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VOLUME XLII.—1880.

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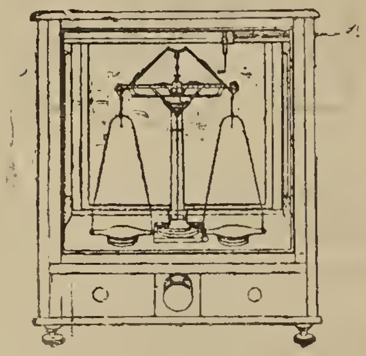

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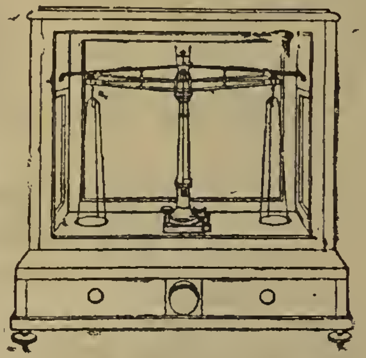

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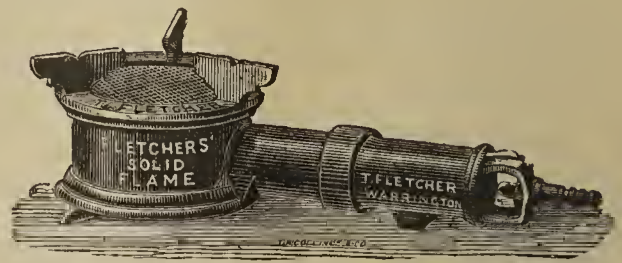
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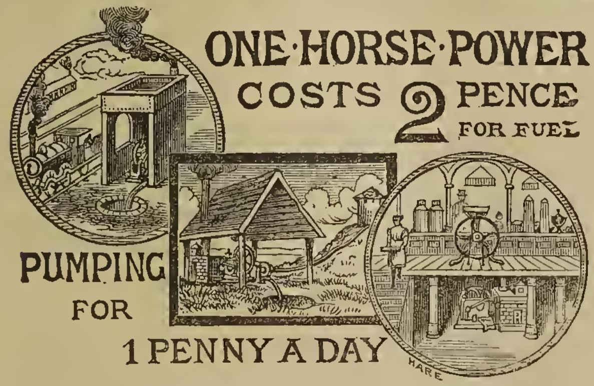

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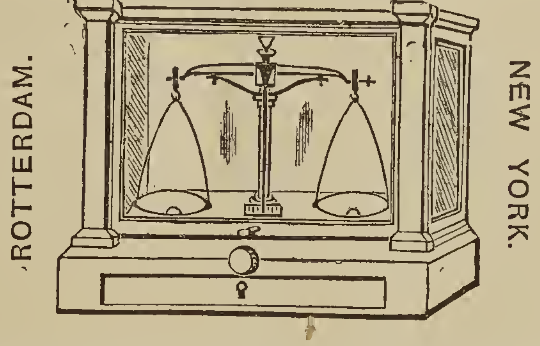

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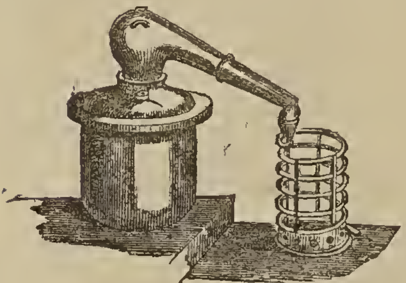

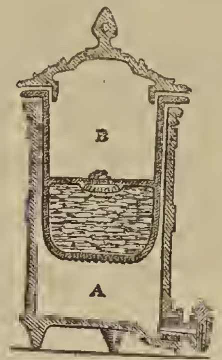

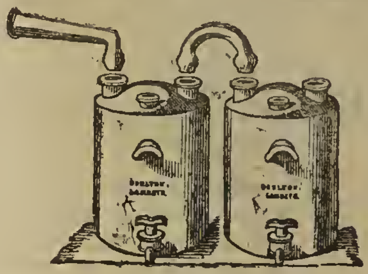

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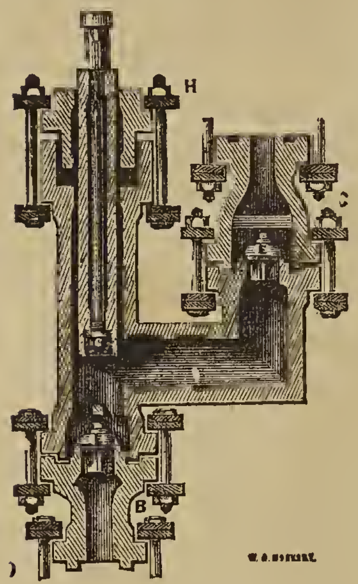

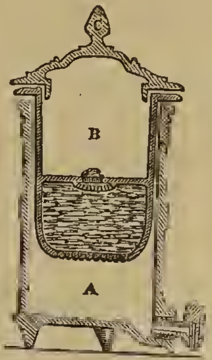

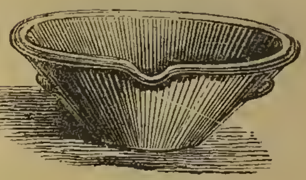

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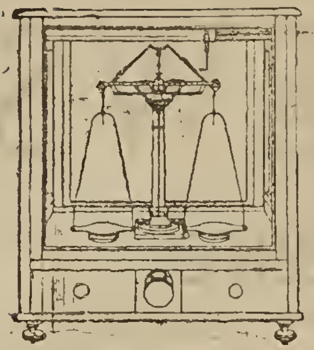

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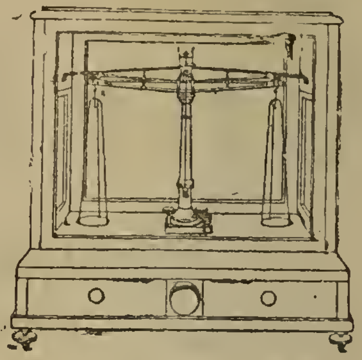

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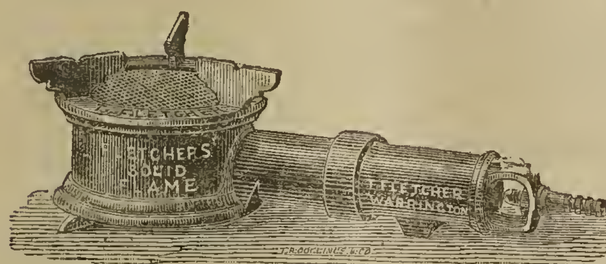

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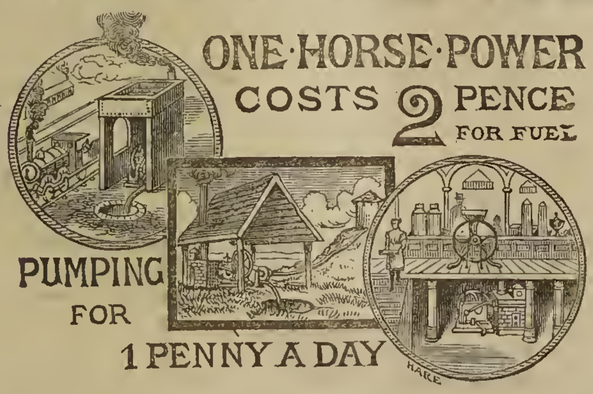

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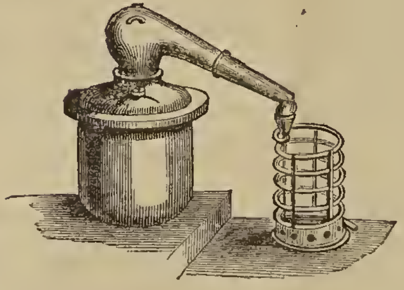

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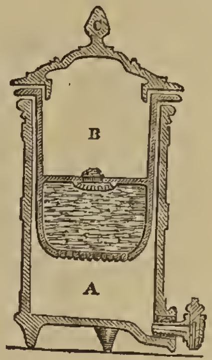

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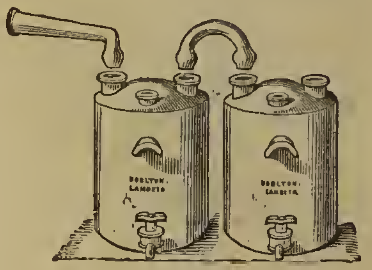

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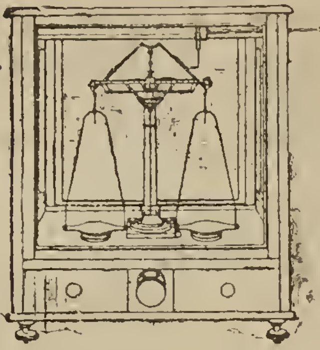

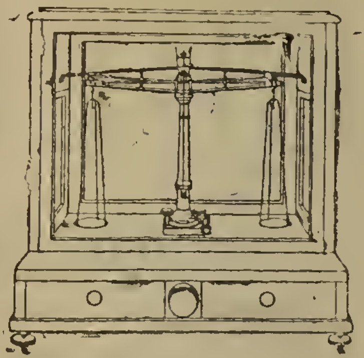

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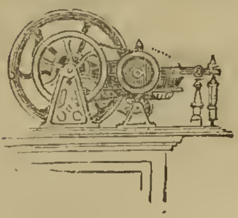

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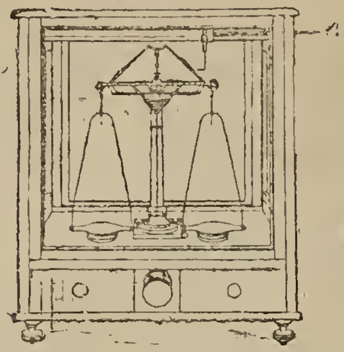

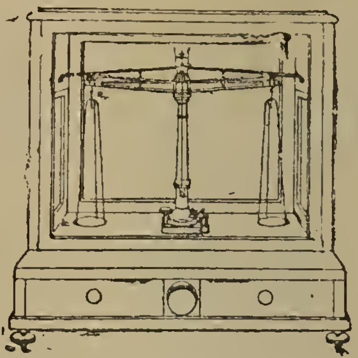

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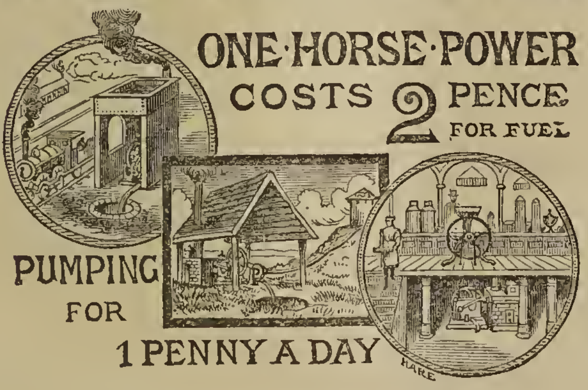

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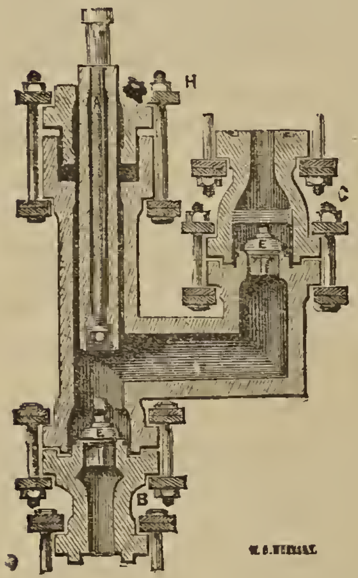

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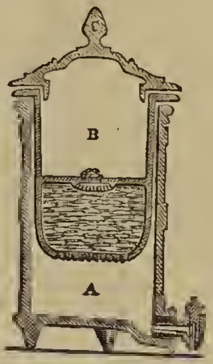

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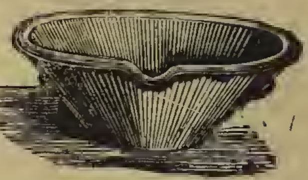

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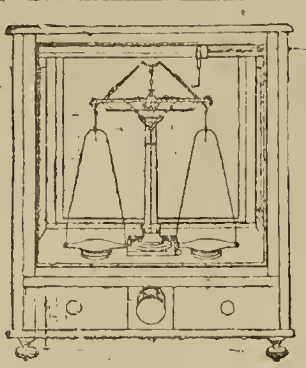

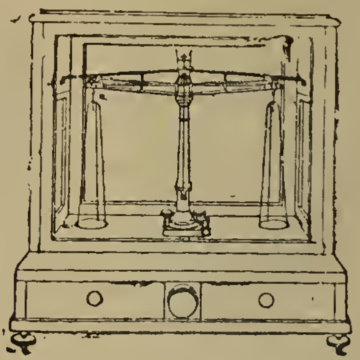

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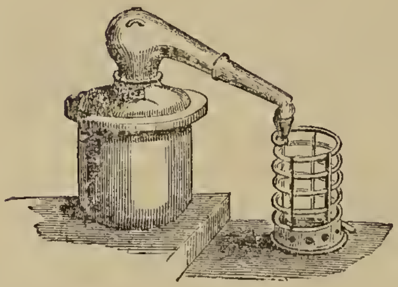

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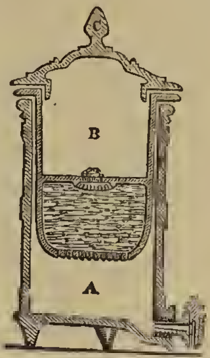

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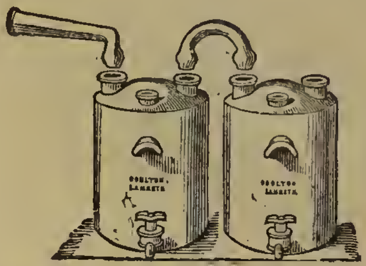

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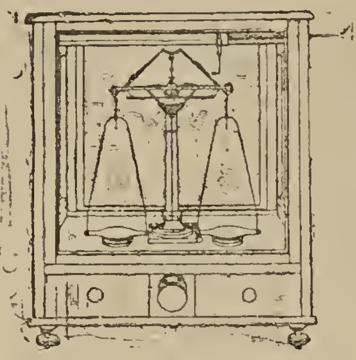

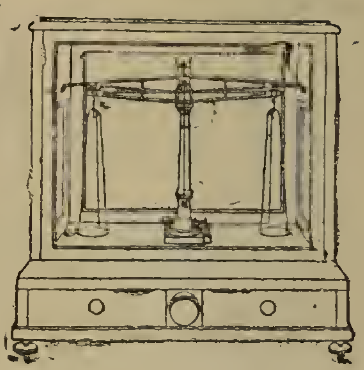

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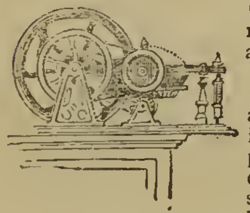

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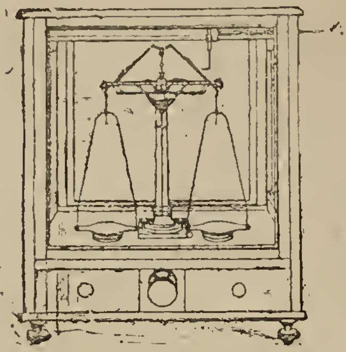

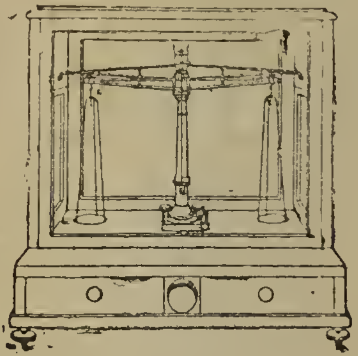

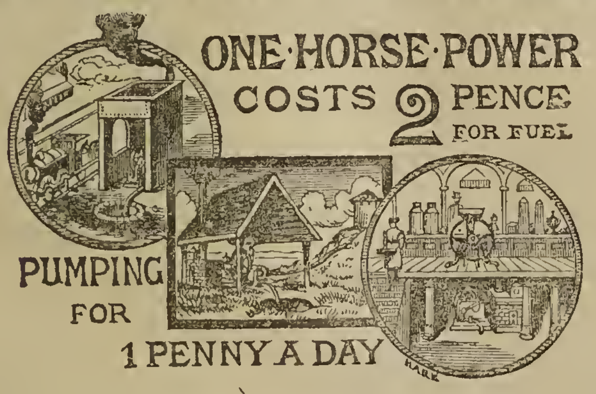
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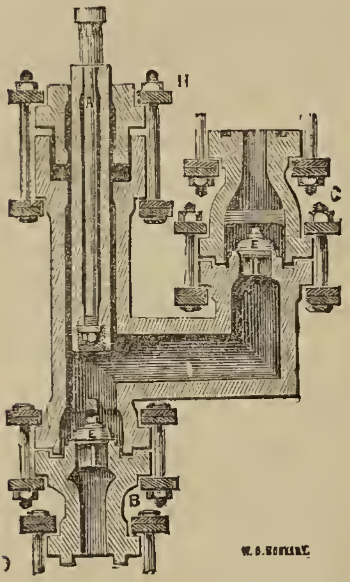

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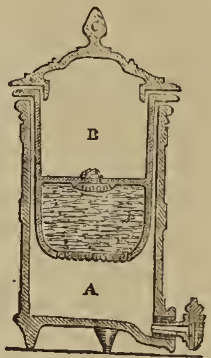

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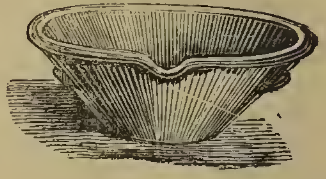

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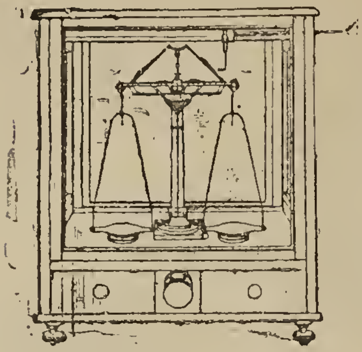

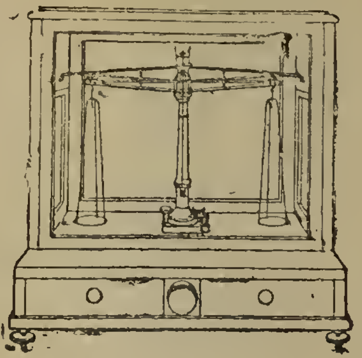

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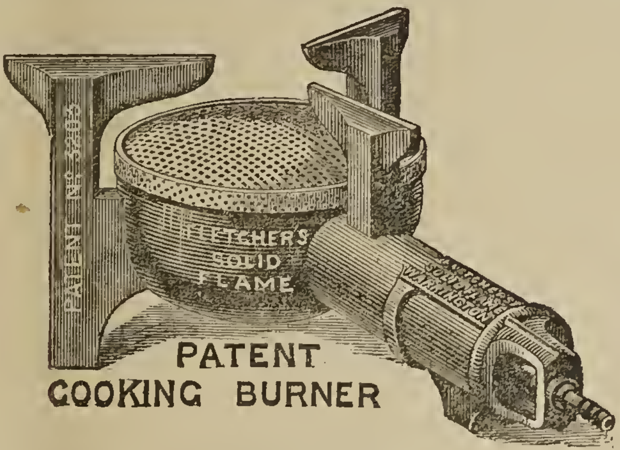

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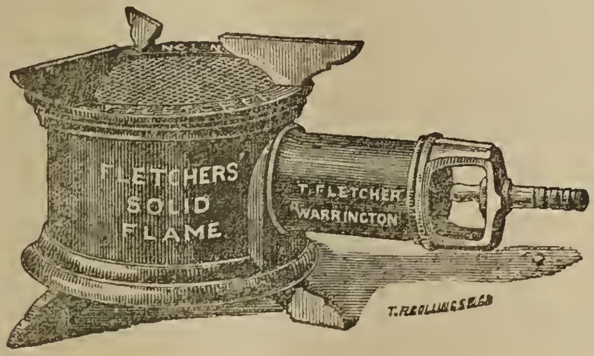

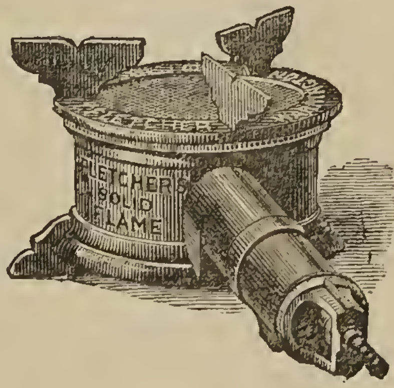

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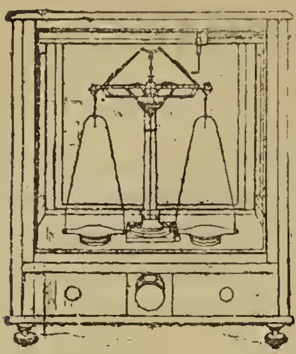

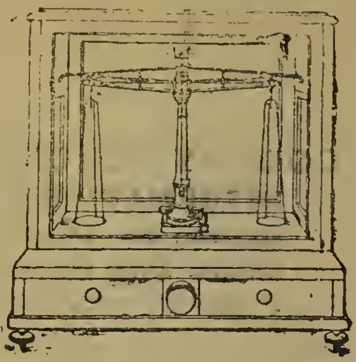

KING'S COLLEGE, London.—MEDICAL DEPARTMENT.

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The College Session, 1880-1, will begin in the Medical Department on Friday, the 1st October; in the Arts, Science, and Law Department, on Tuesday, the 5th October; and in the Evening Classes Department, on Monday, the 11th October. Candidates for admission must not be under 14 years of age, and in the Arts and Science Department those under 16 will be required to pass a preliminary examination in English, Arithmetic, and Elementary Latin. Prospectuses are published for each of these Departments, and may be obtained on application to the Registrar. Fuller details respecting the Courses of Instruction, Fees, Scholarships, Exhibitions, &c., will be found in the Calendar, which may be obtained early in September (price 3s., post free 3s. 6d.), from Mr. Cornish, 33, Piccadilly, and other booksellers in Manchester, and from Messrs. Macmillan and Co., London. The Syllabus of the Evening Classes may now be obtained from Mr. Cornish (price 6d., post free 7d.).

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Other Lecturers—Mr. B. T. Lowne; Dr. Thorowgood; Mr. Foster; Mr. Henry Case.

Two Entrance Scholarships, value £25 and £20 per annum respectively, will be competed for on October 1st and 2nd.

For further information and for a Prospectus, apply to the Dean or the Resident Medical Officer at the Hospital.

THE LONDON HOSPITAL and MEDICAL COLLEGE, MILE END, E.—

The SESSION 1880-81 will commence on FRIDAY, OCTOBER 1st, 1880. Four entrance Scholarships, value £60, £40, £30, and £20, will be offered for competition at the end of September to new Students. Entries on or before September 20. Fees for Lectures and Hospital Practice, 90 guineas in one payment or 100 guineas in three instalments. All resident and other hospital appointments are free. The resident appointments consist of Five House Physiciancies, Four House Surgeoncies, and One Accoucheurship; Two Dressers and Two Maternity Pupils also reside in the Hospital. Special entries may be made for Medical and Surgical Practice. The London Hospital is now in direct communication by rail and tram with all parts of the Metropolis.

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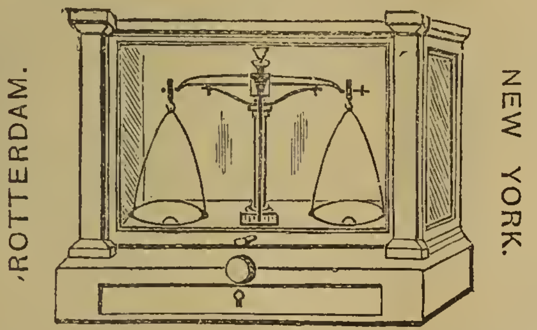

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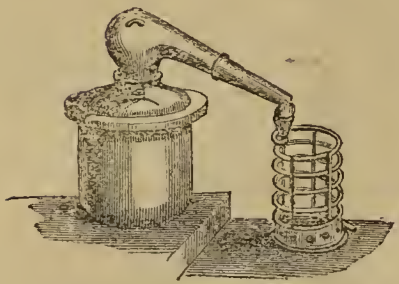

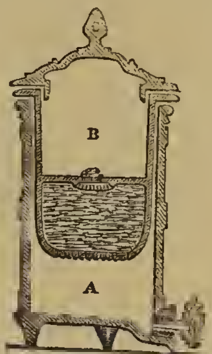

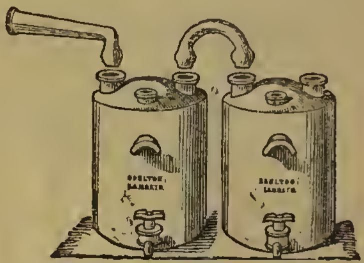

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JOURNAL OF PHYSICAL SCIENCE

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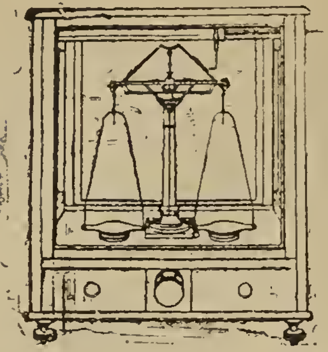

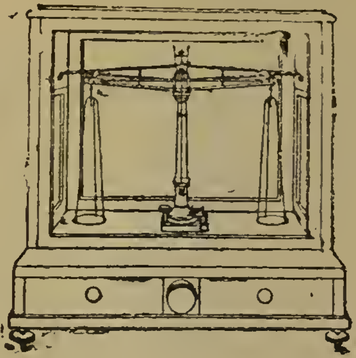

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Anatomy, Descriptive and Practical	MORRISON WATSON, M.D., F.R.S.E.
Comparative Anatomy ..	A. MILNES MARSHALL, M.A., D.Sc.
Chemistry	HENRY E. ROSCOE, LL.D., F.R.S.
Organic Chemistry	C. SCHORLEMMER, F.R.S.
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Surgery..	EDWARD LUND, F.R.C.S.
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Hospital Instruction.. ..	The PHYSICIANS to the ROYAL INFIRMARY. The SURGEONS to the ROYAL INFIRMARY.

SUMMER SESSION.

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Practical Morbid Histology	JULIUS DRESCHFELD, M.D.
Ophthalmology	DAVID LITTLE, M.D.
Practical Chemistry.. ..	HENRY E. ROSCOE, LL.D., F.R.S.
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Demonstrator and Assistant Lecturer in Biology ..	MARCUS M. HARTOG, M.A., B.Sc.

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The College Session, 1880-1, will begin in the Medical Department on Friday, the 1st October; in the Arts, Science, and Law Department, on Tuesday, the 5th October; and in the Evening Classes Department, on Monday, the 11th October. Candidates for admission must not be under 14 years of age, and in the Arts and Science Department those under 16 will be required to pass a preliminary examination in English, Arithmetic, and Elementary Latin. Prospectuses are published for each of these Departments, and may be obtained on application to the Registrar. Fuller details respecting the Courses of Instruction, Fees, Scholarships, Exhibitions, &c., will be found in the Calendar, which may be obtained early in September (price 3s., post free 3s. 6d.), from Mr. Cornish, 33, Piccadilly, and other booksellers in Manchester, and from Messrs. Macmillan and Co., London. The Syllabus of the Evening Classes may now be obtained from Mr. Cornish (price 6d., post free 7d.).

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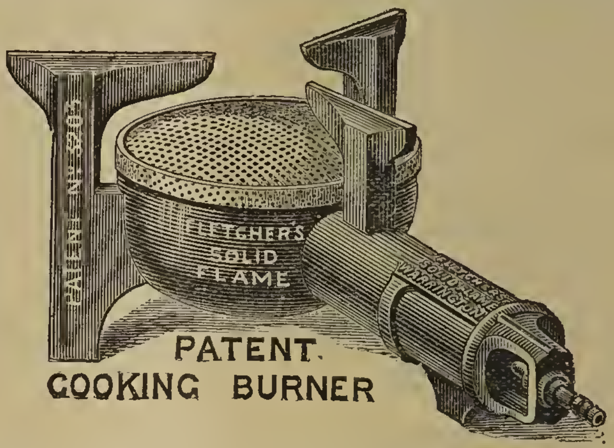

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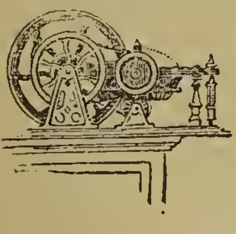

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Edited by Wm. Crookes, F.R.S.

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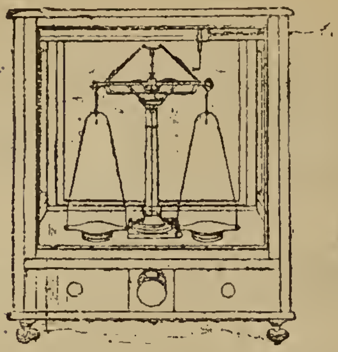

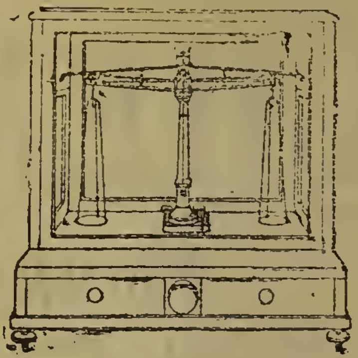

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PROFESSOR T. H. HUXLEY, LL.D., F.R.S., will commence a Course of Eighty Lectures on Biology, or Natural History (including Palæontology), on Monday next, October 4, at 10 o'clock a.m., to be continued on every week-day, except Saturday, at the same hour. Fee for the Course, £4; for Laboratory Practice, £6.

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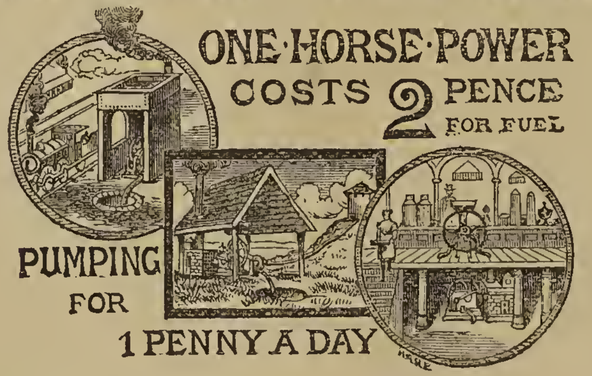
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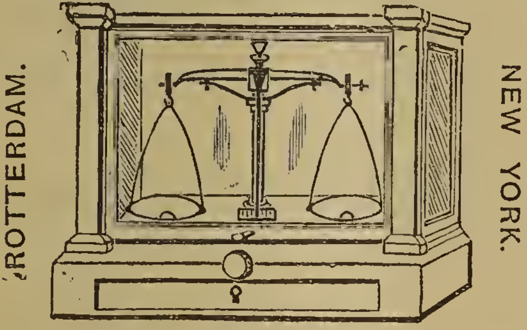

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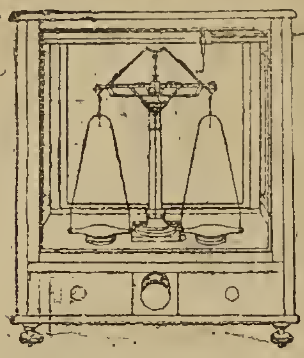

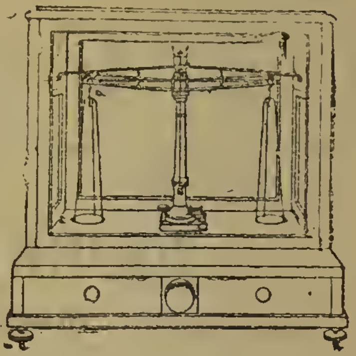

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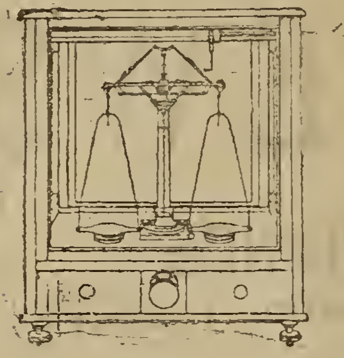

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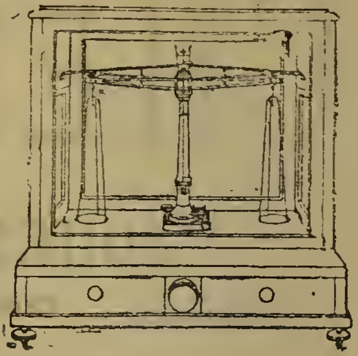

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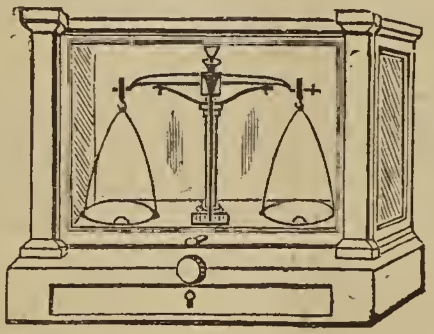

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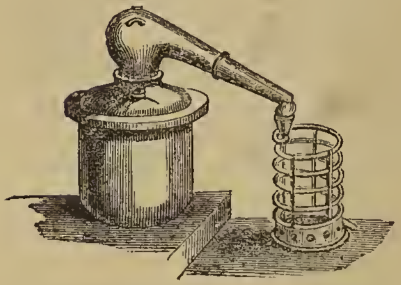

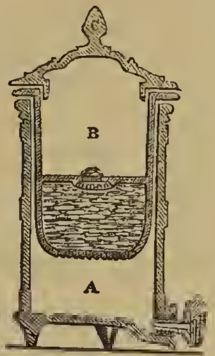

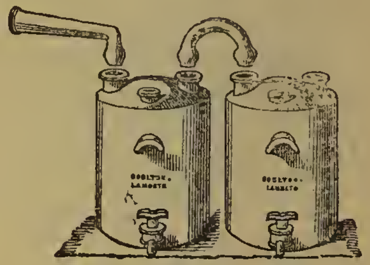

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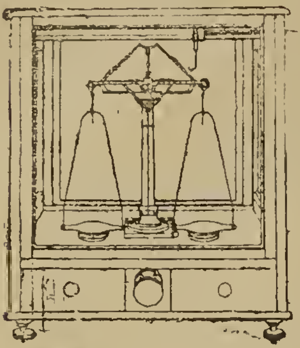

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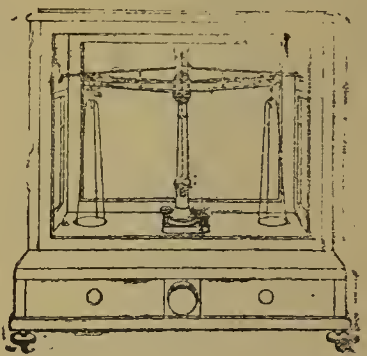

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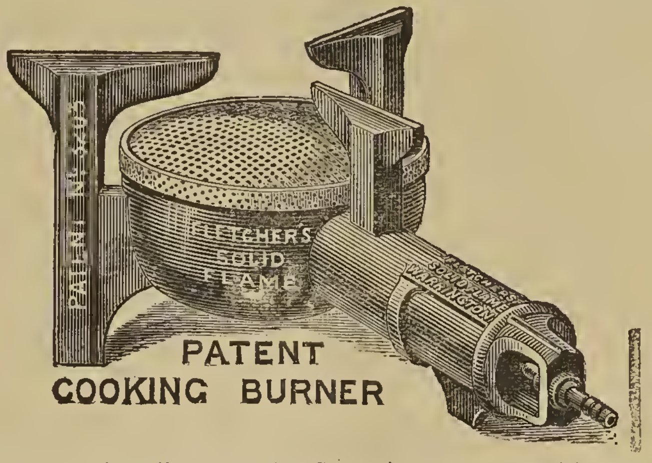

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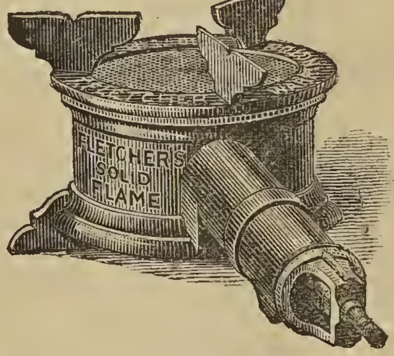

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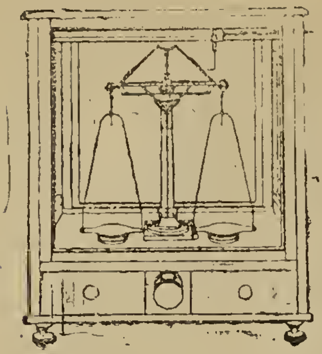

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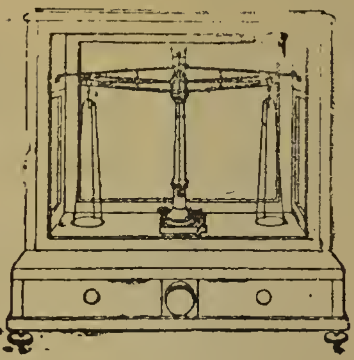

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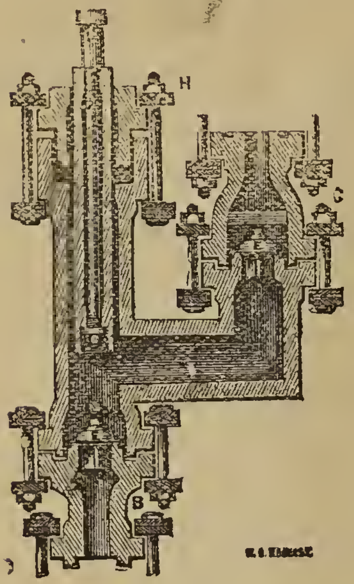

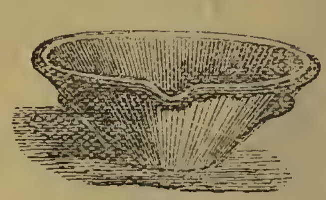

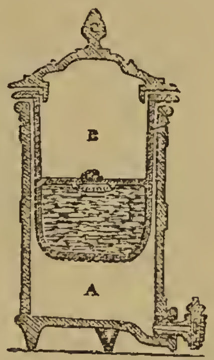

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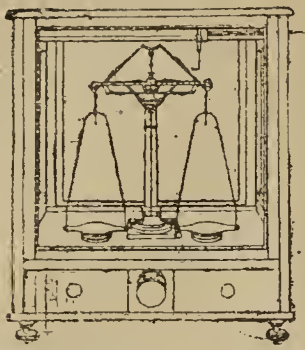

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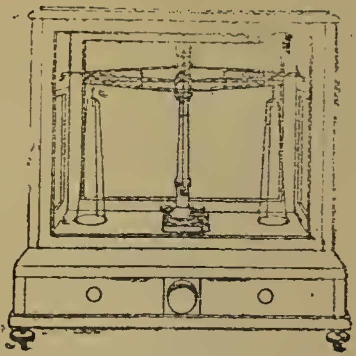

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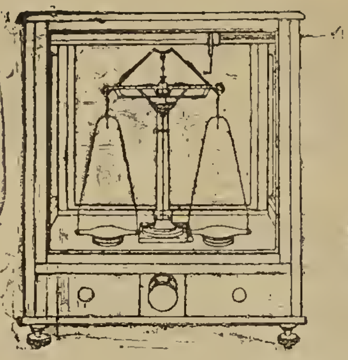

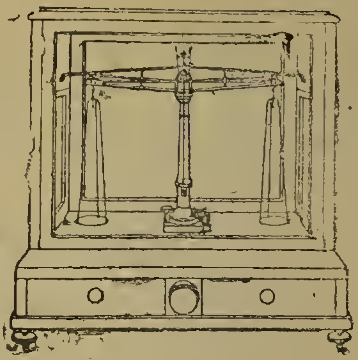

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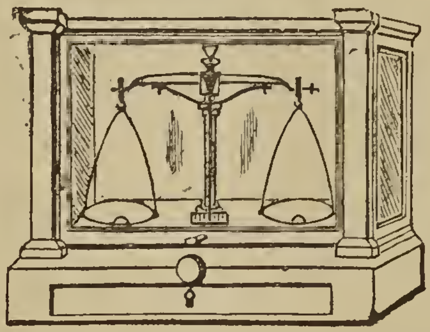

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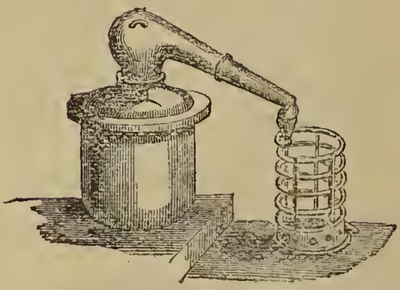

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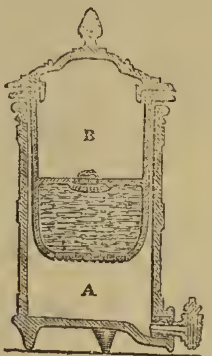

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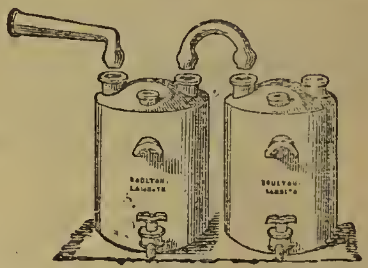

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Medicinal do. Cryst. at 35° C.
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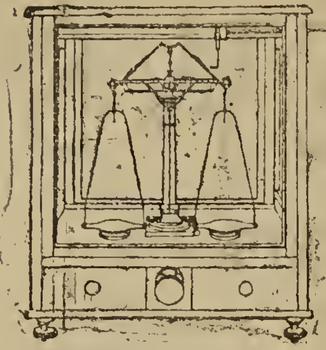

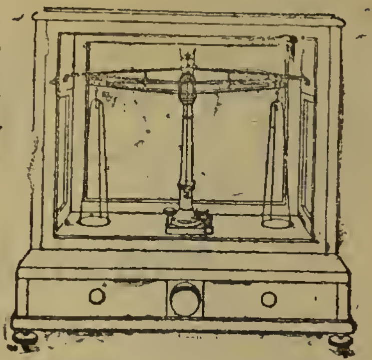

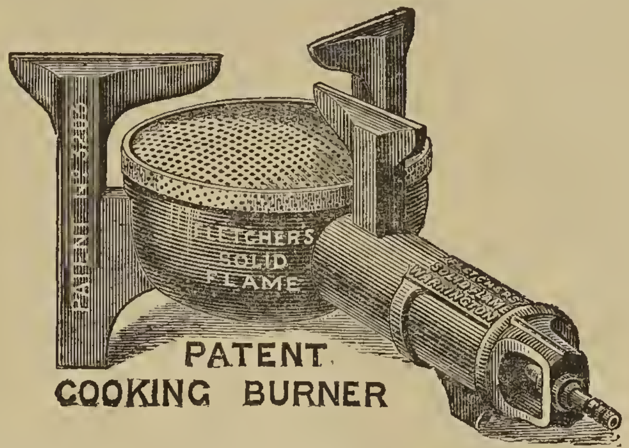
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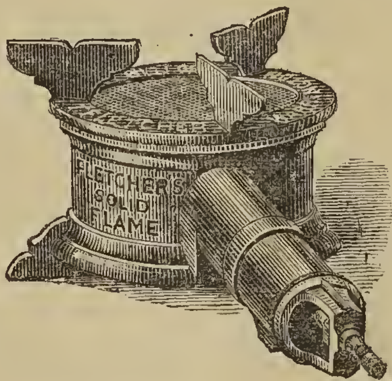
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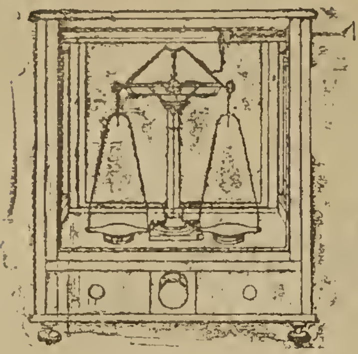

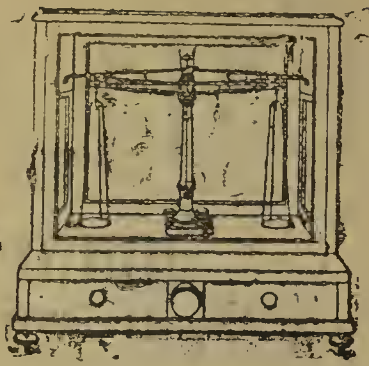

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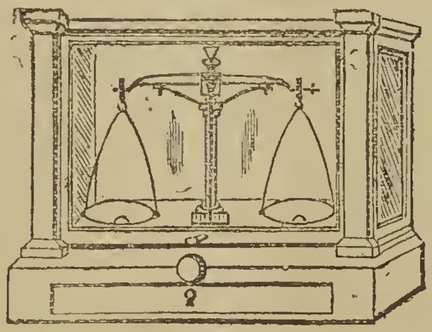

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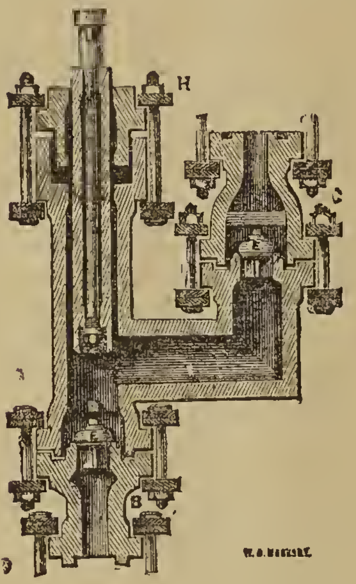

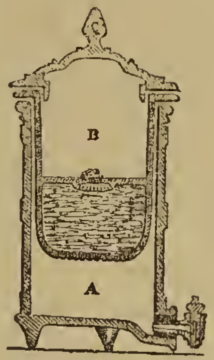

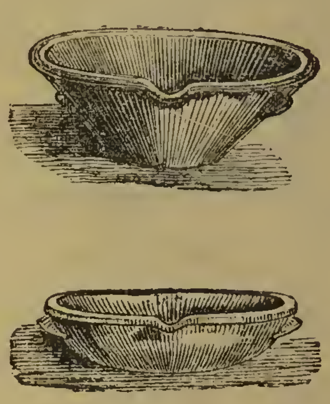

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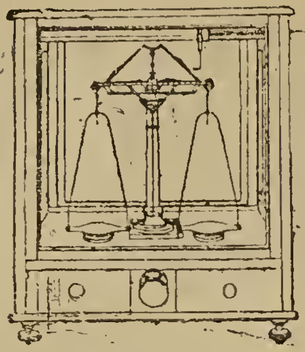

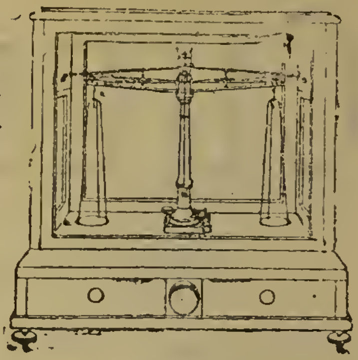

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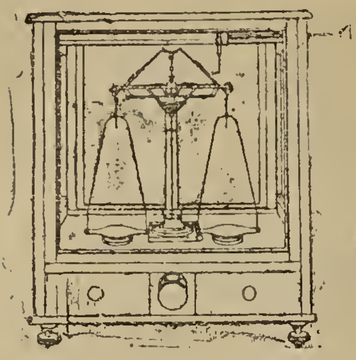

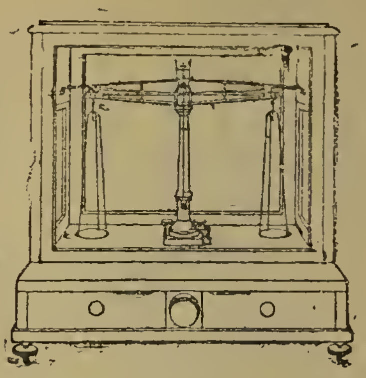

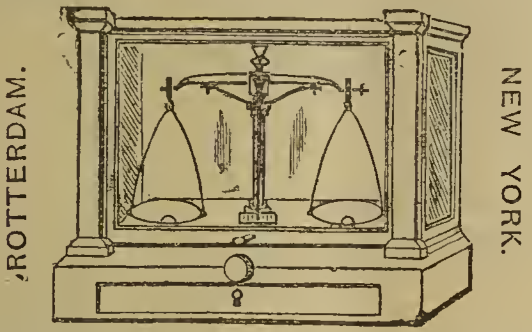
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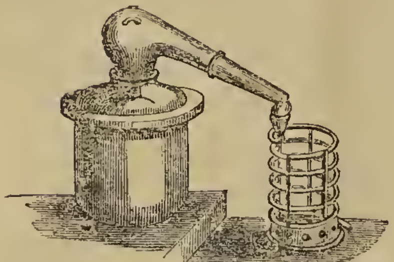

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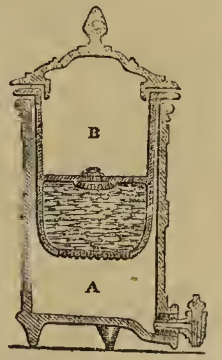

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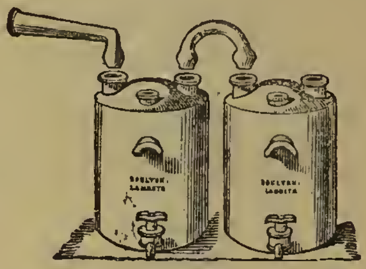

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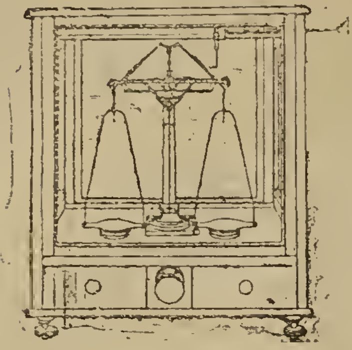

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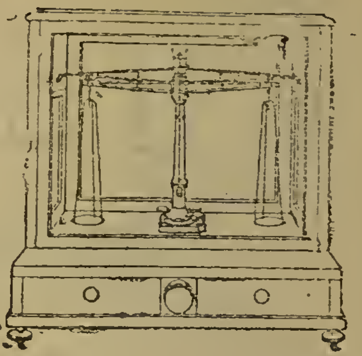

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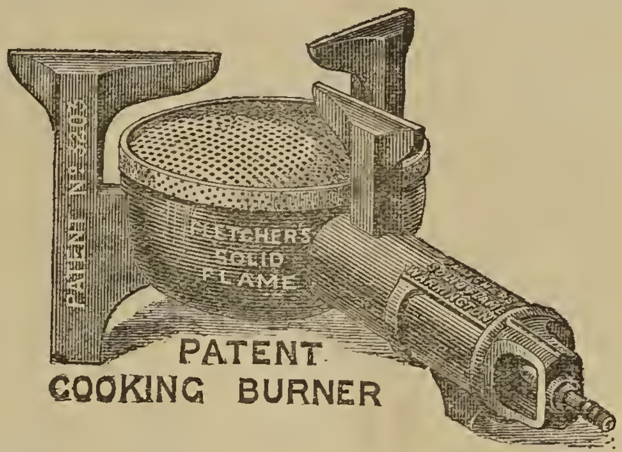

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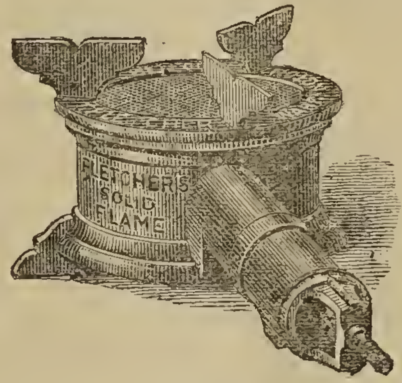

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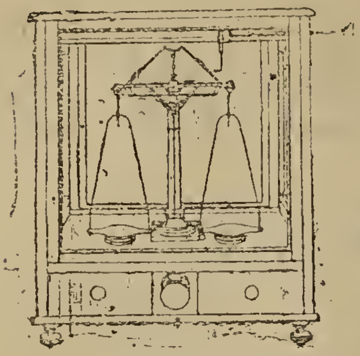

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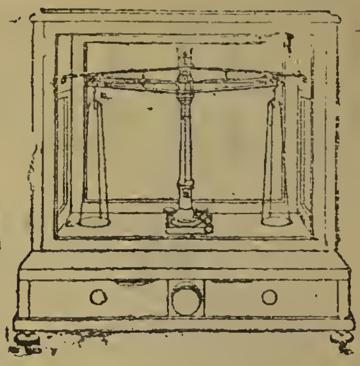

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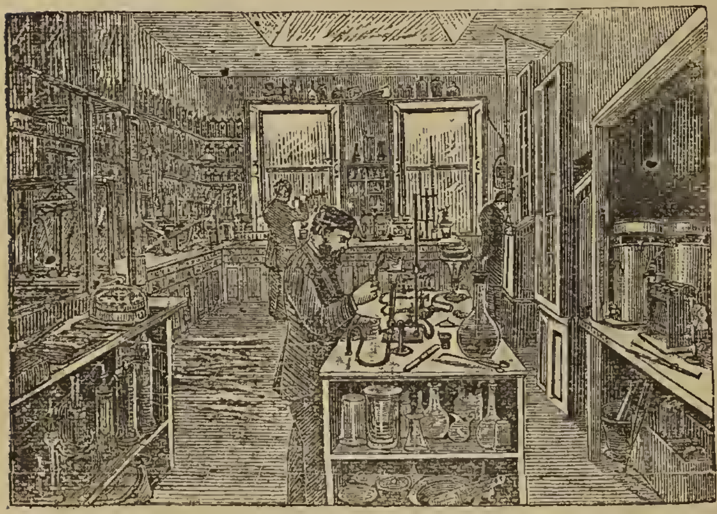

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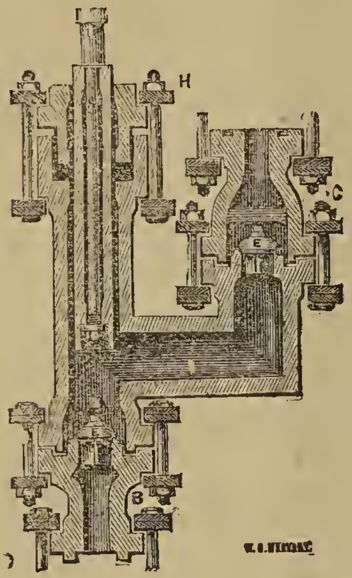

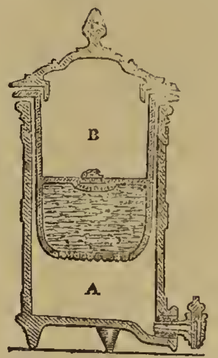

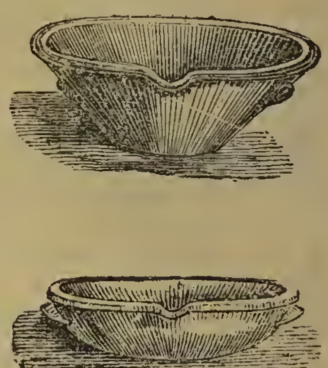

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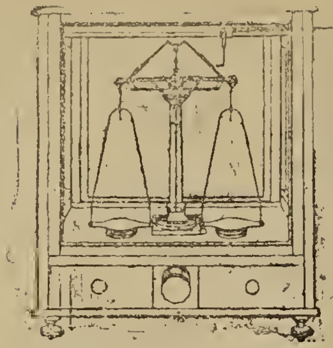

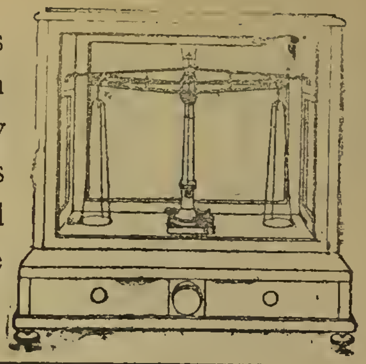

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ON THE LOWERING OF THE FREEZING-POINT OF WATER BY PRESSURE.*

By JAMES DEWAR, M.A., F.R.S.,
Jacksonian Professor of Natural Experimental Philosophy in the University of Cambridge.

THE Cailletet pump may be conveniently employed to observe the thermal effects of compression on solid and fluid substances. Before engaging in an investigation on this subject, it was necessary to test the apparatus, and especially the manometer. For this purpose it seemed, on theoretical grounds, that observations on the lowering of the freezing-point of water by pressure would be a severe test of the accuracy of the pressure gauge, and the constancy of the records of thermo-junctions under pressure. I am not aware of any quantitative experiments on this subject having been made under high pressures. Sir William Thomson carried the proof of the accuracy of Professor James Thomson's great theoretical discovery to a pressure of 17 atmospheres.† The experiments of Mousson (*Pogg. Annalen*, 1858) were not of a quantitative character, being merely intended to show that ice at a temperature of −18° C. might still be liquefied by the application of an enormous pressure. The following experiments appear to show that a convenient manometer for very high pressures, based on the observations of the freezing-point, may be easily constructed.

In all the following experiments the galvanometer, moving to the negative side, showed a cooling effect of the junction inside the bottle. One division on the arbitrary scale represented $\frac{1}{112}$ of a degree C. Two thermo-junctions were used, made of iron-copper wires, which were insulated by a covering of marine glue, the junctions themselves being covered with a thin layer of gutta-percha dissolved in benzol.

One junction was placed in the bottle, to show the effect of pressure under different circumstances, the other being kept outside at the constant temperature of melting ice.

Series I.

One junction, which was fixed in a brass flange with marine glue, frozen in a test-tube containing boiled water, was placed in the iron bottle and surrounded with water at 0° C., the bottle itself being packed round with ice. The pressure was increased by 25 atmospheres at a time. The galvanometer showed a deflection immediately the pressure was applied, rapidly coming to rest at the new position, and it remained perfectly steady as long as the pressure was maintained.

The following is one of many series of experiments, which agreed very nearly. Pressures were recorded by a metallic manometer which had been checked on several occasions. The records of the first 25 atmospheres pressure are not reliable.

Galvanometer zero + 1.

					Increase.
25	atmospheres	= − 26	position	on scale	
50	,,	= − 43	,,	,,	17
75	,,	= − 62	,,	,,	19
100	,,	= − 82	,,	,,	20
125	,,	= − 100	,,	,,	18
150	,,	= − 120	,,	,,	20
175	,,	= − 142	,,	,,	22
200	,,	= − 163	,,	,,	21
225	,,	= − 185	,,	,,	22
250	,,	= − 203	,,	,,	18
275	,,	= − 223	,,	,,	20
300	,,	= − 243	,,	,,	20

This represents a mean deflection of 19·7 on the scale, being 0·18 of a degree C. for 25 atmospheres, or a lowering of the freezing-point by 2·1° C. for the total range of pressure. The mean reduction for one atmosphere pressure is 0·0072°.

These experiments were afterwards repeated, with a stronger pump, to 700 atmospheres. Exactly the same mean deflection was observed, and the galvanometer moved with the same regularity.

Series II.

The junction in the flange was placed in a piece of quill tubing, open at top and bottom, in order to insulate it from the sides of the iron bottle, and surrounded with water at 0° C. instead of ice, the iron bottle being afterwards packed, as before, in ice, and the other junction being outside in ice and water.

This showed a very slight decrease of temperature on applying pressure, the whole deflection for 200 atmospheres being only about 4 divisions, or $\frac{1}{28}$ of a degree C. This agrees with Joule's experiments on the compression of water.

Series III.

A mixture of finely powdered ice and water was placed round the junction inside the bottle, which was as usual,

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* A Paper read before the Royal Society June 17, 1880.

† "The Effect of Pressure in Lowering the Freezing-Point of Water experimentally demonstrated." *Phil. Mag.*, 1850.

packed afterwards in ice. It will be seen, from the following set of readings, that the deflection was about the same as when the junction was frozed into a block of ice, the only difference being that whereas the galvanometer in the first series assumed its new position immediately the pressure was applied, the instrument now required from two to three minutes before coming finally to rest, the action being far more sluggish.

Zero = +6.

Pressure.	Position on Scale.	Difference.
25	− 17	
50	− 36	19
75	− 55	19
100	− 74	19
125	− 91	17
150	−110	19
175	−130	20
200	−149	19

Mean deflection for 25 atmospheres is 19·4 divisions of the scale.

Series IV.

Brine was placed round the junction in the bottle, the latter being afterwards packed in ice and salt, to reduce the temperature of the brine to −20° C., the other junction being placed in ice and salt on the outside. The junctions were then connected up with the galvanometer, and allowed to remain untill the temperatures became equalised:—

Zero +85.

Atmospheres.	Position on Scale.	Increase.
25	+ 98	
50	+106	8
75	+115	9
100	+121	6
125	+128	7
150	+134	6
175	+138	4
200	+142	4

In this case a heating effect was produced, which seemed, however, to decrease as the pressure was increased. It represents an increase of $\frac{1}{25}$ of a degree C. for the whole 200 atmospheres.

Series V.

The junction in the flange was frozen into a solid block of ice, and placed in a bottle, surrounded with brine at −20° C., the bottle being packed in ice and salt. A very slight heating effect was produced, which only amounted to about 1½ divisions for 200 atmospheres.

Series VI.

These experiments were made with the junction frozen into a test-tube with water, and placed in the bottle with mercury surrounding it instead of water. This gave exactly the same deflection of the galvanometer as when water was employed, only the experiment could not be carried on for a long time, as the ice melted with considerable rapidity, from the heating of the mercury by compression.

Series VII.

As in several cases the junction that was placed inside the bottle, and which had been several times subjected to high pressures, appeared to be affected by the compression and worked somewhat irregularly, it was thought advisable to subject both the junctions to the same compression, so as to have them both under the same conditions. For this purpose both junctions were passed through the brass flange, being well insulated by marine glue, the one remaining on the outside of the test-tube in which the other was frozen.

The results with ice were exactly the same as when one junction was placed outside the bottle, but pressure still appeared to have an effect upon the junctions, as after one or two series of compression experiments they could be no longer relied upon, and worked very irregularly. The only way to obviate this difficulty was, therefore, to prevent the junctions being submitted to pressure at all, and this was effected by soldering a stout iron tube, about ¼ inch internal diameter, into the brass flange, which reached to about the centre of the bottle. The tube was closed at the bottom and contained a few c.c. of alcohol, into which the junction was lowered from the outside. The part of the tube which went into the bottle was then frozen into the test-tube with water, and thus sustained all the pressure while the alteration of temperature was conveyed through the iron to the junction.

This worked exceedingly well, giving exactly the same results as before, the only drawback being that the action was very sluggish, the galvanometer taking about two minutes to come to rest after each addition of pressure. The above results seem to prove that the calculated value of the variation of the freezing-point of water, deduced from the observed difference of volume of ice and water, and the latent heat of fluidity at the melting-point under one atmosphere of pressure, is identical with the mean experimental value obtained from a series of observations extending to 700 atmospheres. From Clapeyron's formula we are thus entitled to infer that $\frac{TV}{L}$ = constant, where T is absolute temperature, V is difference of volume, and L is latent heat of fluidity. If V is assumed to be approximately constant, then T and L thus is the latent heat of ice, diminished as the freezing-point is lowered by pressure. This is in accordance with the deductions of Clausius* from other considerations.

THE ALUMINIUM-IODINE REACTION.†

By J. H. GLADSTONE, Ph.D., F.R.S., and
ALFRED TRIBE, F.C.S.,
Lecturer on Chemistry in Dulwich College.

About four years ago we pointed out a reaction of iodide of aluminium which, as far as we are aware, has no precise analogue in the science of chemistry, and which has led to the discovery of several volatile aluminium alcohols. It is well known that neither water, alcohol, nor ether is decomposed by metallic aluminium, and that each of these bodies will dissolve iodine without entering further into combination with it; but we found that by the joint action of these two elements, it was possible to split up the above-mentioned liquids. Water, though forming a definite hydrate with the iodide of aluminium, is decomposed by it in the presence of an excess of the metal, hydrogen being evolved and aluminic hydrate formed. This takes place at the ordinary temperature.

Alcohol in a similar manner is decomposed by the joint action of metallic aluminium and its iodide, with the ultimate production of hydrogen gas, aluminic ethylate, and varying amounts of aluminic iodoethylate proportional to the quantity of aluminic iodide employed. A small quantity of the iodide suffices to bring about the formation of a very large amount of the ethylate; in fact, the process would be a continuous one, were it not that the solid products gradually put a stop to the reaction.

Ether is not affected by aluminic iodide and aluminium, but when exposed to iodine and aluminium simultaneously it suffers a violent decomposition, iodide of ethyl and the aluminic iodoethylate being the ultimate products.

Amylic ether behaves in a similar manner, and the acetates of ethyl and amyl give analogous results; but the reaction is not an ordinary double decomposition, for it

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* *Phil. Mag.*, 1851.

† A Paper read before the Royal Society, June 17, 1880.

does not take place when ready formed iodide of aluminium is employed.

The several changes thus described are typical of our "aluminium-iodine reaction." The reaction is quite distinct from that which has lately given beautiful results in the hands of Friedel; and we have investigated its applicability both for the production of new compounds, and for distinguishing between different classes of organic bodies, as well as for throwing light on their comparative constitution.

Neither zinc nor iron, nor, as far as we know, any other metal can be substituted for aluminium in this reaction; but the chloride or bromide may be used instead of the iodide, though with less advantage.

Recent Results.

For decomposing an alcohol a small quantity of iodine is dissolved in it, the necessary excess of aluminium is added, and the mixture is heated. Evolution of hydrogen gas begins immediately, and proceeds somewhat rapidly until the whole of the metal has passed into combination. In this way the aluminium derivaties of the following alcohols have been prepared:—Ethylic, normal propylic, iso-butylic, amylic, benzylic, phenylic, cresylic, and thymolic. The first four of these aluminium derivatives may be distilled *in vacuo*, and they have thus been separated from the other solid products of the reaction, and obtained in a pure condition. The other aluminic alcohols cannot be distilled, at any rate not without very considerable decomposition. The following alcohols, however, behave in a different way with the reagent:—

Methyl alcohol is not decomposed by aluminic iodide and aluminium, but in presence of free iodine it parts slowly with hydrogen, and the same happens when an aluminium-platinum couple is substituted for the metallic aluminium.

Iso-propyl alcohol.—This is not acted upon in the least by the reagents.

Cetyl alcohol.—On heating this compound with the reagent, hydrogen is slowly set free, untill the temperature reaches about 200° C., when another chemical change is set up, resulting in the formation of cetyl iodide and aluminic hydrate.

Allylic alcohol.—The first action is identical in character with that which takes place with the alcohols of the $C_nH_{2n+1}OH$ series. Instead, however, of the whole of the liberated hydrogen escaping, about 30 per cent of it acts upon the excess of alcohol, splitting it up into propylen and water.

Ethene alcohol.—The reagent does not liberate hydrogen from this substance, and has but a very slight action upon it.

Propenyl alcohol.—No hydrogen is evolved, but a double decomposition ensues at about 140° C., yielding allyl iodide, free iodine, and aluminium hydrate; if aluminium be in excess, aluminic hydrate and allylic iodide are the sole products.

Aldehyd.—Hydrogen is not set free from this compound.

The general result, then, of these observations is that the reagent substitutes aluminium for the basic hydrogen of water, and of all the alcohols hitherto tried, whether of the methyl, allyl, benzyl, or phenyl series, with remarkable exception of iso-propyl alcohol. On the other hand, it does not substitute aluminium for hydrogen in the dihydric or trihydric alcohols, nor yet with aldehyd. The reaction with the ethers and glycerin is of a different character, as, in addition to an aluminium compound, the iodides of the positive radicals are formed.

General Properties of the Aluminium Alcohols.

The aluminium alcohols are solid at the ordinary temperature, and fuse generally into clear liquids. They possess, in a marked degree, the property of remaining fluid far below their melting-points. Those of the methyl series distil unchanged at reduced pressures, affording the first organic compounds which contain both oxygen and a metal and are capable of distillation. They are soluble more or less in ether, alcohol, and benzol, but are decomposed by water with the formation of aluminium hydrate and the alcohols. They were found to have the following specific gravities at 4° C.:—

Ethylate	1·147
Propylate	1·026
Butylate	0·982
Amylate	0·980
Phenylate	1·25
Cresylate	1·166
Thymolate	1·04

Action of Heat.—All these alcohols are decomposed at a temperature somewhere about their boiling-points, and it became an interesting subject of inquiry whether they were resolved into alumina, and the alcohol and its olefine, or into alumina and the ether. Both these actions seem to take place. Thus aluminic ethylate appears to be decomposed by heat in both ways, but mainly according to the following scheme:—

$$(C_2H_5O)_6Al_2=Al_2O_3=3C_2H_4+3C_2H_6O.$$

Aluminic phenylate, on the other hand, is capable of decomposition mainly in the following way:—

$$(C_6H_5O)_6Al_2=Al_2O+3(C_6H_5)_2O.$$

It must not, however, be supposed that these decompositions take place without other reactions. Some of the bodies thus produced are new ones, and we are at present engaged in their investigation.

The most interesting decomposition is that of the thymolate. When heated it splits up into alumina, propylene, and bodies of the cresylic group. One of these is a solid body, which, when re-sublimed or crystallised from alcohol, presents itself in most beautiful pearly plates. Its properties resemble rather those of an ether, and concordant analyses of different specimens and determinations of vapour density give the anomalous molecular formula $C_{14}H_{13}O$, but its real constitution is still the subject of inquiry.

Theory of Reaction.

The reaction by which the compounds described above are formed is by no means clear at first sight. It is difficult to imagine that the presence of a haloid salt should determine the direct replacement of hydrogen by aluminium in water or in alcohol. We believe, however, that the presence of hydriodic acid in the reaction with the aromatic alcohols furnishes the true key to the chemical change. The first action is in all probability that of a reciprocal decomposition between two binary compounds, the alcohol and aluminium iodide,—forming a certain proportion of aluminium alcohol and hydriodic acid. But the metallic aluminium present at the same time decomposes the hydracid with evolution of hydrogen and the production of more aluminium iodide, which brings about a further redistribution of the elements and the formation of more of the aluminium alcohol and the hydriodic acid, and so on till the chemical change is complete. The following equations express these changes, which take place alternately, or rather continuously:—

(1.) $6(C_nH_{2n-7}O.H)Al_2I_6=Al_2(C_nH_{2n-7}O)_6+6HI.$

(2.) $6HI+Al_2=Al_2I_6+6H.$

It is evident that, as the iodine does not enter into the final products, a very small quantity of it is sufficient to carry on the continuous chemical change.

There can be little doubt that the same reactions occur in the case of the alcohols of the $C_nH_{2n+1}O.H$ series, but the amount of hydriodic acid formed in the reciprocal decomposition is perhaps very small, and the tenacity with which it is held by the alcohol has made it impossible for us to prove its separate existence. The same remark applies also to water, which we believe to act in the same way. Indeed we know, from the experiments of Roscoe, that there would be no chance of separating minute quantities of acid from an aqueous solution under the circumstances.

The part which the free elements take in the decomposition of the ethers is doubtless analogous to that which the combined elements play in the action with water and alcohol. The reason why the free elements do, and aluminic iodide does not, attack the ethers, is probably owing to the available energy being greater in the former than in the latter case.

In conclusion, we would commend this aluminium-iodine reaction to the consideration of other chemists who may be investigating organic compounds containing oxygen.

ON VIRULENT DISEASES, AND ESPECIALLY ON THE DISEASE COMMONLY CALLED CHICKEN CHOLERA.*

By M. PASTEUR.

Virulent diseases may be ranked among the greatest of the evils that afflict living beings. To prove this, we have merely to name measles, scarlatina, variola, syphilis, glanders, the carbuncular disease, yellow fever, typhus, and the cattle plague. This list is far from being complete; the pathology of the most important diseases may find a place here.

When the ideas of Liebig on the nature of ferments were in vogue, each virus was considered as a substance undergoing an internal change, which could be communicated to living organisms, turning the constituents of these into a virus of the same nature. Liebig was well aware that the first apparition of the ferments, their multiplication and their power of decomposition, present the greatest analogies with the phenomena of life, but, in the introduction to his "Organic Chemistry," he tells us that these analogies may be considered as deceitful illusions.

All the experiments which I have communicated to this Academy for the last twenty-three years have demonstrated, either directly or indirectly, the inaccuracy of the opinions of Liebig. A single method has guided me in the study of microscopic organisms. This method has been essentially the cultivation of these minute beings in a pure state; that is, by eliminating the heterogeneous substances, living or dead, which accompany them. By the use of this method, the most difficult questions are often solved in the easiest and most decisive manner. I will here recall one of the first applications which I made of this method (1857-1858).

Ferments, according to Liebig, are the nitrogenous substances of organisms, such as fibrine, albumen, casein, &c., in a state of decomposition, resulting from contact with air. There was no fermentation known in which these nitrogenous substances were not present and active. One character of fermentations, as well as of diseases, was that they were spontaneous in their origin and development. In order to show that the hypothesis of the learned German chemist was, to use his own words, "but a deceitful illusion," I made up artificial mixtures whose only constituents were as follows:—Water, the mineral constituents essential to life, fermentable substances, and the germs of the ferments which act on these substances. With these mixtures, fermentation took place with a regularity and a purity, if I may use the word, which are never found in the spontaneous fermentation of nature. As every albuminoid substance had been excluded from these mixtures, the ferment appeared as a living being, which borrowed from the fermentable substance all the carbon of its successive generations, and, from the mineral constituents, the nitrogen, phosphorus, potassium, magnesium—elements, the assimilation of which is an indispensable condition to the formation of all living beings, be they great or small.

After these experiments, not only was the theory o Liebig left without any foundation, but the phenomena of fermentation presented themselves as simple phenomena of nutrition, taking place in exceptional conditions, the most extraordinary of which is the possible absence of any contact with air.

Human, as well as veterinary, medicine made use of the light which shone from these new results. Many investigators made experiments to discover if every virus or contagion was not an animated being. Dr. Davaine, in 1863, endeavoured to show the functions of the *bacteridia* of carbuncular disease, which he had discovered in 1850. In 1868, Dr. Chauveau tried to show that virulence was due to the solid particles previously noticed in every virus. Dr. Klebs, in 1872, attributed traumatic virus to microscopic organisms. In 1872, Dr. Kock obtained, by artificial cultivation, the germs of *bacteridia*, which were similar in every respect to those which I had pointed out in *vibrios* (1865-70), and the causes of several other diseases were ascribed to microscopic organisms. To-day those who are most opposed to the theory of germs are wavering. Still the greatest obscurity prevails on the most important points.

In the great majority of virulent diseases, the virus has not as yet been isolated, and still less has it been shown, by artificial cultivation, that it is a living organism, and everything contributes to make us regard these "unknown quantities" of pathology as mysterious morbific causes. The study of the diseases which they cause presents many extraordinary circumstances, among which the most remarkable is their non-recurrence. Human imagination can hardly venture to present a hypothetical explanation having any experimental foundation. Is it not still more surprising to find that vaccine, a virulent but mild disease, is a preventive, not only of vaccine itself, but of a more serious disease—the small pox? These facts were known from the remotest antiquity. Variolisation and vaccination have been practised in India from immemorial times, and when Jenner demonstrated the efficacy of vaccination, the common people of the locality in which he practised medicine knew that cow-pox was a preservative from variola.

Vaccination appears as an isolated fact, but the non-recurrence of virulent diseases appears to be general. The organism does not go twice through measles, scarlatina, typhus, the plague, variola, syphilis, &c.; at least it may be said that the immunity persists for a certain time.

Although in the presence of such mysteries, it behoves us to be humble, I dare to hope that the Academy will find that the facts which I am about to have the honour of presenting before it throw unexpected light on the problems raised by the study of virulent diseases.

There occurs sometimes in poultry yards a fearful disease, commonly called chicken cholera. The victim overtaken by it loses its strength, and stumbles about with drooping wings. Its feathers stand on end, and give it the appearance of a ball; its seem overcome by drowsiness; if we open its eyes it seems to awake from profound sleep, and soon its eyelids close again. Generally death comes after a dumb agony, without the victim even moving from the position it has occupied during the last stages of the disease. In rare cases, it beats its wings for a few seconds. The internal disorders are of the most serious nature. This disease is caused by a microscopic organism which, according to Tundel's Dictionary, was first suspected by M. Moritz, a veterinary surgeon in upper Alsatia; which was drawn more accurately, in 1878, by M. Peroncito, a veterinary surgeon in Turin; and which was found again, in 1879, by M. Toussaint, professor at the Veterinary School of Alfort, who demonstrated, by cultivation in neutralised urine, that this organism was the cause of the virulence in the blood.

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* Translated from the *Comptes Rendus de l'Academie des Sciences*, of February 9, 1880, page 239, by P. Casamajor. From the *Journal of the American Chemical Society*, vol, ii.

In the study of microscopic parasites, the first, the most useful, condition to fulfil, is to obtain a liquid in which the infectious organism may be cultivated with ease, and without any admixture of other organisms of different species. Neutralised urine, which I have used with so much success to show that the product obtained by the cultivation of the *bacteridia* of Davaine, is identical with the virus of carbuncular disease (1877, Pasteur and Jaubert), does not fulfil the double end in view. But a liquid marvellously adapted to the life of the germ of chicken cholera, is a broth made from chicken's muscles, neutralised with potassa, and made sterile by a temperature superior to 100° C. (110° to 115°). The ease with which the microscopic organism multiplies in this liquid seems prodigious. In a few hours, the most limpid broth becomes turbid, and is filled with an infinite multitude of small articulations of extreme tenuity, slightly thinner in the middle, and which at first sight have the appearance of isolated dots. These small articulations have no motion of their own, and they certainly belong to a very different group from that of *vibrios*. I imagine that they will be classified some day with other forms of virus, now unknown, when we cultivate these, as I hope we are on the eve of doing.

The cultivation of this microscopic organism presents some very interesting peculiarities.

In my former researches, one of the liquids which I used with the greatest success, was a decoction of beer-yeast in water, after filtering it to obtain it perfectly limpid, and after rendering it sterile by a temperature superior to 100° C. The most various microscopic organisms thrive on the food presented by this liquid, particularly after being neutralised. For instance, the *bacteridia* of carbuncular disease multiplies surprisingly in a few hours. It is a strange thing that this liquid is entirely unsuited to the life of the organism of chicken cholera, which dies in it in less than forty-eight hours. Is not this entirely analogous to what happens when a microscopic organism is entirely innocuous towards an animal on which it has been inoculated? It remains inoffensive because it does not develop in the body of the animal, and it does not reach the organs essential to life.

The sterility of the decoction of yeast, with respect to the microscopic organism of chicken cholera, affords us an excellent criterion for the purity of the cultivation of this organism in chicken broth. If the cultivation be pure, upon transferring it to a decoction of yeast, no development takes place, and the yeast solution remains limpid. If, however, other organisms are present, they are developed, and the solution becomes turbid. I will, in the next place, call your attention to a still more extraordinary peculiarity of the cultivation of the germ of chicken cholera. The inoculation of this organism on guinea pigs is not so surely fatal as in the case of chickens. In guinea pigs, particularly in the older animals, the only thing that can be observed is a local lesion, at the point of inoculation, which ends in an abscess, of greater or lesser volume. This abscess opens spontaneously and heals, and meanwhile the guinea pig eats his food as usual, and seems to possess all the characteristics of health. These abscesses last sometimes for several weeks before discharging, being surrounded by a membrane full of creamy pus, in which the microscopic organism exists in infinite numbers, side by side with the globules of pus. It is the life of the inoculated organisms which causes the abscess, which is as a closed vessel, from which we may obtain the organism without endangering the life of the animal. The microscopic organism remains, mixed with pus, in a great state of purity, without losing its vitality. This may be proved by inoculating on chickens a small portion of the contents of the abscess. From the effect of these inoculations, the chickens very soon die, while the guinea pig, which has furnished the virus, is entirely cured after a short time. This is an instance of the localised evolution of a microscopic organism, which causes the formation of pus and of a closed abscess, without, at the same time, causing internal disturbances or the death of the animal on which it exists. It is, however, always able to cause the death of other species on which it may be inoculated, and even the death of the animal on which it exists in a closed abscess, if through some fortuitous circumstances, it should pass into the blood or into the viscera. Chickens and rabbits, living in company with guinea pigs, affected with abscesses of this kind, might, all at once, sicken and die, without any great change being observable in the health of the guinea pigs. This could easily happen if the abscesses of the guinea pigs discharged a small portion of their contents on the food of the chickens and rabbits.

An observer who witnessed these facts, and was ignorant of all the points, might well be astonished to see chickens and rabbits die in great numbers, without any apparent cause, and he would be apt to believe in some spontaneous disease. Certainly, he would not suppose that the guinea pigs were the cause of all the trouble, when he saw them all in good health, and particularly if he knew that the guinea pigs themselves often suffer from the same disease. Many of the mysteries in the history of contagions will some day be solved in easier ways than the one I have just mentioned. We may reject theories which are in contradiction with known facts, but we must not reject them solely because some of their applications elude our grasp. The combinations of nature are both simpler and more varied than those of human imagination.

I may easily convince you of the truth of these statements, if I add that, if a few drops from a cultivation of our microscopic organism, be placed on bread or meat given to chickens, they are sufficient to propagate the evil to their intestines, in which the little organism propagates with such remarkable rapidity, that the excrements of chickens so poisoned cause the death of those on whom they are inoculated. These facts enable us to understand the manner in which this fearful disease develops in poultry yards. Evidently, the excrements of the diseased chickens have most to do with the contagion. Nothing would be easier than to prevent the spread of the disease by isolating the chickens for only a few days; by washing the poultry yard with plenty of water, and particularly with water containing a little sulphuric acid, which kills the germ of this disease. The excrements should be carried off to a distance. After a few days, the chickens that are still alive could be brought together again with perfect safety, because this disease is so rapidly fatal that in a short time all the diseased animals would be dead.

If the cultivation of the infectious organism in chicken broth is repeated many times over, by passing from one cultivation to the next by sowing an infinitely small quantity, such as may be gathered on the point of a needle, the virulence of the germ is not weakened by the process. This is analogous to the ease with which it multiplies in the bodies of the *Gallinaceæ*. This virulence is so great, that the inoculation of a minute fraction of a drop will cause death in two or three days, and most generally in less than twenty-four hours.

Having established these preliminaries, I now come to the most important portion of this communication.

By operating certain changes in the process of cultivation, the virulence of the infectious germ may be much lessened. This is the vital part of the subject. I beg the Academy's permission to withhold a description of the processes by means of which I determine this diminution of virulence. My object is to insure independence in my studies.

The diminution of virulence is seen in cultivations by a slower development of the infectious organism, but, in reality, the two varieties of virus are identical. In the first or very infectious state, the inoculated germ may kill twenty times in twenty. In the milder state, it may twenty times in twenty give rise to illness, but not to death. These facts have an importance which is easily understood, as they allow us to form an opinion, in this particular disease, of the problem of its recidivation or non-recidivation. If we take forty chickens, and inoculate twenty of them with the very virulent virus, these twenty will die. If we inoculate the other twenty with attenuated virus, these will all be ill, but they will not die. We let the twenty chickens be entirely cured, and then if we inoculate them with the very infectious virus, they will not die. The conclusion from this is evident. The disease is its own preventive. It has the character of virulent disease, which do not recidivate.

Let us not be astonished at the singularity of this result. All things are not here as new as they appear at first. In one important particular, however, there is a novelty which will be pointed out. Before the time of Jenner, who himself practised this method, as I have already mentioned, there was a practice of inoculating variola to preserve from variola. In our day, sheep are inoculated with murrain to preserve them from murrain, and cattle are inoculated with peripneumonia to preserve from this fearful discease. Chicken cholera shows us an immunity of the same kind. It is an interesting fact, but it does not show any theoretical novelty. There is, however, an important novelty in the preceding observations, a novelty which gives food for reflection on the nature of virus. It consists in this, that we have here a disease whose virulent cause is a microscopic parasite, which may be cultivated outside of the animal economy. The virus of variola, the virus of vaccine, those of glanders, syphilis, the plague, &c., are unknown in their nature.

This new virus is a living organism, and the disease to which it gives rise has one thing in common with virulent diseases, properly so-called, a quality heretofore unknown in virulent diseases, caused by microscopic parasites: it is that it does not recidivate.

The existence of this disease is a connecting link between virulent diseases caused by a living virus, and other diseases, in the virus of which life has never been recognised.

I would not have it believed that the facts present the constancy and mathematical regularity which I have mentioned. To believe this would be to ignore the great variability in the constitution of animals, taken at hap hazard from among domestic animals, and also the variability in the manifestations of life in general. The very virulent virus of chicken cholera does not always kill twenty times in twenty. Sometimes this virus only kills eighteen times in twenty, but generally twenty times in twenty. We may also remark that the virus, when reduced in virulence, does not save life twenty times in twenty. Sometimes this happens only eighteen times in twenty, and even sixteen in twenty. Neither is it an *absolute* preservative by one inoculation. We may more surely prevent recidivation by two than by one inoculation.

If we compare the results above stated with what is known of vaccine and its relations to variola, we may see that the less vigorous organism which does not cause death, is analogous to a vaccine, relatively to the one that kills, for it gives rise to a disease which may be called mild, as it does not cause death, and, at the same time, it preserves from the disease in its most deadly shape. What other condition must this organism fulfil to be a true vaccine like that of cow-pox? This condition is that it should be a definite variety, and that we should not be obliged to prepare it *de novo*, when we wish to use it. We find here the same difficulty which presented itself to Jenner. After he had demonstrated that inoculated cow-pox is a preservative against variola, he thought that it was necessary to start from the cow-pox of a cow. Jenner soon discovered, however, that he could get along without cows, and make vaccine pass from one arm to another. We may try to do the same by causing our germ to pass from one cultivation to another. Under these circumstances, will the germ become actively virulent or will it remain moderately so? Although this may appear very astonishing, I can say that the last supposition is the correct one. The virulence of the germ, in the small number of cultivations which I have attempted, has not increased, and everything seems to point to the existence of a true vaccine. I may even add that one or two trials favour the idea that the attenuated virus keeps its character of mildness after passing through the bodies of Guinea pigs. Will the same thing happen after repeated cultivations and repeated inoculations? Only by experiments can such a question be answered.

At any rate, we now know of a disease caused by a microscopic parasite, which may be obtained in such a condition that it does not recidivate, as other diseases caused by similar parasites. Moreover, we have a variety of its virus, which behaves towards it as vaccine towards variola.

The Academy may allow me a digression worthy of attention. From what has been said, we can easily obtain chickens affected with the disease called *chicken cholera*, in which death is not a necessary consequence of the disease. We may then witness as many cases of cure as we may wish. Now, I do not believe that experimental surgery has ever met with more curious phenomena than those which are present when the animal returns to health, after inoculations have been made in the large pectoral muscles. The germ of the disease multiplies in the substance of the muscle as it would in a vessel. At the same time the muscle swells, hardens, and becomes bleached at the surface and below the surface. It becomes filled with globules of pus, but does not suppurate. Its histological elements are easily torn, because the parasitical germ is scattered through them in numerous pockets, and it feeds on a portion of their substance. I will, later on, exhibit coloured figures showing the disorders caused by the parasitical germ in case of cure. The parasite is gradually arrested in its development and disappears, while, at the same time, the portion of muscle which has been attacked unites, hardens, and lodges itself in a cavity whose surface resembles that of a healthy granulating wound. The portion which has suffered from the disease finally forms a sequestrum, and is so well isolated in the cavity that holds it that it may be felt by the finger under the skin, and, by the least incision, it may be seized with forceps and extracted. The small wound left in the skin heals immediately, and the cavity is gradually filled by the renewed elements of the muscle. I will now place some of these demonstrations before the Academy.

I have now to close by an explanation relating to the non-recidivation of the disease which occupies our attention. Let us take a chicken thoroughly vaccinated by one or more previous inoculations of the enfeebled virus. What will happen if we inoculate the same chicken again? The local lesion will be insignificant, while the first inoculations, and, in particular, the very first, had been the cause of such marked change in the muscle, that a large sequestrum can be easily felt by the touch. The cause of the difference in the effects of these inoculations is to be found entirely in a greater relative facility of the development of the germ of the disease at the first inoculations, and, in the last inoculation, in the development being either entirely wanting or very feeble and promptly stopped. The consequence of this seems evident, and it is that the muscle, which has been seriously diseased, has become, even after it has been cured, unfit for the cultivation of the germ of the disease, as if this germ, by a preceding cultivation, had suppressed some principle which life does not bring back, and whose absence prevents the development of the microscopic organism. I have no doubt that this explanation, to which we are led by palpable facts in this case, will be found to be generally applicable to all virulent diseases.

It must appear superfluous to point out the principal consequences of the facts which I have had the honour to present before this Academy. There are, however, two of these which may be mentioned. One is, that we may hope to obtain artificial cultivations of every virus, and the other is, the idea of obtaining vaccines of the virulent diseases which afflict humanity, and which are the greatest plague of agriculture in the breeding of domestic animals.

It is a duty and a pleasure for me to add that in these delicate and lengthened researches I have been assisted with great zeal and intelligence by Messrs. Chamberland and Roux.

ON THE PREPARATION OF METHYLENE CHLORIDE, AND DIOXYETHYL-METHYLENE.

By W. H. GREENE, M.D.

With the exception of the diethyl ether of methylene glycol, all of the oxyethyl substitution compounds of methane have already been described. Ortho-formic-ether, $CH(OC_2H_5)_3$, was studied by Kay and Williamson, and is generally known as Kay's ether. Ortho-carbonic ether, $C(OC_2H_5)_4$, was discovered and described by H. Bassett. Methyl-ethyl ether has long been known.

By a reaction similar to that by which these ethers are formed, I have isolated dioxyethyl-methylene, the reaction between sodium ethylate and methylene chloride taking place as indicated by theory.

The chief difficulty lies in the preparation of pure methylene chloride. The process described by Perkin, and depending upon the reduction of chloroform by zinc and ammonia, yields only small quantities of methylene chloride, and the direct chlorination of methyl chloride gives equally unsatisfactory results. The method which, after numerous experiments, I have found to answer best, consists in the reduction of an alcoholic solution of chloroform by zinc and hydrochloric acid.

The zinc and chloroform (mixed with several times its volume of alcohol) are placed in a flask connected with a suitable condensing apparatus, and hydrochloric acid is added in small portions. The reaction develops considerable heat, and methylene chloride and chloroform distil over; when the reaction has somewhat subsided, and no more liquid distils, more hydrochloric acid is added, and a moderate heat applied, if necessary. In any case, the mixture is heated towards the close of the operation, until alcohol begins to distil in quantity. The operation is then arrested, and the product in the receiver is washed, dried, and rectified, that portion which passes below about 53° being retained. The residue is returned to the flask, and again submitted to the action of the zinc and hydrochloric acid. By several careful rectifications of the product passing below 53°, pure methylene chloride, boiling at 40—41°, is obtained.

By several operations in this manner, the yield of methylene chloride may be brought up to about twenty per cent of the chloroform employed.

Little or no advantage is gained by attempting to fractionate the product as it distils from the flask, so that the chloroform may flow back into the reducing mixture, for such distillation necessarily takes place in the stream of hydrogen, which carries with it about as much chloroform as methylene chloride.

Dioxyethyl-methylene.—This compound was prepared by gradually introducing one molecule of sodium into a mixture of one molecule of methylene chloride and about four times the theoretical quantity of absolute alcohol, contained in a flask connected with a reflux condenser. After all of the sodium has been introduced, the mixture is heated on a water-bath for about an hour, and is then distilled. The distillate is fractionated, and the portion which passes below 78° contains all of the diethyl ether. It is agitated with a tolerably concentrated solution of calcium chloride, and the light etherial layer is separated, dried over calcium chloride, and carefully rectified, that portion being retained which passes between 86° and 89°.

Dioxyethyl-methylene, so obtained, is an etherial liquid, having a penetrating, pleasant odour, somewhat recalling that of mint. Its specific gravity at 0° is 0·851, and it boils at 89°, under a pressure of 769 millimetres. It is slightly soluble in water, from which it may be separated by the addition of calcium chloride; it mixes in all proportions with ether and alcohol, and cannot readily be separated from its alcoholic solution if much alcohol be present; in such a case, fractional distillation, and treatment of the portion which passes below 78°, with solution of calcium chloride, effect the separation.—*Journal of the American Chemical Society.*

ACTION OF BENZO-TRICHLORIDE ON PRIMARY AMINES.

By JAMES H. STEBBINS, Jr., B.S.

O. Doebner has shown, by his experiments on "malachite green" (*Ber. Chem. Gesell.*, ii., 1236), that benzo-trichloride will only unite with tertiary amines, in the presence of zinc or aluminium chlorides.

Now, it occurred to me, that some of the primary amines might unite directly with benzo-trichloride, without the aid of metallic chlorides, to form some new and interesting compounds.

Therefore, acting on this principle, I took para-toluidine as my starting point, as I happened to have quite a quantity of the latter at my disposal, and I think that the following experiments justify (as far as I have proceeded) the above-stated theory.

If equal molecules of benzo-trichloride ($C_6H_5C{=}Cl_3$) and para-toluidine ($C_6H_4CH_3NH_2$) be allowed to react on one another, at the ordinary temperature, the mixture soon grows very warm, and gives off copious fumes of HCl, mixed with aqueous vapour. The reaction is ended when the fumes are no longer evolved, and the white granulated mass thus obtained is dissolved in strong alcohol, from which it crystallises on cooling in two forms, namely—those obtained by slow cooling are long, white needles, having a strong, vitreous lustre, and melting at 155° C., and those produced by rapid cooling are small, white, rhombic prisms.

If the substance be heated a little over its melting-point, it sublimes, producing small, white needles which appear to be the original compound unaltered.

It is only sparingly soluble in boiling water and HCl, but is quite soluble in acetic acid.

Strong H_2SO_4 dissolves it readily, giving off fumes of hydrochloric acid, and it is precipitated, probably free from chlorine, by the addition of cold water.

This compound, then, is neither a base nor an acid, and its constitution can only be explained by converting it by oxidation or reduction, into a substance of known composition, and I think that this problem can easily be solved by its property of producing, first a red, then a black precipitate, with a solution of potassic permanganate.

The precipitate thus formed is collected on a filter, washed with a little water, and then boiled for a short time with strong alcohol, and filtered rapidly. On cooling, fine yellow-coloured needles make their appearance, the constitution of which, as well as that of the mother substance, I hope to lay before you at the next meeting.—*Journal of the American Chemical Society.*

ARTIFICIAL INDIGO.

It is said that Prof. Baeyer has discovered a method of obtaining artificial indigo which is practicable on a commercial scale. The patent has been assigned to the Baden Aniline Company. The indigo is obtained from isatine chloride, which in turn is produced from benzol.—*M. Reimann's Faerber Zeitung*, No. 2.

LABORATORY NOTES.

By H. ENDEMANN, Ph.D., and GEO. A. PROCHAZKA, Ph.D.

I. Standard Soda Solution.

Gerresheim has published in the first volume of the *Annalen der Chemie*, for the year 1879, an article on ammoniacal mercury compounds, wherein he calls attention to the pronounced basic properties of the so-called Millon's base, which is obtained by the action of ammonia upon mercuric oxide. He states that a soda solution to which hydrochloric and sulphuric acid, &c., have been added, can be freed from these by shaking with Millon's base. Mercury is not found in the solution. We have made use of this method in order to obtain a chemically pure standard soda solution from the ordinary so-called pure caustic soda, and have found all which the author claims verified. The soda contained large quantities of chlorine, sulphuric acid, and, also, silicic acid and carbonic acid. We employed for 2 litres soda solution about 30 grms. of Millon's base. The chlorine disappeared first, than carbonic acid, silicic acid, and sulphuric acid. The process was complete after about one week's standing, the mixture being shaken about once or twice each day.

We allow the Millon's base to settle in the solution, and draw off with a syphon as required. Shaking from time to time removes any carbonic acid which may be absorbed by the soda in the course of time. Millon's base absorbs carbonic acid from the air rapidly, and is, also, not easily filtered and washed. It was, therefore, not completely washed free from the last traces of ammonia, as we preferred to remove these by the addition of a small quantity of mercuric oxide when the Millon's base was brought into the soda solution.

II. Hydrobromic Acid as a very Delicate Test for Copper.

A solution of cupric bromide is blue; if the same is evaporated, the solution turns, at first, dark reddish-brown, leaving finally the anhydrous bromide as an almost black mass.

The dehydration can also be produced by the addition of a concentrated solution of hydrobromic acid; if, therefore, to a dilute solution of a copper salt concentrated hydrobromic acid is added, a dark brownish-red or violet colour is at once produced.

This reaction is so delicate that $\frac{1}{100}$ milligrm. of copper can be detected with certainty. One drop of a solution containing this small quantity of copper is brought on a watch glass, then one drop of hydrobromic acid is added, and the solution is then allowed to evaporate slowly by standing the glass on a warm place. When the whole has been concentrated to about one drop, this will distinctly show a rose-red colour. The colour thus produced is about three or four times as distinct as the one which is obtained by the addition of ferrocyanide of potassium. Of other metals which are examined in this direction, we find only iron to be apt to interfere with this reaction, and then only when it is present in considerable quantity.

We think that this reaction may also be utilised as a colorimetric test for the quantitative determination of small quantities of copper.

III. Observations made in an Examination of Sweet Potatoes.

(*a.*) *Parasites on Batata edulis.*—We will first mention here *Acarus sacchari*, the sugar mite, which is commonly found in raw sugars. A second parasite is *Mucor mucedo*, which produces a sweet potato rot very similar to that produced by perenospera on the ordinary potato.

The mycelium of *Mucor mucedo* travels soon into the interior plant, and can be followed with the microscope to a certain depth; it then, however, disappears, and from that point on the potato is rapidly destroyed by bacteria. An infection by either *Mucor mucedo*, or the bacteria, is equally effective in destroying the potato, which soon becomes soft throughout.

A similar, but by far not so rapid, result is reached if the infection is produced by *Aspergillus niger*. *Aspergillus glaucus* and *Penicillium glaucum* do not produce the sweet potato rot. The question whether *Mucor mucedo* has merely been effective in preparing the ground for a healthy development of the bacteria, or whether these organisms are merely a lower organised shape of *Mucor mucedo*, we will not decide, but must leave this to the study of botanists.

A peculiar odour produced during this fermentation bears great resemblance to the odour of rose geranium.

(*b.*) *Chemical examination.*—In making this, it was our endeavour to obtain a certain substance, presumably a glucoside, in larger quantities. This we obtained, but not in such purity as we desired. A certain quantity of fat has adhered to it all along, and our efforts to find a method of separation have thus far been in vain.

The sugar of the sweet potato has never been designated—most authors calling it simply sugar, without going into details. Ledoux, in his last report on the work of the experimental station at Chapel Hill, N.C., calls the sugar, glucose. We will state, therefore, that we have obtained this sugar in crystals, which, to all appearances, are cane sugar.—*Journal of the American Chemical Society.*

ANALYSES OF SOME HAIR-DYES.

By J. F. BRAGA.

(Continued from vol. xli., p. 278.)

Silver.—One dye I found to consist of simply a solution of nitrate of silver in water, the quantity of salt being at the rate of 10 grms. per litre. On enquiry I ascertained that this dye, though entirely successful when first applied, produced after a time a peculiar reddish brown tint upon the hair stained with it, this disagreeable hue being very noticeable in some aspects, as when the light reflected meets the observer's eye at a wide angle. This defect is entirely remedied by the presence of a minute quantity of copper in the solution, though it appears that copper salts alone are not successful as hair-dyes.

Silver and Copper.—Of this sort I have examined several. They are all blue in colour and alkaline in reaction, these characteristics being due to the presence of an ammonio-salt of copper, either nitrate or sulphate. One specimen contained a considerable excess of ammonia, whereas the others contained not much more than sufficient to re-dissolve the hydrates of silver and copper at first thrown down by it.

I have not found the quantities of the ingredients to vary much, so I need only quote the following formula, which is typical: by it I succeded in imitating one specimen after an analysis followed by an accurate determination of the metals:—

Nitrate of silver	26 grms.
Nitrate (or sulphate) of copper ..	2·5 ,,
Ammonia	a sufficiency.
Distilled water	to one litre.

The salts being dissolved in about 250 c.c. of water, strong liquor ammonia is gradually added till the precipitate is *just* re-dissolved, the solution being then diluted to the volume of 1 litre. Some of them contained a little alcohol, but I have not detected glycerin in any.

Another silver dye—called instantaneous, and sold under a great number of different names—consists of an aqueous solution of silver and copper salts, of about the strength given above, in one bottle, and a solution of pyrogallic acid, strongly acidified with acetic acid in another bottle. This latter solution, it appears, is first applied to the hair, which is then suffered to get almost dry; when the other solution is put on the well-known reaction occurs, the hair becoming immediately stained *jet black* if the solution of pyrogallic acid be strong, or a brown if that solution be very weak (about 1 to 2 grms. per litre). I have found the strength of the pyrogallic acid solution to vary in different specimens from about 10 grms. to 50 grms. in 1 litre, while the quantity of silver nitrate varied 10 to 60 grms. per litre, and that of the copper salt from 0 to 13 grms. per litre. Two specimens of this dye, obtained from different makers, had this solution of metals made alkaline with ammonia, a sufficiency being used to just re-dissolve the hydrates, as before mentioned.

PHYSICAL SOCIETY.

June 26, 1880.

Prof. W. G. Adams in the Chair.

Mr. C. V. Boys read a paper by Prof. Guthrie and himself "*On the Measurement of the Conductivity of Liquids by means of Magneto-Electric Induction.*" The liquid is suspended in a glass vessel by a fine iron wire in the centre of a cylindrical electro-magnet formed of two semicircular parts. This electro-magnet is rotated at a velocity not exceeding 3000 turns per minute, and the liquid being drawn round in the direction of rotation, the wire is subjected to torsion which, under correction for certain errors, is proportional to the resistance of the liquid. The torsion is observed by means of a scale and microscope. The results plotted in a curve agree very closely with those of Kohlrausch obtained by alternate currents, and Dr. Guthrie thinks that they are probably more correct and trustworthy than Kohlrausch's, for the motion would seem to be superior, and the curve contains fewer eccentric points than his.

Dr. Gladstone read a paper "*On the Refraction Equivalents of Isomeric Bodies*," in which he described the present state of the subject and his own contributions to it. He showed that the refractive power of bodies over light was of great importance to chemists, since it depended on their essential structure.

Dr. Huggins described his latest results of star-spectra, and illustrated his remarks by photographic spectra taken by his improved method. From these it appears certain stars, such as Vega, gives a complete spectrum of hydrogen. Others, more yellowish in colour, show a thinning of these lines, such as Sirius and Ursa Majoris. Others show the intrusion of more refrangible lines—for example, Arcturus, Aquila, and Virginius, while Capella gives a complex spectrum like that of the sun. Dr. Huggins also showed a spectrum of the flame of a spirit-lamp, which present a strong group of lines at S, and he considered it to represent the light emitted by the molecules of water. He further observed that the spectrum offered a highly sensitive test of the presence of carbon.

Mr. Liveing exhibited a new fire-damp indicator, capable of detecting ¼ per cent. of marsh-gas in air. It is based on the fact that an incandescent body shows more brilliantly in proportion to the amount of marsh-gas in the air, and consists of two fine platinum wires kept incandescent by a magneto-electric current sent through them in one circuit. One wire is excluded from the fire-damp; the other is exposed to it, and the relative intensities of the two glowing wires is compared by a photometric screen placed between them, and adjustible to a position between them at which the reflections of the wires on the screen are as equal in intensity. The position of the screen relatively to the wires is given by a scale, and measures the proportion of fire-damp in the air. The contrivance is more advantageous than the safety lamp, which only indicates 2 per cent. of marsh-gas in the air.

Dr. Stone explained a new vacuum shunt for induction currents devised by himself and Dr. Keiler. It consists of a mercury barometer tube with Torricellian vacuum and a movable cistern, suspended by a cord over a pulley, and supported by a counterpoise. By raising or lowering the cistern (which is connected to the tube by an india-rubber pipe) the mercury column is also raised or lowered, and the length of the vacuum—that is, the resistance of the shunt—altered.

Mr. Macfarlane Gray read a paper "*On Specific Heats calculated from Entropy.*" The author arrives at some remarkable results, which will be found in his paper in the Proceedings of the Society. He also explained a new graphic method of representing entropy.

Mr. Clark communicated a paper "*On the Behaviour of Liquids and Gases near their Critical Temperatures.*" When a sealed tube containing a capillary tube dipping into alcohol, ether, or sulphurous anhydride, is heated, the liquid sinks in the capillary tube and rises by expansion in the outer tube. Between 2 and 3° C. below the critical temperature of these liquids both surfaces become level, and, on continuing to heat, the concave capillary meniscus is seen below that in the outer tube. The extent of this depression depends upon the diameter, &c., of the capillary tube, and on the nature of its internal surface. As stated in June, 1879, when the capillary tube dips but very slightly below the surface the liquid disappears at the same level in the capillary and external tube at the critical temperature. Long contact of the liquid with the capillary tube may also cause the depression to be absent the first time of heating. (1.) The depression results from a surface action between the liquid and inner glass surface of the capillary tube. (2.) Surfaces exercise an appreciable action in determining the position at which the condensation of liquid takes place in the external tube on rapid cooling. (3.) Near the critical temperature the surface of a liquid (particularly in an external tube 20 m.m.) appears convex when really slightly concave. This is the result of refraction. (4.) The black, ill-defined band which immediately succeeds the disappearance of the liquid surface is a result of too rapid heating, and probably due to the mixture of the liquid and its vapour when of nearly equal density. Very slowly heated, the defined concave surface is gradually obliterated, and is last seen as a very fine and often waving line. Under these circumstances the disappearance-volume of the liquid is much greater than it is when rapidly heated. Rapidly cooled the liquids reappear at a lower temperature (about 2° C.) and smaller volume than that at which they become gases under the same conditions on heating.

Mr. Winstanley explained two new varieties of air thermometers and a thermograph actuated by an air thermometer on the principle of his radiograph exhibited at the last meeting. The first thermometer consists of a tube with terminal bulbs, and the left leg of much finer bore than the right. Mercury is in the right leg; sulphuric acid surmounted with air in the left. The apparatus is a barometer to the air inside the left bulb and a thermometer to that outside. A similar combination of an air thermometer and an Aneroid barometer constitutes the second instrument. The expansion or contraction of the air in the stem by external temperature expands or compresses a small aneroid chamber in the bulb.

Mr. Gee and Mr. Stroud made a communication "*On a Modification of Bunsen's Calometer*," which will be found in the Proceedings of the Society. The meeting then adjourned till the winter session commences.

The Distribution of Light in the Spectrum.—J. Macé and W. Nicati.—In order to measure the distribution of luminous intensity in the spectrum it is necessary to take, as the starting point, a physiological fact, well defined, and independent of colour. They consider two quantities of light as respectively equal when they, illuminating one and the same colourless object, placed at the same distance from the same observer, enable him to see its details with the same distinctness.—*Comptes Rendus.*

CORRESPONDENCE.

COMPOSITION OF WELDON MUD.

To the Editor of the Chemical News.

Sir,—Dr. Post, in the Chemical News (vol. xli., p. 276), attributes to me a formula, to satisfy which, he says, would require that the 0·58 per cent of Mg, which he found in a certain sample of Weldon mud, should be combined with 4·20 per cent, or 2·0 equivalents of MnO_2, and would further require that the 0·60 per cent of Fe_2O_3, which he also found in that sample, should be combined with 1·305 per cent of MnO_2, or in the proportion of 4·0 of MnO_2 to 1·0 of Fe_2O_3. Will you allow me to say that I have never given any such formula. On the contrary, while I have proved that MgO and MnO_2 can be made to combine in the dry way, though not in the proportions given by Dr. Post, but only in that of 1·0 of MnO_2 to 2·0 of MgO, I have also proved that MnO_2 does not combine with MgO at all in the wet way under the conditions which obtain in the Weldon oxidiser, and under which CaO, SrO, BaO, Na_2O, and K_2O, all combine with MnO_2, and that neither in the wet way nor in the dry way, under any conditions yet realised, will MnO_2 combine either with Fe_2O_3 or any other sesquioxide whatever.

Alike in his communication to you, and in four papers previously contributed to German journals, Dr. Post maintains that his determination of the quantity of CaO in certain samples of Weldon mud, prove that the explanations which I have given of what takes place in the Weldon process are incorrect. I reply that Dr. Post failed to find in his samples all the lime really contained in them, and that if his acquaintance with the chemistry of the Weldon process were as complete as that of one who is endeavouring to set us all right about it should be, he would know that the theory that the results of that process are due to the formation of compounds of MnO_2 with CaO is quite compatible with the non-existence of any lime at all in finished mud, made, as all Weldon mud is now-a-days, with what is called "final liquor."

Some fifty thousand samples of Weldon mud are now analysed per annum. Since the process was first introduced, more than half a million samples have been analysed. Dr. Post has analysed only three or four samples. The half million results are on one side, Dr. Post's three or four results are on the other. It does happen sometimes that one man is right and all the rest of the world wrong. But in this case there are circumstances which make it immeasurably improbable that Dr. Post is right. Weldon mud is made for the purpose of obtaining, by means of it, chlorine-products for sale. Important elements in the cost price of chlorine-products are the quantities of lime and limestone required to produce the Weldon mud, and the quantity of acid required to dissolve it. These quantities are therefore recorded regularly in every chlorine-work in the world, and from them the composition of the mud can be calculated, due note being taken of any loss of free chlorine; and calculation from these data confirms the results of the half million analyses, and utterly condemns Dr. Post's. If Dr. Post's analyses were accurate, to produce the quantity of chlorine now manufactured per annum would require some 14.000 tons less lime, and some 70,000 tons less hydrochloric acid, than we know that it actually does require.

In a paper replying to Dr. Post's first two German papers on this subjects which I read before the Newcastle Chemical Society on the 25th March last, and to which I would beg to refer any of your readers who may care to enquire into the evidence for the existence of manganites, I have shown that the contents of the Weldon oxidiser, when oxidation has ceased to advance, before the addition of the "final liquor," consist partly of Mn_2O_3, but mainly of MnO_2 combined with CaO, partly as $CaOMnO_2$, or $CaMnO_3$, and partly as $CaO(MnO_2)_2$, or $CaMn_2O_5$. Now, when $CaOMnO_2$ is treated by a sufficient quantity of $MnCl_2$, two successive reactions take place; the first, which I will call A, being represented by the equation:—

$$2CaOMnO_2+MnCl_2=CaO(MnO_2)_2+CaCl_2+MnO;$$

and the second, which I will call C, by the equation:—

$$CaO(MnO_2)_2+MnCl_2=MnO(MnO_2)_2+CaCl_2.$$

Reaction A takes place extremely readily, and is always completed before reaction C begins. Indeed, if there be any MgO present, aed if the $MnCl_2$ be added gradually, as it always is in practice, after reaction A has been completed and before reaction C begins, what I will call reaction B takes place, this being simply the decomposition of $MnCl_2$ by MgO. As reactions A and B yield free MnO, which can absorb oxygen, while reaction B moreover affords a simple means of avoiding the serious loss and inconvenience which would otherwise result from the accumulation in the mud of the MgO contained in the lime used, there is practical advantage in adding, as "final liquor," enough $MnCl_2$ to effect reactions A and B, and then continuing the injection of air until the resulting MnO has absorbed as much oxygen as it can; but there would be no advantage in adding more than that quantity of final $MnCl_2$, because the MnO resulting from the reaction of $MnCl_2$ on $CaO(MnO_2)_2$ simply replaces the CaO of that compound, converting it into $MnO(MnO_2)_2$, the MnO in which cannot be made to take up oxygen in the wet way. In well-conducted operations, therefore, no more final $MnCl_2$ is used than will convert all $CaOMnO_2$ into $CaO(MnO_2)_2$, and all MgO into $MgCl_2$. Still it does happen sometimes, exceptionally and by accident, that enough final $MnCl_2$ is added to produce, not only reactions A and B, but also reaction C, with the effect of replacing by $MnO(MnO_2)_2$ the $CaO(MnO_2)_2$ which would otherwise have been contained in the final product, and which is the principal constituent of the final product of operations made in the ordinary way. It follows that the complete absence of lime from finished Weldon mud would not, *of itself*, in the least vitiate the theory that the results obtained in the Weldon oxidiser are due to the intermediate formation of calcium manganites, and that whatever lime exists in finished mud which has been made with "final liquor," whether its quantity be great or small—and it may be very small indeed, even when reaction C has been entirely avoided, if the degree of oxidation of the manganese is low, owing to too small an "excess" of lime having been used—exists therein as $CaO(MnO_2)_2$. This is a very different theory from that which Dr. Post attributes to me, and it can neither be proved nor disproved by even accurate determinations of the quantity of CaO in the final product. To be in a position to discuss it at all, one must know the whole history of the product, and have observed qualitatively and quantitatively, all that has taken place from beginning to end of the operation by which it was made. And yet I am privileged to read, in an "abstract" in a recent number of the "Journal" of the Chemical Society, that Dr. Post's estimation of the CaO in certain samples of finished mud, "show the theories of Weldon . . . . to be inaccurate."

If, however, finished Weldon mud may contain no lime at all, and yet my theory as to the part which lime plays in the oxidiser *not* be inaccurate, why do I hold that the samples analysed by Dr. Post really contained more lime than he found in them? Let me put the answer as follows:—According to the half million analyses, the average proportion of "base" in Weldon mud is about 0·7, that is to say, per equivalent of MnO_2 in it there is usually seven-tenths, or nearly seven-tenths, of an equivalent of protoxides. This result of the half-million analyses is confirmed by the manufacturing result that for every kilogramme of chlorine liberated by means of Weldon mud, the quantity of hydrochloric acid at 32 per cent which is decomposed and neutralised by the mud, averages ten and a half kilogrammes, less the quantity afterwards neutralised by about 0·8 of a kilogramme of $CaCO_3$. But Dr. Post's analyses show very much less than 0·7 of base. Some of them show as little as 0·39 of base. If he had operated upon exceptional samples, in which the $CaO(MnO_2)_2$, of normal finished mud had been replaced by $MnO(MnO_2)_2$, his samples might have contained as little lime as he found in them, or even none at all; but they would then have contained an exceptionally large proportion of MnO. He finds in them, however, only the normal proportion of MnO. If, then, his determinations of MnO are accurate, his determinations of CaO must be wrong.

Dr. Post imagines that his results are confirmed by an analysis of Dr. Lunge's. From Dr. Lunge's figures he draws the conclusion that Dr. Lunge's sample contained "no trace of CaO." But he arrives at that conclusion only by completely ignoring reaction B, and assuming that what almost certainly was $MgCl_2$ was MgO. Moreover, while the "base" in normal Weldon mud unquestionably consists chiefly of CaO, and only to a smaller extent of MnO, it may, as I have shown above, consist entirely of MnO; and while Dr. Lunge's figures do not show what the base in his sample consisted of, between Dr. Lunge's results and Dr. Post's there is the essential difference that Dr. Lunge's results do show a proportion of base in perfect accord with the quantity of lime required to produce Weldon mud and the quantity of acid required to dissolve it, whereas if Dr. Post's results were true, some of the matter put into the oxidiser, in the course of the operation by which his samples were made, must have become annihilated. Analyses which show that some of the matter used in making the product analysed has ceased to exist, cannot be accurate.

Believing, as he does, that he has proved that Weldon mud contains very much less base than corresponds with the quantity of lime used in making it, it is strange that Dr. Post should not have tested the soundness of that belief by ascertaining how much hydrochloric acid it decomposes and neutralises per unit of chlorine liberated by it therefrom. To omit doing that is like omitting to ascertain whether or not supposed diamonds are attackable by hydrofluoric acid.—I am, &c.

WALTER WELDON.

Rede Hall, Burstow,
June 18, 1880.

CELLULOID.

To the Editor of the Chemical News.

SIR,—W. Donald would probably obtain full information by applying to the patentee, Daniel Spill, Esq., Chingford Hall, Leyton, Essex.

S. E. P.

THE NEW PATENT BILL.

To the Editor of the Chemical News.

SIR,—As an English inventor and patentee who has resided for some years in the United States, permit me to tender you my hearty thanks for your spirited and sagacious remarks on this subject, and especially for your comparison of the influence upon invention of the cheap American patent system (total Government tax £7 4s.) with that of our own expensive and, indeed, prohibitory tax (total Government charges £175).

It is as plain as a pike-staff to thinking men that in the coming years England cannot hold her own in the struggle with other manufacturing nations if legislation is to continue in this way to handicap the brain of the inventor.

As a labourer in saw mills, and at other times as mining and chemical engineer in Arizona, California, and Vancouver's Island, and thus mixing with Americans in all grades of life, I was constantly struck with the progressive spirit and inventing animus of the American artisan. Everywhere he appeared destitute of the class feelings and ideas which, in the form of struggles for higher wages or shorter time, seem alone to fill the head and heart of his British confrère. It is notorious that American strikes are in almost all cases got up, not by American, but by British or Continental workmen who have emigrated to the States. Whilst the Republican character of American life and institutions is, to a large extent, accountable for this peculiar spirit and sentiment of the native American artisan, there cannot be a doubt that one of its most important factors is the extreme facility with which he can secure possession of his inventions.

As a consequence, his brain is always at work scheming some new labour-saving or other appliance, whereas in England, as a rule, the tremendous Government tax of £25 (amounting, with agents' fees, principally for useless Government formalities, to nearly £40) during the first six months makes it hopeless for the workman to protect his discovery without first revealing it to some capitalist whose aid he is compelled to invoke.

In the chemical works where I write this (the largest of its kind on the globe) there is not one working man to whom I could truthfully point and say—"He has the inventing animus." The afflatus may exist, but in the entire absence of motive it is now hopelessly asleep.

Taking it as conceded, then, that the door to invention should be opened to the working man by lowering the initial charge for patents, we are met next by the consideration that if the tax in subsequent years be made extremely low, dead and worthless patents may be kept in force merely on the dog-in-the-manger principle of blocking the path for others. This is, I believe, recognised to be a very serious practical objection to the plan of low subsequent charges, and I would venture, therefore, after much thought upon the subject, to suggest the following scheme through the medium of your columns:—That, taking the duration of the patent to be 21 years, as now proposed, the first year's Government tax should be £1, the second year's £2, the third £3, and so on till the last year, when it would be £21. Under this arrangement no working man would be deterred from fully securing his invention. Impracticable or unprofitable patents would die a natural death (everybody knowing when they had given up the ghost), and the Government upon every patent which was worth keeping up for the full term would collect a tax (and that, too, in the least oppressive way) of £231.

The resulting revenue might be well utilised in perfecting the Patent Office publications, say by a prompt and regular weekly issue of abridgments of patents whose close time had expired. These abridgments to be drawn up by experts in every department, and to have an index attached giving full references to every abridgment issued since the appearance of the last yearly volume. The roll of special class abridgments should also all be completed up to within a year of date, and every other facility should be given to the intending inventor to ascertain exactly what had been done up to date in his special field. For want of these reasonable and simple arrangements incalculable loss is, to my certain knowledge, done every day to British invention, as inventors are, in numberless cases, afraid to dive into the mass of complicated indexes and unabridged patents (generally behind date) at present provided. A few years ago I obtained the signatures of leading British inventors and manufacturers to a memorial asking a weekly issue of abridgments with index incorporating weekly all previous indexes since the issue of the last yearly volume, the object being to keep every inventor thoroughly posted up to date in his particular department. The request was granted by the Patents' Commissioners, but as the abridgments given were the optional ones of the inventors (in many cases being mere titles!), and as the Commissioners would spend nothing in employing technical men to prepare the abridgments, the publication soon proved practically worthless, and was abandoned.

If, in addition to such perfecting of the publications, it were considered necessary or desirable for illiterate inventors to provide official technical examiners, the revenue would, doubtless, defray their expenses, but such examiners should only have advisory powers. They might point out to an inventor where they considered his invention weak, but he should still have the full privilege of patenting his invention and taking the consequences of his act.—I am, &c.,

Frank.

TAR DISTILLERS *v.* GAS COMPANIES.

To the Editor of the Chemical News.

Sir,—In your article on this subject in your last number you do not treat the tar distiller with your usual justice when you assert that he, unlike other manufacturers, has not been alive to the advantages of improvements in the processes of his business, and that few industries have suffered more in remaining so long in the background for want of scientific aid and direction. The success of the Gas Light Company with their tar works at Beckton, which you prophecy may possibly rival those of the Cie. Parisienne at some future time, and which are now actually on a much larger scale, does not warrant you, I think, in deciding so emphatically that the tar distillation of the future must be taken out of the hands of tar distillers and placed in those of gas companies.

I freely admit that there are tar distillers who are jogging on in the old ruts despite the great progress that has been made in the different improvements of the different processes of late years, but these men are in every case in a very small way of business, with very limited capital to carry out experimental work or improvements, even when they have the technical knowledge to do so; but I contend that these men have in the past been as successful and efficient distillers, and more so, than managers of small provincial gas works would have been if they had had this industry in charge.

The larger tar distillers of England and Scotland, I contend, will compare favourably with any other body of manufacturers, either as commercial men, or as possessing the technical knowledge of their business. Those of them who have not had scientific training themselves generally engage scientific assistance to ensure their working in the best manner and to improve on old processes.

That English distillers are not behind our French neighbours in adopting a new process or manufacturing a new product is evidenced by one instance, viz., the manufacture of anthracene, as this product was manufactured commercially by English distillers some considerable time before the Cie. Parisienne made it, although they had the assistance of their talented chemist, M. Audouin.

In conclusion, I would point out that the success of the Beckton Tar Works, so far from justifying you in your strictures on English tar distillers, on the contrary, points to the opposite conclusion, as the works were designed by a tar distiller for the company, and are now managed by a gentleman who, I believe, has been brought up to the tar distilling business.—I am, &c.,

Tar Distiller.

ACTION OF A NARROW SLIT ON LIGHT.

To the Editor of the Chemical News.

Sir,—I have just been reading "Effects of Light Passing Through a Slit," by Lieut.-Com. Michelson. It seems to me that no great difficulty obtrudes itself if we apply the vertical and horizontal theory of waves in light. The article closes by saying, "the experiments in any case seem to prove (1) that a very narrow slit polarises light, as described, and (2) that it lets the shorter waves of light pass more freely than the longer ones." This is just what we should expect had the premises been put before us. (1) The rays are in our plane only, the narrowness preventing* those at right angles getting through: this is borne out by the fact that the different reflecting angles of the different metals tried did not influence the result; it was purely, then, confined to some action in the ray itself (shown to be polarised, &c.). (2) This would (seem to) result in the smaller *amplitude* of the waves, when of course the lesser would get where the greater could not: this is proved by the violet succeeding to the blue.—I am, &c.,

D. G. Cliff.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Note.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 22, May 31, 1880.

An Automatic Electric Lamp.—M. Jamin.—This memoir cannot be intelligibly abstracted without the accompanying illustrations.

Heat of Combustion of the Principal Gaseous Hydrocarbons.—M. Berthelot.—The author gives for formene, 213·5; ethylene hydride, 388·8; ethylene, 341·4; acetylene, 318·1; methyl oxide, 344·2; propylene hydride, 556·0; propylene, 507·3; allylene, 466·5. The author concludes that the heat of combustion of a hydrocarbon is never equal to that of its elements.

Synthesis of Citric Acid.—E. Grimaux and P. Adam.—The authors prepared dichlorhydrin by means of glycerin and sulphur chloride, and oxidised it by means of potassium bichromate and sulphuric acid. The symmetric dichloraceton thus obtained was purified by combination with sodium bisulphite, and then heated in the water-bath with concentrated hydrocyanic acid. The cyano-dichlor-aceton was treated with hydrochloric acid; the product distilled in a vacuum, and exhausted with ether. The dichlor-acetonic acid obtained was saturated with sodium carbonate, and heated with 2 mols. potassium cyanide in a concentrated solution. The liquid contain sodium dicyan-acetonate which was saturated with gaseous hydrochloric acid, heated in the water-bath for fifteen hours, and distilled in a vacuum. From the residue the citric acid was extracted by careful treatment with milk of lime.

Researches on the Albuminoid Matters of the Crystalline Lens and Non-Identity of the Soluble Portion with Egg and Serum Albumen.—A. Béchamp.—The soluble albuminoid matters in question are *phacozymace*, a substance which remains soluble in water after it has been precipitated by alcohol. The solution, at a certain state of concentration begins to coagulate at 55°. It fluidifies starch paste. The solution in hydrochloric acid, takes a violet colour, after being heated for a few seconds to the boiling-point. *Crystal albumen* becomes insoluble in water after precipitation by alcohol, but this property is not manifested instantaneously. Both these bodies are thrown down in a plumbic combination by basic lead acetate, but these precipitates, unlike those formed by egg and blood albumens are not decomposed by carbonic acid.

Telephone with Magnetic Super-excitation.—M. Ader.—The new telephone is founded on the principle that if a slender blade of iron or steel is placed before the poles of a magnet, it is much more strongly acted upon if there is placed behind it a piece of soft iron than if this is not the case. Thus a greater sensibility is obtained by placing in front of the diaphragm of a common Bell's telephone, an iron disc pierced in its centre, with a hole corresponding to the mouth of the apparatus.

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* Or supposing the planes at all angles, it only allows one to pass: thus the width is obtained.

On Astigmatism.—C. J. A. Leroy.—A mathematical paper not susceptible of useful abridgment.

Heat Evolved in the Combustion of Certain Isomeric Alcohols of the Fatty Series, and of Œnanthol.—W. Louguinine.—The author's object is to ascertain if the kind of isomerism presented by the various alcohols of the fatty series has an appreciable influence on their heat of combustion.

Refrigerant Mixtures formed of Two Crystalline Salts.—A. Ditte.—The phenomena observed establish the possibility of forming refrigerating mixtures with two saline bodies, one of which retains much crystalline water, when the double decomposition resulting from their contact is effected without any marked liberation of heat.

Crystalline Hydrofluosilicic Acid.—M. Kessler.—The author has obtained a compound to which he assigns the formula $F_2SiFH+2HO$. It is probably the only known hydrated hydracid which crystallises at common temperatures.

Proportion of Carbonic Acid in the Air.—M. Marié Davy.—A reply to the second communication of M. Reiset. The author considers that many well-established facts are opposed to the theory of the uniform distribution of carbonic acid.

Preparation of Malonic Acid.—E. Bourgoin.—The author dissolves 100 grms. monochloracetic acid in double its weight of water, and saturates the solution with potassium bicarbonate, about 110 grms. He adds then 75 grs. pure potassium cyanide, and, after dissolving, heats carefully in the water-bath. To the liquid are added two vols. concentrated hydrochloric acid; the deposit of potassium chloride is removed, and the liquid is supersaturated with gaseous hydrochloric acid. The salts deposited are strained off through a plug of asbestos, and the mother-liquor which they hold back is displaced by a little hydrochloric acid. The washings are added to the acid solution which is evaporated at first at a boil and then on the water-bath. The residue, almost dry, is exhausted with ether, from which pure malonic acid is obtained on distillation or evaporation.

Preparation of Neutral Sulphuric Ether.—M. A. Villiers.—This neutral ether is prepared by distilling in a vacuum a mixture of sulphuric acid and absolute alcohol; about 200 grms. of the former with thrice its volume of the latter. The distillation should be exceedingly slow. The liquid which passes over is divided into two strata, of which the lower is the pure neutral colourless ether.

Presence in Soya hispida of a Substance Soluble in Alcohol and readily Convertible into Glucoose.—M. A. Levallois.—This substance is soluble in alcohol, does not readily reduce Fehling's liquid, and closely resembles glucose in its optical characters.

Justus Liebig's Annalen der Chemie,
Band 201, Heft 1.

Salts of Chloro-chromic Acid.—G. Prætorius.—The author has succeeded in obtaining barium and strontium chloro-chromates by using as a solvent glacial acetic acid instead of water. He has also formed and examined the chloro-chromates of magnesium, nickel and cobalt, zinc, sodium. He did not succeed in obtaining the cadmium salt.

Communications from the Laboratory of Würzburg University.—These consist of a memoir on butallyl-methyl-carbinol, by J. K. Crow; a memoir on diallyl-acetester and its derivatives, by C. Wolff; contributions to the history of methyl-crotonic acid, by Aug. Rücker; an account of diethyl-β-oxy-butyric acid, by H. Schnapp; and a paper on the reaction of zinc and iodallyl upon acetester and diethyl-acetester.

Communications from the University Laboratory of Halle.—These include an account of the behaviour of triacetonamin with iodethyl, by W. Heintz, and a paper on the existence of acetonin, by the same author. The conclusion arrived at is negative.

Molybdic Acichlorides.—W. Puttbach,—The author considers that six acichlorides of molybdenum are known, of which four are saturated.

Reimann's Färber Zeitung,
No. 14, 1880.

The Maritime Court of Bremerhaven has decided that the fire on board the *Mosel*, on October 3, 1879, was due to the spontaneous combustion of certain black silk yarn, which formed part of her cargo.

Dr. Göppelsrœder has resigned his Professorship at the Ecole de Chimie of Mulhouse.

No. 15, 1880.

This number contains nothing of general interest to our readers.

MISCELLANEOUS.

Agitation of Liquids.—For this purpose an india-rubber cover is constructed fitting air-tight to the working beaker and having two tubulures, through one of which a glass tube is passed to the bottom, thereby covering its lower end with the liquid, and its upper end is open to the air. The other tubulure is connected to an aspirator of some kind (in my laboratory a Bunsen's water pump is used with advantage), which, being put in motion, draws a stream of air bubbles through the liquid, thereby keeping it in a constant state of agitation. By means of this apparatus the precipitation of phosphoric acid, as ammonio-phosphate of magnesia, is complete after half an hour's agitation.—H. B. Yardley.

Washing Flask.—In the former communication I believe I may claim some originality, but in this present my desire is simply to draw the attention of brother chemists to the advantages of using an india-rubber ball with tubulure attached to the mouth-piece which is bent round to the side of the flask, so that by holding the neck of flask with the fingers, and pressing the ball with the thumb, the desired stream of liquid is obtained. The ball is provided with a small air hole, over which the thumb is placed, and which allows air to re-enter immediately on removal of the pressure. The stream of liquid can be kept up any length of time, and at a pressure varying at will and in any direction. I also always fit my wash flask with a movable jet by means of india-rubber tube. I may mention that Messrs. Townson and Mercer supply both these balls and the india-rubber covers previously described.—H. B. Yardley.

NOTES AND QUERIES.

Dissolving Shoddy.—A correspondent would be glad to know as to the best apparatus for dissolving shoddy for the manufacture of manure.—H.B.D.

Bright Red Writing Ink.—Will any of your learned correspondents kindly give me, through the medium of the Chemical News, a thorough good recipe for making a bright red writing ink.—J. Smith

MEETINGS FOR THE WEEK.

Monday, 5th.—Royal Institution, 5. General Monthly Meeting.

TO CORRESPONDENTS.

Analyst.—We cannot give the information required. Probably an advertisement would get you what you want.

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King Edward's School, 22nd June, 1880.

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THE CHEMICAL NEWS.

Vol. XLII. No. 1076.

ON THE CRITICAL POINT OF MIXED VAPOURS.*

By JAMES DEWAR, M.A., F.R.S.,
Jacksonian Professor of Natural Experimental Philosophy in the University of Cambridge.

The following record of experiments regarding the behaviour of carbonic acid in presence of different vapours above the temperature of the critical points of the pure gas were undertaken to see if any optical discontinuity could be observed in such mixtures above this temperature. The object was intentionally a qualitative investigation, and thus many of the pressure observations have been taken from the metallic manometer.

The Liquefaction of Carbonic Acid in presence of other Bodies.

1. *Carbonic Acid and Bisulphide of Carbon.*—Carbonic acid liquefied in presence of bisulphide of carbon, at a pressure of 49 atmospheres and a temperature of 19° C., floated on the convex surface of the bisulphide, the line of separation being sharp and well defined, and remaining so.

At 35° C. liquid condensed on the surface of the bisulphide, in the same way, at a pressure of 78 atmospheres; at 40° C. it still appeared, at 85 atmospheres; at 55° C. there seemed to be a distinct appearance of two liquids, and at 58° C. there was still the same apparent separation under a pressure of 110 atmospheres. Observed at 47° C., and a pressure of 80 atmospheres, there was a distinct layer of a separate fluid on the surface of the bisulphide: the bisulphide surface, however, was not so well defined.

By keeping the temperature at 47° C., on increasing the pressure to 110 atmospheres, the upper surface of the liquid floating on the bisulphide almost entirely disappeared. By reducing the pressure again to 80 atmospheres the surface of demarcation disappeared; but in reducing the pressure another 5 atmospheres, the line of demarcation again appeared very sharply, and remained. A quick withdrawal to 58 atmospheres, and again gradually increasing the pressure to 85 atmospheres, did not make the liquid remain, but on reducing it again slowly to 80 atmospheres the definition became perfectly sharp.

2. *Carbonic Acid and Chloroform.*—Carbonic acid in presence of chloroform at 18° C. liquefied at 25 atmospheres, forming a distinct layer on the surface of the chloroform. On further compression the manometer rose rapidly to 50 atmospheres, at which pressure the two liquids mixed completely together, after being left for a few minutes. When the pressure was rapidly withdrawn distinct layers of what appeared to be carbonic acid were always formed, which, however, became rapidly dissolved in the chloroform on standing a few seconds.

At 33° C. liquid began to appear at a pressure of 35 atmospheres, and on increasing the pressure to 55 atmospheres it behaved in exactly the same way as at the lower temperatures, except that the layer was, if anything, more distinct and mixed more rapidly with the chloroform on standing.

At 55° C. a layer of liquid was still formed at a pressure of 50 atmospheres; at 67° C. it behaved in the same way, except that there was a smaller quantity of liquid formed, and the pressure rose to 85 atmospheres.

In every case it rapidly mixed with the chloroform when left for a few seconds.

3. *Carbonic Acid and Benzol.*—At 18° C. the carbonic acid commenced to liquefy at a pressure of 25 atmospheres, and at the moment of liquefaction the surface of the benzol became violently agitated, the carbonic acid falling through the benzol in an oily stream, and becoming completely mixed with it.

When by further condensation more liquid was formed the agitation almost entirely ceased, the liquid carbonic acid forming a distinct layer on the saturated benzol. On leaving this for about five minutes the line of demarcation disappeared, and the two liquids formed a perfectly homogeneous fluid.

On again increasing the pressure so as to get a layer of carbonic acid, and then releasing the pressure gradually, the liquid carbonic acid on the surface first fell in oily streams through the saturated benzol; but when all this had disappeared the carbonic acid then commenced to boil from the bottom of the benzol, and continued to do so until it was again entirely vapourised.

At 35° C. liquid commenced to appear at 35 atmospheres, forming a distinct layer on the surface of the benzol, which was not in the least agitated. On further compression the liquid layer increased in volume, but no oily streams were seen to fall through the benzol; and on leaving it for about ten minutes the layer of liquid was almost just as distinct, showing that it was not nearly so soluble in the benzol at this temperature.

On the pressure being now reduced the liquid quietly evaporated away from the surface; but when all this had disappeared carbonic acid commenced to boil out of the benzol, showing that it had dissolved a considerable amount.

At 52° C. the liquid appeared at 60 atmospheres, forming a layer which mixed with the benzol on standing; and at 70° C. and 85 atmospheres a distinct layer was also formed, which, however, rapidly mixed with the benzol.

4. *Carbonic Acid and Ether.*—A tube was filled with carbonic acid, and a little ether introduced. At 20° C., and at a pressure of 20 atmospheres, the carbonic acid liquefied and fell through the ether, mixing with it in all proportions.

At 42° C. liquid was condensed on the surface of the ether at a pressure of 55 atmospheres, forming a distinct layer; the upper surface of the ether was, however, kept in continual oscillation, from the apparent solution of the carbonic acid in it. No currents due to the falling of the carbonic acid through the ether were visible. At 68° C., and a pressure of 110 atmospheres, a perfectly separate layer of fluid was found on the surface of the ether, and no currents were descending through the ether.

5. *Carbonic Acid and Nitrous Oxide.*—When a tube was filled with equal volumes of carbonic acid and nitrous oxide, and the gases were liquefied, they mixed together in all proportions, no difference at all being perceptible; but when the pressure was suddenly withdrawn the one gas boiled before the other, and for a few seconds a distinct line of separation was seen.

Liquefaction of Carbonic Acid with Trichloride of Phosphorus.

At 16·20° C., and 42·95 atmospheres pressure, the carbonic acid commenced to condense on the sides before the trichloride of phosphorus came in sight, and when the latter was visible a slight indistinct layer of CO_2 was seen on the surface, only distinguishable by the different refractive indices of the two liquids, there being no sharp line of demarcation. On standing a few minutes the liquids became quite homogeneous. On increasing the pressure more carbonic acid was condensed, forming a more or less distinct layer on the surface of the trichloride of phosphorus. This, however, rapidly disappeared on standing. On releasing the pressure the carbonic acid boiled first on the surface, but afterwards through the liquid, the trichloride of phosphorus at the same time falling in heavy striæ to the bottom.

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* A Paper read before the Royal Society, June 17, 1880.

At 23° C. the carbonic acid appeared to liquefy at 46·91 atmospheres, exactly the same appearances taking place as at 16° C.

At 30° C. the carbonic acid liquefied at 49·94 atmospheres, forming a rather more distinct layer, and not mixing so readily. On increasing the pressure to 90 atmospheres the surface of the carbonic acid disappeared, it being near its critical point, the top part of the tube being filled with a homogeneous mass. The trichloride of phosphorus could also not be distinguished on the surface of the mercury, its upper surface being entirely mixed up with the carbonic acid, the whole space above the mercury forming one homogeneous mass.

On releasing the pressure a cloud first appeared, and then the surface of the carbonic acid became visible; it boiled away first from the surface, and afterwards through the trichloride of phosphorus.

At 33° C.	liquid carb. acid appeared at	50·84	atmos.
,, 40 ,,	,, ,, ,,	56·88	,,
,, 50 ,,	,, ,, ,,	66·53	,,

At all the temperatures above 30° C. the appearances were the same, except that as the temperature increased the quantity of carbonic acid liquefied diminished, and it took a great pressure to make the surface of the trichloride of phosphorus disappear.

The following are pressures taken with a smaller quantity of carbonic acid and trichloride in the tube, and were read off when the surface of the trichloride was first agitated, thus showing that the carbonic acid had commenced to condense:—

At 10·5° C.	22·70	atmospheres.
,, 16·5 ,,	24·70	,,
,, 22·8 ,,	32·18	,,
,, 30·0 ,,	33·88	,,
,, 40·0 ,,	36·36	,,
,, 50·0 ,,	49·67	,,
,, 70·0 ,,	76·61	,,

Liquefaction of Carbonic Acid with Tetrachloride of Carbon.

The quantity of tetrachloride of carbon was a little less than the volume of the carbonic acid when liquefied.

At 12·8 C. the surface of the liquid appeared agitated as soon as it appeared in sight, and on increasing the pressure, a distinct layer of carbonic acid was formed on the surface of the tetrachloride; on increasing the pressure a still more distinct layer was formed, which, however, on standing, rapidly commenced to dissolve in the tetrachloride, and in about ten minutes it was perfectly homogeneous.

At 21·4° C. the surface of the tetrachloride appeared agitated when it came in sight, a layer of liquid being formed on increasing the pressure, as at 12° C.

At 30° C. liquid was also formed, which, however, rapidly diffused into the tetrachloride of carbon.

At 40° C. the liquid also appeared agitated, and on increasing the pressure rapidly a small quantity of fluid was condensed, which, however, rapidly disappeared in the tetrachloride.

At 52° C. the liquid again became agitated, and on increasing the pressure a distinct layer of liquid was formed.

The same took place at 58° C.

Liquefaction of Carbonic Acid and Chloride of Methyl.

When chloride of methyl was compressed in a tube by itself it became liquid before the pressure could be registered, and must have been below 10 atmospheres.

When compressed with about twice its volume of carbonic acid, at 13·5° C.,—the chloride of methyl, of course, liquefied first,—and about 27·67 atmospheres, its surface became agitated, showing that the carbonic acid had commenced to liquefy; but the exact point was difficult to ascertain, as it dissolved so very rapidly in the chloride of methyl.

At 20·05° C. this point appeared to be at 28·57 atmospheres.

At 30° C. some liquid was also condensed, but the pressure at commencement of liquefaction could not be taken, as it mixed so rapidly with the chloride of methyl.

That more liquid was in reality condensed was seen by the lengthening of the liquid column, and by its boiling out of the chloride of methyl when the pressure was reduced.

At 40° C. exactly the same took place.

Carbonic Acid and Acetylene.

About equal volumes of these gases were compressed together; they liquefied and mixed completely at all the temperatures given below, and no appearance of two different gases being liquefied was visible, except that the liquid was strongly agitated during condensation. The pressure at the point of liquefaction was, as in the former cases, lower than either of the gases liquefied by themselves. Thus—

At 13·5° C.	the pressure was	25·23	atmospheres.
,, 21·0 ,,	,, ,,	26·8	,,
,, 26·8 ,,	,, ,,	34·1	,,
,, 31·9 ,,	,, ,,	40·26	,,
,, 39·0 ,,	,, ,,	55·3	,,
,, 41·0 ,,	,, ,,	75·32	,,

The critical point was, on the other hand, heightened as usual, being 41° C.; that of carbonic acid being 31° C., and of acetylene 37° C.

Carbonic Acid and Hydrochloric Acid Gases.

A mixture of equal volumes of these gases was filled into a tube, and the liquefied gases mixed completely together at all temperatures below the critical point, which was 36° C., forming a perfectly homogeneous fluid; in fact, it was impossible to tell that two different gases were present, as even at the point of liquefaction no difference was discernible.

The following are the pressures at which the mixture liquefied:—

At 0·0° C.	36·0	atmospheres.
,, 5·0 ,,	39·0	,,
,, 8·0 ,,	43·8	,,
,, 10·1 ,,	48·2	,,
,, 18·5 ,,	59·0	,,
,, 34·0 ,,	83·0	,,
,, 35·5 ,,	90·0	,,

Carbonic Acid and Bromine.

A tube filled with a mixture of carbonic acid and bromine vapour, by passing dry carbonic acid through a tube containing dry bromine before entering the liquefying tube. A little strong sulphuric acid was also introduced to protect the mercury. On compressing this mixture at 11·5° C. the sulphuric acid appeared in sight with a layer of liquid at 50 atmospheres, the liquid having a decidedly red colour.

On increasing the pressure the liquid became more highly coloured, while a little pure bromine liquid fell through the carbonic acid liquid, remaining a short time on the surface of the sulphuric acid, through which a globule also sank.

As this tube was spoiled through the amount of bromine which fell through the sulphuric acid, another was put up in the same way, and heated at once to about 40° C., before compressing the mixture.

On the sulphuric coming in sight a small layer of bromine was seen on the surface, which was surmounted by a layer of darkish red liquid, about ½ inch long. The pressure was about 60 atmospheres.

On increasing the pressure to 90 atmospheres the upper liquid increased a good deal in quantity, and then disappeared entirely, but immediately reappeared on reducing the pressure a few atmospheres, and remained permanently.

Carbonic Acid and Camphor.

Some small pieces of camphor were placed in the capillary part of an ordinary Cailletet tube, near the end, and fused so as to adhere to the sides. The tube was then filled with carbonic acid gas.

On compressing this mixture in the pump, at 12° C., the camphor was seen to melt and run down the sides of the tube before the mercury appeared in sight. (In this experiment it was not observed whether liquid carbonic acid had commenced to form in the tube, and thus dissolve the camphor, or whether the latter was dissolved in the gas.) On continuing the pressure, so as to almost fill the tube with liquid, two distinct layers of liquid were seen, the lower one being slightly cloudy, containing the dissolved camphor, the upper liquid being perfectly clear. On compressing at different temperatures up to 55° C. the lower cloudy liquid was always present, the upper layer diminishing gradually in quantity as the temperature rose; but at 40° C. there was still a slight layer on increasing the pressure to about 125 atmospheres.

On withdrawing the pressure very suddenly, when the tube was full of liquid at 50°, the sides of the tube became coated with crystallised camphor, which rapidly dissolved again on increasing the pressure. After doing this several times a very small quantity of camphor was seen to crystallise out, and in taking down the tube the most of it was found to have crystallised out in the top part of the reservoir near the joining with the capillary part, thus allowing the mercury to get past it on again increasing the pressure.

Carbonic Acid and Camphor (second tube).

Another tube was filled in the same way as the last (a rather larger quantity of camphor being placed in the capillary part), with this exception, that the carbonic acid, after being dried, was passed through a tube containing fragments of solid camphor, which were gently heated, so as to fill the tube with carbonic acid gas saturated with the vapour of camphor.

When this tube was compressed in the pump at a temperature of 15° C., and when the pressure reached 27·7 atmospheres, the camphor was seen to gradually melt and run down the sides of the tubes before the mercury appeared in sight.

On increasing the pressure to 37 atmospheres the mercury appeared in sight, with about ¾ of an inch of a turbid liquid on the surface. On still further increasing the pressure two distinct layers of fluid were formed, which, however, became quite homogeneous after a short time. On mixing them up by a rapid decrease and increase of pressure, the two fluids soon mix. At 28° C. two layers of liquid were distinctly visible at 65 atmospheres, the lower layer being visible as soon as the mercury appeared in sight.

At 35° C. carbonic acid was condensed on the surface of the lower liquid at 80 atmospheres, but when the pressure was increased to 100 atmospheres the surface of the carbonic acid became undefined.

At 45° C. carbonic acid is seen to condense on the sides of the tube at a pressure of 100 atmospheres, running down and forming a slight layer on the surface of the camphor liquor, which, however, disappears on still further increasing the pressure.

When the temperature was 42·5°, and the pressure was suddenly reduced, the inside of the tube became covered with crystals of camphor. On now increasing the pressure very carefully the camphor was seen to melt or liquefy and run down the sides of the tube at a pressure of 37·6 atmospheres; and this pressure scarcely increased at all until all the camphor was thus liquefied.

A diminution of the pressure by two or three atmospheres was sufficient to bring out the crystals of camphor.

At 60° C. the lower layer of liquid still remained, and on increasing the pressure to 100 atmospheres there was an appearance of liquefaction of carbonic acid on the surface.

Carbonic Acid, Air, and Camphor (third tube).

A quantity of camphor was placed in the capillary part of a tube as before, which was afterwards filled with a mixture of 4 volumes of carbonic acid, saturated with camphor vapour, and 1 volume of air.

The tube was surrounded with water at 25° C., so as to be far above the critical point of the carbonic acid and air mixture, and on now increasing the pressure the camphor liquefied and ran down the sides of the tube as before. At 50° C. a quantity of liquid, about ½ inch long, appeared on the surface of the mercury when it came in sight, the pressure being now 65 atmospheres.

On leaving the pressure the same, the mercury being just in sight, and increasing the temperature gradually to 60° C., the inside of the tube above the liquid became covered with camphor crystals, which on increasing the pressure another 5 atmospheres (viz., to 70 atmospheres) again dissolves.

At 65° C., the pressure standing at 70 atmospheres and the mercury and liquid being in sight, on reducing the pressure to 65 atmospheres the camphor crystals separated out, being again suddenly dissolved on again increasing the pressure to 73 atmospheres. On now rapidly letting down the temperature to 15° C. a white mass of camphor separated out from the liquid, which again dissolved on a slight increase of pressure, although it could not be again separated out by diminishing it. These actions may be due to super-saturation and the effect of pressure in aiding solubility when contraction takes place during solution.

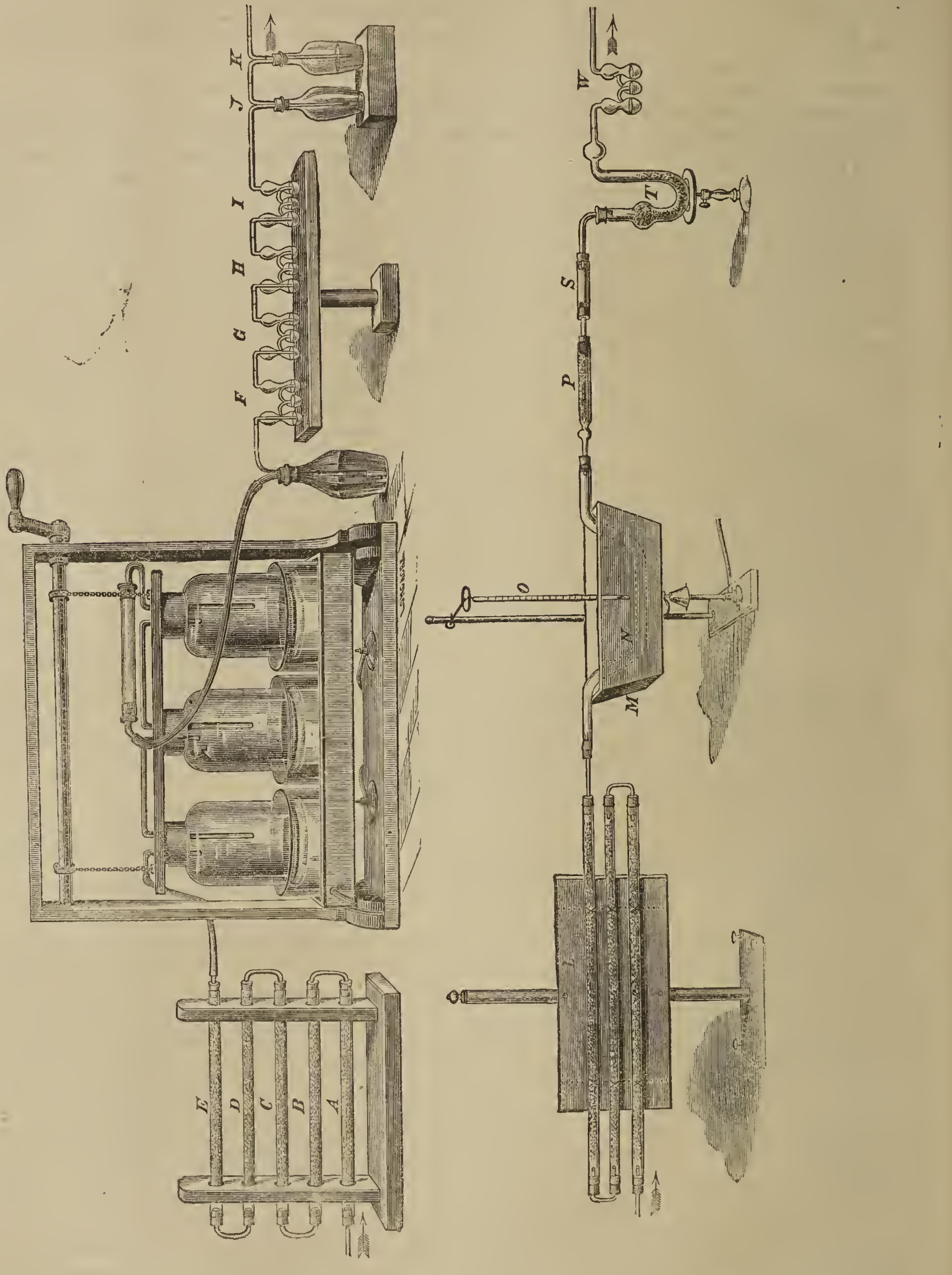
These experiments show that carbonic acid at high pressures, in presence of various substances, acts as if it produced a series of unstable chemical compounds, which are decomposed and re-composed according to the conditions of temperature and pressure in the medium.

ON THE FORMATION OF HYDROGEN PEROXIDE AND OZONE DURING THE ACTION OF MOIST PHOSPHORUS UPON AIR.

By Professor A. R. LEEDS.

In various earlier memoirs, and especially in one published in the *Annalen der Chemie und Pharmacie* (200, 286) entitled "On Ammonium Nitrile, and on the By-products obtained during the Ozonisation of the Air by Moist Phosphorus," I have referred to the fact that the ozone obtained on oxidising moist phosphorus in air always contains an admixture of hydrogen peroxide. I have showed, further, that the quantity of hydrogen peroxide evolved under known conditions of time, temperature, surface of phosphorus, &c., bear a certain proportion to that of the ozone produced, which in the experiments quoted, amounted to $\frac{1}{400}$. The hydrogen peroxide was determined by titration on the assumption that it is completely absorbed by the washing-water, the ozone being first expelled by concentrating the washing-water down to two-thirds of its original volume, the absence of nitrious acid being then ascertained by Griess's method with meta-diamido-benzol, and the quantity of hydrogen peroxide which has been contained in the current of ozonised air being determined in the residue. The purpose of this paper is to show, by a method totally different from that which I formerly employed—

1. That both hydrogen peroxide and ozone are formed during the action of air upon moist phosphorus.
2. That in this great dilution they are not perfectly decomposed even after a prolonged state of mixture.
3. That if the current of ozonised air is conducted through a tube heated to various temperatures, the



quantity of water obtained by the decomposition of the hydrogen peroxide increases with the rise of the temperature.

4. That, under these circumstances, the proportion of ozone regularly decreases, and that no ozone reaction is to be obtained above 200°.
5. That if, after reaching this point, instead of the neutral solution of potassium iodide employed for the titration of ozone, and perfectly free from potassium iodate, there is used a solution mixed with sulphuric acid, it undergoes a slow decomposition, due not to ozone, which has been completely decomposed at a temperature of 200°, but to the spontaneous decomposition by oxygen of an acidified solution of potassium iodide.

The chief points of the new method of examination were :—

1. Filtered and purified air was brought in contact with a great surface of phosphorus partially immersed in distilled water, free from compounds of ammonium and nitrious acid, and maintained during the ex-

periment at the temperature most favourable for the formation of ozone (24—25°).

2. The hydrogen peroxide is removed as far as possible from the ozonised air by means of a sufficient series of washing-bottles.
3. The current of air, after passing these bottles, must be freed from every trace of water.
4. Both the hydrogen peroxide and the ozone are then decomposed by a gradual increase of temperature.
5. Lastly, any ozone which may still exist after the action of the heat is titrated and the water which has been formed is weighed.

This chain of thought is realised in the apparatus represented by the accompanying wood-cut. The air is here filtered through a series of purifiers, of which A contains cotton-wool, which in B, C, D, and E are glass globules, those in B and C being steeped in soda-lye and those in D and E in sulphuric acid. After the air has been ozonised in the phosphor-ozonator, it is drawn by the connecting tube into the first washing-bottle and thence into the four Geissler bulb apparatus, F, G, H, I, which all, like the washing-bottle, are filled with water. It passes then first through the empty washing-bottle, J, then through a bottle filled with sulphuric acid, K, and the three drying tubes, L, which for the length of 2·5 metres contain glass beads steeped in sulphuric acid.

From the drying-tubes the ozonised air passes into the U-tube, N, placed in the oil-bath, M. Its middle part contains for the length of 0·25 metre asbestos which has been previously ignited in order to offer the largest possible heated surface to the stream of air. Next follows a weighed drying-tube, P, charged with sulphuric acid; another of U-shape, T for safety, and lastly a bulb apparatus, W, filled with neutral potassium iodide. Between P and T, there is introduced for convenience, an empty tube, S, closed with corks at each end.

The following experiments were conducted under circumstances as identical as possible, twelve litres of air being drawn through the apparatus in every two hours, whilst the ozonator was heated to 24°. The increase of weight of the drying-tube, P, corresponds to the weight of the water formed by the decomposition of the hydrogen peroxide when heated to different temperatures. The numbers obtained are shown in the following table:—

	Expt.	Temp.	Water.	Hydrogen Peroxide.	Ozone.
		°	Grm.	Grm.	
Series I.	I.	100	0·0015	0·0028	not determined
	II.	50	0·0010	0·0019	,,
	III.	24	0·0000	0·0000	,,
Series II.	IV.	200	0·0010	0·0019	0·0000
	V,	200	0·0011	0·0020	0·0000
	VI.	150	0·0002	0·0004	0·0011
Series III.	VI.	100	0·0015	0·0028	0·0013
	VII.	50	0·0003	0·0006	0·0044
	IX.	21	0·0000	0·0000	0·0037
Series IV.	X.	200	0·0018	0·0034	0·0000
	XI.	100	0·0002	0·0004	0·0011
	XII.	22	0 0000	0·0000	0·0051

These results seem to me to prove the correctness of the first four propositions; they show that both hydrogen peroxide and ozone are formed, and that on heating the ozonised air the quantity of water thus found increases regularly, whilst that of the ozone as regularly decreases, till at 200° all the hydrogen peroxide is transformed into water and all the ozone into ordinary oxygen.

After the conclusion of the twelfth experiment the intermediate apparatus, dryers, &c., were removed, and the bulb-apparatus, W, containing potassium iodide, was placed in direct connection with the washing-bottle, F. On drawing through it the same quantity of ozonised air, in the same time and under the same conditions, there were obtained 7·94 m.grms. instead of the 5·1 m.grms. obtained in experiment 12. The difference of 2·84 m.grms., = 36 per cent of the quantity of ozone originally formed, corresponds to the loss due to the decomposition of the hydrogen peroxide simultaneously present.

Thus proposition 2 is confirmed, and we see that hydrogen peroxide and ozone in a dilute condition can exist together without great loss,[*] but that the quantity of the hydrogen peroxide existing in the ozonised gas bears a not inconsiderable proportion to that of the ozone itself. If we neglect for the moment the trifling quantities of hydrogen peroxide retained in the washing water, its proportion to the ozone is as 1 : 3.

The quantities of hydrogen peroxide absorbed by the washing water during the entire series of experiments were as follows:—The bulb-apparatus, F, containing 47 c.c. of water, after evaporation down to two-thirds of its original bulk in order to expel any dissolved ozone, yielded 0·2 m.grm H_2O_2; G, containing 26 c.c., gave after similar treatment a reaction indicating 0·08 m.grm. H_2O_2; H yielded 0·01 m.grm. The entire quantity found in all the washing waters was 0·29 m.grm. H_2O_2. Further, as hydrogen peroxide in a so dilute gaseous condition is not entirely without influence upon a pure neutral solution of hydrogen, the values given in the table are slightly too high.

Finally, in order to decide whether the supposition 5 is correct, the experiment was repeated with the tube N heated to 200°. I will again mention that in all earlier experiments at this temperature a neutral solution of iodide was employed, and no liberation of iodine could be observed, whilst in this experiment a decomposition took place corresponding to 0·2 m.grm. ozone. Herewith it is proved that in a current of air from which every trace of hydrogen peroxide and ozone have been removed by a strong heat, a seeming ozone reaction is produced by the oxygen present, in the case that the potassium iodide solution used for titration is acidulated.[*]—*Berichte der Deutschen Chem. Gesellschaft.*

ON WELDON MUD.

By G. LUNGE.

Dr. Post has in the Chemical News, vol. xli., p. 276 undertaken to state the results of a discussion between us concerning the composition and analysis of Weldon mud, which took place in *Dingler's Journal*; but he has supplied little more than his own part of it. It would be incumbent upon me now to state what, in my opinion, controverts Dr. Post's position, if this question had not been treated very fully by Mr. Weldon himself in a paper read before the Newcastle Chemical Society. I shall, therefore, content myself with a few remarks, which will, however, practically deal with the whole of Dr. Post's paper.

Dr. Post seems to object to my defending the analytical processes described by me some years ago in *Dingler's Journal*, which are those of Mr. Weldon, modified in a few less essential points, on the ground that he (Dr. Post) had never used these processes for Weldon mud, nor expressed any opinion about their value. But this is immaterial, since he has employed one of these processes, viz., the testing for MnO_2, for the product recovered by his own plan and declared it to be incorrect, and since his assertions about the freedom of Weldon mud from lime necessarily carry the inference that the usual testing for base is also altogether wrong. The latter would refer both to Mr. Weldon's prescription and to my own modification of it. In the former case he tried only my plan (which substitutes permanganate for bichromate), and asserted to have found an enormous difference between

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[*] This point has been established by Schöne in opposition to the statements of Schönbein. *Journ. Prak. Chemie*, 76, 130, and *Ann. Chem Pharmacie*, 196, 240.

[*] See *Phil Mag.*, April, 1879; *Journal Amer. Chem. Soc*, i., 18.

the results of this and of Bunsen's iodine test. In order to dispel any confusion that might arise on these points, I felt myself called upon to re-investigate those methods, and I found that they were perfectly correct, Dr. Post's assertions notwithstanding. This forms the subject of my former paper, in which I also showed that the presence of HCl, if it made any difference in the permanganate iron test, would have just the opposite effect ascribed to it by Dr. Post. I do not feel that I need any further justification for the course which I have taken.

My objection that the great bulk of Dr. Post's work on Weldon mud is quite worthless for deciding the question of the presence of calcium and other manganites therein, because he worked on a substance changed by contact with an enormous amount of water, is practically acknowledged to be valid by Dr. Post himself, who now relies upon one single analysis of unwashed Weldon mud to prove his case. His attempt to turn even my own analysis against myself has been fully answered by me in *Dingler's Journal.* For the reason given above I shall refrain from repeating my own arguments, as well as from going into the question whether I believe Dr. Post's analytical methods to have been inexact, or their execution faulty, or both. I shall simply put the whole case broadly as it stands apart from the question of the accuracy of his or my individual assertions.

If Dr. Post's work is correct, that is, if Weldon mud, in spite of the large excess of lime present, contains no calcium other than as chloride, carbonate, and sulphate, it cannot, *a fortiori*, contain a "manganite" of calcium; consequently the compound MnO_2 (or its polymer, which is immaterial to the question at issue) does not possess any acid properties. Dr. Post tries to "guard himself against this conclusion in advance," but I do not see how he can reconcile his analyses with the contrary opinion. Now, if that is so, Dr. Post's *work* (whatever his *theory* may be) is contradicted by the unanimous testimony of the large number of chemists who have investigated the behaviour of manganese dioxide during the last few years, of whom I shall only name Gorgen, Pattinson, Wright and Luff, Beilstein and Jawein, Volhard, Stingl and Morawski, not to speak of Mr. Weldon himself, who has probably worked more on this subject than all of them put together, although he has published comparatively little of his results. All these chemists agree that MnO_2 carries down basic oxides of all kinds, MnO, ZnO, CaO, K_2O, &c.; it even decomposes alkaline chlorides and nitrates in this way, setting free HCl and NO_3H. Some of them describe definite compounds, in which MnO_2 plays the part of an acid. In the recovery of MnO_2 by Weldon's process *all* conditions are present which determine MnO_2 in its formation to carry down, not merely MnO, but CaO, MgO, &c., in a combined state, whether we call it a "manganite" or not. Shall we believe that all these chemists are mistaken, and that Dr. Post is right in denying calcium to be an *essential* constituent of Weldon mud, which it cannot help being if the acid properties of MnO_2 are granted? But if this assumption is declined, then Dr. Post's *work* cannot be correct.

Can we believe, then, that really Weldon mud occurs in practice with a base below 0·5, because Dr. Post asserts in *one* case to have found 0·46, not by direct estimation of the base, but by a most complicated analysis? Since I asserted, both in English and German periodicals, that "to my knowledge" such muds do not occur in practice, and since Dr. Post put forward his own and only analysis in attempted refutation of my assertion, several months have elapsed, but not a single case has been made known by an independent observer which would contradict me, nor has Dr. Post evidently been able to find such a case apart from his own analysis. Now, there exist, without exaggeration, on the one hand in factories, hundreds of thousands of estimations of base, not one of which is known to show below 0·5; on the other hand there is *one* indirect estimation of the base by a chemist whose analytical work is otherwise irreconcileable with the evidence of all other chemists in the field. Whether that is enough to prove the practicability of obtaining less than 0·5 base, the reader may judge for himself. But even if some cases of lower base were proved to occur by less doubtful evidence, this would not by itself disprove the main point, viz., that Weldon mud contains CaO chemically combined with MnO_2.

The question, What becomes of the excess of lime in the Weldon process? has long since been fully dealt with by Mr. Weldon, of whose work Dr. Post seems to know very little. How imperfect is his acquaintance with the Weldon process is best shown by his claim (*Berlin Ber.*, 1879, p. 1458) to have discovered the presence of CO_2 in Weldon mud, which, in his opinion, had been overlooked by everybody up to that time. But not merely had every practical man known all about that, but I had actually, many years ago, in that publication which has formed the basis of an accurate knowledge of the Weldon process in Germany (*Dingler's Journal*, ccxv., p. 149), mentioned the presence of CO_2 in Weldon mud, and described the means adopted by some manufacturers to overcome this drawback.

Zürich, June 25, 1880.

THE ANALYSIS OF PYRITES BY THE GRAVI-VOLUMETRIC METHOD.

By M. A. HOUZEAU.

One grammme of the powdered ore is attacked with a mixture of 4 parts pure potassium nitrate and 3 parts sodium carbonate, likewise pure. The saline mass is dissolved in hot water, and the ferric oxide is filtered from the alkaline sulphate. The washing waters are added to the filtrate, and after cooling it is made up with distilled water to half a litre. A portion is then taken—10 c.c.—and acidulated with a few drops of pure acetic acid, and the sulphuric acid is rapidly determined, as the author has elsewhere indicated for selinitic waters by making use of a standard solution of barium chloride, applied by the aid of the *gravi-volumeter* in place of the ordinary burette, the use of which, in such determinations, yields merely erroneous results. In the gravi-volumeter the weight of the standard solution shows the quantity of the reagent which has been used. But each drop of the barytic liquid delivered by the gravi-volumeter weighs at the temperature of 15° exactly 0·050 grm. The author then proceeds to show that series of determinations made by this process, vary less than do series made by the ordinary gravimetric method. After oxidising a specimen of pyrites with nitric acid and precipitating with barium chloride, he found the weight of the precipitate 0·343 grm. = 47·1 per cent sulphur. By three successive fusions with pure sodium carbonate and re-precipitations with barium chloride, the weight of the barium sulphate was reduced to 0·331 grm., representing 45·4 per cent of sulphur. In thus purifying barium sulphate by fusion it is necessary to employ wood charcoal as a source of heat, since coal-gas besides reducing a part of the sulphate furnishes sulphur, which is ultimately transformed in the crucible into sodium sulphate. In fine, for the exact determination of the sulphur in pyrites by the gravimetric method, several days are required, whilst after the samples are dissolved two determinations can be effected by the gravi-volumetric method in half an hour.—*Comptes Rendus.*

Aromatic Arsenical Compounds.—A. Michaelis and W. la Coste.—This treatise is a continuation of the researches of the authors on the combinations of the elements of the nitrogen group with the radicles of the aromatic series. From its great extent it is incapable of useful abstraction.—*Liebig's Annalen.*

PROCEEDINGS OF SOCIETIES.

THE AMERICAN CHEMICAL SOCIETY.

The May Conversazione of the Society was held at University Building, Washington Square, New York, on Thursday evening, May 20th, 1880. Among a number of very interesting exhibits the following are worthy of notice:—

Dr. **Arno Behr** exhibited a solution of copper sulphate containing an abundant growth of filamentous Fungi. This solution contained about 3½ per cent of ordinary pure copper sulphate, and the fact that copper salts are usually considered inimical to living organisms makes this exhibit interesting.

The same gentleman also exhibited a remarkable leather-like deposit found in a dust-flue of a sugar-refinery. It was made up of layers of filamentous tissue, and was probably formed by the growth of Fungi which exude a kind of glue-like material that cements the various layers together. This material was quite tough, like thin leather, and of a nut-brown colour. It often occurs covering a surface several feet in area, having the appearance of a coat of paint.

Some black scale from the interior of the retorts used in making bone-black was also exhibited by Dr. **Behr**. This material appears to eat into, and finally through, the retorts, making it necessary to replace them occasionally. It consists of some carbon, together with sulphide of iron, and appears to act upon the metal of the retorts by giving up its sulphur to the metallic iron, becoming reduced to a lower sulphide, which in turn acquires new sulphur from the sulphates in the bones burnt and also from the albumen which they contain.

A filter-press, of Wegelin and Hübner, for use in laboratory experiments, was also shown and explained by Dr. **Behr**. It consisted of a filter-press, with the usual frames for paper, cloth, or wire, and a monte-jus attachment. The monte-jus is simply an air-tight vessel, into which the liquid to be filtered is placed, and thence forced through the filter diaphragms by means of compressed air. The air is compressed upon the surface of the liquid in the monte-jus, and a tube from the bottom of the vessel delivers the fluid with its suspended matter to the filter frame. The pump for compressing the air is attached to the side of the monte-jus, and a gauge dial shows the pressure in the interior of the apparatus.

In answer to a question as to the kind of industries in which these presses were used, it was stated that they could be adapted to every conceivable want, and that they were now being used in the United States to filter beer.

Dr. **Behr** said that the advantage of this press with the monte-jus attachment was that the flow of liquid through the press was steady, while in using the simple pump press without the monte-jus attachment the intermittent action of the pump often caused a turbidity in the filtrate.

A specimen of the new metal Gallium, made by the discoverer, Lecoq de Boisbaudran, was exhibited by Dr. C. F. **Chandler**, of Columbia College. It was only a few millimetres in area, but was interesting as the first specimen seen in this country. It is a hard white metal, melting at the heat of the hand.

Dr. **Chandler** also exhibited a specimen of naphthaline taken from the main leading from the retorts to the gasometer of the Municipal Gas-Light Company, of New York. This company makes gas by passing steam over red-hot anthracite coal, and the resulting mixture of carbonic oxide and hydrogen is carburetted by passing it through petroleum naphtha, and then through red-hot retorts again. The naphthaline deposit exhibited shows the conversion of the hydrocarbons of the paraffin series into those of the aromatic series by heat.

Prof. A. R. **Leeds**, of the Stevens Institute, exhibited a beautiful piece of glass-work by Prof. Richards, of the Massachusetts Institute of Technology. It consisted of a very ingenious regulator, to maintain a constant temperature in a hot-air oven for laboratories. It was made entirely of glass, and much admired for its fine finish. Dr. Leeds said that this apparatus worked very satisfactorily.—**Arthur H. Elliott**, *Recording Secretary*.

NOTICES OF BOOKS.

A Theoretical and Practical Treatise on the Manufacture of Sulphuric Acid and Alkali, with the Collateral Branches. By G. **Lunge**, Ph.D., F.C.S., Professor of Technical Chemistry at the Federal Polytechnic School, Zürich (formerly Manager of the Tyne Alkali Works, South Shields). Vol. II. London: Van Voorst.

In introducing to our readers this second volume of Dr. Lunge's great work we are happy to say that it shows no falling off in point of value and interest. It is devoted to the alkali manufacture according to Leblanc's process, from the manufacture of salt-cake to the utilisation of the vat-waste. A third volume is announced, giving an account of the Solvay process, the manufacture of soda from cryolite, the preparation of bleaching-lime and chlorate of potash, followed by an estimate of the cost of plant for every department of an alkali works.

The account of sodium sulphate—or, as it technically called, salt-cake—commences with a very minute description of its composition and properties, especially its solubility, the methods used for its analysis, and the processes for manufacture. It is remarked that its production in cylinders as a by-product from the manufacture of superior hydrochloric acid has almost entirely ceased—a statement with which we can scarcely agree, since most of the hydrochloric acid used in the preparation of dyers' spirits is still obtained in this manner, that from the alkali works (technically known as *tower* salts) being often found untrustworthy. We notice, also, that the manufacture of aluminium chloride from the reaction of common salt and alum is spoken of as a process actually in use and yielding crystalline sodium sulphate as a secondary product. It is remarked that the manufacturers of Glauber's salt at Stassfurt prepare it only in frosty weather, as the use of ice-making machines seemed too expensive.

The bulk of the salt-cake in the market is of course obtained by the decomposition of common salt, either on the ordinary Leblanc process or by the action of sulphurous acid and atmospheric air—Hargreaves and Robinson's method—or by heating the salt with metallic sulphides, as proposed by Berzelius as far back as 1836, and known even in the last century. It is a curious fact that the French Commission of 1792 in their official report pronounced this process superior to that of Leblanc.

Concerning the present extent of the production of salt-cake it is stated that whilst in 1869 the quantity of salt decomposed in England was 326,000 tons, in 1676 (doubtless 1876), it had risen, for the United Kingdom, to 647,000 tons. Dr. Lunge does not agree with the commonly-received theory of the decomposition of the salt, according to which only 50 per cent of the total hydrochloric acid ought to be given off in the decomposing-pan. In practice the amount eliminated is 66 to 70 per cent, though the heat reached never even approaches redness.

In the manufacture of salt-cake the form or condition of the salt is a matter of importance. Neither rock-salt nor the salt obtained by evaporation at a boiling heat, and known as butter-salt, is suitable, but a coarse grained pan salt, which presents a large surface for the attack of the acid, and open spaces for its absorption. This is, indeed, a general rule of some importance in manufacturing chemistry, that where agitation is not practicable fine compact-lying powders are less readily dissolved or otherwise acted on by liquids than such as are of a coarse grain. The purity of the Cheshire salt—not the rock-salt—is said to be a great advantage which British alkali makers possess over their rivals on the Continent. Ferric oxide and alumina rank among the most troublesome impurities. Salt at the Lancashire works costs 6 to 8 shillings a ton; and though rock-salt—which is less suitable for alkali making—may be had at 3 shillings per ton, yet on the Rhine it costs 15 to 16 shillings, and in Saxony 17 to 28 shillings per ton.

As regards the sulphuric acid, its impurity of arsenious acid does not remain in the salt-cake, but is evolved with the hydrochloric acid. The best strength of the acid for decomposing salt is given as 140° to 144° T., which agrees well with our own observations in well-managed alkali works. Too great dilution is not always avoided if the works are provided neither with a concentrating-pan nor a Glover tower. Concerning the apparatus, close roasters are recommended where the muriatic acid is sold off the works, and where a high strength is consequently required to economise carriage. The arrangements for heating vary according to the relative prices of fuel and labour in different localities. Where the purity and colour of the sulphate are important lead pans are still used, as originally proposed by Leblanc; but where the salt-cake is for alkali making iron pans are preferred, for very obvious reasons. The various designs of decomposing-pans, and their arrangement in the furnaces, are described and figured at considerable length. The structure figured in Muspratt's "Chemistry" (iii., p. 908), and copied into several text-books, was never general in Lancashire and has been abandoned for some years. A good pan will last 2500 tons of sulphate, and sometimes even up to 4000 tons. Their construction is a secret known only to a few iron-founders, and requires a mixture of several brands of pig-iron. On the Continent the pans rarely last above 1500 tons of salt-cake, and will rarely bear a direct heat. Both the setting the pans and the way of working them have a great influence on their duration. It is very important that the sulphuric acid should not be run into the pan at temperatures below 80° to 100° C. If cold, cracking is almost unavoidable.

Heating blind-roasters with waste heat, as common on the Continent, is considered not economical, as the saving in fuel is outweighed by the increased labour and the multiplication of furnaces. But whether blind or open roasters are preferable is still a debated question, each system having its advantages and drawbacks. There is no proof whatever of a more complete condensation of the hydrochloric gas from blind-roasters. Dr. Lunge sums up to the effect that where it is needful to get as much strong acid as possible, whether for sale or for generating chlorine, blind-roasters ought to be employed. The gas-furnaces of Fletcher and of Gamble, intended to combine the advantages of open- and blind-roasters, are also described and figured.

The mechanical salt-cake furnaces, which are not dependent for their efficiency upon the skill, sobriety, and good-will of workmen have scarcely reached perfection. The arrangement patented by Cammack and Walker is pronounced extremely ingenious in its plan, but the mechanical difficulties in working have not been entirely overcome.

The yield upon the salt is calculated in England at 110 per cent. Absolutely pure and dry $NaCl$ should give 121·45 per cent, and at New Stassfurt 120 has actually been obtained. The plan lately adopted in England of referring the yield of salt-cake, not to the sulphuric acid produced, but to the pyrites consumed, the author—very justly in our opinion—condemns as untrustworthy.

The Hargreaves process is described with great minuteness and accuracy, but its merits are left an open question. Among its drawbacks are mentioned a much higher first cost of plant, a probably larger cost of maintenance, a greater consumption of coal, and inequality in the character of the product.

We regret that we are not able to carry any further our survey of this excellent work. We are convinced that all competent judges who examine it will be fully satisfied with its accuracy and thoroughness, and regard it as a great boon to the trade.

A Manual of the Alkali Trade, including the Manufacture of Sulphuric Acid, Sulphate of Soda, and Bleaching-Powder. With 232 Illustrations and Working Drawings. By John Lomas. London: Crosby Lockwood and Co.

It might be supposed that of two books covering so nearly the same ground as the volume before us and Dr. Lunge's great treatise the one must necessarily render the other superfluous. This is, however, not the case. Though both works deal with the same subjects, and both are written by practical men, their point of view is in many respects different, and the one is no mere echo of the other. Mr. Lomas writes more as the manager, and seeks to produce a manual which alkali manufacturers may put into the hands of foremen "as a useful guide in their daily rounds of duty." He makes no attempt to give the general chemical characters of the materials used or produced. Theories of decomposition, analytical procedures, suggested improvements beyond such as have actually established themselves in practice, are rarely touched upon. But all arrangements which have on a fair trial proved themselves successful are described carefully and fully. Sound advice is given on many points which fall quite outside the usual scope of technical manuals. We may instance the selection of a site for intended works: the means of avoiding Sunday labour—a grave evil at many alkali works, as it always involves the demoralisation of the men, and renders them reckless, if not positively desirous of doing mischief. The question of time-work *versus* piece-work, and the adaptability of the latter to various departments of the alkali manufacture, is also discussed. Even such matters as the manufacture and cost of the casks used for packing the soda-ash are taken into consideration. It may surprise outsiders to learn that this apparently insignificant item represents close upon 10 per cent of the production-cost of a ton of alkali.

In passing the author refers to the uncertainty of the Liverpool alkali tests, as compared with those of Glasgow, Newcastle, and London. Hereby hangs a tale. We have heard that in the early days of the Lancashire alkali trade a chemist—whose name we forget, and who has long ago, as old Helmont put it, "been resolved *in terram damnatam*—made use of a most peculiar indicator in alkalimetry. When he judged that the operation was nearly complete he took up a drop of the liquid with the stirring-rod, put it in his mouth, and decided by taste whether the soda was exactly neutralised. It is, we think, to be regretted that the old and incorrect atomic weight of sodium, 24 (soda =32), is still maintained in the trade.

Mr. Lomas enters at some length into the noxious vapours' question. He quotes the "recommendations" of the Commission which sat from 1876 to 1878, and which would probably, but for the recent dissolution of Parliament, have ere this been embodied in a statute. With these recommendations we are only partially satisfied. They seem to aim at one fixed uniform standard for all parts of the country; enforcing the same degree of purity in a manufacturing district—where even in the absence of chemical works vegetation is destroyed by the fumes of sulphur dinoxide given off by burning coal rich in pyrites—as in agricultural districts, where no source of contamination exists. If, *e.g.*, a cotton-spinner burns in the furnaces of his steam-engines 20 tons daily of a coal containing 0·5 per cent of sulphur—no outside supposition—he will daily pour into the atmosphere the equivalent of 660 lbs. of oil of vitriol, equal to a million and a half of cubic feet of air containing 1 grain of sulphur per foot, with entire impunity, whilst his neighbour, the chemical manufacturer, may be subjected to pains and penalties, or even to the total suppression of his business, for a small fraction of such pollution. The recommendations, moreover, make no mention of a recently introduced nuisance which is carried on in the outskirts of London with intolerable effrontery in localities completely surrounded by houses. We refer to ballast-burning, a process in which exceedingly offensive fumes are emitted, not from the tops of high chimneys, but so as to roll along the surface of the ground.

Another important feature of the work before us is the attention which the author pays to the construction of the various buildings and the strength of foundations for every part of the plant.

In case of simple facts, numerical results, &c., there is generally a fair agreement between Dr. Lunge and Mr. Lomas. Mr. Lomas is probably the less sanguine of the two. Though by no means an enemy to improvements, and though he points out various departments of the Leblanc process where there is scope for invention, he is apt to judge patents very rigorously. His opinion of the Glover tower is given as follows:—"It concentrates the chamber acid, increases the produce of the pyrites, and preserves the chamber itself. And yet it cannot be called by any means perfect. The plant is expensive and short-lived, and after all only saves some two-thirds of the nitre consumed. Probably its greatest virtues consist in the concentration of the chamber acid as aforesaid and in the preservation of the chambers."

The Deacon process for the production of chlorine Mr. Lomas pronounces "beautifully simple in reaction and productive of an enormous yield of cheap chlorine," and as "likely in some improved form to supersede all other chlorine processes."

On the Weldon magnesia process he remarks that "the whole of the chlorine is made available, and is actually utilised, so rendering possible an immense production of bleaching-powder. The apparatus required is of a simple description, and in no special way liable to get out of order. The amount of loss from leakages and other inevitable causes may be reduced to 2 or 3 per cent. But the process has never been practically successful, probably on account of the unremitting care required throughout. If the proper proportions between manganite and acid in the stills are not kept, so much magnesia has to be added to the liquors that the furnaced product contains an undue amount of this substance, and is worse than useless in the chlorine stills. Again, if the amount of protoxide in the manganite be too large a waste of acid in the still operation is incurred. The regulation of furnace temperature and of the admission of air are likewise delicate matters requiring constant attention.

Of the processes for the recovery of manganese from the still liquors Mr. Lomas pronounces Weldon's by far the best, its esseential feature being merely the addition of lime in large excess.

The Hargreave's process for the production of salt-cake he considers beautifully self-acting, but requiring very constant and intelligent watching. Many of the complaints made against this process are due to a careless preparation of the salt, especially the admission of fine dust into the cylinders.

To sum up, we may pronounce this a decidedly useful work, especially adapted for manufacturers, managers, and foremen. It should be kept for reference in all those establishments—becoming now, fortunately, more numerous—where "book-learning" is not scouted in favour of "rule of thumb."

CORRESPONDENCE.

CHEMICAL SOCIETY RESEARCH FUND.

To the Editor of the Chemical News.

Sir,—The following grants have been made from the research fund of the Chemical Society:—

£10 to Mr. Kingzett, for Experiments on the Atmospheric Oxidation of Phosphorus.

£25 to Watson Smith, for the Investigation of the Dinapthyls and Phenyl-napthalene.

£25 to Messrs. Bailey and Munro, for the Investigation of the Colour Reactions of Certain Metals and Metallic Solutions.—I am, &c.,

W. J. Russell.

Chemical Laboratory,
St. Bartholomew's Hospital, E.C.
July 5, 1880.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Note.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 23, June 7, 1880.

A Bromated Derivative of Nicotine.—A. Cahours and Etard.—The authors have not succeeded in reproducing C. Huber's compound, $C_{20}H_{13}N_2Br_5$. They have, however, obtained another bromine derivative of nicotine by dissolving 1 part of nicotine in 50 of water, and adding, with agitation, 4 atoms bromine per mol. of nicotine. An abundant yellow flocculent precipitate was formed. On heating the liquid to 65° to 70° by means of a current of steam, filtering and cooling the precipitate, long fine red crystals were deposited. If these crystals are dissolved in hydrobromic acid, the solution, on cooling, deposits a crystalline product less red than the former, probably a hydrobromate of the primitive derivative. This compound has the composition $C_{20}H_{14}N_2Br_4$. Huber's compound is probably a hydrobromate of this derivative formed by addition.

Direct Measure of the Interior Resistance of Magneto-electro-machines in Motion.—G. Cabanellas.—The intensity of the current circulating in the ring being i webers per second, and the difference of the potential between the two extremities of the connected rings being represented by ε volts, the resistance of the ring is given by $\frac{\varepsilon}{2i}$.

Transformation of War Powder in the Metal Cartridge Boxes of the Infantry.—E. Pothier.—It appears that gunpowder preserved in contact with certain metals, especially zinc and copper, undergoes a gradual change. Potassium sulphide, sulphate, and carbonate, and ammonium sesquicarbonate are produced along with sulphides and basic salts of the metals derived from the decomposition of the brass of the boxes. The strength of the powder was found considerably reduced.

Colloidal Oxide of Iron.—L. Magnier de la Source.—The author concludes that ferric hydrate is in certain conditions soluble as such without there being any need to assume that it enters into combinations of greater or less complexity.

A New Aluminium Sulphate (Sesquibasic).—P. Marguerite.—On examining aluminium sulphate resulting from the decomposition of ammonia alum by heat a new sulphate has been obtained, answering to the formula $Al_2O_3,2SO_3,12HO$. If this formula is calculated for 3 equivs. sulphuric acid it would be $\frac{3}{2}Al_2O_3\,3SO_3,18HO$, and if compared with the monobasic, bibasic, and tribasic sulphates it would be sesquibasic salt. It occurs in well-defined crystals, rhombohedra, sometimes simple and sometimes terminating in four-sided pyramids. It dissolves readily in hot and cold water. It is almost neutral to litmus paper, and is without action upon ultramarine. It contains 21 per cent of alumina, that is twice as much as alum and one half more than the ordinary sulphate. The new compound is the tenth simple aluminium sulphate which has been obtained and analysed.

Action of Chlorine upon Chromium Sesquioxide.—H. Moissan.—The author has examined the behaviour of chromium sesquioxide in a current of chlorine gas, dry or moist. If the oxide has been previously ignited there is no action. If the hydrated sesquioxide is employed, and heated progressively to 440°, vapours of chloro-chromic acid are evolved. The sesquioxide in a current of oxygen behaves with a corresponding difference in the reaction. Chromium sesquioxide is the type of the oxides in which a change of properties coincides with a disengagement of heat.

A Compound of Allylic Alcohol with Anhydrous Baryta.—MM. C. Vincent and Delachanal.—The authors show that allylic alcohol forms with baryta a compound analogous to those formed by the other alcohols, and exceedingly instable.

Fixity of the Composition of Plants: Relation between Starch, Phosphoric Acid, and Mineral Substances in the Potato.—H. Pellet.—The author concludes that there is a constant ratio between the total phosphoric acid contained in the entire plant and the starch. There is also a constant ratio between the starch and the total mineral matter absorbed, less silica. There are great differences in the proportions of lime and potash with relation to 100 parts of starch. There is an equivalent substitution of these alkalies, so that the quantity of sulphuric acid required to saturate all the bases is approximately the same. Silica and nitrogen range between wide limits.

Analysis of Beet-root Seed.—H. Pellet and M. Liebschutz.—The results of the analyses are given in a lengthy table.

Disinfection and Preservation for Agricultural Purposes, and especially of Blood, by means of Aluminium Bisulphate and Nitric Acid.—E. Vautelet.—The author adds to the blood, &c., sulphate of alumina, sulphuric acid, and nitric acid. He considers that on adding sulphuric acid to sulphate of alumina there is formed a bisulphate less soluble than the sulphate, which quickly determines a perfect coagulation of the blood.

No. 24, June 14, 1880.

On Papaine: A Contribution to the History of Soluble Ferments.—A. Wurtz.—An account of the composition and behaviour of papaine, the active principle of the juice of *Carica papaya*. As regards its action upon albuminoid matter papaine approaches the pancreatic ferment which has been studied and named trypsine by M. Kühne. Papaine dissolves fibrine in presence of prussic and boric acids and of phenol, *i.e*, under conditions which exclude the formation and agency of microbia. The author has also extracted from Carica juice a saponifiable fatty matter and a crystalline nitrogenous principle.

M. Stas has been elected, by a large majority, corresponding member of the Academy for the Chemical Section in place of the late M. Zinin.

Experimental Researches on Rotatory Magnetic Polarisation in Gases.—H. Becquerel.—The author has studied five gases—oxygen, nitrogen, carbonic acid, nitrous oxide, and olefiant gas. In all these, except oxygen, the author finds that the magnetic rotations of the planes of polarisation of rays of different wave-lengths are almost inversely as the square of the wave-length, as is the case with non-magnetic bodies, solid and liquid. Oxygen gives for the red rays a rotation very little superior to that of the green rays. It might be supposed to be a mixture of two bodies, the one magnetic and the other diamagnetic.

Constancy of the Proportion of Carbonic Acid in the Air.—T. Schlœsing.—The author considers that the sea exercises a regulating action upon the proportion of carbonic acid present in the atmosphere as well as upon that of watery vapour and ammonia. It is admitted that the sea if extended in a vertical layer over the whole surface of the globe would have a depth of 1000 metres. The quantity of carbonic acid contained in a vertical prism of this stratum, having a base of 1 square metre, is 98·3 kilos. These 98·3 kilos. forming bicarbonates, half the quantity, 49 kilos., is retained by the bases. Supposing that the atmosphere has a uniform composition, and contains in volume 2-10,000ths of carbonic acid, a vertical prism of this atmosphere with a base of 1 square metre would contain only 4·7 kilos. of carbonic acid. The sea thus holds in reserve a quantity of carbonic acid available for exchange with the air ten times greater than the total quantity contained in the latter, and, *a fortiori*, very much greater than the variations of such quantity.

The Tensions of Saturated Vapours have Different Modes of Variation, according as they are emitted above or below the Melting-point.—Paul de Mondesir.—This paper is not adapted for useful abridgment.

Action of Methyl Iodide and Bromide upon Monomethylamine.—E. Duvillier and A. Buisine.—The action of methyl bromide yields tetra-methyl-ammonium bromide. That of the iodide is similar. As the most advantageous source of dimethylamine, the authors recommend the trimethylamine of commerce.

Transformation of Terebenthen into Cymen.—M. Bruère.—Terebenthen and ethyl sulphate form an unstable compound, which, if decomposed by heat, yields cymen and ether.

Preparation of Indoline and of its Compounds.—E. Giraud.—Flavindine is dissolved in a very dilute solution of caustic soda and is placed in contact for two or three days with sodium amalgam. A dirty yellow powder is deposited, which is freed from excess of soda by washing in water, dissolved in alcohol, re-precipitated with excess of water, and washed.

Justus Liebig's Annalen der Chemie,
Band 201, Heft 2 and 3.

Communications from the Laboratory of the University of Würzburg.—These communications comprise a paper by G. H. U. Harrow on the products of the decomposition of diacet-succinic ester by sulphuric acid, *i.e.*, carbo-pyro-tritaric acid and pyro-tritaric acid, and a memoir by W. R. Hodgkinson on the action of sodium upon isobutyric benzyl-ester.

Certain Ultramarine Compounds (Second Memoir).—Karl Heumann.—The author here describes the behaviour of silver ultramarine with dilute acids, with solution of sodium chloride, with soda lye, with fused haloid alkali metals, and the conversion of silver ultramarine into the potash compound. Then succeeds an account of potassium and lithium ultramarine, of blue sodium ultramarine and ammoniacal solutions of silver, green ultramarine and its behaviour with the silver salts.

The Constitution of Cinchonin and Cinchonidin.—Zd. H. Skraup.—Not suitable for abstraction.

Alizarin Blue.—C. Graebe.—The author ascribes to this compound the formula $C_{17}H_{11}NO_4$. Its formation from nitro-alizarin and glycerin depends on the simultaneous elimination of water and oxygen. The latter does not appear in the free state, and is doubtless concerned in the formation of the abundant brown secondary products. Alizarin blue is easily attacked by oxidising agents, such as nitric acid and permanganates, the chief product being phthalic acid. With reducing agents in an alkaline solution it yields a reduction-vat, analogous to the indigo-vat, from which the colouring-matter is re-precipitated with a blue colour on exposure to air. The author considers alizarin blue as an anthracen derivative, in which the hydrogen atoms of one benzol nucleus are not replaced by other elements.

Colour Bases from Furfurol.—Hugo Schiff.—The author describes the compounds which furfurol forms directly with the aromatic amine acids, such as the amido-benzoic and amido-cuminic.

Guanidin, an Oxidation Product of Albumen.—F. Lossen.—The substance obtained nearly thirty years ago by Béchamp, by the oxidation of albumen with potassium permanganate, and supposed by him to be urea, is guanidin.

Reply to the Communication of H. Kessler "On the Supposed Non-existence of Pentathionic Acid."—W. Spring.—The author contends that the phenomena adduced by Kessler in support of the existence of pentathionic acid tell in the opposite direction.

Reimann's Färber Zeitung,
Nos. 17 and 18, 1880.

These numbers contain nothing of general interest.

No. 19, 1880.

It is announced that Dr. Nölting succeeds Dr. Goppelsrœder as Professor at the Ecole de Chimie of Mulhouse.

The question is raised whether a course of two years in this institution is a sufficient training for colourists. It is suggested as an improvement that pupils before entering must produce evidence of a satisfactory proficiency in general and analytical chemistry, so that the two years may be entirely devoted to the chemistry of colour.

No. 20, 1880.

This number contains nothing of general interest.

Les Mondes, Revue Hebdomadaire des Sciences.
No. 17, Apil 22, 1880.

Considerations on the Experimental Possibility of the Dissociation of Certain Non-metallic Bodies.—Raoul Pictet.—The author proposes to submit the elementary bodies to be decomposed to the heat of the sun, concentrated by a parabolic reflector of 10 metres in diameter. The "solar chamber" for the reception of the bodies is to be made of lime or zircon.

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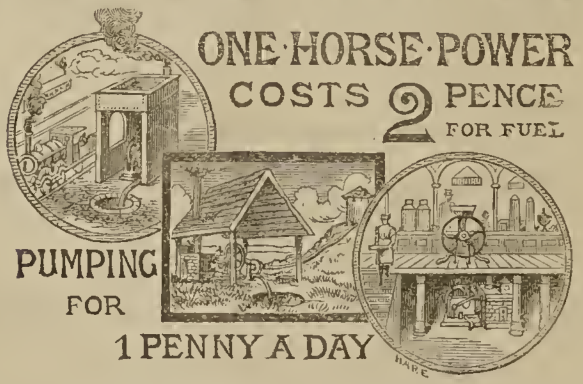

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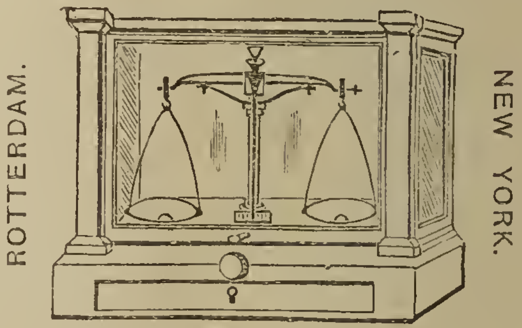

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THE CHEMICAL NEWS.

Vol. XLII. No. 1077.

DETECTION OF COTTON-SEED OIL IN ADMIXTURE WITH OLIVE.

By BENJAMIN NICKELS, F.C.S., F.I.C.

A continental firm (consumers of oils) consulted with me on a recent occasion as to the simplest method of detecting admixtures of "cotton-seed oil" with "Lucca or Gallipoli," making a strong point of the want of such a test amongst consumers. Without entering upon the admitted difficulty of the case as regards the chemical aspect of the question, it has occurred to the writer that something might be done in the direction indicated by spectroscopic examination.

Pure "olive or Gallipoli," as examined by a Browning "direct vision" or pocket instrument, presents a deep shadowing or cutting-out of the blue and violet ray, with a fine, almost indistinct, line in the green, and a strong deep band in the red.

Refined cotton-seed oil similarly examined presents exactly the same appearance, but as regards the blue and violet ray only, the green and red being continuous.

Now if we take as a standard a given stratum of pure olive or Gallipoli, say in a test-tube ⅝ or ¾ in. in diameter, and a similar stratnm or thickness of the standard oil in admixture with cotton-seed, there is no discernible difference as regards the shadowing in the blue and violet ray, but an almost entire fading out of the delicate line in the green, and a considerable diminution in the depth and intensity of the strong band in the red, consequent upon "dilution" or "thinning down." With 50 per cent in admixture the loss in intensity is considerable; with 25 per cent the variation is marked and discernible.

A suspected sample compared with, and differing thus from, the standard, and in the absence of any direct chemical evidence as to the nature of the oil in admixture, might fairly fall within the range of strong presumptive evidence pointing towards "cotton-seed" oil as the probable dilutant.

Laboratory, 104, Leadenhall Street, E.C.

A NEW LECTURE EXPERIMENT— THE CUPELLING OF GOLD AND SILVER

Mr. Holman, the Actuary of the Institute, has constructed a lantern for the oxyhydrogen light, which combines a great many advantageous qualities. It may, at a moment's notice, be changed into a vertical lantern for showing precipitations, the action of a magnet on iron filings, &c., on the screen. It may be converted into a projecting microscope in a moment, and with equal facility it becomes a megascope for projecting the image of solid objects. A course of lectures by Mr. A. E. Outerbridge, Jr., of the U.S. Mint, was recently delivered before the members of the Institute on "Coins and Coinage." By the aid of Mr. Holman's apparatus the enlarged images of rare and valuable ancient and modern coins were projected upon the screen with great sharpness and brilliancy, having the lustre and effect of relief of the coins themselves, and the lecturer was enabled to show the cupellation of gold and silver, as performed in the assaying of the precious metals at the Mint, in a very beautiful manner to the entire audience, thus opening up a new field of usefulness for the projecting lantern in illustrating lectures on metallurgy.

A little "cupel" or crucible, made of calcined bone-ash, was held in the focus of the light from the condensing lenses of the lantern, by means of a ring of thick copper wire. The image of the cupel appeared upon the screen greatly enlarged. The cupel was then heated to a white incandescence by means of an oxyhydrogen blowpipe. A weighed sample of gold alloy containing base metal was enclosed in an envelope of sheet-lead, pressed into the form of a bullet; this was dropped into the cupel and was immediately melted. As the lead became oxidised it was gradually absorbed in the cupel, forming a dark ring in the bottom. A little sheet of light was noticed moving over the surface of the molten metal as the non-oxidisable precious metal became exposed to view; then, at the moment that the lead became completely absorbed, carrying with it all the base metal originally contained in the alloy, the purified precious metal became visible as a brilliant globule, reflecting the light falling upon its surface like a mirror.—*Journal of the Franklin Institute.*

STRONG'S WATER-GAS SYSTEM.

By GEORGE S. DWIGHT.

Some two years since, the representatives of the Strong process publicly announced that it was possible to convert the best anthracite coal to a gas, which should possess a greater heating value than the fuel from which it was derived, for practical purposes. The objection was at once raised that here was an absurd attempt to extract from a given weight of carbon more calories than it contains. The charge was based upon the undeniable truth that the burning of C to CO implies the expenditure of a proportion of its heat, and hence the new form must represent less calorific energy than the old.

Prompt answer was made, admitting this last fact, but explaining that the objection was theoretic and not practical; that the comparison must not lie between the potential energy of a fuel and that of its gaseous product (except for certain limited purposes), but between their several *actual* energies. It was shown that the theoretic heating-powers of fuels are determined by delicate laboratory methods, which, however precise and valuable, are utterly unattainable by ordinary practice; that the steelmakers of Sheffield utilise only 3 per cent of the value so ascertained, consumers in the domestic industries not over 10 per cent; while the most economical use of fuel ever attained, namely, in the largest and most perfect blast-furnaces, does not exceed 36 per cent. The better result claimed for some boilers was excluded from the comparison on the apparently just ground that, as the generation of steam is specifically for the driving of engines, the utilisation of heat should be judged by the power developed rather than the weight of water evaporated, and that, so judged, the loss is as great as in other fuel applications.

Evidence was then offered that in the combustion of the Strong gas it is possible to utilise within 10 to 12 per cent of its full theoretic value, and it remained only to make a comparison of results, which was done substantially in the following manner:—

One pound of anthracite possesses a potential energy of 13,500 units of heat, of which the *actual* energy, namely, that realised in practical operation, is, as we have already seen, from 3 to 36 per cent thereof, or 405 to 4960 units of heat, according to the manner of its use. The gaseous products of 1 lb. of anthracite, by the Strong system at that time, was 22·33 cub. ft. × weight 0·04116 = 0·919 lbs., possessing a potential energy of 0·919 × 8798 = 8002 units of heat, and an actual energy of from 83 to 90 per cent thereof, or 7121 to 7290 units.

Assuming for simplicity what is, perhaps, not an unfair assumption, that the labour-cost of conversion is compensated by collateral advantages in the reduced labour of using gas-fuels, the consumer was left to decide the simple question, from which form of fuel he would realise the greater number of heat-units.

This answer on the parts of the advocates of the new system seemed intelligent and correct; at all events, it has never been controverted. Admitting it to be so, we must acknowledge also that the new method of converting carbon into superheated steam, instead of atmospheric air, represents an important advance in the art of combustion.

More recently, additional facts of great interest have been developed by further experience with the Strong furnace more perfectly constructed. The opinion that a great variety of fuels could be utilised by it has been justified by the results, and the gratifying fact established that all yield nearly identical gases, the variation being not in quality but in volume, which is consistently in proportion to the amount of carbon contained in the fuel. The special excellency of this system in rendering available certain cheap and abundant forms of fuel, heretofore deemed inferior and so neglected, such as small coals, culm and peat-dust, and which were employed in the proportion of 3 lbs to 1 lb. of the better grade, is now ascertained to have a more important significance than the utilisation of cheap material, important as that is.

The facts now to be stated will certainly revive the original objection that the method is claiming to get more out of the fuel than it contains. Let the proposition be stated in the clearest possible manner. In the earlier operations the advantage was claimed to be a practical one, shown as already explained, by a comparison between the energies actually derived, it being admitted that the potential energies of the fuel and the gas were related as 8092 is to 13,500; that is, that the loss by conversion was 5408 units of heat, equivalent to about 40 per cent of the theoretical calorific value of the fuel.

Now, it is claimed that this percentage of loss has, by greater skill and experience in applying the principle, been steadily reduced, till, in some instances; it almost disappears. In other words, the potential energies of the crude fuels and their gaseous products, as heretofore determined by scientists, are approaching so closely as to indicate that the Strong system will yet develop what is now understood to be the maximum theoretical energy of the fuel in its gas. Here are the facts:—

1. If 1 lb., say, of English coke is blown to a red heat upon the grate of the Strong furnace, and the heat of the resulting products of combustion are stored in its regenerating chambers, and used to make and superheat a proper quantum of steam, and this latter is led directly back to the coke, it is ascertained that the largest yield of gas thus far attainable is 25·79 cubic feet.

The potential energy of this gas is as follows:—

25·79 cubic feet × 0·04116 = 1·06 lbs. × 8798 = 9326 units.
The potential energy of the 1 lb. coke is 13550 units.

This, it will be observed, is an improvement upon the earlier results of 1234 units of heat per pound of fuel, and the loss by conversion is so reduced to 31 per cent of the potential energy.

2. If 1 lb. of English coke is treated as before, but the superheated steam is intercepted before coming in contact with the incandescent coke by a shower of pulverised fuel, it is possible to convert a larger weight in that form, and the yield in gas, proportionally to the purity of the dust-fuel (that is, its percentage of carbon), will be greatly increased. Take peat-powder of the following composition:—

Water	17·90
Combustible gas	20·12
Non-combustible gas	20·05
Carbon	26·87
Ash	15·00
	99·94

When 3 lbs. of this material are used, the total gas produced is 101·96 cubic feet. Deducting from this volume the 25·79 cubic feet, which, as already shown, are obtainable from the coke, leaves 76·17 cubic feet as derived from the peat, or 25·39 cubic feet of gas for each pound of peat used.

The total gas product possesses the following potential energy:—101·96 × 0·04116 = 4·196 × 8798 = 36,916 units. The potential energy of the fuels from which it was derived is:—

3 lbs. peat × 7854 = 23,562
1 ,, coke = 13,550—37,112

Showing a loss of potential energy of 196 units of heat only. This is equivalent to a loss of but 49 units of heats for each pound of fuel, as against 4224 where the coke was used alone without dust-carbon.

If the calorific value of the peat is compared directly and separately with that of its own gas, we meet a surprising result, namely:—

76·17 cubic feet × 0·04116 = 3·135 × 8798 = 27,581 units.
3 lbs. peat × 7854 = =23,562 ,,

Here the gas stands related to the fuel as 1·17 to 1, equivalent to a gain of 4019 units of heat, or 17 per cent even upon the basis of potential energies! What becomes of the law of loss in conversion? It is not disturbed, but most strikingly illustrated. The seemingly impossible result is not beyond an explanation, and one which appears to vindicate a theory advanced by the advocates of the new system, and yet to be generally admitted, namely, that in the conversion of carbon from the solid to the gaseous state there is a proportion of the energy of the fuel expended in its own gasification, which is productive of no other effect, and which has never entered into the laboratory tables concerning calorific values. In other words, that the ultimate maximum potential energy of carbon is greater than the accepted estimate.

Take, as example, the conversion of the peat just explained, the heat essential to the act was furnished entirely by the coke, exerted through the medium of the steam, and hence the carbon of the peat went directly and without reduction into union with the oxygen of the water to form gas, representing 9193 units of heat per pound. When the analyst sought to determine the calorific energy of the peat, he obtained 7854 units only. The difference of 1339 units, or a considerable proportion thereof, we must infer was expended in the gasification of the crude fuel, which, according to this theory, is not merely a part of combustion, but a pre-requisite to it. This theory is based upon the proposition that only the gases contained in fuel materials are combustible; and that whatever heat was, during the processes of nature, expended in their solidification from their original gaseous form, must be repaid to restore it and render them available in combustion.

Let us for the moment regard heat-units as a merchantable article, like any other commodity of purchase and sale, and, comparing the above figures upon this commercial basis, see in what market or in what shape we can most advantageously buy.

Assume that lump anthracite coal or coke is selling at four dollars per ton of 2240 lbs., and coal-dust or peat at one dollar for the same weight, and that we have one dollar to spend for practical heat-units, that is, for such as we actually get the value of in combustion:—(See Table on next page).

Bear in mind that the figures are *not* theoretic (except in case 4), but show the calorific values *actually obtained in practical combustion*. The prices, of course, represent the material cost of manufacturing at wholesale only, but they indicate that the gas-maker has a margin for liberal profit without being unreasonable. Naturally the gas purchaser will always determine for himself, as in any other transaction, whether the price charged is in excess of the advantages gained, into which calculation many collaterals, as to the convenience, comfort, and reduced labour attending the use of gas fuels, must enter. And he will always have the alternative of a return to crude fuels, if the gas manufacturer becomes exorbitant in his demands.

	Units of Heat.	Cost per Unit in Mills.	P.c. of Potential Energy of Fuel Utilised.
1. If we pay one dollar for lump coal, applied as in Sheffield steel-making, we obtain	226,800	0·00440	3
2. The same sum, as applied in domestic uses, gives	756,000	0·00132	10
3. The same in the most perfect blast-furnace ..	2,716,100	0·00036	36
4. One dollar's worth of lump coal, converted by the Siemens generator, will yield gas representing theoretically* ..	2,451,874	0·00040	30
5. One dollar, for lump coal or coke, converted to water-gas, as explained, and burned with 10 p.c.	4,706,894	0·00021	62
6. The same sum, expended for $\frac{1}{4}$ the lump coal or coke and $\frac{3}{4}$ peat, and converted by Strong's system, as explained, yields, after allowing 10 per cent loss	10,623,630	0·00009	89

The table represents both clearly and accurately the advantage possessed by the Strong principle, not only as compared with general methods of employing crude fuels directly, but also shows at a glance how far it stands in advance of all preceding gas methods, including even those that rely upon the conversion of carbon in an atmosphere of steam. This last remark may be emphasised in passing by the statement that careful investigation indicates that, where lump fuel is exclusively used in the decomposition of the steam, as described earlier in this paper, little or no further increase of heat-units—that is, no greater volume of gas—will be probable. A slight gain may result from a reduction of loss by radiation and a further utilisation of the sensible heat of the products of the gas furnace; but beyond these trivial gains, the process cannot apparently advance.

Not so, however, when the principle is applied to the conversion of carbon dust. The principle itself is so perfect as to admit of no improvement, and yet an increasing experience in its management may still further develop its efficiency. It is manifest, from what has been stated, that the conversion of a small additional proportion or weight of dust beyond that already successfully used will enable this system to deliver gas representing the full potential value of the fuels as at present estimated. This is a daring promise to make; but we may be trustful of a method which has hitherto kept its pledges so well, and predict a not distant fulfilment of this latest one. The application of the steam principle has had an almost immeasurable influence upon the industries of civilisation. The Strong system of conversion is certainly no less important than that, and may, indeed, be said to be the development of that principle to a yet grander plan of beneficent usefulness.—*The Engineering and Mining Journal*, May 29, 1880.

Stockholm, February 23, 1880.

Chemical Society.—Dr. J. H Gladstone, F.R.S., has presented £100 to the Research Fund of the Chemical Society.

CONTRIBUTIONS FROM THE CHEMICAL LABORATORY OF THE UNIVERSITY OF MICHIGAN.*

By ALBERT B. PRESCOTT.

I. ALUMINATES OF POTASSIUM AND SODIUM IN SOLUTION.

THE composition of certain solid aluminates of metals of the alkalies and alkaline earths was well established long ago. Also, there are recorded observations of the action of water upon alkali aluminates, either effecting complete solution, or partial dissociation. Not finding statements of the proportions of potassium hydrate and sodium hydrate necessary to dissolve precipitated aluminium hydrate, the writer instituted determinations of these data, in order to show the atomic ratio of K and Na, to Al, in the aluminate solutions with different quantities of water, in the conditions of re-dissolved precipitate so common in chemical operations.†

Potassium Aluminate.

Two solutions of aluminium salt were used, one a decinormal solution of potassium aluminium sulphate, $K_2Al_2(SO_4)_4.24aq.$; the other, one-third stronger, or tenth of $1\frac{1}{3}$ normal solution of the same salt—this being a nearly saturated solution. These aluminium salts solutions were treated respectively with normal and decinormal solutions of potassium hydrate, KOH, until the precipitate at first formed had just all dissolved. The determinations were made at 30° C., then at 50°, and then at 70°; and each determination was made by triplicate trial. The three trials, accepted for each determination, did not vary one from another by as much as two-tenths cubic centimetre. The trials at the different temperatures gave the same results. When the results were attained, that the aluminium precipitate had just dissolved under the continued addition of the potassium hydrate, it was found that the addition of the first drop of decinormal solution of sulphuric acid, in all trials, reproduced the precipitate. In most cases, the quantity of aluminium salt solution taken was 5 c.c. It was the single result, under all the conditions of dilution and temperature named, that *1 c.c. of a decinormal aluminium salt solution required 8 c.c. of decinormal potassium hydrate solution.* (Thus, 5 c.c. of decinormal Al_2 solution took 40 c.c. decinormal K solution, and took 5 c.c. normal K solution; and 5 c.c. tenth of $1\frac{1}{3}$ normal Al_2 solution required 53·5 c.c of decinormal K solution, and again 5·4 c.c. of normal K solution). This gives the proportion of potassium to aluminium, in the soluble aluminate—

$$K_2Al_2O_4, \text{ or } Al_2\left\{\begin{array}{l}O\\O\\(OK)_2\end{array}\right. \qquad \text{And the equation is:}$$

$$K_2Al_2(SO_4)_4+8KOH=K_2Al_2O_4+4K_2SO_4+4H_2O.$$

Calculation gives the following as the number of parts of water (*i.e.*, water solution of potassium sulphate) present for one part of the soluble potassium aluminate, in the several degrees of dilution:

			Solution.
With Al_2	tenth of $1\frac{1}{3}$ normal, and	K normal	79 parts.
,,	decinormal,	,, ,,	91 ,,
,,	tenth of $1\frac{1}{3}$ normal,	,, decinor.	445 ,,
,,	decinormal,	,, ,,	456 ,,

Over fifty years ago the compound $K_2OAl_2O_3(K_2Al_2O_4)$ was obtained by saturating potassium hydrate solution with aluminium hydrate, evaporating to a small bulk, and removing all excess of alkali by alcohol.‡ Fremy obtained crystals of $K_2Al_2O_4.2aq.$, after fusing the alumina-saturated potash residue.§ It was stated by Fremy that the di-

---

* In this last computation, the coal is credited with three times the volume of gas, namely 150,000 cubic feet per ton, stated by Percy in *Metallurgy* (p. 528). But as recent investigations prove this gas can only be burned with great loss, if used without the regenerative system, and to be entirely inefficient for general distribution, only its theoretic value is given in this table.

* From the *Journal of the American Chemical Society*, vol. ii.

† These operations for determination of potassium aluminate and sodium aluminate, were entrusted to the execution of Mr. J. N. Ayres.

‡ *Pogg. Annalen*, 7, 723; "Gmelin's Handbook," 3, 320.

§ *Ann. Chem. Phys.* (3), 12, 362 (1846); *Comptes Rendus*, 15, 1106.

potassium aluminate, $K_2Al_2O_4$, is decomposed by much water, precipitating aluminium hydrate, and leaving in solution an aluminate probably $K_6Al_2O_6$. The symmetry of the compound last named, $Al_2(OK)_6$, may have had some influence in its frequent adoption, to represent the potassium hydrate solution of alumina. In nature, the form $R''Al_2O_4$ is represented by the spinels; and dibasic aluminates are formed by precipitating alkali aluminate solutions with barium or calcium hydrate solution, as is well known.

Sodium Aluminate.

Trials were made with decinormal and normal solutions of sodium hydrate, corresponding in all particulars to the trials above described for potassium aluminate. The results were not different in any particular. 1 c.c. of the decinormal aluminium solution, or its equivalent, gave a precipitate exactly dissolving on addittion of 8 c.c. of decinormal sodium hydrate solution, or its equivalent. Therefore, the soluble sodium aluminate, in the stated conditions of dilution and temperature, is a disodium dialuminium tetroxide,—

$$Al_2\left\{\begin{array}{l}O\\O\\(ONa)_2\end{array}\right.$$

The equation will be:—

$$K_2Al_2(SO_4)_4+8NaOH=$$
$$=Na_2Al_2O_4+K_2SO_4+3Na_2SO_4+4H_2O.$$

The greatest dilution was 1 part of the soluble sodium aluminate in 546 parts of solution; the least dilution, 1 in about 95 parts solution.

Fremy did not obtain crystals of the sodium aluminate. Tissier[*] obtained four different combinations, $Na_2Al_2O_4$, $Na_6Al_4O_9$, $Na_4Al_2O_5$, and $Na_6Al_2O_6$—all in some degree soluble in water, the last-named yielding sodium hydrate to alcohol. Schaffgotsch[†] found that one molecule of aluminium oxide, fused with sodium carbonate, expels rather more than one molecule of carbon dioxide.

II. ZINC OXIDE IN ALKALINE SOLUTIONS.

In 1834, Laux[‡] obtained crystallised potassium zinc oxide of the symmetric composition, $K_2O.ZnO$. The crystals were soluble in cold water; but boiling water caused precipitation of $K_2O(ZnO)_2$. Fremy[§] obtained crystals of $K_2O(ZnO)_2$, these being decomposed by water.

Ammonium zinc oxide was crystallised by Malaguti, as he reported,[‖] with nearly the composition,

$$NH_3.ZnO.3H_2O\ [NH_4OH.Zn(OH)_2.H_2O;\ \text{or}$$
$$Zn(OH)(NH_2)3H_2O].$$

Weyl,[¶] by dissolving zinc in aqueous ammonia, in contact with iron, obtained the product, $(NH_3)_2ZnO$. This differs only by a molecule of water from zinc diamine, $(NH_2)_2Zn$. Neither the product of Malaguti, nor that of Weyl, appears to have been wholly soluble in water, without the help of ammonia.

I have obtained some determinations[**] of the quantities of potassium hydrate, sodium hydrate, and ammonium hydrate, respectively required to re-dissolve their precipitates in solution of zinc salt, in certain conditions of dilution and temperature. It was found that, in all conditions, some excess of the alkali needed to dissolve the precipitate, could be neutralised by an acid before the precipitate reappeared.

At 17° C., each c.c. of a normal solution of zinc sulphate, required 8 c.c. of a normal solution of *Potassium hydrate*, to re-dissolve the precipitate at first formed. (In four trials, the numbers were, of c.c., 8·0, 8·1, 8·0, and 8·0.) In taking up the excess of alkali, with semi-normal solution of sulphuric acid (each c.c. equivalent to a c.c. of the normal solution of potassium hydrate), it was found that 4 c.c. could be added before causing a precipitate. So that 4 c.c. of the KOH solution were required to retain in solution the zinc from 1 c.c. of the $ZnSO_4$ solution. The titrations with alkali and acid, then, sustain the following equations:—

$$\text{(1.)}\ ZnSO_4+8KOH=K_2OZnO+4KOH+K_2SO_4+2H_2O.$$
$$\text{(2.)}\ K_2OZnO+4KOH+2H_2SO_4=$$
$$=K_2OZnO+2K_2SO_4+4H_2O.$$

The solution with excess of alkali (not titrated back with sulphuric acid) was precipitated on adding about twelve times its volume of water. A decinormal solution of zinc sulphate required, for each 10 c.c., an average of 12·6 of normal solution of potassium hydrate, to re-dissolve the precipitate; or about fifty per cent more, by reason of the dilution of the zinc solution from normal to decinormal. Decinormal solution of potassium hydrate would not re-dissolve the precipitate which it produced in normal solution of the zinc salt, however much was added. A solution of 16·5 grms. potassium hydrate to the litre—

$$\left(\frac{16{\cdot}5}{56{\cdot}1}\ \text{normal}\right)$$

was the weakest solution that would at all re-dissolve a precipitate in the normal zinc solution. A stronger solution of potassium hydrate, 121·121 grms. to the litre, was tried, and 3·8 c.c. of this, equivalent to 8·2 c.c. of normal solution, were required to dissolve the precipitate from 1 c.c. normal solution of zinc sulphate. So it appears that dilution, as far as the "normal" standard, does not lessen the solubility of the precipitate in aqueous alkali; beyond the strength of the normal solution, dilution rapidly diminishes the solubility.

The above trials were all made at 16° to 17° C. In trials at 50° C., about three times as much of the potassium hydrate solution was required to dissolve the precipitate, as at 17°, and this was true with both normal and decinormal zinc solutions. It is well known that boiling precipitates zinc from its alkali hydrate solutions.

The sodium hydrate solution was prepared from metallic sodium and adjusted to normal strength. Of this normal solution at 17° C., 7 c.c. were needed to dissolve the precipitate made with 1 c.c. zinc sulphate normal solution. Then 3 c.c. of half normal sulphuric acid solution were added, before a precipitate resulted. Subtracting the sodium hydrate neutralised by the acid, short of re-precipitation, we have this equation (corresponding to the result with potassium hydrate)—

$$ZnSO_4+4NaOH=Na_2OZnO+2Na_2SO_4+2H_2O.$$

The solution bears dilution with 10·5 times its volume of water before precipitation, the trial being at 17° C. At 50° C. each c.c. of the normal solution of zinc sulphate took 18 c.c., and each 10 c.c. of the decinormal zinc solution took 27 c.c. of the decinormal sodium solution to re-dissolve the precipitate. It appears, then, that solution, under different conditions, requires somewhat less proportional excess of sodium hydrate than of potassium hydrate.

The normal solution of *ammonium hydrate* was adjusted in the burette, which was kept corked as much as possible. After the work the solution was again tested, and found not to have lost strength to an appreciable extent. Each c.c. of the normal solution of zinc sulphate took 6·6 c.c. of the normal solution of ammonia. (In four trials the results were, 6·6, 6·5, 6·6, 6·6.) Of the seminormal solution of sulphuric acid, 1·6 c.c. were added in reaching the point of precipitation. So 5 c.c. of the ammonia solution remain united with the zinc oxide and sulphuric acid of 1 c.c. of the zinc solution, indicating the equation—

$$2ZnSO_4+10NH_4OH=$$
$$3(NH_4)_2O2ZnO+2(NH_4)_2SO_4+5H_2O.$$

The solution can be diluted with nine times its volume of water before precipitation. 10 c.c. of decinormal solution of zinc sulphate required an average of 10 c.c. of

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[*] *Comptes Rendus*, 48, 627; *Jahresb. d. Chem.*, 1859, 143.

[†] *Pogg. Annalen*, 43, 117; "Gmelin's Handbook," 3, 320.

[‡] *Ann. der Chem. und Pharm.*, 9, 165.

[§] *Comptes Rendus*, 15, 1106 (1846).

[‖] *Comptes Rendus*, 62, 413 (1866).

[¶] *Jahresb. d. Chem.*, 1864, 165; *Pogg. Ann.*, 123, 353.

[**] These estimations were made by Mr. F. S. Wilson, under my direction.

normal ammonia solution to re-dissolve the precipitate. At 50° C. no more of the ammonia was required to re-dissolve the precipitate than at 17° C., a marked difference from the results with the fixed alkalies. It will be observed, a much smaller proportional excess of ammonium hydrate than of potassium or sodium hydrate is required to dissolve the precipitate.

We have, then, apparently, the following alkali-zinc oxides, intact in water solutions not too dilute:—

Di-potassium zinc oxide, of a chemical identity sustained by crystalline separation:—

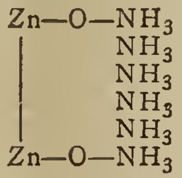
$$Zn\left\{\begin{matrix}OK\\ OK\end{matrix}\right..$$

Di-sodium zinc oxide, less certainly individualised:—

$$Zn\left\{\begin{matrix}ONa\\ ONa\end{matrix}\right..$$

Di-zinc-hexammonium dioxide, inferred from the constituents of its solution:—

$$\left.\begin{matrix}3(NH_4)_2O.2ZnO=\\ (NH_3)_6(ZnO)_2+3H_2O\end{matrix}\right\}$$



$$\begin{array}{l}Zn\text{—}O\text{—}NH_3\\ \quad\quad\quad\; NH_3\\ \quad\quad\quad\; NH_3\\ \quad\quad\quad\; NH_3\\ \quad\quad\quad\; NH_3\\ Zn\text{—}O\text{—}NH_3\end{array}$$

III. Silver-Ammonium Oxide in Solution.

The proportion of ammonium hydrate required to dissolve the precipitate it first forms in silver nitrate solution, at various degrees of dilution and temperature, was carefully determined.* The results show that pretty nearly two molecules of ammonium hydrate are, in every case, required for one molecule of silver nitrate, in attaining solution of the precipitate. Conditions of dilution and temperatures do not materially influence the result. The variations in results were greater than those found with corresponding determinations for fixed alkalies, possibly owing to vaporisation of ammonia, in spite of precautions. The detailed results are given below, as well as the particulars of the work. It is impossible to say how much influence is exercised by the ammonium nitrate in dissolving the silver oxide. If this influence be disregarded, the action of the ammonia is represented by the following equation:—

$$2AgNO_3+4NH_4OH=(NH_3Ag)_2O+2NH_4NO_3+3H_2O.$$

The hypothesis of silver-ammonium oxide,—

$$\left.\begin{matrix}NH_3Ag\\ NH_3Ag\end{matrix}\right\}O,$$

is adopted as more in accord with known compounds than the silver and ammonium oxide, NH_4AgO, or silver-ammonium hydrate, NH_3AgOH.

The solutions of silver nitrate used were the normal and decinormal. The ammonia solutions were of indefinite strength, one near normal and one near decinormal, and their exact strength was found after each trial, or set of trials, by titrating with solutions of oxalic acid, normal and decinormal. The burette was kept closed as far as possible by rubber tube and pinch at the top. Five beakers were set, each with 1 c.c. of the normal solution of silver nitrate, and one beaker for titration with normal solution of oxalic acid. In the first estimation there were required to dissolve the silver precipitate, respectively, 1·6, 1·7, 1·5, 1·6 c.c. of the nearly normal solution of ammonia, and 5 c.c. of the ammonia solution required 6·5 c.c. of normal solution of oxalic acid. Then—

$$5{\cdot}0 : 6{\cdot}5 :: 1{\cdot}6 \text{ (average of five)} : x = 2{\cdot}08.$$

In like manner other estimations were made, mostly starting with 2 c.c. silver solution, giving results as follows:—

			C.c.				C.c.		
1.	5	trials	1	normal	Ag sol.	requires	2·08	normal	NH_3 sol.
2.	5	,,	1	,,	,,	,,	1·95	,,	,,
3.	5	,,	1	,,	,,	,,	1·90	,,	,,
4.	5	,,	1	,,	,,	,,	1·95	,,	,,
5.	5	,,	1	decinormal	,,	,,	2·23	decinormal	,,
6.	5	,,	1	,,	,,	,,	2·24	,,	,,

The 1st set of trials were made at 30° C.; the 2nd, at 23° C.; the 3rd, at 24° C.; the 4th, at 23° C.; the 5th, at 30° C.; the 6th, not noted.

The silver-ammonium solutions were not precipitated by normal or decinormal solution of nitric acid at any point of its addition, and were not precipitated by decinormal solution of sulphuric acid until the neutral reaction was reached.

PROCEEDINGS OF SOCIETIES.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, April 6, 1880.

J. P. Joule, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

E. W. Binney, F.R.S., F.G.S., said that in vols. xvi. and xviii. of the *Proceedings* of the Society he had given accounts of a *Eucalyptus globulus* which he had planted in his garden near the sea at Douglas, Isle of Man. During the winter of 1878 and 1879 it suffered in its foliage and young branches to a considerable extent; but during the past winter, although the temperature of the month of December, in the Isle of Man, was lower than that of the same month in 1878, the tree has almost escaped damage, and is at the present time growing vigorously and giving out a strong odour throughout the surrounding air. It has not grown much in height during the last two years, as it is now considerably higher than the sea wall near to which it grows, but it has much increased in the diameter of its stem. Up to this time it has shown no signs of flowering.

"*Note on modified Chlorophyll from the Leaves of* Eucalyptus globulus," by Edward Schunck, Ph.D., F.R.S.

Whoever has seen the *Eucalyptus globulus* growing must have been struck with the peculiar glaucous appearance of the foliage, such as few European plants show. I thought it would be of interest to ascertain whether this peculiar appearance might be in any degree due to the state in which the chlorophyll exists in the leaves. A very simple experiment, however, sufficed to prove that the peculiar appearance referred to is owing to a covering of fatty matter, such as is seen on fresh plums and other fruit, which, though exceedingly thin, is sufficient to modify the green colour of the leaf. On washing the leaves with a little ether the film of fatty matter disappears instantly, the leaves then appearing green like ordinary leaves. The ether leaves on evaporation a white semi-crystalline fatty residue, which melts at a temperature much lower than that of boiling water, and is partly soluble in dilute alkaline lye boiling, so that it probably consists in part of some fatty acid. The leaves after washing with ether do not differ in appearance from other leaves. The alcoholic and ethereal extracts of these leaves show, however, a peculiarity, as regards the chlorophyll contained in them, which I have not observed in any other green leaf extract, though the same thing may have been observed by others and recorded in one of the numerous memoirs on chlorophyll, the whole of which I do not profess to have read. If a few of the smallest and latest-formed Eucalyptus leaves from the tips of the branches are extracted with ether a green solution is obtained which

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* All of these determinations were carried out with much care and discrimination by Mr. D. E. Osborne.

show the usual absorption-bands of ordinary unchanged chlorophyll. If, however, a little of the extract contained in a tightly-corked test-tube be kept in a dark cupboard for several days, it gradually acquires a yellowish tint, and now shows absorption-bands coinciding with those of so-called "acid chlorophyll," that is, of the modification which is produced at once by adding a few drops of an acid, such as acetic acid, to a solution of ordinary chlorophyll. The change in the spectrum produced by the action of acids consists in the disappearance of the chlorophyll band III., and the intensification of bands II. and IV., which now more nearly approach band I. in strength. An alcoholic extract of more fully developed, but still quite fresh and vigorous leaves (I took for the purpose the pair next in order of development to those at the summit of the branch), showed the ordinary chlorophyll bands, though on attentive examination band IV. was found to be a little more defined than usual. After being kept in the dark, however, for twenty-four hours, the extract became yellowish, and now showed the bands of "acid chlorophyll" as distinctly as did the ethereal extract after several days. An alcoholic extract of fresh grass after being kept in the dark for several days had not changed in the least, and still showed the ordinary chlorophyll bands. On exposing the extract of grass for a few hours to sunlight it gradually lost its green colour, became of a pale yellow, and then showed hardly a trace of absorption-bands, the band I. being only just discernible. On exposing the alcoholic Eucalyptus extract, after being kept in the dark for twenty-four hours, to sunlight, it also became much paler in colour; but the bands I., II., and IV. *a* were quite as distinct as before insolation, and in addition to these the broad band IV. *b*, between the lines E and F of the spectrum, which also belongs to the so-called "acid chlorophyll," now came out very neatly, it having been previously invisible on account of the great obscurity in that part of the spectrum. It appears, then, that an alcoholic or ethereal extract of Eucalyptus leaves undergoes, even in the absence of light, a change whereby the normal chlorophyll contained in it is converted into a substance which shows the same absorption-bands as, and is perhaps identical with, "acid chlorophyll." I am inclined to attribute the change which takes place to the large amount of essential oil contained in the leaves, and consequently in the extracts. Essential oils, it is well known, convert inactive oxygen into ozone, and ozone, according to Gerland, produces a change in alcoholic solutions of chlorophyll similar to the one I have described, whilst under the same conditions ordinary oxygen is without effect, which Sachsse seeks to explain by supposing that the ozone in Gerland's experiments led to the formation of some organic acid which reacted on and modified the chlorophyll. Fresh Eucalyptus leaves contain no acid soluble in water, for a watery decoction of the leaves does not redden blue litmus-paper, and remains neutral even on exposure to the air for some days; but the alcoholic extract shows after insolation a marked acid reaction. It would be interesting to ascertain whether alcoholic extracts of other leaves containing much essential oil behave in the same manner as Eucalyptus extract. I made the experiment with leaves from the orange tree, in which, as in Eucalyptus leaves, numerous oil-cells may be seen under a lens; but the alcoholic extract, on being kept for several days in the dark, remained unchanged. After insolation it differed slightly from an extract of grass made at the same time, and exposed along with it to sunlight, bands I. and IV. *a* remaining visible, while the corresponding bands of the grass extract had disappeared.

"*On the Chemical Composition of the Ink on Letters and Documents as Evidence in Legal Cases,*" by WILLIAM THOMSON, F.R.S.E.

The ideas which I propose to bring before you are not entirely new. They are based on the examination of the ink on letters and documents as a valuable mode of investigation in civil and criminal law cases.

It frequently happens that circumstantial evidence of a very simple character, which is often overlooked, might occasionally have the effect of conclusively proving the innocence or guilt of an accused person, or of pointing in some definite direction towards tracing the culprit. In civil legal cases the same class of evidence may prove equally useful.

If, for instance, a person be murdered on the highway, and any weapon or instrument found with which the deed had been committed, it is needless to say that such implement would be carefully examined for name, mark, or number, or in fact any peculiarity by which it may be traced to its former owner. Again, recent foot-prints in the snow or in soft clay often present sufficient individuality about them to make them useful in the detection of crime, and these means are usually employed. Seldom, however, is it supposed that a common substance such as the ink used in writing a letter or document has any special individuality about it. All ordinary inks are black or nearly so, yet it is conceivable that the name and address of the writer of an anonymous letter may, under a given combination of circumstances, be contained within the black fluid with which the letter was written. If, then, a murderer has left his trace behind him in the shape of paper and ink, it seems to me not improbable that, by a judicious use of the latter, some important information may be obtained respecting him which may ultimately lead to his capture. Some years ago the information obtainable by the examination of the ink on papers or documents would probably have been comparatively insignificant, owing to the fact that the number of different kinds of ink in the market at that time would be small, and the modes of preparation of the ink simple, and the materials used not specially subject to variation. At the present day, however, a large number of different articles, many of which are subject to variation, are employed in the manufacture of ink, and a large number of different inks are in general use; and from these reasons the testimony capable of being obtained by the chemical examination of the ink on letters or documents at the present time may, in some cases, prove to be of the greatest importance.

The reagents which I have found to act best in the testing of the ink on papers are nine, viz.—

1. Dilute sulphuric acid.
2. Strong hydrochloric acid.
3. Slightly diluted nitric acid.
4. Sulphurous acid solution.
5. Caustic soda solution.
6. Cold saturated solution of oxalic acid.
7. Solution of bleaching-powder.
8. Solution of protochloride of tin.
9. Solution of perchloride of tin.

The method which I have adopted in applying these reagents is to moisten one or more strokes or letters of the writing with each reagent, and then to absorb by blotting-paper the excess of fluid a few seconds afterwards.

By thus treating the ink on different envelopes lately sent to me I find they give very diverse results: with sulphuric acid, for instance, the black colours of the different inks are changed, in some to bright crimson, in others to deep red, whilst some become blue, green, violet, and grey of different shades, and some remain practically unaltered; and when, as sometimes happens, the same or nearly the same colours are produced in two different inks by one reagent, the colours produced by another are very different, thus showing clearly that the letters were not written by the same inks.

The same kind of ink, sold by the same maker, but made at different times, also varies more or less in its behaviour with the reagents, as shown by the comparisons of three samples of Lyon's ink, sold in small penny bottles, which were bought at different places.

Differences between inks which give nearly the same reactions may sometimes be observed by noticing the lengths of time which the reagents require to bring about the ultimate changes, and by the shades of colour through which the ink passes after applying each reagent. Changes also continue to go on gradually for days and weeks after the reagents have been applied. The colours and shades of the same colour can be much more distinctly seen by the aid of a good pocket lens.

It is evident that if two persons use ink made by the same maker, at the same time, the reactions would be precisely similar; but it is easy to understand that after such ink has been in use for some time, owing to the different habits of the users, each may acquire a distinct individuality. One, for instance, may have been more exposed to the air or the direct sunlight than the other, and some of the colouring matters present may thus have been more or less altered or destroyed. One person may have a habit of leaving his steel pen in the fluid, so that some of the iron may be dissolved, thus altering the character of the ink, whilst the other may not do so, or may employ a quill in writing. Again, some persons may allow their inks to dry up to a certain extent, and then add to them any fluid which may be at hand, such as tea, coffee, wine, beer, water, &c., each of which would alter the character of the writing fluid, whilst others may use mixtures of two or more different inks, in different and characteristic proportions. One can therefore understand that many persons may have in their ink-bottles fluids which are so peculiar in chemical composition that they may have as much individuality about them, when treated with reagents, as the faces of their owners. I have tested the same ink on different kinds of paper, and the resulting shades of colour produced were identical in each case.

To make use of this mode of investigation it would be necessary to get the ink or inks used by a suspected person, or preferably some writing made by him at or about the same time as any letter or document in question, and test the two side by side with each other. The resulting shades of colours may agree precisely, and may thus tell strongly against the suspected person, or they may differ very much, and so point towards exonerating him.

A case lately occurred in which the expert M. Chabot was called, and gave evidence to the effect that the handwriting in a certain libellous letter was that of the person who was indicted as the writer of it. As a witness for the defence, another person came forward and swore that he was the writer of the letter in question, and on that evidence the case was dismissed. One can, however, under some circumstances, understand that a suspicion of such a witness having perjured himself may be justifiable, and such a suspicion may possibly be removed from an innocent person by his producing for chemical examination a paper written about the same time and with the same ink as that said to have been used in writing the letter in question.

I have arranged on two sheets of paper the writings on envelopes of fifty different persons, lately sent to me. One sheet contains twenty-four, written in Manchester and the suburbs, and the other contains twenty-six, from London and the provinces; and from a minute inspection of these it will be observed that most of them are very different from each other, whilst no two give exactly similar shades of colour with all the different reagents.

NOTICES OF BOOKS.

An Introduction to the Study of Chemistry, specially designed for Medical and Pharmaceutical Students. By A. P. Luff. London. J. and A. Churchill.

We have here a work which, as we are informed in the preface, is "not a manual nor even a manualette of chemistry," but is intended for study "previous to working at a manual of chemistry." The author's object has been to give "the elementary principles of the science, the exposition of its laws, the *modus operandi* in the construction of chemical formulæ, and the writing out of equations." Accordingly he gives an account of the three forms of matter, of latent heat, of the distinction between mixtures and chemical compounds, and the characters of chemical attraction. He then passes over to the elements and their compounds, to metals and non-metals, the physical conditions of the elements, symbols, the atomic theory, atomic weights, the hypothesis of Avogadro and Ampère, atomicity, &c. All these subjects are discussed briefly, of course, but clearly and in such a manner that no justifiable exception can be taken to the expositions given. It would indeed be strange if in treating subjects upon which there is such general accord amongst chemists a man of Mr. Luff's standing and experience had been guilty of any inaccuracy or error; but without seeking to draw any invidious comparisons, it seems to us that we have seen all these questions dealt with quite as simply, quite as clearly and accurately, in some of the very numerous chemical manuals and manualettes which are in the hands of students. In the author's concluding advice to his readers he remarks, very truly—"An elementary knowledge of the various branches of physics is indispensable as a preliminary to the study of chemistry; their exposition in this work was, however, *rendered unnecessary by the numerous books on physics already written.*" Does not the author, in the words we have italicised, substantially cut away the ground from under his own feet? For if works on physics are numerous, introductory treatises on chemistry are not less so. Indeed, without any special or exclusive reference to the work before us, we would ask professors of chemistry seriously to consider what good end is to be gained by the indefinite reproduction of books, one of which can scarcely claim any very decisive superiority over others.

Handbook of the Rational Utilisation, Recovery, and Working-up of Residual Matters of every kind. (Handbuch der rationellen Verwerthung Weidergewinnung und Verarbeitung von Abfall stoffen jeder Art.) By Dr. Theodor Koller. With 22 Illustrations. Vienna, Pesth, and Leipzig: Hartleben.

The utilisation of waste products has been for years a favourite subject with technologists, especially since coal-tar has proved itself under judicious scientific treatment such a mine of valuable products. The appearance of a work like the present is therefore quite in accordance with the spirit of the age, especially as there prevail very crude notions as to what may and what may not be profitably extracted from refuse. The author has attempted to select from a superfluity of materials only such processes as the practical man may apply with advantage—a task which he recognises as difficult. Such a work, to be really valuable, should contain nothing but what will readily approve itself on a working scale, and nothing which is already thoroughly familiar to manufacturers and technologists. We are bound, however, to declare that the book before us sins greatly in the second respect. The first section on the production of ammonia contains scarcely a trace of what is not well known to manufacturing chemists. The utilisation of the residues from the preparation of coal-tar colours—a subject on which the reader might expect important instructions—is dispatched in three-and-a-half small pages, two-and-a-half of which are devoted to the recovery of arsenic from magenta residues. Little more satisfactory is the treatment of brewery residues. Save the yeast, which is to be converted into cakes for sale to bakers, the advice given is to employ the refuse for feeding cattle and for composts—all which, we submit, is already in general practice.

The section on blood and the other waste-products of slaughter-houses is very much more satisfactory. Dr. Koller describes the preparation of blood-albumen, blood-charcoal, Dippel's animal oil, butyric acid and its derivatives, fatty matter, glue, Campe's "blood-poudrette" (obtained from the clot after removal of the albumen), does not seem to us a very happy mixture. To the pulverised clot are added dried human excrement and ground "knoppern"—an inferior kind of gall-nuts, from which the tannin has doubtless been already extracted. Why not use superphosphate? The author points out that dried blood as a plastic material has been superseded by certain preparations of cellulose. It may, perhaps, slightly nauseate the public to learn that certain admired artificial fruit-flavours—such as pineapple ether, apricot-ether, strawberry-ether, &c.—derive their origin from the residues of the slaughter-house.

The shells of the cacao-bean are recommended as the source of a brown pigment. We fear that in England they generally find their way into the stomach of the public in the guise of cocoa and chocolate.

The uses to which iron slags may be put are the preparation of slag-wool, bricks, glass, and certain salts, including sulphate of alumina.

Sewage is very slightly touched upon, but one of the processes of General Scott is described in such a manner that he appears as having devised the process of treatment with lime along with salts of iron and aluminium.

Under the title "Utilisation of Waste Matters for the Production of Colours" we find an account of the so-called patent dyes of Croissant and Bretonnière, otherwise known as "cachou de Lavalle." These dyes have not proved very successful, and their manufacture has been abandoned by several firms who had taken it up.

The brief chapter on the utilisation of the waste-waters of dye works contains a notice of the patent of Higgin and Stenhouse for recovering arsenic and phosphoric acids, which have been used in dung-substitutes, and of that of Remmer's for precipitating alizarin and purpurin from the rinsings of Turkey-red works.

In the section on treacle and "vinasses" we find a notice of the recent invention of Camille Vincent, but the application of the methyl chloride obtained by his process for the extraction of perfumes from flowers has escaped mention.

The production of oxalic acid from the waste of parchment paper is regarded as hopeful, since the resulting crystals will not be contaminated with the colouring matters which are formed by the action of alkalies upon the sawdust of hard woods.

Petroleum residues as a source for the production of aniline, alizarine, &c., as shown in the researches of Lettny, are slightly touched upon.

The various known processes for recovering tin from tinners' waste are described at considerable length.

As a whole the work can scarcely be pronounced satisfactory. The statements advanced are indeed trusworthy, and there is little of that confusion which we have met with in another book on the same subject. But the various processes mentioned are, as a rule, described in a slight and sketchy manner. Much of the information given is merely obtained from well-known and public sources, such as the specifications of patents and from such works as Muspratt's "Chemistry." There are few suggestive hints and very few traces of private and special observation or inquiry in the various branches of industry concerned.

CORRESPONDENCE.

TIN IN PRESERVED FRUITS.

To the Editor of the Chemical News.

Sir,—Has tin any action, deleterious or otherwise, when taken into the human system? I can scarcely imagine that it is particularly beneficial in large quantities. I have found that the juice contained in tins of preserved pine-apple has a very corrosive action upon the lining of the containing vessel. I first noticed that, as the contents were consumed, a ring was formed on the tin at each successive level of the juice. Upon testing it I found abundance of a stannous salt present, which speedily oxidised to stannic when the juice was filtered. There was no lead present, but a trace of iron.

Thinking that perhaps the tin was not attacked till it was opened, and air had access, I punctured another tin and poured off the juice; but this also contained the same metal, and upon opening there was an encrusted ring where the level of the juice had stood.

I also tested the juice from a tin of apricots, and found the same contamination, but to a less extent. The juice of the latter was less acid and much thicker with sugar than that of the former.—I am, &c.,

A. Percy Smith.

Rugby, July 8, 1880.

THE NEW PATENT BILL.

To the Editor of the Chemical News.

Sir,—If the MS. of my last week's letter had been more legible you would have made me say that "the volumes" (not "the roll") "of special class abridgments should all be completed up to within a year of date," by the Patent Office authorities.

As a practical comment upon this recommendation, it should be noted that the only volume yet issued of abridgments of chemical patents is the one so well known to and consulted by many of your readers—"Abridgments: Acids, Alkalies, Oxides, and Salts. A.D. 1622—1866." How invaluable to chemical inventors would have been a continuous issue of these chemical abridgments indexed up to present date!

If my previous letter had not been so long I would have gone on to show that the large revenue derivable from a patent tax, commencing at £1 per annum, but increasing £1 with every year of its twenty-one years' duration, would maintain a staff of technical men not only to prepare abridgments of British patents, but to ascertain exactly what had been patented in foreign countries, the abridgments in both cases being issued in a weekly publication fully and continuously indexed to date.

The former weekly publication of indexed abridgments proved the entire practicability of the plan, if the Commissioners of Patents would only employ a sufficient number of experts to prepare the abridgments.—I am, &c.,

Frank.

HYDROGEN PEROXIDE AND OZONE.

To the Editor of the Chemical News.

Sir,—Your last issue has made me acquainted with the further experiments of Prof. A. R. Leeds upon the atmospheric oxidation of phosphorus.

Will you allow me to state that, having early in this year commenced a similar study, I was able to announce at the meeting of the Chemical Society, on June 3rd last, the following facts:—

1. Both ozone and peroxide of hydrogen are formed by the action of air upon phosphorus in the presence of water.
2. The formation of ozone is substantiated by the observations that the active substance in the gas is destroyed at 300° C., and is totally absorbed by oil of turpentine.
3. The ozone is not decomposed by passing it through a 10-volume solution of peroxide of hydrogen.
4. The peroxide of hydrogen produced bears a greater relation to the ozone formed than stated by Prof. Leeds. That gentleman estimated the peroxide "on the assumption that it is completely absorbed by the washing water," whereas I find it chiefly resides *in the water in which the phosphorus is oxidised.*

At an early meeting of the Chemical Society, next session, I hope to communicate the details of these and other experiments with regard to processes of slow oxidation. Meantime the publication of this note will much oblige.—I am, &c.,

C. T. Kingzett.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Note.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 25, June 21, 1880.

Effects of the Reversal of Photographic Images by the Prolongation of Luminous Action.—J. Jamin.—Photographic images may be reversed, and pass from a negative to a positive by the prolonged action of the light to which they owe their origin. This reversal is produced most readily in images of the sun on account of its enormous radiant power. But other reversals are possible, and even easy. Thus, besides solar images of 0·10 metre in diameter showing the sun with a white disc and black spots, the author has obtained landscapes, presenting the objects as they are. The time of exposure is from one to three hours. He has also produced counter types of the same nature as the original type; positive, if the type is positive, and negative if it is negative. In these photographs the same spectral rays which give at first the negative image effect its conversion into a positive image.

Formation-heat of the Oxides of Nitrogen and those of Sulphur.—M. Berthelot.—The author discusses the agreements and the differences between his thermochemical determinations and those of Prof. Thomsen. For the formation-heat of SO_2 he gives as the mean of three determinations +34·63. This result applies equally to octahedral, prismatic, and insoluble sulphur.

Luminous Spectrum of Water Vapour.—Dr. Huggins.—The author gives a table of the wave-lengths of the principal rays of the luminous spectrum of water vapour.

Proportion of Carbonic Acid in the Air: in Reply to M. Marié Davy.—J. Reiset.—The writer maintains the accuracy of his results, asserting that he has found a variation between the extreme proportions of 7·1 per 100,000. The minimum, 27·99, was found July 10, 1873, in a field of barley and lucerne. The maximum, 35·16, occurred at Paris, January 27, 1879.

New Meteoric Mineral, with Information on the Fall of Meteorites in Iowa, May, 1879.—J. Lawrence Smith.—The meteorite in question, to which Prof. Smith gives the name of peckhamite, is—

Silica	49·59
Ferrous oxide	17·01
Magnesia	32·51
	99·11

At least 3000 fragments of the Iowa meteorite have been discovered, their joint weight amounting to 30 kilos. They consist of nickel-iron, with very small quantities of earthy matter.

Certain Modifications in the Construction of the Bunsen Lamp and of Mono-chromatic Lamps.—A. Terquem.—The author sums up the following defects in the Bunsen burner. The flame is hollow in almost the whole of its length, and the temperature is not uniform. The entrance of air can only be increased within the limits permitted by the total opening of the vents at the base of the tube. If the air enters in a great quantity the flame is shortened, the hollow cone contracts, and takes the green tint of hydrocarbons burning with an excess of air, but the flame is very unsteady, and the ignition is easily communicated to the jet which introduces the gas. The same accident happens if the pressure of gas is weak, and can only be avoided by partially closing the vents to increase the draught. In the author's improved lamp there are no side openings; the air enters between the foot of the lamp and the bottom of the tube, which can be raised or lowered at pleasure, 6 to 7 m.m. being sufficient for admitting the largest quantity of air necessary. The shortening of the hollow cone and the combustion of the gas at the base of the flame are thus easily effected. To avoid the oscillations of the flame and the ignition of the gas at the inferior aperture ("striking down"), the inventor places at the summit of the tube, two small vertical plates at right angles to each other, dividing the exit aperture into four parts. In place of the hollow cone there are then four smaller cones. The author then gives an analysis of the products of combustion. A very pure and intense mono-chromatic light may be obtained by placing a bead of sodium chloride in the flame of the lamp thus modified.

Etherification of Hydrobromic Acid.—A. Villiers.—The following results are arrived at:—The limit of etherification is not equal to that corresponding to the organic acids. The limit is not fixed, but rises with the temperature. The etherification ceases completely in mixtures containing a certain proportion of water. The limit of dilution beyond which the etherification cases is not fixed, and rises with the temperature. The two latter facts may be explained by the existence of definite hydrates of hydrobromic acid, and by the dissociation of these hydrates under the action of an elevated temperature.

A Hydrated Methyl Iodide.—M. de Forcrand.—The compound obtained has the composition C_2H_3I,HO. The author has prepared similar hydrates of chloroform, ethyl bromide, and ethyl iodide.

The Artificial Reproduction of Analcime.—A. de Schulten.—This mineral has been reproduced by heating a solution of sodium silicate along with aluminous glass to 180° to 190° in a closed vessel. The operation lasts eighteen hours. The product thus obtained agrees with the natural mineral in its optical and chemical characters.

Biedermann's Central-Blatt für Agrikultur-Chemie. February, 1880.

The Reversion of Phosphates and the Behaviour of Phosphoric Acid in the Soil.—H. Joulie, H. Albert, and Dr. Vollbrecht.—An examination of the fact that a part of the phosphoric acid in superphosphates originally soluble in ammonium citrate becomes gradually insoluble. The authors incline to the opinion that finely-ground phosphates, or phosphates from which only one-third of their lime has been withdrawn by means of sulphuric acid, give a better result when composted with farm-yard manure, peat earth, &c., than ordinary superphosphates. The great proportion of sulphate of lime in superphosphates may easily lead to undesired results.

Agricultural Action of Reverted Phosphoric Acid and Neutral Phosphate of Lime.—Prof. A. Petermann, Dr. M. Fleischer, Dr. E. Wein, and Dr. de Leeuw.—On these questions there is still much uncertainty. Aluminic and iron phosphates seemed in some experiments to have a more beneficial action than tribasic lime phosphate. On light, calcareous soils reverted phosphoric acid is found equal, if not superior, to such as is soluble in water.

Digestibility and Nutritive Value of the Locust Bean or Carob.—Dr. H. Weiske.—In experiments made upon sheep it was found that the carobs, though eagerly eaten, depressed the digestive power of the animals.

Determination of the Commercial Value of Grain.—Prof. E. Wollny.—The specific gravity of grain is of less value than the absolute weight and size of the individual grains.

Injury to Vegetation from Acid Gases.—Dr. J. Schrœder.—The action of sulphurous acid is always intensified by heat, light, and moisture. Sulphurous acid is always much more destructive than hydrochloric acid, or than an equivalent proportion of sulphuric acid.

Soft and Hard Water in Brewing.—E. R. Southly.—The author pronounces that the value of calcium and magnesium sulphates as constituents of water depends on their promoting the clarification of beer, and on their antiseptic action.

Correspondenz-Blatt des Vereines Analytischer Chemiker.

No. 4, February 15, 1880.

Arsenophobia.—Dr. Ulex, of Hamburg, complains of the exaggerated and sensational outcry which is being raised concerning the alleged presence of arsenic in textile fabrics, paper-hangings, &c.

Modification of Classen's Apparatus for Determining Carbonic Acid in Mineral Waters.—Dr. W. Borchers.—Not capable of reproduction without the accompanying illustrations.

Determination of Glycerin in Beer.—V. Griessmayer.—100 c.c. of beer are slowly evaporated in a flat capsule on the water-bath at 65° to 75°; after expulsion of the carbonic acid 5 grms. magnesic hydrate are added, and the mixture is often stirred with a flat rod, so as to yield finally a homogeneous tough semifluid mass. The evaporation is not pushed to complete dryness. The capsule is then withdrawn from the water-bath, 50 c.c. of absolute alcohol are added, and the mass with the stirrer is digested, finally decanting into a large beaker. The residue is stirred up with 20 c.c. of absolute alcohol, and filtered. To the mixed liquids there are now poured, with constant stirring, 300 to 350 c.c. of absolute ether, when a voluminous flocculent deposit of parapepton and maltose is formed. The whole is filtered, washed with a little ether-alcohol (3 : 1), and the filtrate is exposed to spontaneous evaporation. The alcoholic liquid is then placed in a glass capsule, evaporated on the water-bath to the consistence of a syrup, and placed in an exsiccator connected with a water air-pump, so as to admit of rapid exhaustion. After twenty-four hours the capsule is removed, digested with 15 to 20 c.c. absolute alcohol, filtered through a very small filter into a tared glass capsule, again evaporated down on the water-bath, placed in the exsiccator, and weighed after twelve hours.

Hofmann's Eupittonic Acid as a Volumetric Indicator.—Paper saturated with an alcoholic solution of eupittonic acid indicates 1 part of caustic soda in 89,000 to 90,000 parts of water, whilst the best litmus paper does not go beyond 1 part in 60,000.

Journal für Praktische Chemie.

Nos. 4, 5, and 6, 1880.

On Cyanamide.—Dr. G. Prätorius Seidler.—The oxyguanidin hydrochlorate formed from hydroxylamin hydrochlorate and cyanamide has neither been obtained as such, nor as a free base, but its platinum double salt has been prepared. All further attempts were frustrated by the instability of this salt. It was found practicable to obtain cyanamides with acid radicles by the action of hydrated acids upon cyanamide. The dehydrating action of cyanamide is sufficiently energetic to decompose formic acid into carbonic oxide and water, which latter converts the cyanamide into urea. This experiment proves at the same time, that only water which is becoming chemically free, is capable of converting cyanamide into urea; as if steeped in water for a length of time it yields not urea, but dicyandiamide. The results obtained by the action of cyanamide upon lactic and salicylic acids are similar. With phenol, the products are not urea and the ethyl-ether of phenol, but dicyandiamide. With thiacetic acid the results are sulpho-urea and acetyl-sulph-urea.

Action of Phenols upon Halogen-substituted Fatty Acids.—Dr. L. Laarbach.—Phenol and phenoloid bodies act upon halogen-substituted fatty acids in presence of soda-lye in such a manner that the oxy (phenol, &c.) radicles are substituted for the halogen atoms. New acids of great permanence are thus produced, the oxyphenyl-propionic, the oxythymyl-acetic and the oxyeugenyl-acetic. The phenoxacetic acid of W. Heintz is to be regarded as the first member of this series. Bibasic phenols, such as orcin, act in an analogous manner.

On the Plato-iodo-nitrates.—L. F. Nilson.—This paper does not admit of useful abstraction.

Alleged Conversion of Albumen into Fat during the Ripening of Roquefort Cheese.—Nadina Sieber.—The author shows that no such change takes place. A part of the albumen is decomposed into leucin, tyrosin, volatile fatty acids, and ammonia.

On Salireton.—P. Giacosa.—Salireton is a crystalline body, obtained by heating saligenin to 100° with equal equivalents of glycerin, or with methylal, and in less abundance with mannite.

Behaviour of the Sugars with Alkaline Solutions of Silver and Mercury.—Dr. F. Soxhlet.—This paper does not admit of useful abstraction.

Verhandlungen des Vereins zur Beforderung des Gewerbfleisses. No. 2, February, 1880.

Lithium Glass.—Dr. Otto Schott.—Glasses composed of a mixture of soda and lithia show a great power of resisting acids, and do not differ essentially from known kinds of crown glass, either in the absolute value of the refraction or in the total dispersion. Lithium in glass does not serve as a substitute for the alkalies, but for the alkaline earths.

Processes of Griessmayer and Lallieu for Determining Glycerin in Beer.—Prof. C. Liebermann.—The method of Lallieu has a certain value if it is required rapidly to prove the absence of glycerin in beer. It can be used for an approximate quantitative determination only in the exceptional cases, when it is certain that the impurity is nothing but glycerin.

Gazzetta Chimica Italiana.

Anno 10, Fasc. 1, 1880.

Crystallographic Examination of some Bodies of the Aromatic Series.—G. La Valle.—The bodies thus described are tri-bromo-benzol, bi-iodo-benzol, mono-nitro-chloro-phenol, and bi-nitro-iodo-benzol.

Further Observations on Digallic Acid.—Ugo Schiff.—A reply to the memoir (*Gazz. Chim.*, 9, p. 327 in which the existence of artificial tannic acid is called in question.

Contribution to the Chemical History of Stereocaulon Vesuvianum.—Prof. Michele Coppola.—Not suitable for useful abstraction.

New Method of Determining the Melting-point of Organic Bodies.—Dr. G. Roster.—The author's method, suggested by that of Terreil, is to place the substance directly upon the bulb of the thermometer, substituting for liquid baths, or for the method of heating used by Terreil, a bath of confined air.

On Podophyllin.—Prof. Icilio Guaresci.—The substance known as podophyllin is a mixture of two substances a glucoside and a resin. The glucoside is contained in the portion insoluble in ether and the resin in the soluble part. The action of melting potassa yields products belonging to the aromatic series. The portion insoluble in ether resembles in some of its characters convolvuline and the turpentine of Spirgatis.

The Meteorite of Albarello.—Pietro Maissen.—An analysis of a meteorite which fell in July, 1766.

Researches on the Yellow Incrustations of the Lava of Vesuvius, of 1631.—Arcangelo Scacchi.—An account of the substance to which the author gives the name of vesbium, and which he considers as a new metal. Vesbium forms an acid which yields with the alkalies soluble salts, but with the other metallic oxides salts soluble in water, but soluble in acids. The acid solutions of aluminium and iron vesbiates are of a green colour, which is changed by sulphuretted hydrogen to a blue. The alkaline solutions of the alkaline vesbiates are yellow, and yield with the solutions of other metallic salts precipitates of various colours. The silver salt is of a yellowish red, and the zinc vesbiate is greenish.

* * *

Les Mondes, Revue Hebdomadaire des Sciences.
No. 18, April 29, 1880.

Adulterated Wines.—A controversy is raging between a Hungarian wine merchant and the wine growers of Dalmatia, each party accusing the other of making a fraudulent use of magenta.

Reproduction of Certain Mineral Bodies by the Superposition of Liquids of Densities and Capable of Forming with each other Insoluble Precipitates.—E. Monier.—The difference between the respective densities of the liquids must be great. The strata are not separated by parchment paper or any other porous septum. In this manner the author has obtained artificial resinite quartz by the action of aluminium sulphate upon an alkaline silicate, and has decomposed carbon disulphide by alkaline silicates.

No. 19, May 6, 1880.

The chemical matter in this number is taken from the *Berichte der Chem. Gesellschaft.*

No. 20, May 13, 1880.

Researches on the Relative Intensity of the Spectral Rays of Hydrogen and Nitrogen, with reference to the Constitution of the Nebulæ.—C. Fievez.—The spectra of the nebulæ display the rays of hydrogen and nitrogen which resist extinction longest, and warrant the supposition that these bodies are present in the nebulæ.

The Use of the Term "Indigo" by Newton, and its Relations with the Colouration of the Spectrum.—Prof. Rood.—The author contends, in common with Prof. Von Bezold, that "ultramarine" should be substituted for "indigo" as the name of a spectral colour. He shows that indigo is in reality a greenish blue, and contains a considerable element of blackness.

* * *

Bulletin de la Société Chimique de Paris,
No. 5, March 5, 1880.

Properties of Bicalcic Phosphate.—A. Millot.—This salt prepared in the cold and dried at 100° contains 5 equivs. water, and only begins to lose its crystalline water at 115°. It is completely dissolved in an hour in a sufficient quantity of ammoniacal ammonium citrate. Bicalcic phosphate with a single equivalent of water dissolves more slowly in this reagent, and the solution is only complete after a digestion of about twelve hours. A specimen of bicalcic phosphate with 5 equivs. of water preserved in a bottle for five years lost an equivalent of water, and no longer dissolved entirely in the ammoniacal citrate in an hour. The bicalcic salt loses its crystalline water, and is decomposed in boiling water. If we mix bicalcic phosphate and water, and heat to a boil, the liquid becomes acid, acid phosphate of lime dissolves, and a mixture of bi- and tri-calcic phosphate remains.

There are no other original papers in this number.

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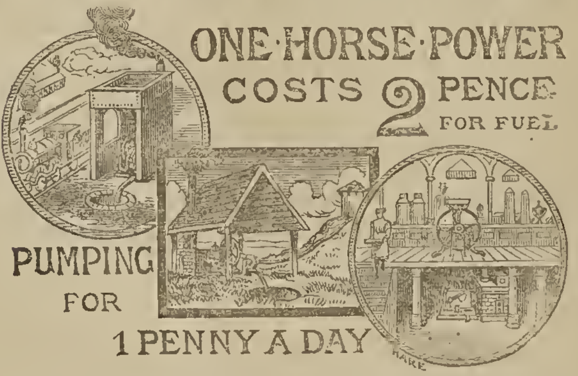

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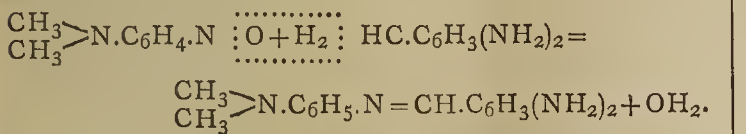
THE CHEMICAL NEWS.

Vol. XLII. No. 1078.

ON A
NEW CLASS OF COLOURING-MATTERS FROM THE PHENOLS.

By RAPHAEL MELDOLA.

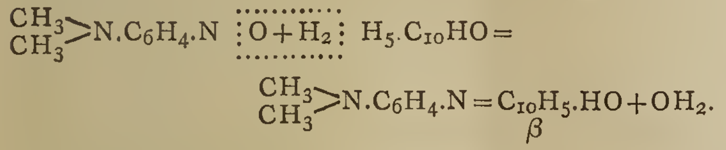
Last year Dr. O. N. Witt published a paper (*Journal Chem. Soc.*, vol. xxxv., p. 356), in which he described the action of nitroso-dimethyl-aniline on meta-toluylene-diamine, from which it appeared that the oxygen of the nitroso-group attacked the hydrogen of the methyl group in the following manner:—



$$\begin{matrix}CH_3\\CH_3\end{matrix}\!>N.C_6H_4.N\ \boxed{O+H_2}\ HC.C_6H_3(NH_2)_2=$$

$$\begin{matrix}CH_3\\CH_3\end{matrix}\!>N.C_6H_5.N=CH.C_6H_3(NH_2)_2+OH_2.$$

At the time this research was published I was independently occupied in investigating the action of nitroso-dimethyl-aniline on phenols, and in order to eliminate the possibility of the formation of compounds built on the type of "toluylene blue," I have restricted my experiments to phenols not containing the methyl group. I have already given a description of the method employed in a preliminary note published last year (*Ber. Deut. Chem. Ges.*, 1879, p. 2065). In the formation of this new class of compounds I have since convinced myself that the oxygen of the nitroso-group *directly attacks the hydrogen of the aromatic nucleus*, as shown in the following equation:—



$$\begin{matrix}CH_3\\CH_3\end{matrix}\!>N.C_6H_4.N\ \boxed{O+H_2}\ H_5.C_{10}HO=$$

$$\begin{matrix}CH_3\\CH_3\end{matrix}\!>N.C_6H_4.N=\underset{\beta}{C_{10}H_5}.HO+OH_2.$$

The new compounds are splendidly crystalline violet colouring-matters, forming extremely oxidisable leuco-bases, and as their complete investigation promises to be very laborious, I have thought it of sufficient interest to record their general structure, as typified by the above β-naphthol compound, in anticipation of future work.

Atlas Works, Hackney Wick, London, E.,
July 13, 1880.

DETECTION AND DETERMINATION OF HEAVY MINERAL OILS, OF RESIN OILS, OF THE FATTY OILS, AND OF RESIN IN THE OILS OF COMMERCE.

By M. A. RÉMONT.

The qualitative analysis ought to be preceded by an examination of the organo-leptic properties of the oil, the manner in which it behaves under the influence of heat, and of its specific gravity, which often gives useful indications. Thus, if the specific gravity of the sample is below 0·900 it certainly contains a mineral oil; if from 0·900 to 0·975, we may be in presence of the most complex mixtures; but if it is above 0·975 we have certainly an oil of resin.

We begin by treating the sample with carbon disulphide, freshly prepared, which gives a clear solution with all the oils. If oleic acid or a fatty oil has been mixed with alkali to raise its specific gravity by the formation of a portion of soap, there will appear a precipitate. In this case the liquid is filtered, and the residue is washed with carbon disulphide. It may be proved to be soap by its solubility in water, its alkalinity, and the turbidity, more or less marked, which is caused by an acid poured into the solution.

The filtrate is freed from the carbon disulphide by distillation: 1 c.c. of the residue is taken and mixed with 4 c.c. of alcohol at 85°. If solution takes place fatty acids are present, pure or mixed, and an excess of alcohol is gradually added. If after having poured in 50 c.c. the liquid is limpid, or there is produced a very slight cloud, which disappears on adding a drop of hydrochloric acid, the sample consists of oleic acid, pure or mixed with resin. If the speciffc gravity does not exceed 0·905 at 15° the sample consists entirely of oleic acid. If the specific gravity is higher the oleic acid contains resin. By way of confirmation the substance is examined with the polarimeter, either directly or dissolved in carbon disulphide, and if there is a deviation we may be certain of the presence of a resinous mixture.

If we find a persistent cloudiness in the alcoholic solution it is because the fatty acids contain an oil sparingly soluble in this solvent, and in so much the greater quantity as the cloud appears earlier. This very sensitive process renders it possible to detect 2 or 3 per cent of heavy mineral oil, or of resin, or fatty oil in the oleic acid known in commerce as oleine. The turbidity produced in the alcoholic liquid resolves itself in some time into little oily drops, which line the sides of the vessel, and which can be made by a shock to fall to the bottom of the tube. The volume of this residue shows approximately the proportion of insoluble matter.

The most common case is when 4 parts of alcohol do not completely dissolve 1 part of oil. A larger quantity of the latter is then taken, and agitated with an equal volume of alcohol. After settling for a time the alcoholic solution is decanted and evaporated in a capsule. The nature and the quantity of the residue serve as a clue to the nature of the mixture.

I submit the oil to the action of caustic soda, employing the method indicated by M. Dalican for the analysis of tallows. In a capsule of porcelain, or preferably of enamelled cast-iron, there are weighed about 20 grms. of oil, and heated to 100° to 110°. There is then poured in a mixture of 15 c.c. soda-lye at 36° B. and 10 c.c. of alcohol: the mixture is stirred and heated until the alcohol and the chief part of the water have disappeared. Then 150 c.c. of distilled water are added, and the boiling is kept up for half an hour, when three cases may occur:—

1. The oil, under the influence of the alkali, is merely emulsified, and on the addition of water it separates distinctly: this indicates either a mineral oil, a resin oil, or a mixture of the two. The aqueous solution is decanted off, and it is mixed with sulphuric acid. If there is no precipition, or if a mere slight cloudiness is produced, the sample is a pure mineral oil. If there is a considerable precipitate which collects in brown viscid drops giving off a strong odour of resin, and soluble in an excess of alcohol, we have a resin oil, pure or mixed. The oil is examined with the polarimeter, and if it acts upon polarised light this is a confirmation of the presence of resin oil. If the specific gravity is below 0·960 there is probably a mixture of mineral oil. Good indications may also be got by distilling if one of the oils is not in too trifling proportion. The distillation ought to be fractionated as far as possible and conducted slowly. As the resin oils boil, as a rule, at lower points than the mineral oils, it follows that in place of having specific gravities which increase with the boiling-points, as happens with the heavy mineral oils or pure resin oils, there are observed, with their mixtures, very abrupt transitions. The sample ought to be tested with stannic chloride, and if the violet colouration is not very distinct the same reagent should be applied to the

first products of distillation, since the colourable product contained in the resin oils is there chiefly met with.

2. Or there is formed by the action of caustic soda a paste-like mass of soap, which on treatment with water and boiling for some time gives a clear liquid. It is diluted with cold water and then supersaturated with an acid. The fatty acids liberated collect on the surface after decantation of the water, and if exposed to cold they crystallise. A small portion is melted in a tube at a gentle temperature, and 4 parts of alcohol at 85° are added first and afterwards an excess. Here two cases are possible :—

A. If no precipitation takes place it is because the fatty acids are pure, which shows that the oil examined is a pure fatty oil, or, which rarely happens, mixed with resin. The specific gravity of the fatty acids may here give good indications, but it cannot be taken at common temperatures, at which fatty acids are solid. They must be melted, and the specific gravity taken at a known temperature. M. Baudouin, Chemist at the Arnavon Soap Works, at Marseille, has drawn up a table of the specific gravities of the fatty acids of certain oils taken at 30°. Except those of linseed oil, which mark 0·910, those of the other fatty oils have specific gravities ranging from 0·892 to 0·900. To reduce the specific gravities of the fatty oils examined to the temperature of 30°, deduct from the density found, calculated on a litre, as many times 0·64 grm. as there are degrees below, or if the temperature is higher to add to the density found as many times 0·64 grm. as there are degrees above. If the specific gravity indicates that the neutral oil contains resin an attempt may be made to separate it, in part at least, rapidly by agitating 5 or 6 c.c. of the original oil with an equal volume of alcohol, decanting after settling, and evaporating in a capsule. There is thus obtained a solid or semi-fluid residue in case of resin. Further examination is then made with the polarimeter.

B. The fatty acids derived from the decomposition of the soap give a precipitate if treated with an excess of alcohol. If it is not re-dissolved by 1 grm. of hydrochloric acid, and if after some time it is resolved into oily drops it is due to a mineral oil or a resin oil. A fatty oil containing 10 to 15 per cent of one of these oils is completely saponified, and yields with boiling water not an emulsion, but a soap completely soluble. The turbidity should resolve itself into oily drops, for there are certain fatty acids—those, amongst others, of the oil of earth-nuts (*arachis*)—which are soluble in a small proportion of alcohol at 85°, but an excess of the alcohol precipitates a sparingly soluble portion, arachidic acid, in small flocks. These flocks may be collected on a filter, and examined as to their complete solubility in alkalies. If their melting-point is near 73° they may be attributed to the oil of earth-nuts.

3. Or, lastly, the oil, on treatment with soda, may give a paste more or less firm, which if placed in boiling water for half an hour allows oily drops to rise to the surface, which are due to a mineral oil or a resin oil. After settling for some minutes a part of the supernatant liquid is decanted and mixed with an excess of a saturated solution of common salt. There is produced a precipitate of soap, which is filtered off on cooling. The filtrate is supersaturated with an acid. If there is produced a slight turbidity, and if the liquid which was almost colourless when alkaline gives off an odour of fatty matters we have a neutral oil mixed with a non-saponifiable oil. If, on the contrary, the solution was highly coloured after filtration, and gives when acidified a flocculent precipitate of a resinous odour, the sample is a mixture containing resin. In these two cases, the components of the mixture may be recognised by means of the procedures indicated above.

Quantitative Analysis.—If it is desired to know the elements attacked by alkalies, and of those which are not, the following procedure is to be followed :—If the sample has yielded anything insoluble in carbon disulphide, it is separated as already said, and the operation is confined to the residue of the distillation. Let it be assumed that the composition of the residue is as complex as possible, containing fatty oils, mineral oils, resin oils, and solid resin.

The mixture is saponified. Into a flask closed by stopper, through which passes a long tube, are introduced 20 grms. of the oil, and a mixture of 15 c.c. of soda at 36° B., and 15 c.c. alcohol at 90 to 95 per cent. The flask is then set upon the water-bath for half an hour and is often shaken. At the end of this time the whole is poured into a funnel fitted with a tap and previously warmed, and which is left in a stove at 50° to 60° until a complete separation of the non-saponifiable oil from the alkaline liquid has taken place. The latter is then decanted into a porcelain capsule, and in its stead is poured 15 c.c. of boiling water which has served to rinse the flask. It is shaken well so as to wash the non-saponifiable matter, and decanted anew after settling. Finally, it is washed a third time with boiling water. The oil in the funnel is received into a tared capsule and weighed. As for what adheres to the sides, it is washed with a little ether, and the solution is received in another capsule, which is exposed to the air till the bulk of the ether has disappeared. It is then gently heated to expel the rest, and weighed.

The alkaline liquid is kept at a boil for some time to expel the alcohol, and after cooling it is mixed with an equal volume of a saturated solution of common salt, freed from magnesia by being boiled for a few moments with caustic soda and filtered. In this manner, the soap is precipitated in firm clots, carrying with it the last portion of non-saponifiable matter. The saline solution, after settling, is decanted by means of a pipette and neutralised with an acid. If a notable turbidity is produced, which collects in flocks, it is due to resin. The flocks are collected, dried, and weighed. The clots of soap are thrown upon a filter, washed twice with salt water, the last traces of which are removed by pressing the mass between blotting paper. The soap is then placed in a glass cylinder, moistened with about 100 c.c. of carbon disulphide recently rectified, stoppered, gently shaken at intervals three or four times, so as not to break the clots, and left to settle. After an hour or two the carbon sulphide which is coloured yellow by the dissolved oil, separates in the lower part of the cylinder. It is decanted by means of a pipette, and in its stead is added a fresh portion of the solvent. It is shaken, left to settle, decanted, and so on till the carbon sulphide runs off almost colourless. The whole is then thrown upon a filtered and washed for the last time. A portion of this last washing, if evaporated upon a watch-glass, should leave an insignificant residue.

The soap on the filter is exposed to the air till the carbon disulphide with which it is saturated has escaped.

As for the carbon disulphide solution, it is distilled gently on the water-bath. The last portions of the solvent are expelled by blowing air into the flask while placed in boiling water. When cold it is weighed.

The last portion of the non-saponifiable matter thus obtained ought to have the same appearance as the first portion. If it is less fluid it still contains a portion of soap. In this case it is again taken up in carbon disulphide at a gentle heat in presence of a few drop of water to hydrate the soap which, without this addition, would again be partially dissolved. It is then filtered and the washed soap is added to the chief mass.

The non-saponifiable oil may consist of mineral oil, resin oil, or a mixture of both. The means of detection have been given above, and I have not yet come upon a process for their separation.

The soap insoluble in carbon sulphide which lies on the filter contains resin and fatty acids combined with soda.

The separation of these substances, so similar in their properties, presents many difficulties. Several methods have been published, but none of them gives satisfactory results. That of M. Jean, one of the most recent, consists in exhausting the barium soap with ether, which ought to dissolve the resinate and leave the soaps of the fatty acids untouched. On following exactly the author's instructions, I have never been able to avoid the partial solution of the barium oleate. I have modified the process by substituting for the ether boiling alcohol of 85 per cent, which certainly dissolves much less of the oleate, but still takes up enough to render the results inaccurate.

As far as possible the soap is separated from the filter and placed in a capsule. The filter is put back in the funnel and filled with boiling water. The solution is effected slowly and filters by degrees; it is received in the capsule where the detached portion has been already placed.

The solution of soap, after cooling is mixed with caustic soda until precipitation no longer ensues, and left to settle. All the soap of the fatty acids is deposited, drawing down with it the chief portion of the resinate, a part of which, however, remains in solution and colours the liquid strongly. The whole is filtered, the filtrate accurately neutralised with sulphuric acid; the flocks of resin deposited are received upon a tared filter which is weighed anew after washing in water and drying at a low temperature. The soap is re-dissolved in a little luke-warm water and an excess of barium chloride is poured into the solution with agitation. The clots of barytic soap are drained in a filter-pump, replaced in the capsule in which the precipitation has been effected and thoroughly dried in the water-bath or the stove. The mass is then powdered, and treated with 50 or 60 c.c. of alcohol at 85 per cent, which is kept near the boiling point, working it up with a pestle. It is left to settle for a few moments and the supernatant alcoholic liquid is then decanted into a phial. 20 to 25 c.c. of alcohol are again poured upon the residue, let boil, decanted after settling, and so on till a portion of the alkali which has been used leaves, on evaporation, scarcely any residue, which happens generally after 120 c.c. of alcohol have been used.

The alcoholic liquids are mixed and distilled till there remains only about 50 c.c. Hydrochloric acid is added to decompose the resinate, and the resin set at liberty floats in the liquids. On cooling it collects in a solid mass at the bottom of the vessel. It is thrown into a capsule, melted under water, and weighed after desiccation on the water-bath.

The residue insoluble in alcohol is treated in a similar manner to obtain the fatty acids.—*Bulletin de la Société Chimique de Paris.*

DETERMINATION OF ASH IN COAL, AND INCINERATION IN GENERAL.

By Dr. FR. MUCK.

Incineration and an accurate determination of ash are, in many substances, attended with not inconsiderable difficulties. These may arise from the sparingly combustible nature of the bodies, as in case of anthracite or graphite; from the formation of a sparingly combustible carbon (in fusible organic substances); from the presence of certain mineral substances which impede incineration, as silicic acid, phosphates, and fusible salts; from a tendency to decrepitation observed in many vegetable matters and in all coals; from the partial volatility of various constituents of the ash, and from the chemical changes which the ash may undergo according to the degree and the duration of the heat and the supply of air.

The first three of these difficulties affect principally the rapid execution of the process, whilst the three remaining affect the composition of the ash and the accurate determination of its weight. Peat and lignite require no special precautions; they are readily inflammable and rarely cake together, often smouldering away like tinder. In true coal all the above-mentioned difficulties may occur. All varieties of coal decrepitate more or less. The losses thus occasioned may be obviated by conducting the incineration at first in covered crucibles. This, however, is inconvenient where a great number of ash determinations have to be performed simultaneously. The incinerations are sometimes performed in porcelain crucibles, where unburnt coal is rarely projected over the edge. In general, flat platinum vessels are preferred, in which the process is more rapid but the chance of loss much greater. To prevent this source of error, it is advisible to reduce the coal to a very fine powder, and above all to heat very gradually. The latter precaution, especially in case of caking-coal, prevents the formation of coke, the incineration of which requires a much higher temperature, and a very considerable outlay of time. Every "caking" coal loses this property completely if very gently heated for a moderate time, and may then be almost completely incinerated at a temperature little above the melting-point of lead.

Not every ash can be at once assumed quite free from carbon, and unburnt particles cannot be readily detected by stirring with a needle. They may readily be found on moistening the ash with alcohol, when any unburnt portions are detected not merely by their colour, but by floating on the surface. Moistening with alcohol has the additional advantage that the ash, which lies very loose, adheres afterwards more closely to the sides of the crucible, and the complete incineration is thus accelerated. Unburnt carbonaceous particles escape the eye most readily when the coal contains very little ash.

When sulphur and iron are present in large proportions prolonged or repeated ignition is often necessary to reach a constant result. The question remained how the weight of the ash might be affected by the presence of a large proportion of lime. Lime may be present in the coal itself as silicate, as sulphate, and as carbonate; not frequently as sulphate, since gypsum rarely occurs in the carboniferous strata, though it is frequently found in solution in the subterranean springs.

Calcium carbonate frequently occurs in coal, sometimes in large laminæ, and where these do not occur the presence of carbonates may be frequently observed on moistening the coal with acids. The ash of coal (Westphalian at least) never effervesces with acids, and when this occurs to the slightest extent it is partially due to sulphuretted hydrogen. This is evolved from calcium sulphide formed to a slight extent by the reduction of calcium sulphate by carbon. The evolution of sulphuretted hydrogen can be less easily ascribed to iron sulphide, which less readily escapes oxidation during incineration than calcium sulphide.

Coal ash often contains very much more lime than what is combined with sulphuric acid or present as calcium sulphide. The residual lime, along with other bases is combined with silicic acid.

Ash containing much ferric oxide may be not at all, or but slightly, red if much lime is simultaneously present, for calcium ferric silicate is colourless or yellowish, but not red. The following is the composition of a pale reddish coal-ash, which, if strongly ignited with the coal in question, melts to a vitreous slag. The ash scarcely effervesced with acids, and after being moistened with sulphuric acid and re-ignited it became red:—

Silica	32·17
Alumina..	17·87
Ferric oxide	17·42
Lime	17·83
Magnesia	6·97
Sulphuric acid (saturating 4·018 lime)	5·94
Matter not determined	2·00

1·049 grms. of this ash moistened with alcohol, and then ignited for a considerable time in the muffle at a white heat, decreased to 1·0465 grms. On treatment with ammonium carbonate and gentle re-heating the ash, weighed 1·056 grms. The ash effervesced a very little more with acids; a trace of calcium carbonate had been decomposed during incineration. The practical results of the author's experiments may be stated as follows:—

1. The chemical changes during the incineration of coal

can only serve in rare and especial cases as an explanation of great differences in the determination of ashes. For instance, when coal contains very much sulphur, and at the same time very little lime. In presence of much lime the combustion-products of the sulphur are more or less completely retained, whilst if little lime is present variable amounts of sulphur are driven off.

2. In case of a thoroughly homogeneous material, *i.e.*, a finely-powdered and well-mixed average sample accurately and carefully analysed, the determinations of ash may be expected to agree to 0·1 to 0·2 per cent.

Larger differences, except in the peculiar cases just referred to, must be referred to defective work or great difference of the samples.

It is understood that the proportion of moisture must be taken into consideration. In a thoroughly air-dry coal—not lignite—the highest limit of moisture is from 1 to 2 per cent.

The platinum crucible, according to the position of the flame, may be coated with soot. This coating may be easily burnt away, but care must be taken not to wipe a sooty place, nor a place that has been sooty without carefully checking the weight of the crucible after weighing the ash. In the places where soot has been formed by the dissociation of the hydrocarbons the platinum becomes disaggregated and is coated with a black-grey deposit of finely pulverulent metal, which is easily rubbed off. The author has observed variations exceeding 8 m.grms. produced in this manner. He refers to the method of Schlösing, who places the substance to be incinerated in a platinum boat, introduces it into a combustion-tube, and distils in a current of dry carbonic acid. When the development of inflammable gases has entirely ceased a slow current of oxygen is substituted for the carbonic acid, and the incineration is completed. Decrepitating substances are previously charred in a closed crucible.

A remark becomes here necessary on the incineration of filters. A previous complete drying of the precipitate as Bunsen has proved, is not merely a loss of time, but a disadvantage, whilst introducing the still moist precipitate into the crucible requires the application of a very gentle heat at the outset, and thus ensures the most favourable conditions for the easy and complete incineration of the filter-paper. Precipitates not washed upon the filter-pump can be readily brought to a sufficient degree of dryness if laid for a short time upon blotting-paper or unglazed earthenware.

Dry filters may be also much better incinerated after previous charring at the lowest possible temperature, than by rapid carbonisation or direct ignition in the flame. How advantageous it is to char previously very slowly may best be seen on incinerating filters, whose contents impede the complete combustion of the paper by the old process, *e.g.*, silicic acid, ammonio-magnesium phosphate, &c. Charred paper obtained by rapid heating is deep black and of a silky lustre, whilst if slowly carbonised it is brownish black, dull, and smoulders away like tinder. Charred paper of the first kind appears under the microscope perfectly amorphous, whilst the other displays the carbonaceous skeleton of the fibre.

A careful removal of precipitates from the filter—with the exception of cases like zinc and cadmium, where volatile reduction-products may be formed—is quite useless, since the errors which it was hoped to obviate are not really avoided. On incineration with the filter, wet or dry, an error due to reduction may be easily corrected, *e.g.*, in barium sulphate with sulphuric acid; in lead sulphate with nitric and sulphuric acids; in iron and copper oxides with nitric acid; in silver chloride with nitric and hydrochloric acids, &c.—*Zeitschrift für Analytische Chemie*, vol. xix., p. 131.

Occurrence of Iodine in Curaçao Guano.—H. Steffens.—In rendering this guano soluble violet vapours of iodine were evolved when the mass had reached a temperature of 110° to 120°.—*Zeitsch. f. Anal. Chem.*

THE HEAT OF THE COMSTOCK LODE.[*]

By JOHN A. CHURCH, E.M., Ph.D., New York City.

In May, 1878, I had the honour of presenting to this Institute, at the Chattanooga meeting, some observations upon the heat of the Comstock Lode, and since then the subject has attracted some attention and criticism, in the course of which I have observed errors of apprehension which I beg leave to correct now. Mr. John Arthur Phillips discussed it before the Geological Society, his paper being printed in the *Quarterly Journal* for August, 1879.[†]

His criticism is based upon that part of my paper in which I quote a number of analyses of the mine waters, to illustrate the fact that the heat does not come from the oxidation of pyrite. I pointed out that these waters contain only an insignificant proportion of sulphuric acid, and not enough to account for even one per cent of the heat observed, the calculation being based upon the oxidation of the quantity of pyrite which corresponds to the amount of sulphur in the water. Mr. Phillips endeavours to disprove my theory of kaolinisation, by applying the same reasoning and calculations to the amount of alkalies and alkaline earths dissolved in the waters. He says:—

"The average proportion of alkalies contained in the rocks of the district is 6·40 per cent, while the mean of the published analyses gives 11·30 grs. of alkalies in 58,373 grs. of mine water. It consequently follows that the 4,200,000 tons of water annually pumped out of the workings must contain 813 tons of alkalies, and that, as these are present in the rocks in the proportion of 6·40 per cent, the felspar in 12,703 tons of rock must be annually kaolinised and the whole of the felspars removed in solution.

"The amount of rock in which the felspar has been kaolinised being 12,703 tons and the number of tons of water 4,200,000, it follows that—

$$\frac{4,200,000}{12,703} = 330$$

is the number of tons of water heated by each ton of altered rock.

"In order, therefore, that one ton of rock should be enabled to heat 330 tons of water only 1° F., and if the specific heat of these rocks be taken at 0·1477, which is that of blast-furnace slags, it would require to be heated by the kaolinisation of its felspar to a temperature above that of molten gold. Consequently to raise the water 85° or to a temperature of 135°, at which it issues, the kaolinisation of the felspar in each ton of rock would require to elevate it to an extent we are unable to estimate, since there is no means of ascertaining the specific heat of bodies at such enormously high temperatures."

Mr. Phillips therefore concludes "that the kaolinisation of felspar is no more than the oxidation of pyrites, an adequate cause to account for the heat of the Comstock Lode." His criticism is, however, founded on fallacious premises. It may be possible to use the analyses of mine waters to determine the function of pyrite in the physics of the lode, because the products of the oxidation of pyrite are completely soluble, and all of the sulpuric acid produced may be expected to appear in the water, except such part as is precipitated as gypsum by the lime of the rocks. Observation showed that the quantity of sulphur removed by this precipitation is very small, and my use of the water analyses to exhibit the inadequacy of pyritic oxidation as a source of heat was, perhaps, pardonable, though it was inaccurate, for the reason that no attempt was made to compute the quantity of heat developed by the *hydration* of the sulphuric anhydride. This omission was allowed because the augument against pyritic oxida-

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[*] A Paper read before the American Institute of Mining Engineers at the New York Meeting, February, 1880.

[†] A Contribution to the "History of Mineral Veins," by J. Arthur Phillips, Esq. F.G.S. *Quarterly Journal of the Geological Society* for August, 1879.

tion as a principal source of the heat did not depend on heat calculations, but on the fact which I repeatedly stated, that the rocks do not contain pyrite or precipitated ferric oxide in quantity sufficient to accouut for the heat by oxidation. The calculation based on the analyses merely gave a *measure* of this inadequacy, and showed that a cause to which 100 per cent of the heat had been ascribed, really did not account for as much as 1 per cent of it.

But the case is very different with kaolinisation. Here we are not dealing with the *least* apparent cause of heat, but with that one which observation shows to be the most extensive result of chemical action in the lode rocks. I believe my theory of heat from kaolinisation is acknowledged on all sides to be well founded as a general fact, and the only difference of opinion is upon the quantity of heat obtainable by the hydration of aluminic silicate. My critics acknowledge that where kaolinisation takes place some heat is produced, but they deny that its effects can be so remarkable as to produce the temperatures found in the Comstock.

The point which I now wish to insist upon is that pyritic oxidation and kaolinisation do not stand upon the same footing as possible sources of heat in the Comstock. Both come under the practical observation of the geologist, and while one is so limited in its quantity as to be insignificant as a factor of heat, the other is found to exist in the excessive magnitude which the student of geology sometimes encounters.

The Comstock rocks may be said to form a mass four miles long by four miles wide, and say two and a half miles thick, and so far as observation teaches us, it shows that every pound of this block has undergone alteration to some extent. A large proportion, say ten per cent, as a *minimum*, has been altered to flaking clay, while a still larger proportion has advanced so far in decomposition that alteration sets in vigorously when a gallery is opened in it, the rock "slakes" and the air of the gallery is highly heated, its atmosphere often becoming temporarily insupportable by man.

It is a mistake to suppose that the results of this extensive kaolinisation are represented in the mine waters. It is probable that much of it is accomplished by aqueous vapour, which is entirely absorbed by the rock and does not give rise to any solution. The rock is not saturated with water, but, on the contrary, analyses show that the hydration is almost everywhere incomplete.

The error in Mr. Phillips's calculation is contained in the words, "as these (the alkalies) are present in the rocks in the proportion of 6·40 per cent, the felspar in 12,703 tons of rock must be annually kaolinised and the whole of the alkalies removed in solution."

This conclusion is based upon the supposition that the result of kaolinisation at a great depth is clay free from alkalies. But that is not the case. The result is merely altered propylite, or a decomposition product which differs from the original rock only in the addition of water and the removal of a small part of the alkalies. The difficulty, and perhaps impossibility, of obtaining rock which has not been altered in the Comstock region, makes it a delicate matter to institute comparisons between the clay and the original rock, but the following analyses given by Mr. Clarence King* will serve to show the change produced. The composition given for the clay is the mean of four analyses of clays taken from different points, and probably resulting from the alteration of two rocks, propylite and andesite. Analyses of both of these rocks are adjoined, but the specimens did not come from the same places, though they are from the same region. They were chosen out of a series because they are the only examples which are nearly free from the water. (See next column).

	Clay.	Propylite.	Andesite.
Silica..	56·24	64·62	58·33
Alumina	15·28	11·70	18·17
Oxide of iron	4·00	8·39	6·03
Lime	4·40	8·96	6·19
Magnesia	3·01	1·18	2·40
Soda	0·95	3·13	3·20
Potassa	3·24	1·95	3·02
Water	6·26	1·02	0·76
Carbonic acid	2·34	—	2·85
Phosphoric acid ..	0·08	—	—
Pyrite..	4·36	—	—
	100·16	100·95	100·95

In order to obtain an analysis of nearly unaltered propylite it was necessary to take one which does not contain the usual proportion of alumina, while the alkalies are in corresponding excess over the proportion usual in the immediate Comstock grounds, so that the comparison is not entirely accurate. Still the analysis of the clay is the main thing, and the mean composition given shows what is invariably the fact in this locality, that the clay retains a large proportion of its iron, alkalies, and alkaline earths. It is impossible to say whether more or less than one-half of the soluble products have been leached out, and therefore it is incorrect to say that the solids present in the mine waters represent the kaolinisation of 12,703 tons of rock, as Mr. Phillips assumes. The true quantity of *leached* rock may be many times as great as this even in wet ground.

But the quantity of rock that yields solids to the mine waters by no means represents the quantity that is kaolinised. These waters do not permeate the whole mass of rock. On the contrary, nine-tenths of it is extremely dry, and the water is confined to narrow courses, which discharge an unceasing flood from a few points. The dry rock seems to be as hot as the rest, and, like the clay, its analysis exhibits the presence of water. One analysis made in a specimen that showed no signs of alteration, and was classed as a typical specimen of propylite, was found to contain 6·53 per cent of water, or more than the mean given above for the clay. The alteration was accomplished without affecting the physical appearance of the rock, and perhaps without lessening its constituents.

I have shown that this rock is permeated by currents of gas, which is frequently discharged from the drill-holes with force sufficient to move the flame of a candle. Probably this gas is partly composed of aqueous vapour, which is not perceptible to the eye in the hot levels of the mines, but is seen as a rushing column of steam forty feet high at the mouth of the shaft, where it encounters the atmospheric temperature. All the upcast shafts of the lode show this column of watery vapour, and it appeared to be just as strong in the Imperial, Bullion, and other mines, where fifty to one hundred men were employed, as in the California and Consolidated Virginia mines, each of which gave work to six or seven hundred men. The quantity of this steam in the upcast is much greater in the Comstock than in any other mines I have ever seen, though they are by no means the most heavily manned, nor the most watery excavations that have come under my observation.

It is extremely probable that even the dry rock is supplying heat by kaolinisation through the action of water vapour over large areas which never receive water enough to be robbed of their alkali by solution.

(To be continued.)

The Transformation of Amylen and Valerylen into Cymen and Benzenic Carbides.—G. Bouchardat.—The author has succeeded in converting the amylic alcohol of fermentation and the valerylen derived from it into benzenic carbides, and into cymen, $C_{20}H_{14}$, by means of successive subtractions of hydrogen, and into a second carbide, $C_{18}H_{12}$. He has also attempted the direct conversion of diamylen into cymen, seeking to withdraw 4 equivs. hydrogen by the successive action of bromine and potassa, but without success.—*Comptes Rendus.*

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* "United States Geological Expedition of the Fortieth Parallel," vol. iii., p. 89, and vol. i., p. 560 and 576.

PROCEEDINGS OF SOCIETIES.

THE AMERICAN CHEMICAL SOCIETY.*

The regular monthly meeting of the Society was held on Thursday evening, June 3, 1880, at University Building, Washington Square, New York. Dr. A. R. Leeds, Vice-President, in the Chair.

Mr. Alexander Cochrane, of Boston, Mr. Hugh Cochrane, of Boston, and Mr. Lucius Pitkin, of New York, were elected Associate Members of the Society.

The following gentlemen were nominated:—

As Members:—Herman Poole, Buffalo, N.Y., Dr. Karl Stahl, Taeony Chemical Works, Philadelphia; G. T. Bihn, Philadelphia; and C. H. Torrey, School of Mines, N.Y.

As Associate Members:—Thomas Harrison, Philadelphia; and J. R. Savage, Philadelphia.

It was resolved that the Society should have a meeting, either formal or informal, in Boston, during the session of the American Association for the Advancement of Science; and that the First Vice-President should appoint five members of the Society as a Committe to carry out this resolution.

Mr. J. H. Stebbins, Jr., S.B., read a paper on "*Some New Colouring Matters Produeed by the Action of Diazo-compounds on Phenols.*" The first part of the paper was devoted to a history of the discovery and development of the diazo-compounds, beginning with the work of P. Griess in 1875 and running rapidly through a number of interesting and valuable researches down to the present time.

Mr. Stebbins then went on to describe a number of new bodies which he had produced, and which are very interesting contributions to this wonderful series of chemical compounds. The first of these was azobenzol-trinitro-oxybenzol, made by the action of diazobenzol-nitrate on picric acid; it is explosive when heated. The next compound was azobenzol-pyrogallol, made by the action of diazobenzol-nitrate on pyrogallol; it crystallises in red-brown needles, slightly soluble in alcohol, and in this form is adapted for dyeing. Another body described was para-azosulphoxylbenzol-phloroglucine, made by acting on diazosulphanilic acid with phloroglucine. This body forms a soda-salt easily soluble in water, and also an acid-barium-salt less soluble in water than the soda-salt. Azobenzol-sulpho-cresol was the next body described. It is made by acting on cresol-sulpho-acid with diazobenzol nitrate; and crystallises in long red-brown needles with metallic lustre, soluble in alcohol and less so in hot water.

The above compounds had already been described by Mr. Stebbins in a previous paper to the Society; but he thought it best to review them and add some new facts. He now went on to describe the new dyes, the results of his recent researches.

The first of these new compounds is dinitro-oxy-azobenzol-ortho-oxysulphoxylbenzol, a compound intermediate between Griess's azosulphoxylbenzol-ortho-nitrophenol and Mr. Stebbins's azobenzol-trinitro-oxybenzol, and may be made from diazo-dinitrophenol and phenol-ortho-sulpho-acid. It crystallises in yellow-brown needles with metallic lustre, sparingly soluble in hot and cold water.

Another new body, made by Mr. Stebbins, is azonaphthalin-sulphoxyl-ortho-nitroxylbenzol, which is similar to Griess's azosulphoxylbenzol-orthonitrophenol. It is produced by treating diazonaphthionic-acid with orthonitrophenol. It forms red-brown microscopic needles, very soluble in water.

Up to the present time no attempts have been made to combine more than two sulpho-groups (HSO_3) in one compound. It occurred to Mr. Stebbins that possibly three such groups might be combined, and with this point in view the following body was produced:—

Para-azosulphoxylbenzol-beta-oxydisulphoxyl-naphthaline.—This body can be produced by several methods, but the simplest of these is by combining diazosulphanilic acid with naphtholdisulpho-acid. The free acid crystallises in pretty orange leaflets with beetle-green lustre, soluble in water with an orange colour. A lead salt of this compound was formed, giving a yellow powder soluble in water.

Azobenzol-disnlphonaphthol was the next body described. It is made by treating the soda salt of beta-naphtholdisulpho-acid with diazobenzol-nitrate. This body forms both soda and barium salts, the former very soluble in water, and the latter only sparingly soluble in that liquid.

By combining diazo-toluol-nitrate and beta-naphthol-disulpho-acid, para-azo-toluol-beta-naphtho-disulpho-acid is formed. It is a fine scarlet dye. The free acid forms red-coloured leaves very soluble in water. The soda-salt is very soluble in water, and the barium salt is only sparingly soluble in that liquid. Mr. Stebbins stated that he had made the ortho and meta compounds of this last body in the same manner as the para compound just described, and that they resemble it more or less, giving only different shades of colour, one more yellow and the other more red. These last are also scarlet dyes soluble in water.

Parazo-sulphoxyl-naphthalin resorcin was produced by treating diazo-naphthionic acid with resorcin. The free acid of this body gives dark brown needles, very soluble in water.

The last body described was parazo-dibrom-sulphoxyl-benzol-beta-naphtha, obtained by allowing para-diazo-dibrom-benzol-sulpho-acid and beta-naphthol to act upon one another. The free acid is pretty soluble in hot water.

This highly interesting paper was illustrated by a number of samples of silk dyed with the new colours, whose richness and beauty of shade caused them to be much admired.

In the discussion which followed, Mr. Stebbins promised to finish up some of the details of the paper in the Fall, and remarked that many of the new colours were much faster than those of aniline.

Dr. A. R. Leeds read a paper "*Upon the Action of Light and Darkness on Standard Tannin Solutions.*" In a previous paper the author had reviewed the labours of Schlœsing and Muntz upon the dependence of nitrification upon the presence of an organised ferment, and R. Warington upon the necessity of darkness to the development of the nitrifying body. The amount of ammonia converted into nitrous acid under the action of a fungoid growth in a standard solution of ammouium chloride placed in the dark for a considerable period, had also been given. In this communication the author stated that similar ammonium chloride solutions, from which, however, oxygen was excluded, did not change under the influence of a fungoid growth, in light or darkness, and with or without chloroform. Tannin solutions containing saproligneous growths, and to which the access of oxygen was permitted, changed little in diffused light, but to a great extent in darkness; the development of fungi being large, and the percentage of tannin diminishing during the lapse of eighteen months as much as 8 per cent. The presence of chloroform in the latter case retarded, though it did not prevent, the fungoid growth. The conclusions drawn from the experiments narrated were that the circumstances most favourable to the indefinite preservation of standard solutions of this character are exposure to light, exclusion of air, and the absence of germs of fungoid growth.

Dr. Leeds also presented a memoir "*Upon certain New Methods in Actino-Chemistry, with their application to the Sun, Electrie and Magnesium Light, and to the Actino-Chemical Analysis of the Atmosphere.*" The results obtained in three preceding communications, made to the Society in 1878 and the spring of 1879 were summarised, and the confirmation of the laws then announced which governs the rate of change of the soluble iodides in

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* Communicated by Arthur H. Elliott, F.C.S., Recording Secretary.

the presence of actinic rays and of dilute acids was afforded by the results subsequently obtained by the use of chemically equivalent solutions of iodides and acids. The amount of iodine set free in the presence of hydrochloric acid in the sunlight was shown to be greater by a definite ratio than that liberated by sulphuric acid, whatever might be the nature of the iodide employed. The amount of decomposition effected by heat, the electric and magnesium light, was also given, and the relative amount of absorption of the actinic rays emitted from these sources, when passed through absorbing media.

NOTICES OF BOOKS.

Kelly's Directory of Chemists and Druggists. This work includes Chemical Manufacturers, Wholesale Druggists, Drysalters, Patent Medicine Vendors, and other Trades connected therewith, of England, Scotland, and Wales, and the principal Towns in Ireland. Fourth Edition. London, Kelly and Co.

In no department is a thoroughly accurate and complete directory more needed than in the chemical arts and manufactures. We therefore opened the present volume hoping that the want might at last be supplied, and that consumers of chemicals would no longer be put to trouble in finding where any given article was to be obtained. To decide on the value of such a work we must consider, firstly, the plan on which the authors have proceeded—what they have omitted and what they have included—and secondly, how this plan has been carried out.

In the first respect we cannot pronounce the compilation satisfactory. Turning to the index, we find no mention of manufacturers of bronze-powders, litharge, white-lead, ink, printers' ink, varnishes, gun-cotton, washing-powders. Soap makers are only to be found under the heading "Fancy Soap Manufacturers," where the enquirer is not likely to begin his search. Oil merchants and oil refiners are also absent. The manufacturers of and dealers in Prussian blue and ultramarine, if included, are not specially mentioned in the index.

On the other hand, valuable space is taken up by the insertion of herbalists, medical botanists, and of the hospitals and infirmaries. These items, indeed, together with the dentists, chiropodists, and veterinary surgeons, ought, we think, to be referred to a distinct medical directory.

Again, there figure here the makers of arrowroot, chocolate, cocoatina, dandelion-coffee, lozenges, extracts of meat, baking-powders, and gluten-bread. If such trades are admitted we see no just reason for not likewise including the manufacturers of sauces, pickles, jam, confectionery, tinned fruits, sardines, German yeast, &c. But the makers of all such substances ought to be classed along with the grocers, provision merchants, &c., and would be quite numerous enough to require a separate directory. We have grave doubts also concerning the photographers, whom we should be disposed to place along with artists, engravers, lithographers, &c.

It will be of course admitted that the more heterogeneous the contents of such a book the greater will be the difficulty of finding what is wanted.

The execution of the directory is much better than the plan. Under many places with which we are minutely acquainted we find the information given is quite correct. But even in this respect there are exceptions. Thus, at Aylesbury, we note the omission of a manure manufacture and of two manure merchants. We find the name of "Wanklyn, James A.," curiously placed among the chemists and druggists. Turning to the copperas manufacturers, we see that some of the most important are omitted, one in Droylsden, one in Leeds, and several in Wales.

The "bisulphite of lime" manufacturers figure here as makers of bisulphate of lime—a confusing error. Under "Tin Solution Manufacturers" we miss four firms of whose existence we are certain. Among analytical chemists now in practice we miss several, two of whom, we believe, are at present holding appointments as public analysts.

Enough has been said, we think, to show that the work requires a careful revision, and if the publishers could be induced to simplify its plan by the omission of trades and manufactures which cannot fairly be considered as chemical its utility would be much enhanced.

A Practical Treatise on Sea-sickness: its Symptoms, Nature, and Treatment. By G. M. Beard, M.D. New York: E. B. Treat.

In this little pamphlet the author combats successfully certain popular errors. He proves that sea-sickness is not a joke or a mere trifle; that instead of being ultimately beneficial it frequently entails prolonged or even permanent mischief, and sometimes proves fatal; that it is an affection, not primarily of the digestive organs, but of the nervous centres, and that it may be successfully combatted by means of alkaline bromides,—preferably the sodium salt—taken in full doses of from 30 to 90 grains. This treatment should be commenced two or three days before going on board, and continued till the danger is over. If this treatment is deferred till the attack has actually set in success is doubtful, as the remedy may then be rejected by the stomach before it can be absorbed into the system. Champagne, brandy, &c., the author pronounces useless. We hope his advice will be taken in this respect, since the atmosphere of a saloon where many of the passengers have unsuccessfully dosed themselves with brandy is truly distressing.

If Dr. Beard's treatment proves as successful as the cases described seem to warrant he may fairly take rank as a benefactor to the human race.

Manual for the Physiological Laboratory. By Vincent Harris, M.D., and D'Arcy Power. London: Baillière, Tindall, and Cox.

The tyro in physiological research is not bewildered with choice among a legion of manuals and handbooks, as is the chemical student. The authors, who have been for some years engaged in teaching practical physiology at the medical school of St. Bartholomew's Hospital, have found that the manuals of Foster and Langley, Schäfer, and Rutherford are too elaborate for the ordinary student, and do not contain any epitome of histology. Hence a deficiency exists, which they have endeavoured to supply by collecting and publishing the papers which have been yearly issued to their students, and which embrace an abstract of the histology and chemistry of the various organs, together with the methods of preparing tissues for microscopic examination. In our opinion, they have succeeded in producing a treatise which will be widely useful.

In the introduction we find directions for the preparation of tissues for microscopic examination—hardening, cutting, staining, and mounting.

In Part I. we come to practical histology. A list is first given of the apparatus and reagents necessary for the student, after which follows the consideration of blood, epithelium, endothelium, the connective tissues, cartilage, bone, muscular tissue, nerve tissue, the blood vessels, the alimentary canal and the glands connected with it, trachea and lung, skin and appendages, genito-urinary organs, the lymphatic system, the eye, the ear, the teeth, the nose, the mammary gland, and the ductless glands. In each case we find instructions for preparation, and an account of the structure of the part or organ.

The second portion of the book deals with physiological chemistry, and begins with a list of apparatus and experiments required, and with memoranda of experiments.

Under each substance are given the general properties and characteristic reactions.

An appendix gives an account of the injection of blood-vessels and lymphatics, the action of gases, chloroform, alkalies, alcohol, and electricity on the blood; the method of feeding the colourless corpuscules, the preparation of glycogen, leucine, biline, cholesterine; notes on the development of teeth; and a classification of staining fluids.

The chief objection we find to this book relates to its getting up. Out of a total of 124 pages, including the index, we find 15 entirely blank and 14 more than half-blank. We thought at first that this space had been purposely left for the introduction of notes by the student, but the position of the blank leaves bears no apparent reference to such a use. Surely useful and appropriate matter might have been found to fill up these "aching voids."

Science Lectures delivered in Manchester, 1879-1880. Eleventh Series. Manchester and London: John Heywood.

On opening this little book we were greeted by the unpleasant announcement that these lectures have been brought to a close owing to the want of interest on the part of the public. The attendance no longer justified the Committee in continuing the experiment which they had taken in hand. Everyone who glances over the list of subjects which have been treated of, and the names of the lecturers, must admit that Prof. Roscoe and his colleagues have done their part, and that Manchester has had a fair trial. The result, then, for the present at least, must be accepted as conclusive. It points, we fear, to the inference that the attendance at first was largely due to the novelty of the undertaking rather than to any widespread and abiding interest in science. We fear, consequently, that the similar courses of lectures which have been established in other large towns will have but a transient success.

In this final series three of the lectures, though of a most valuable and instructive character, scarcely come within the scope of the Chemical News. "Traps to Catch Sunbeams" is a curious title for the lecture on solar radiation, delivered by Capt. W. de W. Abney, F.R.S. The speaker began by pointing out the error conveyed in George Stephenson's dictum that coal is "bottled-up sunlight." He then explained the nature of radiant heat, which he said ought rather to be called radiant energy. He gave a description of the sun's surface, illustrated by one of Janssen's photographs, and entered upon the question of sun-spots and their cyclical recurrence, with their possible influences upon the seasons. In order to arrive at a complete knowledge of these sun-cycles he proposed the systematic measurement of solar radiation as a problem of national importance. He calculated the heat of the sun last year at 25000°, or more than twelve times the heat required to melt platinum. His account of the modifications of the solar spectrum due to atmospheric changes, and by which the approach of rain or of an easterly wind can be foretold, is exceedingly striking. In short, the character of the whole lecture can only serve to heighten our regret and surprise that it should be so little appreciated.

The Spirit of Nature: being a Series of Interpretative Essays on the History of Matter from the Atom to the Flower. By Henry Bellyse Baildon, B.A., Member of the Pharmaceutical Society, Author of "Morning Clouds," "Rosamond, a Tragic Drama," &c. London: J. and A. Churchill.

Mr. H. B. Baildon is a writer of multifarious pursuits and of attainments rarely combined by the same person. Like Apollo, he is devoted at once to poetry and to pharmacy. His acquaintance with chemistry and with botany is very much in excess of mere professional requirements. He is also a metaphysician, a theologian, and a classical scholar. We find in his book conceits quaint and even beautiful, as when he pronounces ferns to be "the Classics of Nature, owing their charm to symmetry, balance, refinement, restraint." The sense of beauty, whether it be a simple or a compound faculty, is evidently predominant in his mind, and gives a tinge to all he writes. Scarcely less prominent is another feature: although describing himself as "naturally approhensive, sensitive, and timid," he is very far from undervaluing Mr. H. B. Baildon.

Turning now from the author to the book, we have to ask what do these "interpretative essays" really interpret? Upon what phenomena or what generalisations do they throw any new light? In the "prologue"—we cannot see why the ordinary and unobjectionable term "preface" is discarded—he declares that his purpose is an attack on materialism. To such warfare we can have no possible objection. But the question sometimes occurs to us whether "scientific materialism," or a materialistic tendency among scientific men, is really so common, prominent, and formidable as a certain school of writers and orators take for granted? We doubt whether the crusades preached against this same materialism may not have objects. We find in the work before us, published in this year 1880, a critique on the world-famed "Belfast Address" of Prof. Tyndall! The very words "promise and potency of all terrestrial life" are brought forward for a searching analysis. We should have thought that this Address, and these words in particular, had been already discussed *ad nauseam*, and that nothing both new and appropriate remained to be said upon the subject. Mr. Baildon seems to be of a different opinion, though whether he has succeeded in saying anything which had escaped previous writers is, to say the least, somewhat doubtful.

An important feature of the work before us is the contention that the law of Evolution, in its strictest sense, is not supported by the analogy drawn from chemical science. This analogy, he declares, "threatens continued hostility to the theory of Evolution so long as that theory maintains a unity of source for all life." His ground for this utterance is that the commonly received elements have not been resolved into one simple ultimate element. Of course it would be premature to declare that any one of these elements has yet been decomposed, and possibly their decomposition may lie beyond human resources. But is it legitimate to found an argument against Evolution upon what is possibly a mere temporary inability? We conceive that the analogy may be reversed: finding Evolution wherever we are able to see with sufficient distinctness, is it unfair to argue that the simple bodies of chemistry have also been evolved, perhaps from a few principles, perhaps from one primordial form of matter? Mr. Baildon, indeed, admits facts which, if furnishing no direct proof, are at least strongly in favour of the hypothesis in question.

The chief novelty in the work before us is a re-statement of the law of combination by weight, which the author proposes to express thus:—"That the weight of the atom of each element is a constant quantity, so that elements can only combine with each other in certain constant proportions or in multiples thereof." To this law is appended the corollary to the effect that:—"Compound bodies behave according to the same law as simple ones, their combining weights being the sum of the combining weights of their constituent elements." This simplification of laws of combination seems to us an improvement. Somewhat curious, however, is the accompanying definition of a chemical atom:—"That moiety of an element which is capable of combining with such moiety or more of any other element." The term "moiety" is here employed in a questionable manner. It is generally understood to signify a half share of anything in number, extent, or value. If we are to estimate a book in proportion to the novel facts or generalisations which it brings forward, or to the new light which it throws upon phenomena and laws already recognised, the "Spirit of Nature" cannot take a very high rank. The author's anti-materialistic polemics, though well meant, savour of an *Ilias post Homerum*. The general impression produced upon our mind is that Mr. Baildon's centre of gravity—if we may use the expression—lies not in science but in poetry.

Pocket Mining Atlas, compiled from the latest Official Surveys and the most authentic sources. New York: Edwin Bolitho. (Engineering and Mining Journal.)

This atlas, as its name expresses, is composed of maps of the mining regions of the United States. Among these we may mention in particular the Dahlonega district, the copper and iron regions of Lake Superior, and the Deadwood, in Dakota, where the different mining properties seem to overlap each other, in some cases three deep. There are further maps of the Leadville district, in Colorado, the Central City and Silver Cliff regions, and of Caribon Hill in the same state. The Comstock, Eureka, Treasure Hill, and Tuscaroora districts in Nevada, and the Bodie in California make also a prominent figure. There are no indications of the geological character of the districts, which in maps of so small a size could perhaps scarcely have been introduced so as to be useful. Speculators in American mining properties will doubtless find this atlas valuable.

Special-Katalog der Königlich-Rheinisch Westfälischen Technischen Hoch-Schule zu Aachen. Aachen: Palm.

We have here a catalogue of the articles exhibited by the celebrated Polytechnic School of Aachen at the Dusseldorf Exhibition of Arts and Trades. Chemistry and physics do not appear to play a very prominent part in comparison with mechanics. We may notice a set of chemical preparations by Dr. A. Polis, and a model of sulphuric acid works for lecture purposes, constructed and presented by Dr. R. Hasenclever.

We find a general view of the School and of its new chemical laboratory, which are certainly most imposing structures. We must not, however, quote Waterton's—

> "The walls are thick, the servants thin;
> The gods without, the devil within,

since the internal appliances, in the shape of apparatus, specimens, models, and every other requisite, are on an even more liberal scale, and the numerous and thoroughly qualified staff of professors, assistants, and curators are a sufficient proof that the establishment is meant and fitted for sound earnest work.

CORRESPONDENCE.

IRISH BOG BUTTER.

To the Editor of the Chemical News.

Sir,—Mr. W. A. Carius, whose letter on "Irish Bog Butter" appeared in the Chemical News, vol. xli., p. 295, may be interested to know that, in the February number of the *Analyst*, there is a report of an interesting paper on this subject by Mr. G. W. Wigner, F.C.S., and Professor Church, M.A., F.C.S., read by the former gentleman at the meeting of the Society of Public Analysts held on January 5, 1880.

This paper contains the results of a careful chemical examination of two specimens of ancient butter, and if my memory does not deceive me, one of them presented evidences of having been enclosed in some kind of keg or tub. Reference was also made in the paper to the specimen of bog butter preserved in the Museum of the Royal Irish Academy.—I am, &c.,

Edwy Godwin Clayton.

TIN IN PRESERVED FRUITS.

To the Editor of the Chemical News.

Sir,—In reply to A. Percy Smith's query in the Chemical News, vol. xlii., p. 34, tin salts act as irritants, but as to the amount necessary to produce anything serious, I am not in a position to say.

It has long been known that tinned fruits, if kept for some length of time, get impregnated with the metal of the containing vessel.

Never having seen any published quantities of the metals so dissolved in the syrup, I give the following as being the highest in their respective kinds I have yet found, in the hope that others, who may have investigated the subject, may do the same.

Per 2 lbs. tin of Pine-apple—0·21 grain SnO_2, 0·39 grain Fe_2O_3.

Greengages—0·36 grain SnO_2, 7·22 grains Fe_2O_3.

Apples—0·34 grain SnO_2, 15·28 grains Fe_2O_3.

The two latter were same age, the exterior of the cans being weather-beaten. They were not edible, the metallic taste predominating so much; in the pine-apple the taste was perceptibly metallic.

The syrup in all the tins had visibly acted upon the containing vessel, not only at the level of the liquid, but all over the covered metal. On pouring out the contents, the surface was beautifully crystalline, similar to the *moiree metallique* produced by the action of HCl or H_2SO_4 upon tin. In no case did I detect lead.—I am, &c.,

George J. Wishart.

18, Picardy Place,
Edinburgh, July 20, 1880.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Note.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 26, June 28, 1880.

Researches on the Determination of the Wave-lengths of the Thermic Rays at Low Temperatures.—P. Desains and P. Curie.—The authors have endeavoured to solve this problem by the use of Fraunhöfer's net-work, employing a tissue of metallic wire of $\frac{1}{8}$ millimetre in diameter. Their results are given in the form of a table.

Evaporation-heat of Sulphuric Anhydride.—M. Berthelot.—The heat evolved by SO_3 solid being, according to the author's former determinations, + 18·7, he concludes that the evaporation of sulphuric anhydride ($SO_3 = 40$ grms.) at about 18° absorbs − 5·9; for S_2O_6, − 11·8, a figure very near that of the heat absorbed by the evaporation of the same volume of solid water at 0° (− 12·3).

On Certain General Relations between the Chemical Mass of the Elements, and the Formation-heat of their Compounds.—M. Berthelot.—The author asks what relations exist between the elementary chemical masses known as equivalents or atomic weights and the quantities or heat evolved, *i.e.*, the molecular work effected in chemical combination? If we examine the compounds formed in multiple proportions, we find that the heat evolved by the successive combinations of two elements, or of two bodies which are themselves compound, decreases gradually as one of the elements accumulates. As examples he takes the combinations of sulphur and of nitrogen with oxygen. The chief work is generally accomplished in the first act, which approximates the heterogeneous molecules. Hence, the systems formed are generally the less stable the greater is their complexity. The last compounds are formed with extremely small disengagements of heat, in as far as these result from a purely chemical cause. When the components are not in the same physical state we obtain, in addition, the heat due to a change of such state, *e.g.*, the liquefaction of a gas or the solidification of a liquid, which is proportional to the number of elements fixed. Organic compounds of the same function evolve about the same quantity of heat when they experience a similar transformation. The close analogy which exists between the equivalents as well as between the majority of the properties of nickel and cobalt occurs also in the approximate identity of the formation-heats of their binary compounds. In the majority of elements a totally different relation prevails.

A New Form of Galvanometer.—L. Gastynski.—This galvanometer is intended for the measurement of thermo-electric currents, and is distinguished by the association of two astatic systems in the same direction. Its chief advantage lies in its proportionality, which may be extended to near 90°, and which dispenses with the construction of tables.

Existence in the Smoke of Tobacco of Hydrocyanic Acid, an Alkaloid as Poisonous as Nicotine and Various Aromatic Principles.—G. Le Bon and G. Noel.—The alkaloid in question has a pleasant odour but is dangerous to inhale, and has proved fatal to animals in doses of 1-20th of a drop. It appears identical with collidine, the existence of which has been traced in the products of the destructive distillation of several organic substances.

Vibrations on the Surface of Liquids.—F. Lechat.—The author, setting out from the general equations of the slight movements of liquids, establishes the mathematical theory of the vibrations on the surface.

Energetic and Constant Galvanic Battery yielding Residues Capable of being Regenerated by Electrolysis.—E. Reynier.—The zinc is immersed in a solution of caustic soda; the negative electrode, which is of copper, is depolarised by a solution of copper sulphate, separated from the alkaline liquid by a porous septum. A couple thus arranged is constant, and its electro-motive force is from 1·3 to 1·5 volt., according to the concentration of the liquids. The author diminishes the resistance of the soda and copper sulphate by the addition of salts suitably chosen. For the porous septum he takes several leaves of parchment-paper laid upon each other. No volatile products are formed.

Mechanical Effects Produced in the Magnetic Nucleus Submitted to the Magnetising Action of an Electric Current.—M. Ader.—The author's experiments show that all bars of a magnetic nature if submitted to pressure, torsion, or traction tend to resume their primitive molecular disposition under the influence of the magnetising current.

Independent Optical Compass for Ironclads.—M. de Fraysseix.—The author proposes to fix a compass at such a height that it is not affected by the iron work of the ship and to render it visible to the steersman by means of a special optical arrangement.

Thermic Study of the Alkaline Polysulphides.—P. Sabatier.—The successive equivalents of sulphur dissolve in dilute sodium sulphide, evolving +0·8 cal. per equiv. This value is the same for the tetra-sulphides of potassium and sodium.

Etherification of Hydriodic and Hydrochloric Acids.—A. Villiers.—Hydriodic acid behaves in a manner very similar to hydrobromic acid, but the etherification is effected much more rapidly. Hydrochloric acid is etherified much more slowly than the two other hydracids and than the organic acids.

Crystalline Anhydrous Lime.—A. Levallois and S. Meunier.—The crystals in question, which contain from 96·5 to 97·3 per cent of calcium oxide, have been obtained from the sides of a lime-kiln, which had been kept at a temperature of 1200° to 1300° for twenty-eight consecutive months.

Zeitschrift für Analytische Chemie.
No. 1, 1880.

Application of the Electric Current in Analytical Chemistry.—C. Luckow.—Inserted at length.

Method of Detecting and Determining Iodine in Presence of Chlorine and Bromine.—E. Donath.—Inserted at length.

The Solution of Arseniferous and Antimoniferous Compounds.—E. Donath.—Already inserted.

Behaviour of the Juices of Fruits of Different Ages with Reagents.—F. von Lepel.—An extensive memoir, not capable of useful abstraction and requiring the accompanying illustrations. It is of considerable value for the detection of sophisticated wines, fruit-syrups, &c.

Detection of Salicylic Acid in Wine and Fruit-Juices.—Dr. L. Weigert.—The well-known reaction with ferric chloride is not available in deeply-coloured liquids, especially if the proportion of salicylic acid does not exceed 2 to 3 per cent. The author shakes up 50 c.c. of the wine for some minutes in a flask with 5 c.c. of amylic alcohol, pours off the supernatant liquid into a test-glass, and mixes with it an equal volume of alcohol, in which the colourless amylic dissolves. To this mixture are added a few drops of the dilute solution of ferric chloride, which produces the usual well-known deep violet colour.

Composition of Various Foods for Children.—Dr. N. Gerber.—A tabular view of analytical results.

Supplement to Dietrich's Table for Calculating Nitrogen in Places where the Barometer is Low.—E. Trachsel.

Communications from the Laboratory of Prof. R. Fresenius at Wiesbaden. A New Method for Determining the Value of Zinc Powder.—V. Drewsen.—The author prepares two solutions; the one of pure fused potassic dichromate—say 40 grms. per 1000 c.c.—and the other of crystalline ferrous sulphate, about 200 grms. in 1000 c.c. The iron solution must be strongly acidulated with sulphuric acid, to prevent oxidation. In order to find the respective value of the two liquids, 10 c.c. of the iron solution are measured into a beaker, a little sulphuric acid is added, and the other solution is dropped in from a burette until a drop of the mixture is no longer turned blue by potassic ferricyanide. About 1 grm. of the zinc powder is then weighed, placed in a beaker with 100 c.c. of the chromic solution, 10 c.c. of dilute sulphuric acid are added, the whole is well stirred, 10 c.c. more of the sulphuric acid are added, and allowed to act for about a quarter of an hour with diligent stirring. When everything is dissolved except a small insoluble residue, an excess of sulphuric acid is added, and 50 c.c. of the iron solution in order to reduce the greater part of the excess of chromate; more of the iron solution is then added from a burette till a drop displays a distinct blue reaction with ferricyanide, and the mixture is then titrated back with chromate till this reaction disappears. From the total number of c.c. of the iron solution consumed the quantity is deducted which corresponds to the ferrous solution employed. The chromate contained in the remainder if multiplied by 0·66113, shows the metallic zinc contained in the powder.

The Determination of Sulphur in Sulphur Ores.—R. Fresenius.—An examination of Dr. G. Lunge's method as proposed in his "Manual of the Alkali Industry," i., 96.

Apparatus for Determining the Specific Gravity of Solid Bodies.—F. Frerichs.—This paper requires the accompanying illustration.

Chromometry.—G. A. Kœnig.—A modified application of the principle of the complementary colorimeter. Instead of dissolving an ore and examining the colour of the solution, the author dissolves a small sample (5 m.gs.), carefully weighed in a bead of borax of known weight. He causes the melted bead to be drawn up into a fine platinum tube, and when cold, grinds off the projecting parts at both ends, so as always to obtain a stratum of equal length. He then looks through it, and through a glass wedge of a complementary colour, and observes what thickness of the wedge renders it colourless. By operating with specimens of known percentage a scale may thus be establised.

Steinle and Hartung's Graphite Thermometer and Pyrometer.—C. Winkler.—An illustrated paper.

Improvements in Orsat's Apparatus for the Analysis of Chimney Gases.—F. Fischer.—An illustrated paper. See *Dingler's Polyt. Journal*, ccxxvii, p. 257; and ccxxx., p. 480.

Assay of Platinum Alloys.—Nilson W. Perry.—From the New York *Engineering and Mining Journal.*

New Reaction for Salicylic Acid.—H. Schulz.—If a watery solution of salicylic acid, or better, of sodium salicylate, is mixed with a little solution of copper sulphate, the liquid, which was previously colourless, turns to an emerald green. The addition of a stronger acid destroys the green colouration.

Varrentrap and Will's Process for the Determination of Nitrogen.—In case of substances rich in nitrogen the addition of sugar is recommended by Prehn and Hornberger. Kreussler points out that in the analysis of proteic bodies, the results yielded by the Varrentrapp and Will process are generally too low, and those furnished by the Dumas process too high. He examines the sources of these errors at some length.

Determination of Zinc in Spring-waters.—E. Mylius.—In cases where iron is absent the author proposes the following colorimetric method:—2·60 grms. potassic ferrocyanide are made up to 100 c.c., and 3·536 grms. zinc sulphate to 1000 c.c., so that 1 c.c. of the latter solution contains 1 m.g. ZnO. Into a test-glass of about 4 c.m. in width there are poured 200 c.c. of the water to be examined, which should be clear and filtered. In another glass are placed 200 c.c. of spring-water free from zinc. To each is added an equal quantity, which may be from 5 to 8 drops of hydrochloric acid, and 2 c.c. of the solution of ferrocyanide. To the water free from zinc there is then added from a narrow burette, drop by drop, so much of the zinc solution as will render the turbidity equal to that in the water containing zinc. After this preliminary experiment a cylindrical glass is again charged with 200 c.c. of the water to be examined, 2 c.c. of the ferrocyanide solution and 8 drops of hydrochloric acid. Five other glasses are then charged each with 200 c.c. of water free from zinc, ferrocyanide and acid as before, and different quantities of the zinc solution. All the cylinders are then allowed to stand for half an hour, protected from too full daylight, and the turdidities produced are then examined. The author remarks that the parish well at Tuttendorf, which contains 0·007 gr. ZnO per litre, has been used for drinking for about a century without any percetible injury.

The Ash in Wines.—H. Kayser.—In genuine wines, the ash bears to the extract the proportion of 1 : 10, with but little fluctuation. Other authorities make the range greater.

Method of Detecting Dyes used in Colouring Red Wines.—A. Gawalovski.—From the *Pharm. Central-Halle.*

Determination of the Extract of Wort and Beer.—V. Griessmayer.—Not capable of useful abstraction.

Analysis of Soaps.—Jul. Loewe.

Les Mondes, Revue Hebdomadaire des Sciences.
No. 1, May 27, 1880.

Contains no original chemical matter.

June 3, 1880.

This issue contains no chemical matter which has not appeared elsewhere.

June 10, 1880.

This issue is filled with extracts from various journals. Such of them as relate to chemistry and physics have been already noticed.

June 17, 1880.

Condensation of Gases on the Surface of Glass.—M. Chapuy.—From a surface of glass of 1·6752 square metre there were evolved, in the author's experiments, from 0°—180°, 0·45 c.c. hydrogen, 0·58 air, 1·03 sulphurous acid, and 1·41of ammonia.

Annales de la Société des Sciences Industrielles de Lyon
No. 1, 1880.

Galena Produced at the Lyon Glass Works.—F. Gonnard.—Well defined crystals of galena have been discovered at the bottom of a disused melting-pot.

Note on Certain Mineralogical Facts observed in the Granites on the Banks of the Saone.—F. Gonnard.—Among the minerals observed are orthose, pinite, pegmatite, garnets, almandine, black tourmaline, chlorophyllite (?), sericite (?), and rutile.

Reimann's Färber Zeitung,
No. 22, 1880.

The Fixation of Colouring Matters by Silica.—The author controverts the theory of G. Engel that the fixation of colours is due to capillarity. Experiment shows that precipitated silica attracts and fixes aniline colours, Turkey-red, &c., more strongly than siliceous infusorial earth, which alone of the two substances can be said to possess capillarity.

No. 23, 1880.

It is announced that Dr. O. N. Witt, the discoverer of chrysoidine, removes to Mulhouse in order to carry on certain investigations in the laboratory of Prof. Noelting.

The celebrated print works "Impressione Alsacienne" are in course of being dismantled and sold piece-meal.

No. 24, 1880.

Bleaching Tussah Silk.—It is asserted that the difficulty encountered in bleaching this silk is due to the circumstance that many parcels of it consist of two distinct kinds of fibre. One portion is readily bleached, and on microscopic examination displays the structure of silk. The other and larger portion is yellow, hard to bleach, and has a flax-like appearance.

Bulletin de la Société d'Encouragement pour l'Industrie Nationale.
No. 76, April, 1880.

This number contains no chemical matter.

Wanted, a Manager who thoroughly understands the making of Bisulphide of Carbon. A man with a little capital can have a share in the business. A first rate chance for a really good man.—Apply to Jos. Harris, 104, Corporation Street, Manchester.

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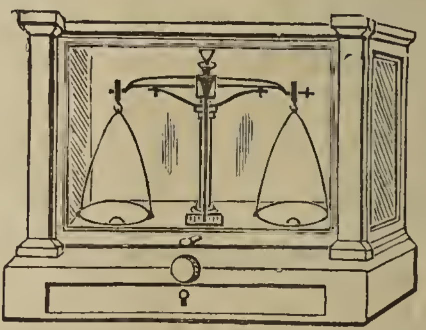

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THE CHEMICAL NEWS.

VOL. XLII. No. 1079.

NEGATIVE BONDS, AND RULE FOR BALANCING EQUATIONS.

By OTIS COE JOHNSON.

PROF. HUGO SCHWANERT, in his new "Lehrbuch der Pharmaceutischen Chemie," gives a rule for the construction of chemical equations. His method depends upon algebraical principles, as does also that of James Bottomley, read before the Manchester Philosophical Society, February 19, 1877, and given in the CHEMICAL NEWS, vol. xxxvii., p. 110.

My method is a chemical one, and is derived directly from principles of oxidation. It is founded upon a system of negative bonds. I here use the term bond in a sense partially new, which can better be understood by the illustrations that follow than by any definition which can be framed.

By the *bonds* of an element is meant the *amount of oxidation* it has sustained, or a *bond* is the *measuring unit of oxidation.*[*] The terms positive and negative are used *not* in an electrical but in a strictly numerical sense. The bonds of an element may always be ascertained with certainty by one of the following rules:—

(1.) Free elements have no bonds, and may be represented by zero, written thus—Cl° or Cl_2°, Ba°, Al°, &c.

(2.) Hydrogen in combination has always one bond, and it is positive, written thus, H^{I}.

(3.) Oxygen in combination has two bonds, always *minus*, written thus, O^{-II}.

(4.) The bonds of the metals in combination are usually *plus*. The most prominent exceptions are their combinations with hydrogen, $Sb^{-III}H^{+III}$, $As^{-III}H^{+III}$.

(5.) The sum of the bonds of any compound is always zero. Thus, $H^{I}N^{+V}O_3^{-VI=0}$; that is, the H has one positive bond, the N five positive bonds, and each atom of O has two negative bonds, and the three atoms have six negative bonds: six positive bonds added to six negative equals zero. In the same way, $H^{I}Cl^{+V}O^{-VI=0}$, $H_2^{II}S^{+VI}O_4^{-VIII=0}$, $H_3^{III}P^{+V}O_4^{-VIII=0}$,

$$Ba^{II}O_2^{-IV}H_2^{+II=0},\ Fe_2^{VI}O_6^{-XII}H_6^{+VI=0},$$

$$Ba^{II}H_4^{+IV}P_2^{+X}O_8^{-XVI=0}.$$

(6.) Acid radicals are always negative, $H^{I}I^{+V}O_3^{-VI=0}$. or $H^{I}(IO_3)^{-I=0}$, $Pb_3^{VI}P_2^{+X}O_8^{-XVI=0}$,

$$\text{or } Pb_3^{VI}(PO_4)^{-VI=0}.$$

The bonds of the radicals being equal to the number of atoms of hydrogen with which it is capable of combining.

An *oxidising agent* is one that can increase the number of bonds of some other substance; hence oxidation of one substance *must* involve the reduction of some other. The number of bonds gained by one are lost by the other. The real bonds are transferred from the oxidising to the reducing agent. Thus in the equation—

$$2PbCrO_4+5Zn+16HCl=2Pb+Cr_2Cl_6+5ZnCl_2+8H_2O,$$

it can be proven that the ten bonds lost by the lead chromate are transferred to the zinc.

From these principles is derived a rule for writing equations, by which every equation involving oxidation may be balanced almost at a glance, if we know the products formed. The rule is:—*The number of bonds changed in one molecule of each shows how many molecules*[*] *of the other must be taken*, the words *each* and *other* referring respectively to *oxidising* and *reducing* agents. A few equations will illustrate:—

$$2HNO_3+3H_2SO_3=3H_2SO_4+2NO+H_2O.$$

The nitrogen in HNO_3 has five bonds, and in NO it has two, losing three, therefore three molecules of H_2SO_3 must be taken. The sulphur in H_2SO_3 has four bonds, and in H_2SO_4 it has six, gaining two, therefore two molecules of HNO_3 must be taken. Again,—

$$3Sn+4HNO_3=3SnO_2+4NO+2H_2O.$$

The Sn gains four bonds (free elements having no bonds), hence four molecules of HNO_3 must be taken, and the N of HNO_3 losing three bonds, three of Sn must be taken.

$$6Sb+10HNO_3=3Sb_2O_5+10NO+10H_2O.$$

Here the rule calls for three of Sb and five of HNO_3, but since the product Sb_2O_5 cannot be written with an odd number of atoms of antimony, we must double the amount of each, and instead of three and five take six and ten.

$$3H_2S+HNO_3=3H_2SO_4+8NO+4H_2O.$$

Here the sulphur in H_2S has -2 (*minus* two) bonds, and in H_2SO_4 it has $+6$, so it has gained eight bonds; hence we must take eight molecules of HNO_3, and we take three of H_2S, because the nitrogen loses three bonds.

$$3Sb_2S_3+28HNO_3=3Sb_2O_5+9H_2SO_4+28NO+5H_2O.$$

In this equation both the sulphur and the antimony of Sb_2O_5 are oxidised; each atom of sulphur gains eight bonds (as explained above), and the three atoms will gain twenty-four bonds. Each atom of the antimony gains two bonds, the two atoms gaining four bonds. These added to the twenty-four bonds gained by the sulphur, make twenty-eight (that is, one molecule of Sb_2S_3 gains twenty-eight bonds); hence we must take twenty-eight molecules of HNO_3, also three of Sb_2S_3, because the nitrogen of the HNO_3 loses three bonds.

$$HIO_3+3H_2SO_3=3H_2SO_4+HI.$$

In this the rule calls for two HIO_3 and six of H_2SO_3, but we take one and three, which are in the same proportion. The rule gives relative quantities only.

$$3Pb+2+6HNO_3=3Pb(NO_3)_2+NO+4H_2O.$$

Here the rule requires two of HNO_3, and shows that exactly two are reduced to NO; but in order to make a solution of lead nitrate six more must be added, which are not reduced.

$$3NaH_2PO_2+7HNO_3=3NaNO_3+3H_3PO_4+4NO+2H_2O$$

Here the phosphorus gains four bonds, and four of HNO_3 are reduced to NO; but three more are required to liberate the hypo-phosphorus acid from $3NaH_2PO_2$, hence we take seven in all.

$$AsH_3+6AgNO_3+3H_2O=6Ag+H_3AsO_3+6HNO_3.$$

The arsenic in AsH_3 has *minus* three (-3) bonds, and in H_3AsO_3 it has *plus* three $(+3)$; the gain is six, therefore take six of $AgNO_3$.

$$3KNO_3+8Al+5KOH+2H_2O=3NH_3+4K_2Al_2O_4.$$

The nitrogen in NH_3 has -3 bonds, having lost eight, therefore take eight of Al; and as the Al gains three, take three molecules of KNO_3.

This article will be followed by another showing the use of the rule in organic chemistry.

University of Michigan,
July 7, 1880.

Carbazol.—C. Graebe.—The author describes the preparation of carbazol and potassium carbazol, and gives an account of the derivatives methyl-carbazol, ethyl-carbazol, ethyl-carbazoline, acetyl-carbazoline, tetra-nitro-carbazol, and the chlorine derivatives.—*Liebig's Annalen*, 202, 2.

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[*] As this definition of *bond* is a new and provisional one, possibly the adoption of a *new term* might be less objectionable than the use of new definitions for the established term, bond, as a character in quantivalence.

[*] In case of free elements, to avoid complexity, atoms are represented as molecules.

THE HEAT OF THE COMSTOCK LODE.*

By JOHN A. CHURCH, E.M., Ph.D., New York City.

(Concluded from page 43.)

I HAVE explained in a previous paper, and also in a subsequent publication,† that the movement of gaseous currents in the solid rock has the effect of bringing each part of the mass under the influence of all the heat produced in the rock below it, for the gas is one of the results of kaolinsation. It is released at the moment of alteration, and has no alternative but to take its way to the surface, carrying with it part of the heat produced, which it distributes through the whole mass of rock.

It is probably owing to the operation of this gaseous current that the immense quantity of heat observed is discharged into the mine levels, and the water may receive part of its heat in the same way. A water-course and a mining gallery are both localised things placed in fixed positions in the pathway of a hot gaseous current, and the heat they receive may be (and in the case of a gallery with the air in it certainly is) derived in part from alteration proceeding in the rock at considerable distances from them.

For this reason the conception of Mr. Phillips, that the rock "would require to be heated by the kaolinisation of its felspar to a temperature above that of molten gold," has no foundation either in the theory or facts presented by me. The problem laid before us in the mines of the Comstock is as complex as it is interesting. I am justified in saying of all other theories that they do not even attempt to explain the facts. The explanation I give is still merely a hypothesis, but at the present day it is unique in being based upon a careful statement of the observed phenomena.

Another of my critics is Prof. G. F. Barker, of the University of Pennsylvania, who is reported to have said that the rock of the Comstock is not uniformly heated to 130° F., the remark being made to Dr. J. P. Lesley, and apparently as a contradiction of a statement made in my paper. Prof. Barker's remark, as quoted by Dr. Lesley, was "that there was no uniform temperature; but, on the contrary, the most remarkable differences, some of the higher levels being much hotter than some of the lower levels." This state of things was fully stated by me; and I showed that, while the great mas of rock near the two-thousand level, had a "pretty uniform temperature of 130° F.," it also contained narrow belts, most of which are hotter, while some are colder than the general mass. In any given mine the drift on a high level may be run for a long distance in a hot belt, and be much hotter than a drift two hundred feet lower, if the latter is carried through what I have called the general mass of rock. I regarded the extreme local temperatures as significant phenomena, the explanation of which would carry with it the whole theory of heat production, and discussed them to that end.

Professor Barker's opinion is, that the heat is a hot-water heat, and that the water is heated by the movement of the rocks. It is true that incessant and great movement of portions of the rock is encountered in the mines; but the opinion in question does not take into consideration the character of this movement. Every miner on the lode will sustain me in saying that the motion consists in the swelling of parts of the rock when its conditions have been artificially altered by the excavations of the mines. The usual explanation is, that this swelling is caused by the admission of air and moisture brought in by the artificial openings, an impression in which I fully concur. There are no indications that this movement takes place at a distance from the drifts, and there is no sign of natural movement at the depth of two thousand feet sufficient to produce as much heat in a year as the complete combustion of 28,601 tons of carbon, this being a low estimate of the quantity of heat withdrawn in the mine waters and air.

In this connection there is great significance in the fact that the first thousand feet of depth does not exhibit unusual increase of temperature, though this is especially the zone of oxidation, solution, and consequently of movement resulting from changes of volume.

I am indebted to Mr. Charles Forman, of Virginia City, Nevada, for valuable facts upon this subject. He is superintending the sinking of a shaft which is designed to reach the lode at a depth of about four thousand five hundred feet, and, with enlightened regard for exact knowledge on this interesting subject, he procured for use in the shaft a Negretti and Zambra slow-action thermometer, of the pattern adopted by the Underground Temperature Committee of the British Association, and standardised at Kew. The shaft is now one thousand feet deep, and I have received from him the following data obtained by the use of this instrument:—

"Temperature of the ground in Forman shaft, from the surface to the depth of one thousand feet, as ascertained by drilling holes three feet deep into the rock and inserting a Negretti and Zambra mining thermometer in the hole; closing the hole with clay, and leaving the thermometer for twelve hours; not less than three holes being tried at each point:—

Depth.	Temp.	Increment.	Decrement.
100 feet	50½° F.		
200 ,, ...	55	4½°	
300 ,, ...	62	7	
400 ,, ...	60		2°
500 ,, ...	68	8	
600 ,, ...	71½	3½	
700 ,, ...	74¾	3¼	
800 ,, ...	76½	1¾	
900 ,, ...	78	1½	
1000 ,, ...	81½	3½	

The increase of temperature is 31½° in nine hundred feet, or precisely 3½° in one hundred feet. This determination has especial value, because the shaft is nearly a mile and a half east of the lode, and therefore not controlled by its physical conditions. The shaft has been quite wet, and its temperature may have been affected by the presence of water. This series of observations promises to be extremely valuable.

I have discussed this subject at length because of the interest which has been shown in it and because of its intrinsic importance. The Comstock Lode is unique as *the* hot lode of the world. I have followed up several reports of hot veins elswhere and have learned that whilst "hot springs" are known in other mines, there is no other metalliferous deposits of which I can hear that presents the peculiarity of this locality and which may be stated as *hot rock*, comprising the whole country of the lode. The Mexican mines are hot on account of bad ventilation, but I have ascertained that the rock is not hot. The quicksilver and sulphur deposits of California have shown great heat in a superficial stratum, but I am informed that the heat dissappeared at the depth of a few feet. Another mine near the Comstock was reported to be hot, but this turned out to be heated by a steam-pipe which the geological observer had not noticed. At Steamboat Springs, seven miles north-west of the Comstock, there is a mass of quartz which has been mined for a small proportion of mercury which it contains. This is really another hot locality, and its heat is the more remarkable because the mass is almost pure quartz. Half a mile away are the springs which steadily pour out a current of boiling water, from which silica, with occasional traces of sulphur, cinnabar, and even gold, are said to be deposited. Probably the heat of the quartz body is intimately connected with the source from which the springs derive their heat, and the whole locality is in the Comstock neighbourhood.

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* A Paper read before the American Institute of Mining Engineers at the New York Meeting, February, 1880.

† "The Comstock Lode, its Formation and History," published by Wiley and Sons, New York, 1879.

Mr. R. Pearce, of Argo, Colorado, has kindly called my attention to the analysis of a hot spring in a Cornish mine, which he investigated several years ago. The analysis was made by the late W. A. Miller, M.D., Treasurer of the Royal Society and Professor of Chemistry in King's College, London. It was published in the Chemical News, vol. x., p. 181, and is given below. It presents two remarkable peculiarities, showing in one gallon no less than 216·17 grains of calcic chloride, and 26·05 grains of lithic chloride. The water is described as forming "a spring" in the mine.

Analysis of Cornish Mine Water.

From a hot spring in Wheal Clifford Mine, Cornwall: Depth, 1320 feet below the mean level of the sea. Flow, 150 gallons per minute. Temperature of water 125° F. Sample collected by Mr. R. Pearce. Analysis by Prof. W. A. Miller, F.R.S.

Specific gravity at 60° F.		1·007	
Gases—			
1 imp. gallon, at 30 in. bar. and 60° F., contains		8·91	cub. ins.
Consisting of—			
Carbonic acid		1·89	,,
Ratio of O to N, 1 : 3	Oxygen	1·72	,,
	Nitrogen	5·30	,,
Solids—			
1 imp. gallon contains fixed salts, by evaporation		646·10	grains.
Consisting of—			
Chloride of lithium		26·05	,,
Chloride of potassium, with a little chloride of cæsium		14·84	,,
Chloride of sodium		363·61	,,
Chloride of magnesium		8·86	,,
Chloride of calcium		216·17	,,
Sulphate of calcium		12·27	,,
Silica		3·65	,,
Oxides of iron, manganese, and alumina		traces	
		645·45	

ON THE ANALYSIS OF SOAPS.

By JUL. LOEWE.

For the determination of moisture the author takes from 8 to 10 grms. shaved very finely, and dries them, first, at 60° to 70°; afterwards, at 100° to 105°. If it is supposed that the soap contains caustic alkali, the process must be conducted in an atmosphere free from carbonic acid. The free alkali may also be determined before the process of drying by exposing a portion of soap, shaved very fine and weighed, upon a watch-glass in a cylinder filled with dry carbonic acid and closed, the proportion of caustic alkali being calculated from the increase of weight. The exposure must not last too long, in order to prevent the formation of bicarbonate.

To determine free unsaponified fat, the portion of dried soap is thrown into a rather high beaker capable of being well covered, and extracted with benzol or petroleum with the aid of heat, according to the method of Perutz, and decanting when clear into a small tared flask. If the decantation is difficult it is passed through a weighed filter, which is afterwards used for the alcoholic solution of the soap. After two or three extractions the filtrates are collected and distilled, the residue dried at 108° in a chloride of sodium bath, and the increase of weight of the flask is noted, which shows the proportion of the non-saponified fat.

The residue freed from such fat is covered with about 8 to 10 parts of alcohol at 90° per cent, and heated to 40° to 50° in the water-bath. Caustic alkali, and that in combination with fatty acids, along with glycerin, are readily dissolved, whilst soda (carbonate), farina, and mineral impurities remain undissolved, and after washing with hot alcohol and drying at 100° may be weighed. In the better class of soaps the residue does not exceed 1 to 1½ per cent.

A moderate stream of well washed carbonic acid is then allowed to play upon the surface of the warm alcoholic filtrate. Caustic alkali, if present, is deposited as alkaline carbonate. The beaker is covered, allowed to stand till clear, heated in the water-bath, the contents filtered, and the filter is washed with warm alcohol. In the aqueous solution of the residue the carbonate of soda may be determined volumetrically.

The second alcoholic filtrate, thus freed from soda, is mixed with sulphuric acid diluted with alcohol as long as a turbidity is produced. When clear the sulphate of soda deposited is filtered off, collected upon a weighed filter, washed with alcohol, dried at 110°, and weighed. The weight shows the alkali which was in combination with the fatty acids.

The filtrate is acidulated with sulphuric acid, mixed with water, and freed from alcohol by evaporation in a platinum capsule. When cold, the acid aqueous extract which may contain glycerin, is separated from the congealed fatty acids by filtration. These acids, as well as the glycerin, may be determined by known processes, the latter after the accompanying sulphuric acid has been saturated with barium carbonate.

The residue, insoluble in alcohol after being weighed, is washed with cold water till the filtrate makes up exactly 60 c.c. The water is then driven out of the filter by means of alcohol, and the residue is dried at 100°.

After being weighed the residue is submitted to microscopic examination in order to detect starch. Mineral impurities are sought for by ordinary analytical methods.—*Zeitschrift für Analytische Chemie.*

REVIEW OF BERTHELOT'S "MÉCANIQUE CHIMIQUE."[*] [†]

By CH. DE MARIGNAC.[‡]

On witnessing the calorific effects which accompany the act of chemical combination, chemists have at all times sought to determine the relations which exist between these two classes of facts; on the one side, affinity, considered as the cause of all chemical combination; and, on the other side, the heat produced when this combination takes place.

A century ago, Lavoisier explained this relation by assuming that simple bodies are only known to us in a state of union with a certain quantity of caloric, which is set at liberty when these bodies enter into chemical combinations. Having his attention more exclusively attracted to the phenomena of combustion, he attributed this too exclusively to oxygen. Later, it was admitted for all the elements.

The brilliant discoveries of Davy, on the chemical decompositions effected by the galvanic current, gave rise to the electro-chemical theory, proposed by this scientist, and afterwards adopted and modified by Ampère and Berzelius, which caused heat and affinity to be looked upon in an entirely new light. According to this theory, affinity is merely the reciprocal attraction of the opposite electricities, which exist in either a free or a polarised state in the elements which tend to combine, and the heat

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[*] Communicated to the Chemical News by the translator, P. Casamajor.

[†] "Essai de Mécanique Chimique, Fondée sur la Thermo-chemie," par M. Berthelot; Dunod, Editeur, Paris, 1879.

[‡] Published in the *Archives des Sciences Physiques et Naturelles*, and re-published in the *Moniteur Scientifique*, of February, 1880.

observed in chemical combinations results from the mutual neutralisation of these electricities.

When chemists abandoned the electro-chemical theory, they returned to the system previously adopted, admitting, at the same time, the pre-existence of caloric in all the elements. But, about this time, an important revolution took place in the physical sciences; heat and electricity ceased to be considered as material fluids, combined more or less intimately with ponderable bodies, and came to be regarded as modes of motion. The discoveries of Mayer, of Golding, of Joule, had demonstrated the mechanical equivalent of heat. This new doctrine necessarily modified the ideas entertained by chemists concerning the origin of the heat and the electricity evolved in chemical reactions, and led them to consider these manifestations as complementary to one another, and as resulting from the loss of molecular impetus attending the act of combination.

Vast erudition, and long bibliographical researches, would be necessary, to follow this change of doctrine, and to apportion to each scientist the share which he had in it. We may, however, cite the memoir of M. Henri Sainte-Claire Deville (1860), and those of Babinet (1866).

A Danish chemist, Mr. Thomsen, whose labours have greatly contributed to the progress of thermo-chemistry, published in 1853 a series of memoirs,* in which he explained the principles of this new science. Without deriving them explicitly from the mechanical theory of heat, he expresses the basis on which they rest in terms which recall this theory. He founds them on the two following principles:—

1. The intensity of chemical energy in a given body is constant when the temperature is invariable.

2. The totality of the heat given out in a chemical reaction is the measure of the chemical energy brought to bear in this reaction.

The chemical energy of a body, or, what is identical, its capacity of developing heat, is what Mr. Thomsen calls its *thermo-dynamic equivalent.*

Mr. Thomsen has since tried to show that all the laws of thermo-chemistry subsequently established by M. Berthelot were derived from the second of these principles. It is true that they may be considered as consequences of it, but only by including conditions and restrictions which are not found in the statement of this principle. The best proof of this is that fifteen years later,† when Mr. Thomsen attempted to establish an agreement between his principles and the manner in which bases and acids are divided in a mixture of salts, he had to introduce a new force, *avidity*, which is an inherent property of these bodies, and in which it is difficult to find anything different from chemical affinity under another form, independent of that which is measured by the heat given out in combinations.

It is in reality to M. Berthelot that the honour belongs, of having stated with perfect precision the fundamental principles of thermo-chemistry, and, above all, of having shown, by a prolonged and numerous series of experiments, that these principles are sufficient to account for all chemical reactions.

Gifted with an eminently philosophical spirit; deeply versed in mathematics and in the physical sciences; trained to the most extended researches by his labours on chemical synthesis, this scientist was better able than any one else to accomplish the task to which he has devoted himself, which is to show the application of the principles of mechanics to the formation of organic compounds, and even to chemical reactions generally.

From this, it was not sufficient merely to determine the heat given out in chemical reactions. A not less important portion of the labours of M. Berthelot has been the study of the conditions under which such reactions take place, and more especially the study of the chemical equilibrium which results from two contrary reactions, which are reciprocally limited. This happens in the formation of ethers, in decompositions and inverse re-combinations produced by electricity, &c.

Through the study of these subjects, the author was led to new and important results in a branch of chemical mechanics, already successfully cultivated at the beginning of this century by Berthollet, under the name of *chemical statics*. He has shown that the theory of Berthollet, by which we are enabled to foretell chemical phenomena from the mere knowledge of the physical conditions of insolubility and volatility, has no foundation, except when there already exists a previous equilibrium, which constantly tends to renew itself. But the permanency of this equilibrium, and even its existence, are dependent on a still more general condition, which had never before been pointed out, and which has been defined in the principle of *maximum work* laid down by M. Berthelot.

These researches have taken sixteen years—since 1864. The results have been given in numerous memoirs, published in the *Annales de Chimie et de Physique*. M. Berthelot acknowledges the great help he has had from the labours of his predecessors, Hess, Graham, Andrews, Favre, and Silbermann, and in numerous series of numerical determinations of the quantities of heat given out in combinations, made by Mr. Thomsen, which are generally remarkable for their accuracy.

This subject is not exhausted, and will not be for many years, but M. Berthelot thinks that the time has come for him to state the laws and general principles, the search of which has led him to this long series of experiments, and to show in what manner the ideas recently acquired on the theory of heat, allow us to bring the whole of chemistry, that is, the formation and reactions of organic as well as of mineral substances, under the same principles of mechanics which govern other branches of physics; whence the title of "Essay on Chemical Mechanics founded on Thermo-chemistry," given by the author to the work which we are reviewing.

In his introduction, M. Berthelot states the three fundamental principles of thermo-chemistry and chemical mechanics, viz.:—

1. *Principle of Molecular Action.*—The quantity of heat given out in any reaction is the measure of the sum of the physical and chemical work accomplished in this reaction.

This principle gives the measure of chemical affinities.

2. *Principle of the Calorific Equivalence of Chemical Transformations, or, in other words, Principle of the Initial and Final State.*—If a system of simple or compound bodies, taken in determinate conditions, suffers physical or chemical changes capable of bringing it to a new state, without giving rise to a mechanical effect exterior to the system, the quantity of heat given out or absorbed by the effect of these changes, depends solely on the initial and final states of the system; it is always the same, independently of the nature and sequence of intermediate states.

3. *Principle of Maximum Work.*—Every chemical change, accomplished without the intervention of a foreign energy, tends toward the production of the body, or system of bodies, which gives out the greatest quantity of heat.

The prevision of chemical phenomena is, by this last principle, brought down to the purely physical and mechanical idea of the maximum labour accomplished by molecular action.

The following principle, deduced from the preceding, is applicable to a multitude of phenomena:—

> Every chemical reaction, which may be accomplished without the help of a preliminary action, and apart from the intervention of any energy foreign to the bodies present in the system, will necessarily take place if it be attended with evolution of heat.

The first two principles above given had not heretofore been stated in such precise terms, but it may be said that they had been implicitly admitted by the previous investigators in thermo-chemistry. It was, however, M. Ber-

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* *Pogg. Ann.*, lxxxviii., 349; xc., 261.

† *Pogg. Ann.*, cxxxvi., 65; also *Archives*, 1869, xxxvi., 301.

thelot, who first called attention to the principle of maximum work, which is the basis of chemical statics.

In the statements made above, we may note the particular care taken by the author in distinguishing, in the calculations of reactions, the heat given out by chemical reactions, properly so-called, from that due to change of state, and which is derived more specially from physical energies, such as the liquefaction of gases, the solidification of liquids, changes of volume and of specific heats of gases, of liquids, and solids; changes of tension in vapours, and of fluidity in liquids, of crystallisation and of crystalline form in solids; also various modifications of the amorphous state.

It is, indeed, only on the condition of keeping account of all these circumstances, and of eliminating the share of all these physical causes, that we are able to ascertain that the heat given out in a reaction, is the exact measure of the affinities brought into play.

The first volume of M. Berthelot is specially dedicated to *chemical calorimetry*, which is the study of the quantities of heat given out in chemical reactions. It forms the exposition and confirmation of the first two principles stated above.

These principles cannot be demonstrated *à priori*. They are founded on a hypothesis, in itself very probable, that the imperceptible molecular motions developed in chemical reactions, obey the same laws as the motions of mechanical motors, which may be seen and measured. The accuracy of this hypothesis can only be established by long series of experiments, and by the constant conformity of the results obtained with those foreseen by theory.

The first volume is divided into three books. The first book develops the rules and general methods of chemical calorimetry. The author reviews the various reactions, combinations of elements, formation of salts, &c. He gives the calculations for each case, also gives rules for taking into account all the physical actions which intervene in these reactions, and shows by numerous examples the agreement of experience with the fundamental principles established by him.

One chapter is devoted to the quantities of heat which come into play in the reactions which take place in living beings. Another chapter treats of the variation of the heat of combination as influenced by temperature.

The second book contains descriptions of experimental processes and of calorimetric apparatus, for the determination of the quantities of heat given out in chemical reactions, of specific heats, and of the heat due to changes of state.

The great experience of the author gives a special interest to this description of the methods and apparatus used by him, in which he has succeeded in uniting simplicity with accuracy, and in which he has tried to eliminate or, at least, to reduce as much as possible, the corrections made necessary by the influence of exterior causes.

The third book is devoted to numerical tables. It includes numbers obtained by experiment, representing the quantities of heat given out or absorbed during physical or chemical changes of state which these bodies undergo in the operations of our laboratories. It therefore contains, besides results of thermo-chemical experiments, all the data relating to specific heats, and to changes of state (latent heats). It forms an extensive and exceedingly useful compendium of the labours of all the scientists who have studied these questions. We also find in this portion very interesting discussions on numerous theoretical questions suggested by the examination of these results. We may cite more specially those which relate to specific heat.

M. Berthelot states that the specific heat is the only physical property by which simple gases differ from compound gases. This property, however, is sufficient to establish between these two groups such an important difference, that it is impossible to suppose that any of the gases now considered as simple, results from the union of several other elements, such as we admit them at the present time, or from the condensation of several equivalents of the same element. This union or condensation must be understood to be comparable to that which gives rise to compound bodies, such as are now known to us. Simple gases differ from all compound gases formed with condensation, by the constancy of their specific heat, while for these last the specific heat increases rapidly with the temperature, and always exceeds in a notable manner, at all temperatures, that of simple gases. They also differ from compound gases, formed without condensation of their elements, such as hydrochloric acid and carbonic oxide (corresponding to four volumes, according to the system of notation of the author), whose moleular specific heat is equal to that of simple gases, and also remains constant at all temperatures, by the fact that the molecular volume always corresponds to one equivalent for these compound gases, while it represents two or four equivalents for all the known elements.*

In the chapter relating to specific heats of solid bodies, occur very interesting observations on the law of Dulong and Petit,† and on the relations which exist between the specific heats of compound bodies and those of their elements.

The second volume of the work of M. Berthelot, is specially dedicated to establishing the bases of chemical mechanics.

The object at which the author aims is the highest that science can propose to itself. Instead of confining itself to the individual description of the properties, the preparation and transformations of chemical species, science must endeavour to discover the laws governing these transformations; the causes and proximate conditions which determine them. The complete solution of this problem of chemical mechanics is not yet possible, and will not be possible for many years. To solve it, we should know, in every reaction, the masses of the particles brought into play, their relative positions, the proper motions of each particle, the exact nature, and the laws which govern the forces which react on each other. Most of these data are wanting, and even if they were known, it is doubtful if any method of calculation would allow us to solve such a complicated problem.

However, some portions of the problem may be attempted. For a long time, all that relates to the proportions in which combinations take place is known by the laws of definite proportions, of multiple proportions, and of equivalents. As to the prevision of reactions, we have only had the laws laid down by Berthollet at the beginning of this century. These only apply to special cases, and meet with frequent exceptions. M. Berthelot replaces them by a more general law, by which the prevision of the reciprocal actions between simple and compound bodies is reduced to the determination of the thermic properties of the reacting bodies. We may, by this new law, foresee the reciprocal actions of compounds towards one another and towards simple bodies, when we know the quantities of heat given out in the formation of

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* This difference is not so much in the specific heats themselves as in the relations of molecular volumes to equivalents. In this respect the reasoning of M. Berthelot does not seem to me unobjectionable. It would be so if the determinations of equivalents rested on fixed rules. But, as he himself acknowledges (Introduction, page 21), "equivalents are only determined within an approximation of a multiple," it would only be necessary to change the multiple which has been chosen more or less arbitrarily, for a given body, to change the relation between its molecular weight and its equivalent, which the author regards as characteristic of simple and compound gases. As an instance, it is not clear why M. Berthelot considers the molecule of carbonic oxide, C_2O_2 (four volumes), as representing one equivalent. In the combinations of this gas with oxygen, chlorine, and sulphur, it would appear that this quantity rather corresponds to two equivalents.

† Although we acknowledge, with M. Berthelot, that the law of Dulong and Petit, when applied to solids, is not rigorously exact, and that it cannot serve, in an absolute manner, for the determination of atomic weights, still we believe that he exaggerates the exceptions which it suffers, and that it must be taken into account in the case of elements whose physical properties are very similar, as in the case of metals whose fusion-points are not very far apart.

each compound. This new law is no other than the third principle of the author, which has already been stated, the law of maximum work.

Whence the division of this volume in two parts:

The first treats of what may be called chemical dynamics. It is the study of combination and decomposition, and contains definitions of the opposing actions, on one side, of chemical forces, and on the other side, of calorific, luminous, and electric energies, which determine the phenomena. In this way, the conditions are laid down which determine the existence and the stability of combinations for each separate body.

The second part is the study of the final state which results from reciprocal actions between simple and compound bodies; this is chemical statics. In this part comes into action the principle of maximum work, which is as remarkable for its simplicity as for its generality. It brings everything down to the knowledge of two things: the heat given out by the transformations, and the stability which belongs to each compound.

The author demonstrates the existence of this principle, by the discussion of the general phenomena of chemistry; afterwards, he develops the application of it to the reciprocal actions of the principal groups of substances, such, for instance, as take place between elements and binary compounds; the reciprocal displacement of binary compounds, and specially of hydracids, when opposed to one another or to water; the reciprocal displacements of acids in salts, and, finally, saline double decompositions.

It may be easily understood that it is impossible to give, in a review of this kind, a clear idea of a work of such importance and of such vast extent. We have only tried to give an idea of the subjects treated in it. The theories of M. Berthelot are well known, as they have been developed by him for the last sixteen years in a series of papers, which have been read by all chemists. Nevertheless, the present work forms an important epoch in the history of chemistry, and shows the necessity of transforming its methods. In the future, it will not be sufficient to state the reactions of which a body is susceptible, as if they were mere experimental data. It will be necessary to derive them from the general principles, of which these reactions are consequences. Doubtless, the edifice of chemical mechanics, whose foundations have been laid by M. Berthelot, cannot be considered complete. Sixty years ago, the laws which govern chemical proportions were established, and nevertheless, the numbers which represent equivalents have frequently to be rectified. We may expect that those which express the quantities of heat of combination, or transformation, will also require change, more especially as many of them cannot be determined directly by experiment, but result from complicated calculations in which a great number of reactions have to be taken into account.

There are, moreover, many theoretical questions of the highest importance, the solution of which is at present beyond our reach.

If the heat given out in the combination of two bodies is the immediate consequence of the loss of momentum which comes from the union of their particles, and if it measures at the same time their mutual affinity, what then is the first cause of affinity? Is it a special force as was once believed? Or is it the concordance in the vibratory motions of the particles?

Those combinations which are attended with evolution of heat are the only ones which can take place directly; but they do not always take place spontaneously. Many of them must be determined by an exterior cause, such as heat, light, electricity, but these agents do not give to the elements an energy of which they have already an excess. How then do they act?

These questions, and many others which will come up, cannot fail to exercise for a long time to come the sagacity of chemists. At any rate, an important step has been made towards their solution when it has been shown that the physical and chemical energies which come into play in chemical reactions, follow the laws of rational mechanics.—*Journal of the American Chemical Society.*

PROCEEDINGS OF SOCIETIES.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Physical and Mathematical Section.

April 13, 1880.

E. W. Binney, F.R.S., F.G.S., President of the Section, in the Chair.

"*Colorimetry, Part IV.*," by James Bottomley, D.Sc.

On the Colour Relations of Nickel and Cobalt.

For some experiments which I was making in colorimetry I wished to obtain a solution which would absorb all the kinds of light in the same ratio, so that whatever sort of light we started with, after penetration through such a solution, it would remain the same in character, the only variation being a change in intensity. Hence through such solutions white surfaces would appear grey of various shades, verging towards blackness as the length of the column increased. Such a fluid we might call a soluble black. I am not aware of any single fluid that fulfils the above conditions. It might be said, Why not use ink? but such specimens of ink as I have examined are bluish or violet on copious dilution. Moreover, the colour alters with the degree of oxidation; also, it seems to be colouring matter in suspension rather than in solution. I had some hopes of succeeding by mixing solutions of nickel and cobalt salts. On reference to the *Phil. Mag.*, vol. vi., p. 15, I find that the colour relations of nickel and cobalt had been studied by Mr. Thomas Bayley with a view to the quantitative determination of these metals founded upon the complementary character of their colours. He states, "The fact will have been observed by chemists that solutions of nickel and cobalt salts are so far complementary in colour that when they are mixed together the resulting liquid, if moderately dilute, is hardly to be distinguished from pure water." After considering the nature of the absorption spectra of nickel and cobalt salts, he states—"If the spectra were exactly complementary, on superimposing the nickel spectrum upon the cobalt spectrum the dark part on the one would cover exactly the light part on the other. This, however, though nearly the case, is not exactly so . . . this is why the solution obtained by mixing strong solutions of nickel and cobalt is not grey, but reddish brown in colour." Some experiments which I made seemed to confirm the opinion of Mr. Bayley; the nickel solutions contained 0·05 grm. of $NiSO_4$ per c.c., and the cobalt solution contained 0·05 grm. $CoSO_4$ per c.c. A mixture consisting of 50 c.c. of cobalt solution with 100 c.c. of nickel solution contained in a white porcelain basin seemed to be a grey tinted with pink in the shallower parts, and having a tendency to pass into a yellowish tint as the depth increased. I now poured the fluid into a tall glass cylinder covered externally with black cloth except a circular aperture of 8 m.m. diameter at the bottom. When I looked through the column of fluid at a white surface the colour was decided, resembling somewhat the pigment known as yellow ochre. Also, with a less proportion of cobalt to nickel—namely, 20 c.c. of cobalt solution to 50 c.c. of nickel solution—I still obtained a tint in which yellow seemed to predominate. Had I employed solutions so dilute that no colour was perceptible in the mixture, this would not strictly imply that the colours were complementary, but that the resulting tint was too feeble to produce the impression of colour, and if we filled a long tube with such a dilute solution the colour might again become manifest. Moreover, my aim was not to mix two coloured solutions so as to obtain a fluid which exercised no perceptible absorption of light, but to obtain a fluid which would exercise a considerable absorption subject to a certain condition. The following consideration seemed to me to render it hopeless to obtain a soluble black by nickel and cobalt only. A solution of cobalt when dilute is pink, but if we look through a considerable thickness or through a concentrated solution, the pink shows a tendency to pass into scarlet. This shows that as the quantity of the salt increases the ratio of the yellow to the red increases. The colour of the undissolved salt is brownish red, and the colour of the solution seems to approximate towards this as the concentration increases. Hence the colour of a solution of cobalt alters not only in intensity but also in kind as the amount of the salt is increased. On the other hand, the green of a solution of nickel varies in intensity, but does not seem to vary in character, at least in any marked manner, as the quantity of the salt increases. In order that it should be generally complementary in character to cobalt, any inconstancy in the ratio of the red to the yellow of the latter would require a corresponding variation in the ratio of the yellow to the blue in the former, and the tint ought to pass from an emerald green to a bluish green. As this does not seem to be the case, it would follow that if we mixed nickel and cobalt so as to obtain a perfect grey for a column of definite length, a column longer or shorter than this would still retain some colour. My experiments seemed to indicate a deficiency of blue in the mixture, and this I thought might be supplemented by another salt; so I tried the addition of sulphate of copper. After some trials I got a solution containing in 1000 c.c. 7·275 grms. $NiSO_4$, 4868 grms. $CoSO_4$, and 11,468 $CuSO_4$; the solutions also contained 30 c.c. of strong sulphuric acid; this I added to guard against any possible formation of sub-salts on copious dilution. This solution seemed nearer to what I wanted than a solution of nickel and cobalt only. It did not, however, appear wholly free from colour, and possibly a variation of the quantities might have given a better result; also the tint seemed to vary somewhat with the nature and intensity of the incident light. When in the failing light of approaching evening I held the containing bottle against the grey sky I thought that there remained a somewhat pinkish tint, whilst in the colorimeter, when looking at an external white surface through a column sufficiently long to produce a perceptible absorption, I thought the solution had a bluish tint. When viewed against gaslight it gave a greenish tint. Within the range of coloured fluids in chemistry there may be some which if combined would yield a mixture absorbing all colours in the same ratio, so as to be truly a soluble black. The preparation of such a fluid would be an interesting problem in physics. It seems to me that we might also have such fluids which, on spectral analysis, would show not an absorption of all colours in the same ratio, but would be resolved into a violet and yellow, or an orange and blue, or red and green, or some other combination of colours of a complementary character.

Remarks on the Formulæ for the Intensity of Light that has passed through Absorbing Media, and on a Method of Experimental Verification.

In my last paper on colorimetry I pointed out that the function which expresses the connection of the intensity of light with the quantity of colouring matter is of the same form as the function expressing the relationship of the intensity and the length of the absorbing column, and if we accept Herschel's formula Σak^t for the latter relationship, then an expression of the form $\Sigma a\kappa^Q$ must be taken to express the former relationship. The connection of these two may be shown more directly than I indicated in my last paper. If we grant one of the laws, the other may be deduced from it as a corollary. Take, for instance, the law as given by Herschel,—

$$T=a_1k_1{}^t+a_2k_2{}^t+,\ \&c.$$

Now it is manifest that if q be the quantity of colouring matter per unit of length, we may write the above formula

$$T=a_1k_1{}^{q\frac{t}{q}}+a_2k_2{}^{q\frac{t}{q}}+,\ \&c.$$

For—

$$k_1{}^{\frac{1}{q}},\quad k_2{}^{\frac{1}{q}},\quad k^{\frac{1}{q}},\ \&c.,$$

substitute new constants, κ_1, κ_2, κ_3, &c. Then we may write—

$$T=\Sigma a\kappa^{qt}.$$

Since q denotes the quantity of colouring matter per unit of length, and t the total length, we shall have $Q=qt$, when Q denotes the whole quantity of colouring matter, so that we finally deduce—

$$T=\Sigma a\kappa^Q.$$

As the basis of a method of colorimetry, I took the relationship, that the length of the column was inversely as the quantity of colouring matter present when the colour was made constant. It may be readily shown to be a consequence of the laws stated above. Suppose C to be the constant colour, then—

$$C=\Sigma ak^{qt}.$$

The form of the equation shows that C is the sum of a number of constants, C_1, C_2, C_3, &c., such that—

$$C_1=a_1k_1{}^{qt}$$
$$C_2=a_2k_2{}^{qt}$$
$$\dots=\dots$$
$$C_n=a_nk_n{}^{qt}$$

whence we obtain—

$$\log\frac{C_1}{a_1}=qt\log k_1$$
$$\log\frac{C_2}{a_2}=qt\log k_2$$
$$\dots\dots=\dots\dots$$
$$\log\frac{C_n}{a_n}=qt\log k_n$$

and by addition we obtain—

$$\log\frac{C_1}{a_1}+\log\frac{C_2}{a_2}+\ \&c.=qt\,(\log k_1+\log k_2+\&c.)$$

Or—

$$qt=\frac{\log\left(\frac{C_1C_2}{a_1a_2}\dots\dots\frac{C_n}{a_n}\right)}{\log(k_1k_2\dots\dots k_n)}=\text{constant.}$$

In my last paper I stated that the law of absorption of light given by Herschel appears to have been obtained *à priori*; I have not found in his memoirs any experimental confirmation of it. The form of the expression has a somewhat formidable appearance, inasmuch as it involves the measurements of infinite varieties of light. But suppose that in the formula Σak^t, k is the same for every species of light; then we may write $T=k^t\Sigma a$ or $T=k^tI$, if I denote the incident light. In such a case the emergent light will be of the same nature as the incident light, and will differ only in intensity. Suppose the incident light to be white, the emergent light will be a white of less intensity—that is, will be a grey, approaching to blackness as the length of the column increases. A fluid medium, affecting white light in this way, we might call a soluble black, and my aim in seeking to obtain such a fluid was to apply it to the confirmation of the law. In a previous note I state that I had tried to obtain such a body; what I got was not wholly satisfactory, but I thought that with it I might obtain some approximate results. The solution I used consisted of 500 c.c. of the previously-mentioned fluid with 500 c.c. of distilled water.

The mode in which I proposed to operate was as follows:—Take two white lights of different intensities, say

W_1 and W_2, and look at them through the liquid. Suppose the lengths of the columns when equality of intensity is obtained to be t_1 and t_2, then—

$$W_1k^{t_1} = W_2k^{t_2}$$

Suppose we alter the lengths of the columns, and in a second experiment we find—

$$W_1k^{t_1} = W_2k^{t_2}$$

by cross multiplication and elimination of the common factor W_1W_2 we obtain—

$$k^{t_1}\,k^{t_2'} = k^{t_2}\,k^{t_1'}$$

or, as we may write it,—

$$k^{t_1+t_2'} = k^{t_2+t_1'}$$

Then taking the logarithms of both sides and dividing by log k we get finally—

$$t_1+t_2' = t_2+t_1'.$$

In this way I proposed to test the law.

I took, as standard tints, a smooth surface of $BaSO_4$, and another of $BaSO_4$ and carbon, in the proportion of 10 grms. of $BaSO_4$ to 0·006 grm. of carbon: these materials were intimately mixed, and the powder reduced by pressure to a flat surface. The colorimeters used were glass cylinders covered externally with black cloth, the circular apertures at the bottom admitting light being 8 m.m. in diameter.

One experiment gave the following results:—Length of column 22·2 c., standard tint $W_1(BaSO_4)$, I now attempted to get the same intensity when looking through the second cylinder at tint W_2 ($BaSO_4$+carbon). The mean of two trials gave 14·8 c. as the equivalent column. Hence we have the following results:—

$$W_1k^{22\cdot2} = W_2k^{14\cdot8}.$$

I now made the length of the column over W_1 13 c. The equivalent column over W_2 was as the mean of two trials 7·5. Hence—

$$W_1k^{13} = W_2k^{7\cdot5}.$$

From these experiments we get as the sum of t_1 and t_2' 29·7, and the sum of t_1' and t_2 27·8.

A second experiment gave—

$$W_1k^{13} = W_2k^{6\cdot55}.$$
$$W_1k^{22\cdot2} = W_2k^{15\cdot45}.$$

Here $t_1+t_2' = 28\cdot45$ and $t_2+t_1' = 28\cdot76$.

A third experiment gave—

$$W_1k^{16\cdot8} = W_2k^{10\cdot65}.$$
$$W_1k^{14\cdot3} = W_2k^{8\cdot5}.$$

Here $t_1+t_2' = 25\cdot3$ and $t_1'+t_2 = 24\cdot93$.

A fourth experiment gave the following results—

$$W_1k^{22\cdot2} = W_2k^{14\cdot9}.$$
$$W_1k^{12\cdot5} = W_2k^{6\cdot2}.$$

Here $t_1+t_2' = 28\cdot4$ and $t_1+t_2' = 27\cdot4$.

A fifth experiment gave—

$$W_1k^{25\cdot1} = W_2k^{16\cdot65}.$$
$$W_1k^{13\cdot7} = W_2k^{6\cdot85}.$$

Here we have $t_1+t_2' = 31\cdot95$ and $t_1'+t_2 = 30\cdot35$.

It seems to me that the above results are as favourable as might be expected considering the difficulties of the enquiry; and even if all external circumstances necessary for the successful completion of such experiments had existed, there would yet remain the difficulty of deciding about the equality of two grey tints. In such matters it is difficult to say where judgment ends and fancy begins.

That any discrepancies might be due to such a cause was shown by the following experiments:—I took the two cylinders, and poured from one into the other with the intention of obtaining the same tint in both. In one trial I could not very clearly distinguish a column 16·4 c. long from one 14·6 c. long, and in a second trial a column 16·2 c. long seemed to give the same tint as a column 14·6 c. long. In the above experiments I used one eye only, namely, the right one.

I also made the following experiments:—I took as the standard of intensity W seen through a column 12·5 c. long. On a former occasion I had made 6·2 as the equivalent column to be used with W_2. On the present occasion I thought 6·5 gave a nearer result, so I took the column at this length. Now if the law of absorption of light be true, if we increase both columns by the same quantity, the intensities should again correspond. So I added 4 c. to each, making one column 16·5 and the other 10·2. I thought that the tints were the same. I now made one column 20·5 and the other 14·2. Again I thought the tints were the same. Finally, I made one column 24·5 and the other 18·2. The tints seemed again to correspond.

THE AMERICAN CHEMICAL SOCIETY.*

The June Conversazione of the Society was held with Dr. C. T. Chandler at the School of Mines, Columbia College, New York, on Thursday evening, June 20. A large attendance answered Dr. Chandler's invitation, and a pleasant time was spent examining a number of new additions to the Chemical Museum of the School of Mines.

Among the most interesting of the objects exhibited was a collection of the celebrated Arita porcelain from Japan. This material is true porcelain, made by the admixture of two natural clays found in Japan, without any preparation or other material. It is susceptible of being worked into the most delicate and artistic forms, and is decorated with all the beautiful and curious skill so characteristic of the Japanese.

Dr. **Chandler** explained the elaborate fittings recently put into the Chemical Lecture Room of the School of Mines for experiments with the electric light. These are so arranged that the room is lighted with two electric lamps, suspended from the ceiling and enclosed in opal glass globes; while by means of an ingeniously devised switch, at the side of the Lecture Table, these lamps can be thrown out of the electric circuit, and at the same instant the current is diverted to the experimental lantern to project objects on the screen. The advantage of this arrangement is that the sudden change from the electric light of the experimental lantern to gas-light, as ordinarily arranged for lecture rooms, is obviated, and there is no sudden strain upon the eyes. The electricity is obtained from a Wallace dynamo-electric machine, located in the Engine Room of the School of Mines, which machine is driven at a speed of about eight hundred revolutions per minute. Eight wires run from the different parts of the machine to the switch-board in the Chemical Lecture Room, thus giving the means of throwing out or using any desired combination of parts. With the lantern Dr. Chandler made a number of experiments in spectrum analysis, showing the lines of silver, copper, zinc, and the reversing of the sodium line. He also showed the effect of the change of form in the slit of the lantern, by using Dr. Henry Morton's admirable contrivance to form round disks, circles, and zig-zag lines of the spectrum on the screen.

Some beautiful examples of the Autotype Process of Photo-printing were exhibited, and much admired for their near approach to ordinary fine photographs, experts only being able to detect that they are printed.

A very fine and large photograph of Lauynmantel's picture of the arrest of Lavoisier by the Officers of the French Revolution was much admired.

Among a number of other minor objects exhibited were some old hand-blocks used in calico-printing. Dr. Chandler said he had visited a print-works, where he found a cord of such blocks stored away among some rubbish, and he secured some of them as relics of a process which is rapidly becoming obsolete, being superseded by the more modern process of machine-printing.

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* Communicated by Arthur H. Elliott, F.C.S., Recording Secretary.

The next regular meeting of the Society will be held on Thursday, September 2, 1880.

NOTICES OF BOOKS.

The Life of Thomas Wills, F.C.S., Demonstrator of Chemistry, Royal Naval College, Greenwich. By His Mother, Mary Wills, and her Friend, J. Luke. London: James Nisbet and Co.

We have here an interesting account of the short career of a young chemist, whose researches have been, from time to time, recorded in the Chemical News, and who, had he survived, would have earned for himself no mean position in the scientific world. The volume includes several of his memoirs and lectures, such as a paper on the "Solidification of Nitric Oxide," read before the Chemical Society, February 20, 1873; an account of a new ozone generator, described in the Chemical News, vol. xxvii., p. 292; a paper on the "Theory of the Spontaneous Generation of Life," communicated to the Westminster Young Men's Association, February 4, 1875; an account of the coal brought home by the late Arctic Expedition, read at the Plymouth Meeting of the British Association (1877); a paper on the "Production of the Oxides of Nitrogen by the Electric Arc in Air,"—a subject upon which he was working at the time of his death—and a course of lectures on "Explosions in Coal Mines," delivered before the Society of Arts in January and February, 1878. In all these productions he appears as a sound chemist and as a clear, highly cultivated, and original thinker. Perhaps the only occasion where he suffered his judgment to be warped was in his paper "More Light," where he comes forward as the champion not merely of gas, but of gas companies, and proclaims the strong public feeling against them an "unreasonable prejudice." The very fact that a gas company, in applying to Parliament for additional powers, has attempted to stipulate that the gas should only be analysed at the works, and *after two hours' notice had been given*, is more than sufficient to justify "prejudices" against such bodies. What would be thought of a shopkeeper who should petition for two hours' notice of the visit of the inspector of weights and measures?

The immediate cause of the premature death of Mr. Wills was typhoid fever, but he had been declining in health for some months previously, and there can be little doubt that overwork was the root of the mischief. His love for science was a perfect passion, and he was most conscientious in the fulfilment of his duties. But he had undertaken too much. One feature in his activity deserves in particular honourable mention. Whilst combatting that materialistic tendency with which science is in some quarters tainted, he ever sought to combat that jealousy with which the progress of research is viewed in the religious world.

University of Tokio: the Calendar of the Departments of Law, Science, and Literature. 1879-80. Tokio: Z. P. Maruya and Co.

This publication throws an interesting light on the progress of higher education in the Japanese Empire. Of the fifteen professors in the faculty of science four are native Japanese, the remainder being composed of four Americans, two Germans, two Frenchmen, and two Englishmen, perhaps the only instance outside the British Empire where a professorship other than of the English language and literature is held by persons of this nationality. The general arrangements of the University are rational. Not only are the three departments of faculties in fact separate institutions, but in the department of Science there are five distinct courses. These are—Chemistry; mathematics, physics, and astronomy; biology; engineering; geology and mining. These courses, however, are for the first year all identical; but for the remaining three years each student is required to elect one of the courses and pursue as his special studies those of the course chosen. Thus the error of "all-roundism," and the superficiality which it necessarily entails, are avoided. The common curriculum for the first year embraces the English language, logic, outlines of mental philosophy, mathematics, elementary mechanics, outlines of astronomy, inorganic chemistry, elementary mineralogy, elementary geology, and drawing. Each curriculum becomes increasingly special as the student advances, and in every case a graduating thesis is required at the close. In other words, the pupils are required to prove their capacity for research in the subjects they have selected, and cannot, as is unfortunately the case in England, attain honours by dint of mere verbal memory. In every course, however, Japanese and Chinese literature figures among the last year's studies—a requirement of doubtful value to a man of science.

It may be interesting to quote the questions in chemical technology given in the second examination of the fourth year:—

1. Give the usual dimensions of O. V. chambers, and show how you would arrange a series of five so as to get the most concentrated acid possible.
2. What chemical examinations are made in this process, and how.
3. Give a brief description of the Stassfurt salt-deposits.
4. Describe the construction of Mactear's carbonator.
5. What processes have been used for the recovery of the sulphur contained in tank-waste. Describe two of them briefly.
6. Describe the methods called respectively Dunlop's and Weldon's methods, used for recovering manganese from the still-liquors.
7. How is iodine obtained from kelp by Stanford's process.
8. Give an outline of the Stassfurt method of preparing bromine.

We do not find any mention of the graduating theses submitted by the students in any of the departments of the scientific faculty—an important omission, as such documents would have been the most satisfactory data concerning the real efficiency of the University.

Memoirs of the Science Department, University of Tokio, Japan. Vol. I., Part I. "Shell Mounds of Osmori." By E. S. Morse, Professor of Zoology, University of Tokio.

This memoir contains much to interest the zoologist, the antiquarian, and the anthropologist, but, as may be judged from the title, nothing of a chemical or a physical nature. Not merely the printing of the book, but the drawing and lithographing of the plentiful illustrations, have been executed, and that very satisfactorily, by Japanese artists and artisans.

CORRESPONDENCE.

CELLULOID.

To the Editor of the Chemical News.

Sir,—In the Chemical News, vol xlii., p. 10, in answer to a question from a correspondent "What is Celluloid, how prepared &c.," you state that it is the invention of D. Spill. Such is not the case, as it is the invention of Alexander Parkes, of Birmingham, who patented it in 1855, and has taken numerous patents since for improvements upon the original mode of manufacture and for a variety of applications, and after whom it was for some time called "Parkesine," and a company was formed called the Parkesine Company Limited, of which D. Spill was the working manager.

Will you kindly insert this correction, in order that the merit of the discovery may be awarded to the real inventor. Medals were awarded to Mr. Parkes for the discovery, at the English Exhibition of 1862, and the Paris Exhibition of 1867.—I am, &c.,—

Alexander Parkes.

3, Wellington Terrace, Sutton Coldfield,
July 19, 1880.

DIAZO-COLOURS WITH MORE THAN TWO SULPHO-GROUPS.

To the Editor of the Chemical News.

Sir,—In the account of the Proceedings of the American Chemical Society (Chemical News, vol. xlii., p. 44), there is a report of a paper by Mr. J. H. Stebbins on "*Some New Colouring Matters produced by the Action of Diazo-compounds on Phenols*," wherein the author lays claim to the introduction of more than two sulpho-groups (HSO_3) into these colouring matters as a novelty. Permit me to point out, through your columns, that last year I took out a patent (May 10, 1879, No. 1864), in which I describe the production of such colours, formed by the action of diazo-sulphanilic and its homologues on the salts of the disulphonic acid of β-naphthol, &c.—I am, &c.,—

R. Meldoda.

Atlas Works, Hackney Wick, London, E.,
July 26, 1880.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Note.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 1, July 5, 1880.

Photography of the Chromosphere.—M. Janssen.—By pursuing the method of reversing images by a prolonged exposition, which I communicated to the Academy at its last session but one, it seems to me that we may succeed in obtaining the photograph of the chromosphere. The luminous solar action must be continued long enough to become positive to the borders without going beyond them. The chromosphere then appears in the form of a black circle, the diameter of which corresponds to 8″ or 10″. I have compared positive and negative solar photographs obtained the same day with the same instrument, and the measure of the diameters shows that the black circle in question is clearly outside the solar disc.

The Consequences of the Experiment of MM. Lontin and Fonvielle.—M. Jamin.—A mathematical paper, not adapted for abstraction.

Vision of Colours.—M. Chevreul.—The author presents to the secretaries a letter addressed to him in 1846 by Colonel Baron Aubert, accompanied by five transparent designs caused by bands of superimposed paper. The optical effects of silk tissues, which form a small octavo volume of lectures delivered at Lyon in 1842 and 1843, are reduced to four principles which it is well to recall.

General Relations between the Chemical Mass of the Elements and the Formation-heat of their Compounds.—M. Berthelot.—Elements belonging to one and the same group, when they unite with a given simple body to form comparable compounds, evolve, in the majority of cases, quantities of heat, so much the less as the chemical mass of these elements is more considerable; the stability of the compound decreases in the same proportion. The decrease sometimes extends as far as a change of volume produced by the combination of solid elements, forming a compound likewise solid.

On a Meteorite which fell November 26, 1874, at Kerilis, in the Commune of Mael-Pestivien, Canton de Callac (Côtes-du-Nord).—M. Daubrée.—The meteorite when entire weighed about 5 kilos., and had been driven into the soil to the depth of 0·78 metre. Its sp. gr. is 3·51. The portion of the mass soluble in hydrochloric acid consists of peridot, pyrrhotine, and nickel iron.

On a Meteorite which fell September 6, 1841, in the Vineyards of St. Christophe-la-Chartreuse in the Commune of Roche-Servières (Vendée).—M. Daubrée.—This meteorite weighs 5·5 kilos., and is unfortunately in the hands of a local *propriétaire*, who refuses to part with it.

Nature of the Immunity of Algerian Sheep against Splenic Fever.—A. Chauveau.—All the indigenous sheep of Algeria enjoy in a more or less marked degree an immunity against this infection, and can communicate it by crossing to European sheep. French sheep do not acquire it by living and propagating in Algeria, and it is not known whether it may not be lost by Algerian sheep propagated in France.

Brilliant Spectral Rays of the Metal Scandium.—R. Thalén.—The rays of scandium are very characteristic, both by their arrangement and their lustre. They are almost all very fine, except certain rays in the yellow and orange portion and the seven strong rays situate in the blue-violet part of the spectrum and which have a certain width. Among these latter there is one, 4374, O, which the author suspected might coincide with a strong ray of yttrium. But on a direct comparison there is found a small but distinct difference of position. The scandium ray is the more refrangible. There are also in this spectrum certain very faint bands between 5900 and 5730. According to the view of Angström these bands may be due to the oxide of the metal.

Improvements in Coils of the Siemens Type.—G. Trouvé.—The polar surfaces, instead of being portions of a cylinder whose axis coincides with that of the system, are in the form of a helix, so that whilst turning round they gradually approach the surface of the magnet up to the point where the posterior margin escapes the pole of the magnet. The action of repulsion then begins, so that the dead point is practically avoided.

The Sensibility of the Eye to Differences of Light. A. Charpentier.—The eye appreciates much better the differences of intensity of two contiguous lights than the differences of two successive lights, since in the latter case the differential sensation is seven to eight times stronger than in the former, in which it is not more than 1-100th part. The sensibility of the eye is constant for the whole extent of the retina.

Thermic Researches on the Ammonium Polysulphides and Hydrogen Persulphide.—P. Sabatier.—The formation-heats of the three polysulphides, if formed from their elements, are given as +34·53, +34·73, and +34·83 calories respectively. Hydrogen persulphide is formed with absorption of heat, like oxygenated water.

The Density of the Vapour of Iodine.—L. Troost.—The density of the vapour of iodine decreases as well at low as at high temperatures. All the hypotheses which have been formed, based either upon a dissociation of iodine or upon an isomeric change, appear to me not very admissible. In the present state of our knowledge nothing warrants us to suppose that a partial vacuum suffices to produce a modification of this nature. The only consequences which necessarily flow from the experiments made at high temperatures or at low pressures, are that the coefficient of the expansion of iodine is variable with the temperature, and that its coefficient of compressibility varies with the pressure. All the hypotheses proposed to explain these results should take this double variation into account.

The Atomic Weight and some Characteristic Salts of Ytterbium.—L. F. Nilson.—As the mean of seven determinations the author gives the atomic weight of ytterbium as 17·301. Ytterbia, Yb_2O_3, is a white infusible powder, of sp. gr. 9·175. It is insoluble in water, but dissolves readily in acids, even if dilute, with the aid of heat. It is not readily attacked in the cold even by concentrated acids. The solutions are colourless, and do not present any absorption-ray. The earth and its salts do not impart any colouration to flame. The anhydrous sulphate is an opaque mass which does not lose all its sulphuric acid below a white heat. The aqueous sulphate forms large brilliant prisms, permanent in the air, but which lose their water at 100°.

The Solution of Platinum in Sulphuric Acid.—M. Scheurer-Kestner.—The author concludes from his experiments that absolutely pure sulphuric acid does not attack platinum, and that by making use of ammonium sulphate as recommended by Pelouze we may almost completely avoid injury to platinum vessels. Sulphuric acid containing traces of nitrous acid dissolves platinum, and the activity of this process increases with the concentration of the process. The acid from the lead chambers, even when containing an excess of sulphurous acid, attacks platinum, the nitrous acid resisting the action of the sulphurous acid, and being in a state of stable combination. Hence the corrosion of platinum is always due to the presence of nitrogenous compounds in sulphuric acid. One part in 10,000 suffices to dissolve a proportion of platinum such as is never observed industrially. To ascertain whether an acid is sufficiently free from nitrogenous compounds the most sensitive reactions are necessary. An acid in which ferrous sulphate scarcely reveals visible traces of nitrogen compounds is coloured blue by diphenylamine. It contains both sulphurous and nitrous acids, and it attacks platinum. During its concentration the quantity of nitrous acid scarcely diminishes, whilst the sulphurous acid is completely expelled.

Remarks on the Etherification of the Hydracids.—A. Villiers.—Etherification results from the equilibrium established between the hydrates which the acids form with water, and the alcoholates or combinations which they form with alcohol, both these classes of bodies being in a state of partial dissociation.

Atmospheric Bacteria.—P. Miquel.—The number of atmospheric bacteria is low in winter, rises in spring, reaches its maximum in summer and autumn, and falls rapidly with the first frosts. This law is equally applicable to the spores of fungi. But whilst the mould germs are most abundant in damp weather, the aërial bacteria are then rare, and become most numerous when the ground is dry. Until it shall be possible to prepare a liquid capable of causing the germs of all Schizophytes to germinate indiscriminately, it will be very difficult to find with accuracy the number of bacteria suspended in the air. On operating with neutral broth perfectly sterilised the mean annual number of bacteria found in a cubic metre of air does not exceed 200, which would lead us to believe that the atmosphere is a hundred times richer in spores of moulds than in germs of bacteria. In summer and autumn we find occasionally at Montsouris 1000 germs of bacteria per cubic metre of air. In winter it is not rare to see this number fall to 4 or 5, and to notice days where the dust of 200 litres of air is incapable of setting up infection, even in the most susceptible liquids. In the interior of houses, in the absence of mechanical causes which stir up dust, 30 to 50 litres are required to determine infection. In the author's laboratory 5 litres are sufficient to infect neutral broth. 1 litre of air from the sewers of Paris is enough to occasion a similar result. It will be seen how widely these results differ from those published by Prof. Tyndall. According to this *savant* a few c.c. of air are generally sufficient to introduce infection into the most various liquids. The author has compared the mortality in Paris due to infectious diseases with the number of bacteria present in the air. From this comparison, carried on from December, 1879, to June, 1880, it appears that every increase of atmospheric bacteria is followed in about eight days with an increase of the deaths from this class of diseases. Prolonged observation must prove whether this fact is anything more than a coincidence.

A Digestive Ferment in the Juice of the Fig Tree.—M. Bouchut.—The researches undertaken by the author in connection with Prof. A. Wurtz on the juice of *Carica papaya* have induced him to inquire whether the properties of papaïne are not connected with a general carnivorous property in the juices of many other plants. For the present he has established this property as existing in the milky juice of the fig tree. This juice is scanty and difficult to collect, and its properties vary in quality according to the more or less advanced stage of vegetation. The author's experiments were made with juice from Provence, collected in the month of April. 5 grms. of the partially coagulated juice were placed in a glass with 60 grms. of distilled water and 10 grms. moist fibrine, and exposed to a temperature of 50°. By evening the fibrine was completely digested. 10 more grms. of fibrine were then added and digested in twelve hours; then 12 grms., then 15 grms., at intervals of a day or two, until 90 grms. fibrine had been acted upon. The solution had a decided odour of good meat-broth, free from putridity.

Justus Liebig's Annalen der Chemie,
Band 202, Heft 1.

Phenyl-naphthyl-carbazol.—C. Graebe and W. Knecht.—The authors have obtained this compound from the pitch remaining after the distillation of anthracen. They have also obtained it synthetically by passing β-phenylnaphthylamine through a strongly-heated combustion-tube. It possesses no basic properties, but forms an unstable compound with picric acid. Phenyl-naphthyl-carbazol sublimes in leaflets, melts at 330°, and boils about 450°. It is almost insoluble in cold alcohol and benzol, but dissolves readily in boiling aniline.

Vapour-Determinations in the Vapour of Phosphorus Penta-chloride.—The author gives several determinations effected by this process.

The Compounds of Phthalic Acid with the Phenols (Second Memoir).—A. Baeyer.—This valuable paper, which extends to 110 pp. is quite incapable of abstraction.

Pereiro Bark.—O. Hesse.—The author finds in this bark the following bases, which are here described:—Geissospermine, $C_{19}H_{24},N_2O_2$; pereirine, $C_{19}H_{24}N_2O$; and a base not yet named, $C_{16}H_{18}N_2O$.

Caroba Leaves.—O. Hesse.—These leaves are obtained not, as was supposed, from the tree yielding the pereiro bark, but from *Jacaranda procera*. They contain no basic compounds.

Remarks on Morphine Hydrochlorate.—O. Hesse.—The author has obtained and analysed an anhydrous hydrochlorate.

Band 202, Heft 2.

Combinations of Phthalic Acid with the Phenols (Third Memoir).—A. Baeyer.—This paper consists of researches on ortho-cresol-phthaleine and its derivatives, conducted by G. Fraude. The principal derivatives here described are diacetyl-ortho-cresol-phthalein, dibenzoyl-ortho-cresol-phthalein, dibrom-ortho-cresol-phthalein, dinitro-ortho-cresol-phthalein, methyl-oxy-anthraquinon, brom-methyl-oxy-anthraquinon, methyl-alizarin, diacetyl-ortho-cresol-phthalin, and dibrom-ortho-cresol-phthalin. The author then briefly notices the phthalidin and phthalidein compounds of ortho-cresol.

TO CORRESPONDENTS.

Brickmaker.—Not a chemical question.

THE JOURNAL OF SCIENCE

for July (Price 1s. 6d.), includes—

The Evolution of Scientific Knowledge. By C. Lloyd Morgan.
Habits and Anatomy of the Honey-bearing Ant.. By C. Morris.
The "Laws of Emphasis and Symmetry." By J. W. Slater.
Instinct and Mind. By S. Billing.
Tuberculosis transmissible through the Meat and Milk of Animals affected with it.
Natural Science and Morality. By S. Tolver Preston.
Analyses of Books. Correspondence. Notes.

London: 3, Horse-Shoe Court, Ludgate Hill.

THE LONDON HOSPITAL and MEDICAL COLLEGE, MILE END, E.—The SESSION 1880-81 will commence on FRIDAY, OCTOBER 1st, 1880. Four entrance Scholarships, value £60, £40, £30, and £20, will be offered for competition at the end of September to new Students. Entries on or before September 20. Fees for Lectures and Hospital Practice, 90 guineas in one payment, or 100 guineas in three instalments. All resident and other hospital appointments are free. The resident appointments consist of Five House Physiciancies, Four House Surgeoncies, and One Accoucheurship; Two Dressers and Two Maternity Pupils also reside in the Hospital. Special entries may be made for Medical and Surgical Practice. The London Hospital is now in direct communication by rail and tram with all parts of the Metropolis.

MUNRO SCOTT, Warden.

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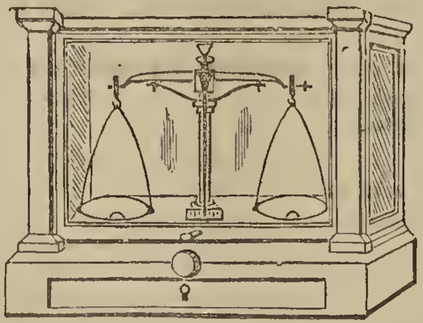

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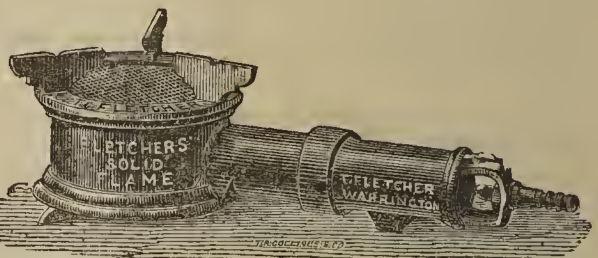

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SOME OBSERVATIONS UPON THE HYDROLYTIC FERMENTS OF THE PANCREAS AND SMALL INTESTINE.[*]

By HORACE T. BROWN, F.I.C., F.C.S., and
JOHN HERON, F.C.S.

We were requested a few months ago by Dr. W. Roberts to verify a statement, recently made by Musculus and De Méring, that *maltose* is a product of the action of an aqueous extract of pancreas upon starch-paste. During the prosecution of the inquiry, and whilst following up certain lines of experiment which suggested themselves from time to time, we have, besides fully confirming the results of the above-mentioned observers, ascertained certain facts which we believe are of some physiological importance in elucidating the still very obscure processes of animal digestion and nutrition.

I. *Hydrolytic Action of the Pancreas.*

The first observation upon the amylolytic action of the pancreatic secretion appears to have been made by Bourchardat and Sandras[†] in the year 1845. The general functions of the gland were more fully studied in 1856 by Claude Bernard,[‡] and a few years later by Cohnheim (*Virchow's Archiv.*, 28, 241, 1863). Danilewski[§] in 1862, and Hüfner (*Journal f. Prakt. Chem.* [2], 5, 1872, 396), ten years later, isolated a soluble amylolytic ferment from the pancreas, the former observer by acidifying the aqueous infusion with phosphoric acid and precipitating with lime; the latter by the glycerin method first described by Wittich.

Hüfner found that the isolated body, which was still doubtless impure, contained 14·95 per cent. of nitrogen, but a less quantity of carbon and a greater quantity of oxygen than an ordinary albuminoid.

Until recently it has always been taken for granted that the fermentable sugar produced by the action of pancreas upon starch-paste is dextrose. Nasse, however, in 1878 (*Pflüger's Archiv. f. Physiologie*, 14, 473), attempted to show that the fermentable and cupric-oxide reducing body, obtained by the action of saliva upon starch, is a specific sugar which he calls *ptyalose*, and that the action of pancreatic juice appears to result in the production of the same body. Nasse did not isolate this hypothetical sugar, nor even approximately determine its specific rotatory power, but relied mainly upon its non-reduction of Barfoed's solution, and its doubling in reducing power on boiling with dilute acid, as evidence of its non-identity with dextrose.

In the early part of last year Musculus and De Méring published an important memoir (*Bull. Soc. Chim.*, 31, 105), upon the action of diastase, saliva, and pancreatic secretion upon starch and glycogen.

They concluded from their experiments that the fermentable sugar produced in all these reactions is a mixture of maltose and dextrose. We have shown in a recent communication to the Chemical Society (*Journ. Chem. Soc.*, 1879, 1, 648; *Liebig's Ann.*, 199, 245), that *dextrose* can certainly not be included amongst the products of the action of malt diastase upon starch. The experiments we are about to describe will afford ample proof of the correctness of that portion of the above statement of Musculus and De Méring referring to the action of pancreatic extract.

Our experiments were made either with the aqueous infusion of the gland, or with the actual tissue itself in a finely divided state.

The first method is by far the most convenient, and is the one which we generally adopted when studying the action of the pancreas, but it will be seen that certain tissues can produce under some circumstances hydrolytic effects which their aqueous infusions are incapable of exercising.

With a clear aqueous infusion of the gland, the course of experiment did not differ materially from that which we employed when investigating the action of malt-diastase upon starch (*loc. cit.*). As a rule the infusion had no power of reducing cupric oxide, hence it was not necessary to apply any correction to the quantity of cupric oxide reduced by the transformation products.

In cases where the transformation was effected by the actual tissue itself, this was previously dried very rapidly in a current of air at 35° C., a process which could easily be carried out without decomposition taking place. The dried tissue was used with a given volume of the experimental liquid containing a known quantity of the carbohydrate. The volume of the liquid was kept constant during the experiment, and the small increase in solid matter due to hydration was taken into consideration. Far more accurate results are obtained by this method than by attempting to estimate the correction for the total solids by digesting a given weight of the tissue in a given volume of water. In the latter case the solvent action of the water upon the tissue always differed somewhat from the solvent action of the solution of the carbohydrate employed.

When a clear infusion of pancreas, obtained by acting for several hours upon one part of the finely divided pancreas of the pig with five parts of cold water, is added to starch-paste at 40° C., complete limpidity is produced with extreme rapidity, and in from ten to fifteen minutes iodine ceases to produce any reaction for soluble starch or erythrodextrin. Careful observations at different periods of the reaction showed unmistakeably that the transformation in its first stages does not differ in the least from transformations of starch-paste made at 60° C. with aqueous malt-extract, which has not been previously heated to a higher temperature. When the specific rotatory power of the transformation products has reached $[\alpha]_{j3\cdot86}$, 162°·5, the cupric-oxide reducing power is found to be κ 49—50. When about 3 cub. centims. of the pancreatic infusion have been used for every grain of starch, the time occupied in fully bringing about this change is from forty to fifty minutes. The liquid now contains *maltose*, and an achroodextrin having all the properties of our *achroodextrin* ζ.

The maltose was proved to be identical with that obtained by the action of malt-extract by isolating it in its characteristic crystalline form, and by determining its specific rotatory and cupric-oxide reducing powers.

The composition of the matter in solution at this point is

Maltose	80·8
Dextrin	19·2
	100·0

which is that required by our No. 8 equation.

$$\underbrace{10C_{12}H_{20}O_{10}}_{\text{Soluble Starch.}} + 8H_2O = \underbrace{8C_{12}H_{22}O_{11}}_{\text{Maltose.}} + \underbrace{C_{24}H_{40}O_{20}}_{\text{Achroodextrin }\zeta.}$$

On continuing to digest the liquid at 40° C., the values of $[\alpha]_j$ and κ undergo a little alteration. which is, however, very slow when compared with the previously rapid degradation of the starch-molecule.

The following is an example of such an experiment:—

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[*] A Paper read before the Royal Society, May 13, 1880.

[†] "Des Fonctions du Pancréas, et de son Influence dans la Digestion des Féculents," *Compt. Rend.*, 20, 1085.

[‡] "Mémoire sur le Pancréas," 1856. "Leçons de Physiologie Expérimentale," Paris, 1856.

[§] "Ueber specifisch wirkende Körper des Natürlichen und Kunstlichen Pancreatischen Saftes," *Virchow's Archiv.*, 1852, 25, 279.

90 cub. centims. of a liquid containing 3·5 grms. of starch-products, already reduced to the above point, were digested at 45° C. for twelve hours, with the further addition of 10 cub. centims. of fresh pancreatic extract.

At the end of that time, the liquid having remained perfectly free from organic life, the starch-products yielded on analysis the following corrected numbers:—

$[\alpha]_{j3\cdot86}$	128°·7
$\kappa_{3\cdot86}$	66°·8

corresponding very closely with the composition

Maltose	55·5
Dextrose	32·7
Dextrin	11·8
	100·0

The united evidence afforded by many such experiments as the above proves conclusively that the *prolonged* action of a pancreatic infusion yields results differing materially from those obtained under similar conditions with malt-extract. With malt-extract the hydrolytic effect upon the starch-products, after No. 8 equation is reached, is confined entirely to the conversion of the achroodextrin into maltose, the latter body being the final product of the reaction. On the other hand, an aqueous infusion of pancreas, whilst converting achroodextrin into maltose, sensibly hydrolyses the maltose to dextrose.

In order to put this important point of difference between the action of malt and pancreatic extract beyond all doubt, a solution of pure maltose was digested with an infusion of pancreas, in just the same manner as maltose had, on many previous occasions, been treated with malt-extract.

100 cub. centims. of a solution of maltose, containing 5·813 grms. of substance, were mixed with 20 cub. centims. of a clear aqueous infusion of pancreas, the infusion being added in successive portions of 5 cub. centims. each. Temperature of digestion 45° C.

In four hours the optical activity of the solution had fallen from $[\alpha]_{j3\cdot86}$, 150°·0, to $[\alpha]_{3\cdot86}$, 148°·7; and after sixteen hours to $[\alpha]_{3\cdot86}$, 135°·1; the value of κ at the end of this time having risen from 61 to 67·3. The composition deduced from these latter numbers is—

Maltose	83·8
Dextrose	16·2
	100·0

This experiment, repeated many times, always gave the same results, thus proving that the amylolytic agent of the pancreas is capable, as stated by Musculus and De Méring, of slowly converting maltose into dextrose, a property which is not shared by malt diastase. Both malt diastase and pancreatic diastase have, however, the property of hydrolysing the lowest acroodextrin of the series, a fact which we have proved by submitting to their action the isolated and purified achroodextrin ζ.

A series of experiments was now made with the object of ascertaining if the pancreas contains any ferment capable of inverting cane-sugar. It will be remembered that a cold aqeous infusion of malt possesses this property to a limited extent. The first experiments were made by digesting solutions of cane-sugar at 40° C., with 25 per cent of an aqueous extract of pancreas (5 of water to 1 of pancreas). As long as the solutions remained free from bacteria we never observed even the slightest inversion of the cane-sugar, the specific rotatory power of the solutions remaining constant throughout the experiment. If the digestion was carried on for a length of time sufficient to develop organic life, and a sensible evolution of gas, a marked inversion of the sugar set in, a result which is not however attributable to any soluble ferment derived from the pancreas.

From some observations made at a later period of the inquiry it seemed possible that the gland-tissue itself might have an effect which was not shared by its aqueous infusion. An experiment was consequently made in which the finely divided pancreas itself was digested with the cane sugar solution. Here again, however, as in the former case, absolutely no inversion took place.

J. Béchamp, in his experiments upon the action of various tissues upon starch-paste and cane-sugar, states that the pancreas has a very slight invertive action upon cane-sugar ("Les Microzymas," p. 68). In an examination of the details of his experiments, however, it is clear that he only obtained evidence of invertive action after the gland had been in contact with a sugar solution for several days, and in no cases without the previous appearance of bacterial life to which doubtless the effects are attributable; the invertive action of some of these organisms being almost as well marked as that of the Saccharomyces.

II. *The Hydrolytic Action of the Small Intestine.*

Claude Bernard first called attention to any distinct hydrolytic action of the small intestine. He found that a solution of cane-sugar, inclosed within a portion of the intestine ligatured above and below, or placed in contact with an infusion of its mucous membrane, speedily acquired the property of reducing a cupric solution.

Bernard found this property common to the small intestine of the dog, pig, rabbit, rat, and various other animals, and looked upon the invertive action as one of the most important functions of the *succus entericus*.

The observations made from time to time upon the *amylolytic* action of the secretions of the small intestine are somewhat conflicting. Thiry (*Wien. Sitzungsber.*, l., 77), in 1864, by isolating a portion of the small intestine of a living animal, obtained an albuminous secretion which he found had no action upon starch, and with this observation Funke agrees. On the other hand, Masloff (*Unters. Physiolog. Inst. Heidelberg* [2], 1879, p. 290), Frerichs, and Busch (*Virchow's Archiv.*, xiv., p. 140) state that the small intestine has the power of transforming starch.

When commencing to investigate the hydrolytic action of the small intestine of the pig, we made use of an aqueous infusion of the tissue, made by acting upon one part of the well-washed and finely-minced intestine with five parts of water for from ten to fifteen hours.

Such an aqueous extract was prepared from three different portions of the intestine.

(1.) A portion of the *duodenum* immediately below the glands of Brunner.

(2.) The *agminated Peyer's glands* (Peyer's patches) cut from the jejunum.

(3.) Portions of the *jejunum* and *ileum* not containing any of the agminated Peyer's glands.

20 c.c. of the clear filtered infusion made with each of the above portions of intestine were added to 100 c.c. of a cane-sugar solution containing 4·557 grms. of sugar per 100 c.c. On digesting for three hours at 40° C., and allowing subsequently to stand in the cold for twenty-four hours, scarcely a trace of inversion was found to have taken place.

The action of equal quantites of the various infusions upon starch-paste was scarcely more marked; about 3 grms. of starch, in the form of starch paste, being employed in each case, at a temperature of 40—45° C. After digestion for sixty minutes, none of the samples of starch-paste showed any signs of limpidity. After sixteen hours, No. 1 was found limpid, but contained only soluble starch; No. 2 was perfectly limpid, and contained a little erythrodextrin; whilst No. 3 was absolutely unacted upon, the gelatinisation being still as perfect as at the outset of the experiment.

The pig, from which was derived the intestine used in the above experiments, had been killed after fasting for thirty-six hours. It occurred to us that the absence of any well-marked amylolytic action might be due to this fact, and that a different result would probably be obtained by infusing an intestine in which the various glands had been more recently active. In order to test this, an animal was killed about two hours after administering a liberal allowance of barley-meal. In this case the aqueous extract of the small intestine possessed a somewhat greater action upon starch than in the previous experiment, but the transforming power was still very feeble, more than two hours being required for the production of limpidity in the starch-paste containing the most active of the three portions of the intestine, which was in this, as in the former case, the region of the jejunum and ileum containing the Peyer's patches.

It is possible, as was first shown by Berthelot, to obtain a clear aqueous infusion of ordinary yeast, which is capable of exercising a very decided invertive action upon cane-sugar. The action of this aqueous infusion is, however, feeble when compared with the inversion produced by actual contact of the yeast-cells themselves.

Reasoning from this fact, it seemed to us probable that, in the case of the intestine, far more pronounced hydrolytic results might be expected from acting with the tissue itself, than from merely using its aqueous infusion. This was found to be case.

In the following experiments the different portions of the small intestine, after thorough and prolonged washing, were dried rapidly in a current of air at 35° C., and were divided into very fine shreds, which were immersed directly in the solutions under examination. The intestine was taken from a pig of eight months old, killed during the period of digestion.

The solution of cane-sugar employed contained 3·020 grms. of sugar per 100 c.c.

To every 100 c.c. of this solution were added 5 grms. of the finely divided dry intestine. Temperature of digestion, 40° C.

All the determinations were made by the optical method.

Comparative Action of the different portions of the Small Intestine of the Pig upon Cane-Sugar.

Portion of the small Intestine.	Percentage of Cane-Sugar Inverted. After 1½ hrs. at 40°.	After 3½ hrs. at 40°.	After 16 hrs. in the Cold.	After further digesti'n for 5 hrs. at 45°.
(1) Duodenum immediately below the pylorus, containing Brunner's glands ..	No action	No action	No action	13·0
(2) Duodenum below the glands of Brunner ..	No action	No action	10·9	13·0
(3) Jejunum, not including any of Peyer's patches	—	14·0	19·5	25·1
(4) Ileum	—	14·0	19·5	25·1
(5) Agminated glands of Peyer (Peyer's patches), cut from the jejunum	9·2	18·4	24·6	26·7

Action of Various Portions of the Small Intestine upon Starch.

The action of the *tissue* of the small intestine upon starch-paste, as upon cane-sugar, is decidedly more energetic than that of its aqueous infusion. Limpidity of the starch-paste is, however, not rapidly brought about, and when produced the resulting soluble starch is very stable, and resists any sensible hydrolysation for a considerable time.

Strictly comparative experiments upon the amylolytic power of the various parts of the intestine were made in the following manner:—30 grms. of potato-starch were gelatinised with 1000 c.c. of water, and 1 c.c. of malt-extract was added to the resulting starch-paste after cooling to 60° C. The moment limpidity was produced, the further action of this trace of malt-extract was arrested by boiling. The liquid, filtered perfectly bright on cooling, and containing, besides soluble starch, only a trace of erythrodextrin and maltose was analysed; its specific gravity, optical activity, and cupric-oxide reducing power being determined. The solution was divided into portions measuring 100 c.c., into each of which were immersed 5 grms. of the dried and finely-divided intestine. The various experimental liquids were digested under exactly similar conditions, in the water-bath at 40° C.

The portions of the intestine taken were as follows, the animal from which they were derived being a young pig, killed during active digestion of starchy food:—

1. A portion of the duodenum taken immediately below the pylorus, and containing numerous Brunner's glands, which were very apparent in this case, owing to active digestion being in progress at the time of the animal's death.
2. Lower portion of the duodenum, not containing any Brunner's glands.
3. Agminated Peyer's glands, taken from the jejunum.
4. Portions of the jejunum, not containing any Peyer's patches.
5. Portions of the ileum, taken at the distance of a few inches from the ileo-cæcal valve.

The solutions gave the following iodine reactions, the dilute iodine solution being slowly added in each case up to an excess, thus ensuring the detection of any erythrodextrin:—

After digestion for fifteen minutes—all deep blue—no production of erythrodextrin.

After thirty minutes—a very slight production of erythrodextrin in all.

After forty-five minutes—all gave a violet reaction. From the larger amount of iodine solution requisite to produce a permanent colouration in No. 3, it was evident that the hydrolytic action was proceeding more rapidly in this than in any of the other solutions.

After three and a half hours—Nos. 1 and 2—deep violet reaction. No. 3 contained only a trace of unconverted soluble starch, and no erythrodextrin.

Nos. 4 and 5, violet reaction, but much lighter in tint than Nos. 1 and 2.

After the digestion for three and a half hours at 40°, and lying in the cold for sixteen hours longer, the various liquids were fully analysed. The corrected results are here given:—

Portion of the Intestine.	3½ hours at 40°. $[\alpha]_{j3·86}$.	16 hours in the cold. $[\alpha]_{j3·86}$.	$\kappa_{3·86}$.
(1) Duodenum, with Brunner's glands	179·8°	149·3°	41·7
(2) Lower part of duodenum ..	163·4	140·7	47·8
(3) Peyer's patches..	148·3	122·3	63·3
(4) Jejunum	159·7	133·0	53·0
(5) Ileum	157·2	134·9	50·6

Upon calculating the composition of the transformation products from the above numbers, a very remarkable fact was brought to light. *Maltose* was present in only one case, that of No. 3, the whole of the cupric-oxide reducing body consisting in the other cases of *dextrose.* The percentage composition of the products is here given.

		$[\alpha]_{j3·86}$.	$\kappa_{3·86}$.	$[\alpha]_{j3·86}$.	$\kappa_{3·86}$.
(1) Dextrose	41·7				
Soluble starch and dextrin ..	58·3	150·3°	41·7	149·3	41·7
	100·0	Calculated.		Found.	

		$[\alpha]_{j3·86}$.	$\kappa_{3·86}$.	$[\alpha]_{j3·86}$.	$\kappa_{3·86}$.
(2) Dextrose	47·8				
Soluble starch and dextrin ..	52·2	140·7°	47·8	140·7°	47·8
	100·0	Calculated.		Found.	

(3) Maltose	16·6	$[\alpha]_{j3\cdot86}$.	$\kappa_{3\cdot86}$.	$[\alpha]_{j3\cdot86}$.	$\kappa_{3\cdot86}$.
Dextrose	53·1	122·0°	63·3	122·3	63·3
Dextrin	30·3	Calculated.		Found.	
	100·0				
(4) Dextrose	53·0	$[\alpha]_{j\cdot386}$.	$\kappa_{3\cdot86}$.	$[\alpha]_{i3\cdot86}$.	$\bar{k}_{3\cdot86}$.
Soluble starch and dextrin ..	47·0	132·6°	53·0	133·0°	53·0
	100·0	Calculated.		Found.	
(5) Dextrose	50·6	$[\alpha]_{i3\cdot86}$.	$\bar{k}_{3\cdot86}$.	$[\alpha]_{i3\cdot86}$.	$\kappa_{3\cdot86}$.
Soluble starch and dextrin ..	49·4	136·3°	50·6	134·9°	50·6
	100·0	Calculated.		Found.	

The question now arises—has the dextrose obtained in these experiments passed previously through the stage of maltose, or has it been derived more directly from the starch?

Direct evidence, as well as that furnished by analogy, points strongly to the former proposition being the correct one. We have seen, in four out of five of the experiments, that the soluble starch resisted very persistently the hydrolytic action of the ferment. Where this resistance to transformation was least strongly marked, as in the experiment with the Peyer's glands, a little maltose was found.

The conclusion seemed a fair one that we had here to deal with a remarkable hydrolysing agent, differing, in its relative action upon starch and starch-products, from that of any other known ferment of its class; an agent which is in fact capable of hydrolysing *maltose* with greater ease than *soluble starch.*

On further experiment this surmise proved correct.

All portions of the small intestine exert at 40° a rapid hydrolytic action upon maltose; an action, however, which varies in intensity in different parts of the intestine, and is far more energetic than that of a similar portion of the intestine upon starch-paste, soluble starch, the higher dextrins, and even cane-sugar itself.

After thoroughly washing the small intestine of a pig, the portions of the jejunum containing the agminated Peyer's glands were cut out, rapidly dried at 35°, and cut into fine shreds, 5 grms. of which were immersed in 100 c.c. of a solution containing 3·107 grms. of pure maltose.

The mixture was digested at 40° for sixteen hours, and at the end of that time it was found that *the maltose had been entirely converted into a dextrose* which possessed the same specific rotatory power and cupric-oxide reducing power as ordinary dextro-glucose, with which it was doubtless identical.

All portions of the small intestine exert, *cæteris paribus*, a much more rapid and complete action upon maltose than upon cane-sugar, and the hydrolytic effect of the agminated Peyer's glands upon either of these carbohydrates is far greater than that of any other portion of the small intestine, either for equal weights or for equal areas of the tissue.

We have satisfied ourselves of the truth of these statements by numerous experiments.

The following results exhibit the relative action upon maltose and cane-sugar of (1) the agminated Peyer's glands of the jejunum, and (2) of the adjoining portions of the jejunum containing, besides the glands of Lieberkühn, only solitary Peyer's glands. The experiments were conducted under exactly similar conditions, 5 grms. of the dried and finely-divided intestine acting in each case upon 3 grms. of the carbohydrate dissolved in 100 c.c. of water.

1.—Action of Agminated Peyer's Glands of the Jejunum upon Cane-sugar and Maltose.

	Percentage of Carbohydrate Hydrolysed.			
	1½ hours at 40°.	3½ hours at 40°.	After 16 hours in the cold.	5 hours more at 45°.
Cane-sugar ..	9·3	18·4	24·6	26·7
Maltose ..	15·4	33·9	52·2	74·3

2.—Action of the Jejunum, without Agminated Peyer's Glands, upon Cane-sugar and Maltose.

	Percentage of Carbohydrate Hydrolysed.			
	1½ hours at 40°.	3½ hours at 40°.	After 16 hours in the cold.	5 hours more at 45°.
Cane-sugar ..	10·9	13·6	21·7	24·4
Maltose ..	4·2	26·6	38·6	57·9

These experiments, in conjunction with those upon cane-sugar previously described, prove that the activity of the small intestine upon saccharose is slow and incomplete, when compared with its power of converting maltose into dextrose; and also that, whilst the conversion of maltose into dextrose under the action of the intestine ferment is as continuous and uninterrupted a process as is its conversion by dilute sulphuric acid, the invertive action upon cane-sugar is decidedly limited, the action being either arrested or proceeding with extreme slowness, when 25 per cent. of the total quantity of cane-sugar has been inverted. The reason for this limited invertive action is by no means clear, and the subject requires further investigation.

Claude Bernard, who first demonstrated the existence in the small intestine of a soluble ferment capable of inverting cane-sugar, considered that in this property resided one of the most important functions of the *succus entericus.* By injecting a solution of cane-sugar into the veins and cellular tissue of animals he demonstrated that this carbohydrate, after traversing the system, is eliminated weight for weight in the urine, without undergoing any modification or assimilation. In order that cane-sugar shall be assimilated by the animal or vegetable economy, it must first be *inverted.* The seat of the invertive action is in the small intestine itself.

If this function of the small intestine has the importance attributed to it by Bernard, it is in the highest degree probable that the relatively far more active maltose-hydrating ferment, coexisting with the invertive ferment, must possess some considerable physiological value.

It must be remembered that, under natural conditions, the amount of cane-sugar which an animal is called upon at any given time to assimilate is very small when compared with the amount of products derived directly from starch.

We cannot consider, under these circumstances, that so well-marked and striking a function of the small intestine as that of converting maltose into dextrose can be useless to the animal economy. The most probable explanation is that maltose is incapable of assimilation in its unaltered state, but has first to be broken down to the smaller moleculed dextrose, just as cane-sugar, prior to assimilation, is converted into the chemically less complex dextrose and levulose. This is rendered the more probable from the known similarity of composition of maltose and cane-sugar, both bodies belonging to the class of sugars represented by the formula $C_{12}H_{22}O_{11}$.

Whether maltose is capable, under any circumstances, of being directly assimilated is a question, the solution of which we must leave in the hands of experimental physiologists. Probably a series of carefully conducted injection experiments, similar to those made by Bernard with cane-sugar, would yield the desired information. It is true that the estimation of maltose in urine would be attended with greater difficulties than the estimation of cane-sugar, but these difficulties are by no means insurmountable.

It will be remembered that the action of artificial pancreatic juice upon gelatinised starch is very rapid, the transformation products in a short time containing 80 per cent of maltose, which is but very slowly and partially converted into dextrose by a continuance of the reaction. The active agent of the small intestine, on the other hand, whilst exerting but little action upon gelatinised or soluble starch, converts with great readiness maltose into dextrose. Thus we see that in the transition from colloidal starch to highly diffusible dextrose, the hydrolytic actions of the pancreas and small intestine *are mutually dependent and complementary to each other*, neither one set of actions alone being sufficient.

The small intestine does not contain a very active *amylolytic* ferment, because it is seldom or never called upon to act upon unaltered starch, the first portion of the work being completed by the pancreatic secretion. Brücke (*Wien. Akad. Ber.*, 65 (3), 126), when experimenting upon dogs fed with amylaceous food, found that the soluble starch and erythrodextrin which were produced in the stomach at once disappeared on passing the pylorus, under the rapid action of the pancreatic juice.

We have now to consider more fully the part played by the three different sets of glands of the small intestine in bringing about the hydrolytic effects which we have described. These glands are known as—(1) the glands of Brunner; (2) the glands or follicles of Lieberkühn; and (3) the glands of Peyer.

Brunner's glands occur only in the duodenum. They are most numerous immediately below the pylorus, and resemble closely in structure the salivary glands, or minute portions of the pancreas. When a portion of the duodenum containing these glands is macerated in water, the liquid becomes extremely viscous, owing to the extraction of the special glandular secretion. This effect is best observed by taking the duodenum of an animal which has been killed during digestion. The viscid secretion, resembling in appearance submaxillary or sublingual saliva, has but a very slight amylolytic action. The portion of the duodenum containing the glands has only a slight invertive action upon cane-sugar, but a somewhat more decided hydrolytic action upon maltose.

The glands or follicles of Lieberkühn consist of tubular depressions in the mucous membrane of the intestine, and are generally supposed to secrete the *succus intericus*, to which, however, the Brunner's glands must also largely contribute. Since Lieberkühn's follicles are pretty evenly distributed throughout the whole of the small intestine, and the hydrolytic effect of equal areas of the intestine varies very much in different parts, it is evident that these glands play no very important part either in the inversion of cane-sugar or in the still more active hydration of maltose to dextrose.

We believe that the variable hydrolytic action of the different regions of the small intestine is mainly, if not entirely, due to the relative frequency of the glands of Peyer. The solitary Peyer's glands occur most scantily in the upper portion of the duodenum, and here the hydrolytic effect is by far the least. As the solitary glands increase in number the action of the intestine becomes more strongly marked, and finally the regions of the jejunum and ileum containing the agminated glands, or Peyer's patches, are the portions of the intestine which exert the most pronounced hydrolytic effect upon maltose, cane-sugar, and starch. These glands consist of small ovoid masses of adenoid tissue, embedded in the mucous membrane, and enclosing vast numbers of leucocytes. The glands are in intimate relation both with the vascular and lymphatic systems, each follicle being penetrated by blood-vessels, and surrounded by lymph sinuses which are in connection with the lacteals of the villi.

The function of the Peyer's glands is by no means established. The opinion held by physiologists up to a few years ago that they discharge, by occasional rupture an intermittent secretion into the intestine, appears now to be abandoned in favour of the view that they are instrumental in absorbing material from the blood and chyle, which they elaborate and again transmit, in a modified form, in part to the portal blood, and in part to the lacteal system.

The property which the glands undoubtedly possess of hydrolysing maltose to dextrose, and of so rendering the starch products of the pancreatic digestion more fitted for nutrition, is probably one of these special functions of elaboration.

Our work on these matters has been necessarily restricted to the chemical side of the subject, and we must now, lest we should incur the charge of overstepping our *métier*, leave further observations in the hands of physiologists.

The following are the main points which we consider have been established by our experiments:—

(1.) The action of artificial pancreatic juice upon starch-paste or soluble starch at 40° C. is, in the earliest stages of the reaction, similar to that of unheated malt-extract acting at 60° C. and under, the composition of the starch products becoming comparatively stationary when 80·8 per cent of maltose has been produced.

(2.) Both malt diastase and pancreatic diastase are capable of hydrolysing the lowest achroodextrin to maltose.

(3.) Pancreatic diastase is capable, by long continued action at 40°, of slowly but sensibly converting maltose into dextrose, a change which malt diastase is incapable of effecting even under the most favourable circumstances.

(4.) Neither artificial pancreatic juice, nor the tissue of the gland itself contains any ferment which is capable of inverting cane-sugar.

(5.) The small intestine is capable of hydrolysing maltose, of inverting cane-sugar, and of acting feebly as an amylolytic ferment.

(6.) The action of the tissue of the small intestine in bringing about these changes is far greater than that of its mere aqueous infusion, and differs materially in different regions of the intestine.

(7.) The variability of the hydrolytic action of different portions of the small intestine is independent of the relative frequency either of the glands of Lieberkühn, or of those of Brunner, but appears to be correlative with the distribution of Peyer's glands.

(8.) In the transition from colloidal starch to readily diffusible and easily assimilated dextrose, the actions of the pancreas and of the Peyer's glands are mutually dependent and complementary.

The pancreas readily breaks down the starch to maltose, but is capable only of a very slow conversion of the resulting maltose to dextrose. The Peyer's glands, on the other hand, whilst almost powerless upon starch itself, take up the work at a point where the pancreatic juice almost ceases to act, and so complete the conversion of starch into dextrose.

ON THE
PRESENCE OF RED OXIDE OF MERCURY IN OLD SAMPLES OF GREY POWDER.

By DAVID LINDO.

Hydrargyrum cum creta, recently prepared, is no doubt justly considered the mildest of mercurial preparations. That it may become poisonous by long keeping, through absorption of oxygen and consequent conversion of the metal into mercuric oxide, is a fact worth noting, and which ought to be pretty generally known; yet, I find no allusion made to it in works in which we should certainly expect to find it mentioned.

In September, 1878, I was asked to examine a sample of grey powder (or rather what was once grey powder), strongly suspected of having caused symptoms of irritant poisoning which exhibited themselves in children who had taken some of it; and of having caused salivation in the case of an adult—doses not specified.

The quantity brought to me weighed about 509 grms. It was contained in a glass bottle, closed with a fairly fitting cork, covered on the under surface and sides with white kid. The bottle was about three-fourths full, and was alleged to be the original package in which the drug had been imported from England in 1862. It had not been excluded from light, and for some time previous had been left neglected on a shelf. The mass possessed a decided reddish colour throughout, and I could detect no globules of mercury in it by the most careful scrutiny with a powerful lens. Pure cold dilute HCl dissolved the substance almost entirely, and solution of stannous chloride produced a copious precipitate when added to the filtered fluid. Further examination being unnecessary, in order to pronounce the sample unfit for use, it was put aside for more complete investigation at some future time.

I have recently estimated the amount of mercuric oxide in this sample, as well as the total mercury. At the same time, I have examined ten other samples of grey powder; one prepared by myself in September, 1878, and the remaining nine collected from different dispensaries in the Island. The sample prepared by myself, has been kept from the light in a glass stoppered bottle, which it nearly fills. The bottle has been rarely opened. It now gives up only 0·14 of 1 per cent of metal to cold pure dilute HCl,[*] equal to about 0·15 of a per cent mercuric oxide.

Of the other nine samples, three were found to contain a large proportion of the metal in the form of mercuric oxide, and two sufficient of this impurity to render them unsafe. The results of the analyses are given below. The remaining four samples may be considered mild preparations, since they contain mere traces of mercuric oxide, if any. No two of these, however, exhibited the same shade of colour, and would, no doubt, be found to vary to the same extent in activity.

The darkest slate among them yielded only 0·18 of a per cent of metal to cold dilute HCl.

	Mercury in form Soluble in Cold Dilute HCl, per cent.	=	Mercuric Oxide, per cent.	Total Mercury, per cent.
No. 1. Supposed to be about 18 years old..	35·26	=	38·08	36·19
No. 2. Age not known	23·67	=	25·56	32·65
No. 3. Said to be 7 to 8 years old	15·79	=	17·05	31·75
No. 4. Said to be 4 to 5 years old	17·05	=	18·41	33·37
No. 5. Age not known	7·40	=	7·99	30·22
No. 6. Age not known	5·33	=	5·75	31·45

No. 1 is the sample first alluded to. As already stated, no globules of mercury were found in it when first examined. I have since failed to detect any, even in the residue left on dissolving it in dilute HCl. Globules of metal were found in all the others. Allowing for increase of weight due to absorption of oxygen, these samples would originally have contained mercury per cent as follows.—

No. 1.	37·24
No. 2.	33·28
No. 3.	32·16
No. 4.	33·83
No. 5.	30·40
No. 6.	31·58

No. 1 must therefore have been compounded according to the formula, 3 of mercury to 5 of chalk, which requires 37·5 per cent of metal, and the others (though most of them not accurately) according to the more usual formula, 1 of mercury to 2 of chalk. The colour of the red oxide of mercury was very apparent in Nos. 1 and 2, and ought to have attracted attention, yet these samples were being dispensed. The others were not sufficiently tinged by the acquired impurity to excite suspicion by mere inspection of there being anything wrong.

The following passages occur in Neligan:—

"According to the recent investigations of many celebrated chemists, grey powder appears to consist of metallic mercury in a state of minute division, a very small proportion of mercurous oxide, and carbonate of lime combined mechanically."

Under Characters and Tests.—"The solution formed with hydrochloric acid is not precipitated by the addition of chloride of tin."

Under Adulterations.—"I have always found this a very pure preparation; the pharmacopœial test with chloride of tin is directed against mercuric oxide, a possible impurity, and which, were it present, would be a most serious one."

I have never met with a sample of grey powder, however recently prepared, that would stand this test perfectly. Though the experiment may be performed at the ordinary temperature, and the acid employed be pure and dilute, a *small* quantity of metal is always found in solution. Whether this is owing to the presence of mercuric oxide produced during the manufacture, or to the conversion of a small portion of the unstable mercurous oxide into mercuric oxide and metal during solution, it is difficult to determine.

In connection with this enquiry the question is of no practical importance, as I have found powders of a dark slate colour which evidently contained notable quantities of mercurous oxide yield mere traces of metal to the acid under the conditions stated.

The conversion of the metal into the higher oxide would be likely to take place more rapidly in a warm moist climate such as this is than in one where a lower temperature and drier atmosphere prevails. I can hardly suppose light to be an indispensable agent in bringing about the change, since it is difficult on that supposition to account for its pervading the entire mass, which was the case with the samples examined by me. I cannot vouch for the accuracy of the statements made respecting the age of these samples. I believe most of them were imported from England. No. 4 I am inclined to think much older than was alleged.

My object in publishing these analyses has been to show that poisonous samples of this preparation may be met with in dispensaries, without taking particular trouble to hunt them up; it remains for medical men to consider whether, under these circumstances, it would not be better to abandon its use, except in cases where they can personally vouch for its freedom from such deleterious impurity.

Falmouth, Jamaica,
June 25, 1880.

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[*] The HCl employed in these analyses, besides being otherwise a very pure sample, was free from any trace of uncombined chlorine.

PROCEEDINGS OF SOCIETIES.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

General Meeting, March 23, 1880.

J. P. Joule, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

Dr. Lloyd Roberts, of Kersal Towers, Higher Broughton, was elected an Ordinary Member of the Society.

Ordinary Meeting, March 23, 1880.

J. P. Joule, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

"*On the History of the Word 'Chemistry' or 'Chemia,'*" by Dr. R. Angus Smith, F.R.S., &c.

The author said that he had been led to a renewed consideration of the meaning of the word chemistry because of papers read lately on the subject by Prof. Schorlemmer, F.R.S., in Manchester, and Mr. Maclear, in Glasgow, a very brief abstract of the latter being seen. He had always been inclined to agree with Olaus Borrichius in his reference to Egypt for the word chemia and the chemical arts, as well as the early attempts at a science, and he was especially inclined to do so on reading of the few Greek manuscripts that have been published or described by sufficient extracts being given. The eyes of the writers were evidently much turned to the land of Chemi, whilst Isis and Osiris were favourites. For many years he had occasionally tried to interest young Greek scholars in the subject, and he had hoped that when Dr. Kopp began his description in his very learned work the "Beiträge zur Geschichte der Chemie" that the examinations would have been exhaustive; but that author was not inclined to go further at the time.

If Formicus used the word chemia in the second, third, or fourth century, and if chemistry were meant, we have a good starting-point; but Formicus was an astrologer, and he gives no definition.

The tract by "Democritus" speaks of several chemical subjects, and he is said to have treated of metals and purple dyeing, having learnt Egyptian and other Eastern wisdom. Dr. Kopp considers that he may be referred to the fourth century. No criticism of any point of Kopp's enquiries was proposed; it was intended to begin where he had ended his enquiries, and to go into earlier times.

The old belief that chemistry has something to do with the soil of Egypt was not considered just. Brugsch says that Egypt is called black in contradistinction to the Desert, which is called red in hieroglyphic inscriptions. But the soil is not really black, and even if it were so chemistry can have no direct relation to it. Still an art might receive the name of the country it came from, as Japan gives us japanning, &c. But so much came from Egypt to the West that we have little reason to suppose that such an obscure subject as chemistry would be the only one to take up the name of such an illustrious country and carry it to the West, whilst only those outside who foresaw the greatness of the science would give it a great name. The Egyptians themselves would scarcely do so. Why should they choose this as peculiarly characteristic of their country?

Dr. Angus Smith came to the idea that the word חֵמָה (Hema or Khema), meaning heat in Hebrew, was much more likely to be the origin of the name *chemia* than any hitherto fixed upon, and hoped by the later researches in Egypt and Assyria to have some light thrown upon it. Prof. Theodores, of Owens College, being asked, gave information which confirmed him in the belief, and also showed that the word was not an Arabic one. "If *Kimia* be an Arabic word the attempt to graft it on hot or black is futile," and he quotes Bochart's proposal to derive it from *Kama*, to conceal, but wisely sets such aside. To quote one of his letters again—"חמם (cognate in character to יחם) means to be hot; from it are derived the forms "Hammah, "heat," also sun, "the receptacle of heat." Another noun is Hema, "fiery anger." This latter form occurs in Deuteronomy, xxxii., 24, in connection with the names of snakes or some such animals." This word is, according to Prof. Theodores, incorrectly translated "poison." It would appear to be *heat*, as in *hot with rage*. The author did not know how far Semitic scholars would allow the next stage, which is to connect this with הָמָה, which certainly softens the first letter, giving us the meaning of *noise and excitement as by intoxicating drink*.

On enquiry of Dr. Birch how such a word could be connected with the Egyptian and Assyrian, he says, "The word for heat is *Hhamamu* or *Khamamu*." Then he gives *qam*, Egypt, or the black land, *qam* meaning in Egyptian *black*.

Having obtained the first idea, *heat*, it was impossible to avoid going thus far, but there was a difficulty, namely, in the connecting-links between Egyptian and Semitic. Still it is allowed that there is frequent connection, and this word links itself with the Hebrew both towards Egypt and Assyria. It was too early to stop here, because we have the province of Egypt, Chem, which would seem to have been the kernel of the Egyptian nationality, the nation keeping, as it expanded, the original name. The metropolitan name of Chem is Chemmis, although Thebes overshadowed it. In Brugsch's "Egypt from the Monuments"* we learn that the district was especially under the God Chem, who was the divine representative of heat as emanating from the sun and producing life in all nature—not the sun as the Glorious Apollo, but the sun, we might say in modern phrase, as an actinic agent. Chem was called Pan by Greeks and Romans from some inferior characteristics. Pan, like Chem, had his high and his undignified position. Chem was "the Lord of Coptos," to which place there was a road from the Red Sea, through the very hot district of *Hammamat* (still from heat), where stones for the temples were obtained, and where attention was paid to the discovery of gold and silver, and where Chem was held as "the Master of the tribes which inhabit the valley," and "the Lord Protector of the mountain."† Many complaints were made of the heat of this valley, where wells were dug in the time of Rameses the Second, because others much older had been closed up. It was a burnt land, and it still preserves its ancient character. It is the earliest place where we know metals to have been studied. On the west of the Nile and opposite was Tini, famous for purple dye, another branch of chemistry. Tini, or *Thinis*, or *This*, was the place where the earliest king *Menes* sprang from. In *Chem* we are in the land of Heat, where the God of Heat was worshipped, where metals were worked, and where other chemical processes were employed, and from which the first chemist, whose writings are clearly on chemical subjects, and whose drawings are clearly of chemical apparatus, sprang,—viz., Zosimus the Panopolite, or Zosimus of Chemmis, for this is the proper name of his city. The translation of Chem into Pan has been misleading, and still more of Chemmis into Panopolis.

We thus see that chemistry has received its name from no trifling accident, but from that great natural agent which has made the science, and the character of which we are continually learning.

Prof. Theodores had quoted for the author Bunsen's "Die Stellun Aegypten's in der Weltgeschichte," v. 5, s. 2, where he says, "'Hem' or 'hêm,' Sieden glühen (*i.e.*, to boil, to glow with heat) = Hebrew, ham," and the next part of the quotation must be kept in view—"h is related to S. The Egyptian Sôm has the same value, and with this the Teutonic Sommer. Summer has some connection through Sun, Sonne." This helps us to bridge over some of the ground to be seen.

Sayce's Assyrian Vocabulary was then consulted in order to see if an Assyrian or Accadian origin could be found for the word. No Accadian word of the kind was found. We are then guided to Semitic Assyrian, which has—

Samsu, the sun.
Samu, the heavens.
Samas, the sun-god.
Khamamu, and } heat.
Khammu, }
Khamsa, fire.
Khamdu, light.

The word then had full force in the East beyond Egypt.

The examination of metals, dyeing, colour-making, and distilling seem decidedly to be referable to Egypt. Distillation leads to alcohol readily, when juices of plants containing sugar are used, and still more so when sugar is added.

In Egypt there were drinks of great potency, but the fullest idea seems to have grown in this direction in Asia, where the Kh or Ch was much softened into h, and converted into S. This is done by laws not to be examined here, but received from those who have studied the tongues of the regions under view (see the quotation from Bunsen).

The study of intoxicating liquors was carried out more fully in Asia, where mysticism was most prevalent. It is

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* "A History of Egypt under the Pharaohs, derived entirely from the Monuments," by Brugsch-Bey. Murray, 1879.

† From Brugsch-Bey.

probable that the love of the spiritual, the unseen, and the immortal, was among some tribes at an early period connected with intoxicating substances. This itself is a great field of historic enquiry full of interest.

We have seen how *hama* is connected with Sôm, and if we go far East we find the great Asiatic drink Soma used by the inferior gods to strengthen them in their fight with demons. Soma is the Sanskrit word for this drink, but there are many Semitic words the same in Sanskrit, and this evidently has the same root, which is seen in Egypt moving towards metals and general chemistry: it had more force in the far East in connection with the mystic drink, although the idea may have arisen in Egypt. The vessel into which the drink flowed, the receiver, was called *kama* ("Indo-germanische Chrestomathie," by Ebel, Leskien, Joh. Schmidt, and Aug. Schleicher). In old Bactrian the word appears as *haoma*, whilst in Russian we find it returned, in all probability, as *koumiss*, to more western lands. We have yet to learn fully the medical effects of the *asclepias acida*, or moon plant, from which the Soma was made. Ordinary alcoholic liquids made by fermentation are notwithstanding much condemned in the Institutes of Menu.

The connection of the earliest thoughts of civilisation and the later is remarkably seen in the history of this word. The hope of happiness from intoxicating liquids is evidently the direction which theory took in the minds of some searchers for truth, and constant deception seems not to have cured men entirely. We have removed the intense faith in the delusion, and retained the name of the substance, *i.e.*, *aqua vitæ*, *eau de vie*, *uisge beatha*, all meaning water of life. The Germans simplified the word by calling it *burnt wine* (Brenntwein) or brandy, and we have cut down the latter and Celtic form into whiskey. This history of the theory is believed to be correct, although quite apart from the derivation of the word chemistry. The connection of chemistry with *moist* or *dry* was considered as too slight to form a basis for an opinion, and the superstition concerning a plant that made gold was simply held to have no solid place in the history. The belief that the word *kem*, in Arabic, alluded to the fifth element, or *ether*, as one of the five existences spoken of in an important part of Indian philosophy, was rejected —but this abstract is already longer than usually allowed.

"*On the Electrical Resistance and its Relation to the Tensile Strain and other Mechanical Properties of Iron and Steel Wire*," by WILLIAM H. JOHNSON, B.Sc.

It has long been known that there is a great variation in the tensile strain of various qualities of iron and steel wire, depending on the mode of manufacture and purity of the material used, and it occurred to the writer that it would be interesting to examine if this difference in quality was accompanied by a corresponding alteration in the electrical resistance.

If electricity is a mode of motion of the particles of matter, then it is only reasonable to suppose that anything which constrains this motion will increase the electric resistance, and *vice versa*. Thus an annealed wire should conduct electricity better than an *un*annealed, and a hardened and tempered steel wire, whose particles are in a state of tension, worse than a bright wire; also, from the analogy of copper wire, any increase of impurities in the iron would probably augment the electrical resistance. The following experiments will be found to confirm these views:—

A series of preliminary experiments were made, beginning with a pure iron, smelted and worked throughout with charcoal, and ending with a highly carbonised steel wire.

The apparatus used was a Wheatstone bridge, with divided metre scale and reflecting galvanometer of low resistance. All the wires except the piano steel were drawn in the same way to No. 18½, and then either hardened and tempered, or annealed and finished by drawing one hole to No. 20 = 0·038 inch. The length of wire tested electrically varied from 8 to 3 metres; care was taken to select such a length that the resistance was about 1 ohm, as then all four arms of the balance were equal. A thermometer was placed close to the wood drum on which the wire was coiled, and the temperature noted at the time of each experiment.

The circuit was first closed by pressing a button, and then connection with the galvanometer was made. By a suitable arrangement of keys the current was allowed to circulate through the wire such a short time that it could not sensibly increase its temperature.

The wire was tested for breaking-strain in an apparatus similar in principle to a steel-yard weighing machine; the weights could be added without any jar, and the elongation recorded at the moment of rupture. The length tested measured always 10 inches between the jaws of the machine.

The diameter was measured by a decimal gauge, manufactured by Elliott Bros., reading to 1-1000th inch.

The tortion tests were made by gripping a wire in two vices 8 inches apart, and turning one of them on to a suitable apparatus till the wire broke.

The results are given in Table A. Charcoal iron is here seen to have the least electrical resistance, or about one-half that of piano steel. We also notice a regular increase of resistance as the impurities augment. Charcoal iron is almost chemically pure, while dephosphorised iron, the next on the list, has some impurities which increase in ordinary iron wire. Again in the samples of steel, piano steel wire, which has the highest electrical resistance, contains two or three times as much carbon as mild steel, while steel wire samples 3 *a* and 2 *a* are intermediate in carbon as they are intermediate in electrical resistance.

Table A shows that the breaking-strain and electrical resistance increase together.

A series of eight sets of experiments were made on various specimens of charcoal iron wire to determine the effect of annealing on the electrical resistance, but without definite result, as the alteration caused by annealing appears to be so small that it is difficult to distinguish it from variations arising from change of temperature.

Annealing diminishes considerably the electrical resistance of puddled iron wire, and has a marked effect on steel, the exception in the case of sample 3 *a* being probably due to temperature.

The annealed samples were cut originally from the same piece as the bright, and the same was the case with the hardened and tempered, so they are identical in chemical composition.

As the electrical resistance of steel wire seemed from these experiments to increase very rapidly, the writer determined to investigate it more closely. A set of seven samples of cast steel wire were prepared, all of them manufactured in the same way, differing only in the amount of carbon and other impurities. These samples were drawn to No. 18½ and each coil divided, one-half being hardened and tempered, then drawn to No. 20 and tested; the other half of each coil was annealed and drawn one hole to No. 20, then tested in the bright state, and afterwards part was annealed and tested. The eighth sample was Bessemer steel, drawn and tempered in the same way as the others. The tempering was done in melted lead at a constant temperature.

The results are given in Table B.

The electrical resistances in this Table, as in A, are the mean of two or more experiments differing very little amongst themselves: the method of experiment and apparatus was the same as used before.

The figures denoting breaking-strain, elongation, and torsion, as in Table A, are the mean of two experiments in each case. These two experiments gave results sometimes the same, at others differing 4 to 6 per cent in the annealed samples, 3 per cent in the hardened and tempered, and 4 per cent in the bright wires, except in sample No. 6, where a difference of 20 per cent was observed.

TABLE A.

Electrical and Mechanical Tests of Various Qualities of Iron and Steel Wires.

Quality.	Sample No.	Length.	Weight.	Resistance 1 Metre Gramme.	Temperature of Experiment.	Breaking-Strain per Sq. Inch.	Twists in Eight Inches.	Elongation per Cent.	Diameter
		M'ts.	Grms.	Ohms.	Degrees.	Lbs.			Inch.
Annealed Charcoal Iron	6*a*	8	47·403	0·7875	13·0	50,280	138·5	27·75	0·039
Bright ditto	7*a*	8	47·391	0·7852	13·5	105,840	57·0	2	0·038
Dephosphorised Iron, Annealed	—	5	28·000	0·8908	10·0	63,504	189·0	23	0·038
Annealed Iron	9*a*	6	35·040	1·0123	13·0	66,740	134·5	25·5	0·038
Bright ditto	8*a*	6	33·231	1·0696	11·5	112,896	49·5	0·75	0·038
Mild Steel, Annealed ..	5*a*	6	32·890	1·1140	12·5	70,268	207·0	19	0·038
Ditto do. Bright ..	4*a*	6	33·291	1·1270	15·5	98,784	46·0	1·25	0·038
Annealed Steel	3*a*	8	46·286	1·2520	15·0	95,256	149·0	16·25	0·038
Bright ditto	2*a*	3·50	20·081	1·2470	14·5	132,600	69·0	1·5	0·038
Hardened and Tempered ditto	1*a*	5·20	29·500	1·2832	13·5	134·244	96·0	1·5	0·038
Annealed Piano Steel ..	—	4	28 930	1·3422	10·25	127,840	152·0	9·5	0·043
Hardened and Tempered Steel (Piano) ..	—	5	35·230	1·4497	11·25	287,540	75·0	2·62	0·043

TABLE B.

Quality.	Sample No.	Electrical Resistance per Metre Gramme.						Breaking-Strain in lbs. per Square Inch.			Twists in 8 inches.			Elongation at Moment of Fracture.		
		Annealed.		Bright.		Hardened and Tempered.		Annealed.	Bright.	Hardened and Tempered.	Annealed.	Bright.	Hardened and Tempd.	Annealed.	Bright.	Hardened and Tempd.
		Ohms.	T.	Ohms.	T.	Ohms.	T.									
Cast Steel	1	2·140	13°	2·157	13½°	2·225	13½°	109,335	166,785	208,330	63·0	16·0	23·0	19·50	1·50	1·75
,,	2	1·903	11¼°	1·913	11½°	1·980	11¼°	105,810	155,140	204,780	67 0	19·0	19·0	17·87	2·00	1·87
,,	3	1·560	10¾°	1·575	11°	1·641	10°	96,075	137,695	169,270	64·0	44·5	63·0	19·25	1·50	1·75
,,	4	1·519	11¾°	1·526	12°	1·597	12°	91,701	132,305	170,160	89·0	59·5	47·0	17·25	1·62	1·75
,,	5	1·450	11½°	1·465	12°	1·526	12°	90,493	135,785	167,015	95·0	77 0	64·5	18·50	1·75	1·75
,,	6	1·430	8¾°	1·444	10¾°	1·515	10½°	89,937	123,590	166,515	81·5	105·0	63·5	19·50	1·75	1·87
,,	7	1·417	9½°	1·437	11°	1·502	11°	88,174	130,549	158,110	76·5	37·0	49·5	19·12	1 87	1·50
,,	8	1·070	9½°	1·098	11°	1·158	10¼°	79,718	135,790	158,110	140·0	43·0	32·0	19·50	1·00	2·00
Total ..		12·489		12·615		13·144		751,243	1,117,539	1,402,290	676·0	40·1	361·5	150·49	13·79	14·24
Average..		1·561		1·577		1·643		93,905	139,692	175,286	84·5	50·1	45·2	18·81	1·72	1·78
Per cent..		100		101·03		105·25		100	148·76	186·66	187·0	110·93	100			

This probably accounts for the wide divergence of this sample.

These variations may seem large, but it must be remembered that there is an essential difference between an observation on electrical resistance and one on tensile strain. If we suppose the length L of wire under experiment divided into a great number of extremely short pieces, l_1, l_2, l_3, &c., each one would have a slightly different breaking-strain, b_1, b_2, b_3, &c., and electrical resistance, r_1, r_2, r_3, &c., depending on the physical structure of each length, l_1, l_2, l_3, &c. Now the observed electrical resistance R of the wire L is the sum of all resistances of the parts l_1, l_2, l_3, &c., or $R = \Sigma(r)$, while the observed breaking-strain is not the *mean*, but the *minimum* breaking-strain of all the pieces l_1, l_2, l_3, &c. It is an experimental fact, that the longer the piece of wire under experiment, the lower will be the breaking-strain.

Some of the results of the experiments are given in Table B. Here we see again that the electrical resistance and breaking-strain increase together in a pretty regular way, the variations observed probably being due partly to differences in temperature, but chiefly to the fact just explained, that in the electrical experiments we register a *mean*, while in the experiments on tensile strain we register a *minimum*, observation. This law must be applied with due care, and only where the samples have been drawn in the same way.

The mode of manufacture of the eight samples of steel leads one to believe that there is a regular increase of carbon and other impurities from No. 8 to No. 1. The writer hopes shortly to analyse these wires, as the resistance of some of the samples is most remarkable, being no less than three times that of pure iron.

No attempt has been made to reduce the electric observations by calculation to some fixed temperature, as it seemed improbable that samples with such varied resistances would increase in the same proportion as pure iron.

Table B shows us that annealing diminishes the electrical resistance of bright steel wire some 1 per cent, while hardening and tempering increases the resistance of annealed steel about 5 per cent. Now we may observe that the greatest increase of electrical resistance, viz., 8·1 per cent, and of breaking-strain resulting from hardening and tempering, is found in piano steel, thus clearly showing that any process like wire-drawing or tempering, which makes the particles of steel more rigid and difficult to separate, increases the electrical resistance.

The torsion tests vary a good deal among themselves, but it is interesting to observe, from Table B, that the mean breaking-strain of hardened and tempered steel is to that of the same steel annealed in the inverse ratio of the number of twists in 8 inches. The electrical resistance of the samples of steel is seen from Table B to be usually inversely proportional to the number of twists in 8 inches.

It is generally allowed that the heat conductivity of metals is nearly proportionate to their electric conductivity. If this relation between the thermal and electric conductivity holds good for different qualities of iron and steel, then the electric resistance is an important factor of all iron and steel plates used in the construction of boilers, fire-boxes, &c. It is important that the plates exposed to the flame should conduct heat well, and one of the advantages claimed for steel over iron is that, as a thinner plate can be used, it allows more heat to pass than the thicker iron one.

Thus a ½-inch steel plate can replace a ⅝-inch iron plate; but our experiments lead us to believe that, as steel conducts electricity worse than iron, the thin steel plate may, after all, let less heat through in a given time than a thick iron one.

Suppose the iron plate has a resistance of 0·90 ohm per metre gramme, and the steel plate a resistance of 1·11 ohms, then a ⅝-inch iron plate will allow as much heat to pass per unit of area as a ½-inch steel plate. If the steel plate be more highly carbonised—say of Siemens-Martin steel—then the ½-inch steel plate will allow no more heat to pass per unit of area than an iron plate 3-16ths inch thicker. There is not space here to discuss this subject fully, but there is no doubt of its importance, and it is probable the time will soon come when our leading boiler-makers will electrically test their steel plates, perhaps by the induction balance, just as they now test them for ductility and tensile strain.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

March 15, 1880.

C. **Bailey**, F.L.S., President of the Section, in the Chair.

Dr. **Tatham**, of Salford, described a new form of Stephenson's Binocular, which has been recently introduced by Messrs. Baker, of Holborn.

The microscope, being mainly intended as a working instrument, is of firm, rather than of elegant construction; the stage is remarkably large and substantial, and is so firmly mounted that it affords efficient support for the hands during the manipulation of an object under the microscope. The stand does not possess the usual axial joint; but inasmuch as that portion of the tubing which carries the eye-pieces is bent at almost a right angle with the perpendicular portion of the body which carries the objective, the axial inclination of the stand is unnecessary. The binocular prism is remarkably well ground and set, and under it objects of considerable thickness, such as injected specimens, stand out in relief, much in the same way as they appear under the Wenham's binocular. The instrument is capable of being used binocularly with high, equally well as with low powers; the prism acts as an erector, and when used in combination with a "combined two- and four-inch objective," recently invented by Zeiss, it forms a most useful dissecting microscope.

Dr. **Tatham** bore testimony to the great comfort he has experienced in the use of this form of binocular, and augured for it an extensive sale, more especially as the price of the binocular, stand complete, does not exceed seven or eight pounds.

CORRESPONDENCE.

ERRATUM.—DIAZO-COLOURS.

To the Editor of the Chemical News.

Sir,—Will you kindly insert an *erratum* in your next issue to the effect that the signature to my letter on "Diazo-Colours" is misprinted "R. Meldoda" instead of

R. Meldola.

August 3, 1880.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Note.—All degrees of temperature are Centigrade, unless otherwise expressed.

Bulletin de la Société Chimique de Paris,
No. 7, April 5, 1880.

Decomposition of Oxygenated Water in Presence of Alkalies, and the Derivatives of Barium Peroxide.—M. Berthelot.—Already noticed.

Analysis of the Waters of Bourboule.—E. Willm.—Already noticed.

Russian Chemical Society. Session, April 17/5, 1879.—MM. Jawein and Beilstein communicated a paper on the electrolytic determination of cadmium.

M. N. Lubavine described his researches on the nuclein of milk (see *Bulletin*, xxix., p. 213). The author considers that the phosphorus found in nuclein and casein does not occur in the state of a phosphate.

M. Flavitzky stated that lævo-rotatory terebenthen yields hydrated terebenthen under the influence not merely of nitric, but of sulphuric and hydrochloric acid.

M. P. Latschinoff pointed out the discrepancies between his results and those of M. Tappeiner on the oxidation of cholalic acid by means of permanganate and sulphuric acid.

The 5th part of Vol. xi. of the *Journal of the Russian Chemical Society* contains a memoir by M. Rudneff on the amines with tertiary radicles.

No. 8, April 20, 1880.

Action of Bromine upon Diphenyl-methane.—C. Friedel and M. Balsohn.—The authors have obtained bi-bromated and mono-bromated diphenyl-methane, the latter of which, if left for some days in contact with water at common temperatures, is completely transformed into benzhydrol.

Constitution of the Salts of Rosaniline and of Analogous Colouring-matters.—A. Rosenstiehl.—In this first memoir the author criticises the views of former writers, especially of MM. Emil and Otto Fischer.

Continuous Method of Preparing Acetic Ether.—J. A. Pabst.—The author introduces into a retort a cold mixture of 50 c.c. of sulphuric acid and the same measure of alcohol, and as soon as the mixture has arrived at the temperature of 140°, he allows to flow slowly into the retort a mixture of equal equivalents of alcohol and of glacial acetic acid. At first a little sulphuric acid distils over, and then there passes a liquid containing 85 per cent of acetic ether.

The Fatty Acids of Butter.—E. Wein.—According to the author butter contains the following fixed acids:—Palmitic, oleic, stearic, myristic, and arachitic, the two latter in small quantity; and the following volatile acids: normal caprylic, capric, normal caproic and butyric. Acetic and formic acids have also been found, but the propionic, valeric, œnanthylic, and pelargonic acids could not be detected.—*Sitzungsber. d. Phys. Med. Soc. Erlangen*, xi., p. 1644.

Moniteur Scientifique, Quesneville.
March, 1880.

Determination of Specific Heats at a Constant Volume in Simple and Compound Bodies.—G. Quesneville.—The author maintains that the method indicated by Clausius leads to results inaccurate, even for simple bodies, and much more so for complex compounds. He has therefore adopted another process, based on the knowledge of the constitution of bodies, which chemical syntheses enable us to foresee. He shows that the combinations of the non-metals among themselves, with the exception of hydrogen, are effected without the loss of atomic heat. Those in which there occur more than two atoms of hydrogen are always attended with a loss of atomic heat, depending on the number of the atoms of this element, which take part in the formation of compounds capable of acting as true radicles. By introducing the single radicle CH_2, he calculates in all cases the specific heats of compounds, and shows that the numbers thus obtained agree with those given by observation, whilst on introducing the usual radicles of the atomists the numbers are always different.

Electricity in the Service of Life.—A summary of the modern applications of electricity, translated from a lecture given by Dr. Werner Siemens, delivered at a General Meeting of the German Congress of Naturalists and Physicians, held at Baden-Baden in October last.

Radiant Matter.—E. Ricklin.—An account of the recent investigations of Mr. Crookes.

History of Benzoyl Green and Malachite Green.—Otto Fischer.—From the *Berichte der Deutschen Chem. Gesellschaft.*

On Ultramarine.—Dr. Knapp.—An abstract from *Dingler's Journal.*

Beer, the Queen of Alcoholic Drinks.—M. Dubrunfaut.—This paper can scarcely be regarded as of a scientific nature.

Sessions of the Chemical Committee of the Industrial Society of Mulhouse.—M. E. Weiss ascribes the corrosion of steam boilers to the action of ozone, which he supposes to be generated by electric action, the result of steam violently impelled against the upper portion of the boiler.

The Process for the Manufacture of Sugar.—An account of a process patented by L. Faucheux.

Remarks on the Analysis of Mixed Tissues of Silk and Cotton, Wool and Cotton, &c.—J. Persoz.—The author recommends the following process:—Supposing a tissue is of silk and cotton, he takes three swatches, Nos. 1, 2, and 3, of arbitrary sizes, and weighs them at the same instant. Let their respective weights be a, b, c. No. 1 is preserved in its original state. The dye is removed as completely as possible by means of acid baths, and No. 2 after being pressed is set acid to be dried. No. 3, whilst still moist is treated with caustic soda or chloride of zinc, and the residue (cotton) is dried in the air along with Nos. 1 and 2. After the lapse of several hours two successive concordant weighings show that the fibres are in hygrometric equilibrium with the air of the laboratory, when the new weights, α, β, γ, of the swatches Nos. 1 and 2 and of the residue No. 3, are noted. These six data, a, b, c, α, β, γ, serve to show the results of the analysis. If we knew the successive weights of the swatch 3, first in its untouched state, then when stripped of dye, and lastly when reduced to a residue of cotton, all in the same atmospheric conditions, we should have all the elements necessary for the solution of the problem. But they are easy to calculate. Let us call these weights, supposed to be obtained at the end of the experiment, x, y, z. Then $x=c.\frac{\alpha}{a}$, the original swatch; $y=c.\frac{\beta}{b}$, the swatch stripped of dye, and $z=\gamma$, the residue of cotton. Hence we find the percentages of dye and dressing, TA, and of cotton, C.

$$TA=100.\frac{\alpha.b-a.\beta}{\alpha.b}.$$

$$C=100.\frac{a.\gamma}{\alpha.c}.$$

The proportion of silk S is found as the difference. This method of operating renders it possible to take account of the variation which may take place in the hygrometric condition of the air during the interval between the first weighing and the last. If this variation $=0$, then $\alpha=a$, and the above expressions are reduced to the form—

$$TA=100.\frac{b-\beta}{b}, \text{ and } C=100.\frac{\gamma}{c}.$$

So-called Spontaneous Decomposition of Hydrocyanic Acid.—Perfectly pure hydrocyanic acid may be preserved for an indefinite time without decomposition. The decomposition in question is due to the presence of a trace of ammonic hydrocyanate or of an alkaline cyanide.

New Process for the Preparation of Nitrous Ethers.—M. Armand Bertrand.—This process depends on the action of aqueous hydrochloric acid upon sodium nitrite in presence of an alcohol.

Verhandlungen des Vereins zur Beforderung des Gewerbfleisses. No. 3, March, 1880.

This number contains no chemical matter.

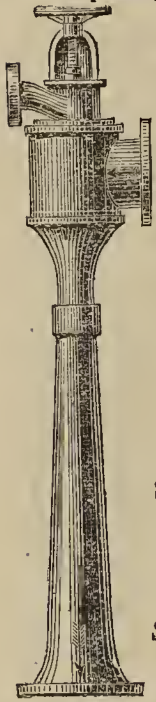
NOTES AND QUERIES.

Vanadium Ink.—Can any correspondent tell me where I can buy vanadium ink?—J. C.

KÖRTING BROTHERS,

ENGINEERS,
17, LANCASTER AVENUE, FENNEL STREET.
MANCHESTER.

Air Compresser.



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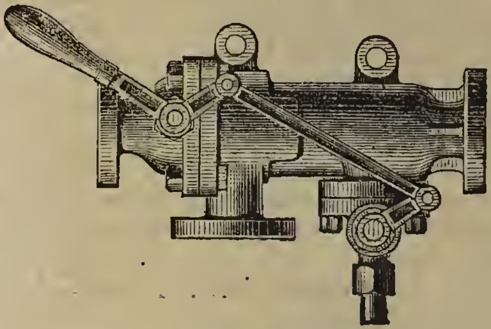
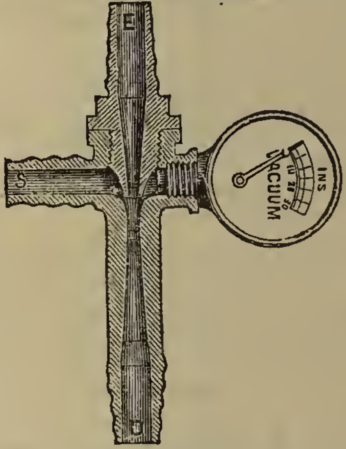


Water-jet Air-pump.



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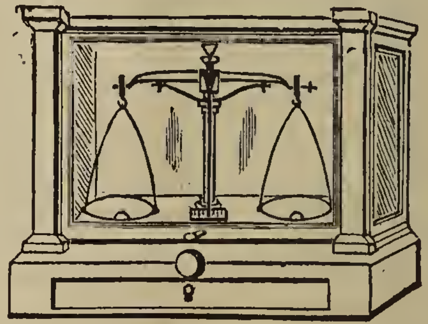

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THE CHEMICAL NEWS

Vol. XLII. No. 1081.

NOTE ON SOME DOUBLE SALTS OF MECONIC ACID.

By EDWARD H. RENNIE, B.Sc. (Lond.),
Demonstrator of Chemistry in St. Mary's Hospital Medical School.

In testing on one occasion for meconic acid, it was noticed, that if to a solution of that acid, chloride of calcium is first added in considerable quantity and then a small quantity of ferric chloride, a red precipitate is produced, and so long as the ferric chloride is not in excess, the supernatant fluid remains colourless. If, however, the ferric salt is added in sufficient quantity, the precipitate first formed re-dissolves on standing, producing the well known blood-red colour. If, instead of adding the chloride of calcium *first*, the meconic acid solution be mixed with some ferric chloride and *then* the calcium salt added, no precipitate is produced at first, but (unless the ferric salt be in considerable excess) a slight precipitate is deposited after a time. As these appearances seemed to point to the formation of a double salt, it was thought worth while to examine the matter a litte further.

The meconic acid used was re-crystallised twice, and gave on combustion numbers which shewed it to be practically pure. A quantity of the red precipitate mentioned above was prepared, and after thorough washing, heated to 100° for some time. Further experiment showed, however, that this temperature was insufficient to drive off all the water. It was then found, that up to 140° the substance gave off water only, that is, up to that temperature the loss of weight of the substance and the gain of weight of a chloride of calcium tube used to collect the water were practically equal, but that when the temperature was raised to 200°, the chloride of calcium tube ceased to gain in weight at anything like the same rate as the substance continued to lose weight. Three separate specimens of the substance were therefore prepared, dried at 140°, and analysed, with the following results, the numbers representing percentages:—

	A.	B.		C.
Carbon	—	29·11		29·45
Hydrogen ..	—	1·66		1·45
Iron	10·21	10·49	10·48	10·32
Calcium	10·69	10·58	10·77	10·91

These numbers appear to represent a substance of fairly constant composition, but it was not found possible to derive from them a very probable formula. The nearest formula appears to be $(C_7HO_7)_4Fe_2Ca_3.5H_2O$, which requires 30·37 per cent C, 1·27 per cent H, 10·09 per cent Fe, 10·81 per cent Ca.

Similar compounds are produced when barium or strontium chloride is substituted for the calcium salt. Two different specimens of the barium compound, also dried at 140°, gave the following percentages, which, however, do not agree nearly so well as the above:—

	A.	B.
Iron	9·63	8·93
Barium	28·80	29·93

These compounds are slightly soluble in cold, more so in boiling water, and separate out from the latter on cooling in an amorphous condition. Their solutions give no precipitate with ammonia and no colouration with potassic ferrocyanide until after the addition of an acid. They give no blue colouration with potassic ferricyanide after the addition of an acid, but only the brownish tint indicative of a ferric salt. Their solutions when treated with sulphuric acid and ether yield to the latter an acid which strikes a blood-red colour with ferric chloride.

ON THE ELECTROLYTIC DETERMINATION OF NICKEL AND COBALT.

By H FRESENIUS and F. BERGMANN.

According to former experimentalists, nickel and cobalt are not precipitated by the electric current from solutions containing free mineral acids, but both metals can be entirely thrown down from ammoniacal solutions and from the solutions of their cyanides in potassium cyanide. According to Luckow nickel and cobalt can be completely precipitated from solutions of their neutral sulphates mixed with alkaline acetates, tartrates, and citrates. Luckow seems to have used this method for the quantitative determination of the above metals, but no particulars are found in his memoir.

All other papers on the quantitative determination of nickel and cobalt by electrolysis refer to precipitation from an ammoniacal solution, which, according to previous experience, yields the metals in the state best adapted for weighing. For the success of the operation it is essential that the solution contains a sufficient excess of free ammonia, since the liquid must remain strongly ammoniacal during the whole course of the experiment. The precipitation is promoted by the presence of ammonium sulphate. Sodium phosphate has, according to M. S. Cheney and E. S. Richards, a similar effect. On the other hand, ammonium chloride retards or hinders the precipitation. The strength of the current and the respective distance of the electrodes are also of importance. The authors used a Clermond's thermo-electric battery of the construction described in *Zeitschrift f. Anal. Chemie*, xv., p. 333, made by Koch, of Eisleben. It consists of 120 elements, and was provided with the porcelain burner described and figured in *Zeitschrift Anal. Chemie*, xv., p. 334. The current produced by the apparatus evolves 300 c.c. detonating gas per hour from water acidulated with sulphuric acid. The platinum electrodes were of the form and size recommended by the Management of the Mansfeld Mines and Smelting Works. (See *Zeitschrift f. Anal. Chemie*, xi., p. 6.)

The electrolysis was performed upon 200 c.c. of liquid, contained in a beaker, covered with a watch-glass, having suitable apertures for the polar wires. The withdrawal of the liquid and the washing after the completion of the electrolysis took place without interrupting the current. The removal of the liquid was effected by means of a water air-pump; the authors using a flask closed with a stopper having two apertures. Through one of these passes a glass tube, not too wide, which projects a little into the flask, and which outside is bent twice at right angles, so that the free descending limb can be introduced into the beaker used for the electrolysis in such a manner that its end reaches nearly to the bottom of the beaker. By the aid of this arrangement, and with a proper regulation of the air-pump, the solution and the washing-water can be sucked out of the beaker into the flask regularly and not too rapidly. The beaker and the electrodes were easily and safely rinsed with the washing-bottle. Not until all this was completed the polar clamps were opened and the electrodes removed from the beaker. The cone (negative pole) with the metallic deposit to be weighed was dried upon a hot iron plate, let cool, and weighed. Rinsing with alcohol before drying, as occasionally recommended, was found superfluous.

In some preliminary experiments the authors confirmed the statements of their predecessors as to the precipitation of nickel and cobalt from ammoniacal solutions, especially as to the beneficial effect of ammonium sulphate and the injurious effects of ammonium chloride. It appeared, further, that quantities of 0·1 to 0·15 cobalt or nickel were completely precipitated by a current of the above strength in from five to six hours.

The nickel solution was prepared by dissolving chemically pure crystalline nickel sulphate in water with the addition of a little dilute sulphuric acid, and diluted to a litre. In 50 c.c. of the solution the nickel was determined by precipitation, and weighed after ignition in a current of hydrogen. The quantity of nickel thus found was 0·1233 grm. = 2·4660 grms. per litre.

For preparing the cobalt solution metallic cobalt was employed, prepared by igniting chemically pure cobaltous oxalate. A suitable quantity was heated with water and nitric acid, the nitric acid removed by evaporation, diluted with water, filtered, and the filtrate made up to 1 litre. In 50 c.c. of this solution the cobalt was determined by precipitation as cobaltous oxide, and weighed as metallic cobalt. The result was 0·1283 grm., corresponding to 2·5660 grms. per litre.

A solution of ammonium sulphate was also employed containing 305 grms. per litre of the anhydrous salt, and liquid ammonia, of sp. gr. 0·96, that is containing 10 per cent NH_3. As the nickel was precipitated by the current the ammoniacal solution, which was at first blue, became paler, and finally colourless. This decolouration does not afford a certain indication of the conclusion of the operation, and it is requisite to ascertain by one of the known tests if nickel is still present in solution. The authors made use of a solution of potassium sulphocarbonate. When this reagent produces in the liquid merely an exceedingly faint rose tint the precipitation is complete. This colouration appears even when the quantity of nickel present has been precipitated to decimilligrammes. In the most successful experiments which gave a result differing from that obtained gravimetrically merely by 0·0005 grm., there were employed 50 c.c. of the ammonia solution, 100 c.c. ammonia, 50 c.c. ammonium sulphate, and 300 c.c. water. The electrodes were at the distance of 1 to 1½ c.m. from each other. Results nearly equal were obtained with 50 c.c. nickel solution, 40 to 50 ammonia, 10 to 20 c.c. ammonium sulphate, and water 100 c.c., the electrodes being distant from each other only ½ to ⅓ c.m.

In the experiments with cobalt the same strength of current was employed, and the conclusion of the process was ascertained by means of potassium sulphocarbonate, which produces a vinous yellow as long as the slightest traces of cobalt are present.

Ammoniacal solutions of cobalt are known to take up oxygen on exposure to the air, and become brownish red in consequence of the formation of roseo-cobaltic compounds. This phenomenon takes place with the solutions undergoing electrolysis. The original red colour becomes a light reddish brown, which darkens for a time until decolouration ensues, when the cobalt is precipitated. The authors' experiments show that cobalt is most perfectly precipitated if 50 c.c. of cobalt solution are taken along with 50 c.c. ammonia, 10 c.c. ammonium sulphate, and 80 c.c. water. The electrodes were at the respective distances of ½ to ⅓ c.m.

The presence of sodium phosphate does not interfere with the accuracy of the result, but it was not found to exert any favourable action, at least as far as cobalt is concerned.—*Zeitschrift für Analyt. Chemie.*

ON THE

ELETROLYTIC DETERMINATION OF SILVER.

By H. FRESENIUS and F. BERGMANN.

It is generally known that silver can be precipitated in a compact metallic state from the solutions of silver cyanide or chloride in potassium cyanide by means of the electric current.

As far back as 1865 Luckow demonstrated that silver may be quantitatively determined in this manner. He pointed out at the same time that electrolysis may be made available in other manners for the quantitative determination of silver; either—

(1.) By the reduction of silver chloride at the negative pole, or—

(2.) By the separation of the silver from a neutral solution of silver nitrate.

Concerning the latter process Luckow states:—"If the current from two Meidinger elements is conducted through a neutral dilute solution of silver nitrate, metallic silver in a spongy state is deposited on the platinum capsule which forms the negative pole, whilst at the same time the edge and the lower surface of the platinum disc forming the positive pole is covered with fine black needles of silver peroxide, which, however, disappear almost entirely on prolonged action of the current. If, when all the silver has been deposited, the supernatant liquid is decanted off, the separated metal is repeatedly washed with water, dried sharply, and the capsule weighed; the increase of weight gives the proportion of silver in the liquid a little lower than the reality. The loss is due to the fact that a small quantity of silver is deposited on the disc of the positive pole owing to the reduction of the peroxide."

In a more recent memoir on the application of the electric current in analytical chemistry (*Zeitschrift Anal. Chemie*, xix . p. 15), Luckow states:—

"Silver is precipitated by the electric current from solutions containing not more than 8 to 10 per cent of free nitric acid, in a very bulky metallic state; at the same time a little peroxide is deposited at the positive pole, the formation of which may be prevented by an addition of glycerine, milk-sugar, or tartaric acid." No further information has been published on the electrolytic separation of silver from nitric solutions.

In accordance with Luckow the authors observe that silver can be easily and completely precipitated from nitric solutions, whether neutral or containing free acid, but that it is disposed to take a spongy or flocculent form, so that it easily falls off from the electrode and cannot be readily weighed. The precipitate assumes this spongy state, especially when it has been deposited from a somewhat concentrated solution, by the action of a moderately strong current. By using dilute solutions and a weak current the authors have succeeded in throwing down the silver in a compact state, adhering firmly to the electrode and capable of being readily weighed. This result was only obtained in presence of free acid. From neutral solutions even a feeble current precipitated the silver in a flocculent state. The authors' experiments were conducted with the same apparatus described in their memoir on the determination of nickel and cobalt.

The following proportions appear suitable for obtaining the deposit of metallic silver in a compact form:—In 200 c.c. of liquid submitted to electrolysis there should be from 0 03 to 0·04 grm. metallic silver and 3 to 6 grms. free nitric acid, the electrodes being at a distance from each other of 1 c m., and the strength of the current such as to evolve 100 to 150 c.c. detonating gas per hour.—*Zeitschrift für Analytische Chemie.*

ON THE

PROBABLE OCCURRENCE OF NORWEGIUM IN AMERICAN LEAD.

By GEO. A. PROCHAZKA.

C. W. Blomstrand reports (*Ber. d. Deut. Chem. Gesell.*, xii., 1731) on the discovery of norwegium by Tellef Dahll, substantially as follows:—

"On a small island Oterö, in the neighbourhood of Kragerö, where Mr. Dahll resides as mining director, towards the end of the year 1878, a calcite gangue, interspersed with small particles of copper, nickel, and nickel glance, was discovered. The nickel glance, on analysis, gave nickel, cobalt, iron, arsenic, and sulphur. The hydrogen sulphide precipitate was treated with ammonium sulphide to remove the arsenic; the black sulphide of a new metal remained undissolved. 10 kilogrms. of the ore were then roasted; the residue was dissolved in acid and precipitated by H_2S. The precipitate was dried and again roasted; the oxide thus produced was reduced to metal.

Further investigation proved that the metal contained copper. To remove the copper the metals were converted into chlorides, and the solution diluted with a large excess of water. The new metal was thereby precipitated as oxychloride, while copper remained in solution. The new metal in its properties bears very close resemblance to bismuth.

	Bismuth.	Norwegium.
Melting-point	264°	254°
Atomic weight (R_2O_3)	210	218·9
Specific gravity	9·8	9·441

The oxides of both metals are fusible, and give in the flame, on porcelain, a metallic mirror. The main difference between the two metals consists in the solubility of the oxyhydrate of the new metal in caustic alkali, as well as in ammonium and sodium carbonate solution, on boiling. The difference is well marked if carbonates are treated in the same manner.

In the course of an examination of American refined lead, containing comparatively large quantities of copper and nickel, the bismuth had been precipitated in the ordinary manner by an excess of sodium carbonate, from the potassium cyanide solution of the 5th group of metals (Fresenius). In the alkaline filtrate, H_2S produced a reddish brown precipitate, which could not be identified as the sulphide of any of the well known heavy metals. It was readily soluble in dilute nitric acid. From this solution a basic salt was precipitated by water. Potassium carbonate produces a precipitate which is soluble in an excess of the boiling precipitant. Hydrogen sulphide precipitates the original sulphide from the alkaline solution.

The quantity of sulphide was very small, amounting to about 1 m.grm. from 200 grms. of lead.

The reactions, as far as they could be verified, considering the smallness of the quantity of material, correspond to the description of norwegium as given by Dahll.

The refined lead was a mixture of leads from various sources, but it is not improbable that there may be a connection between the presence of copper, nickel, and norwegium. Proper attention given to the various lead ores, especially those which yield lead containing larger quantities of copper and nickel, will undoubtedly throw further light upon this subject.

The results obtained seemed of sufficient interest for publication for the benefit of those who may be more favourably situated, both with regard to time and material, than I am at present.—*Journal of the American Chemical Society.*

CONTRIBUTIONS TO THE CHEMISTRY OF BAST FIBRES.[*]

By E. J. BEVAN and C. F. CROSS.

Before proceeding to lay before this Society an account of the results which Mr. Bevan and myself (the paper was read by Mr. Cross) have obtained as the reward of two years' work in a comparatively new field of chemical research, we will devote a few minutes to the consideration of the more general aspects of our subject matter.

Casual observation reveals, in the larger number of the commoner plants, the existence of a fibrous layer, of greater or less tenacity, situated towards the cortical portion of the stem; the fibres themselves composing this tissue descend vertically and rectilinearly, forming thin concentric plates, and to this circumstance the tissue owes its name of liber. The symmetrical distribution of these fibres is especially marked in the stems of certain dicotyledonous plants, the bast of which constitutes the basis of important textile industries. Flax, hemp, jute, China grass, are the isolated liber of the most characteristic of such plants. The mode of separation adopted varies in each case, according to the degree of adhesion between the liber and the cortical elements on the one hand, and the true wood on the other. One of the simplest, and that of the greatest antiquity, having been employed and brought to considerable perfection by the Chinese and Japanese long before our civilisation had dawned, is the manipulation of the paper mulberry (*Broussonetia papyrifera*), for the production of the paper peculiar to these nations. The stems of five years' growth are cut down and boiled for twenty-four hours in water, the cortex and bast are then easily stripped together from the wood. By further soaking in water for twenty-four hours, and drawing the strips through a scraper, the cortical cellular tissue is easily removed. Subsequent boiling in an alkaline liquid, obtained by lixiviating wood-ashes, and washing in a running stream brings about the removal of the last traces of cellular tissue and the complete isolation of the bast fibres. These are easily felted by striking them with wooden mallets; lastly, a natural size obtained by boiling the root of the *Hibiscus manihot* is applied, and, after drying, the remarkable paper which is so extensively used in Japan is obtained. An equally simple manipulation, for the purpose of a more curious application of the bast which is thereby isolated, is extensively practised by the South Sea Islanders; the bread fruit tree (*Atrocarpus incisa*) similarly treated, readily yields its bast. This is worked, by the simple process of felting, into a cloth capable of use for clothing and other purposes. Perhaps the most curious and rudimentary of these simple processes is that which is applied to the *Antiaris saccidora*, a tree which is common in the jungle of several districts of the Indian peninsula. A stem or branch is cut, soaked for a short time in water, and beaten with clubs. The liber is then stripped as a perfect cylinder to within a few inches of one extremity, the wood is sawn through at this point, and the bast restored to its original position, now forming, together with the disk of wood, a perfect sack, which is used for carrying rice and for other similar purposes. The isolation of the textile fibres previously mentioned involves a more complicated process, which moreover requires to be varied to suit the requirements of each case. A brief description of the method employed in the preparation of the jute fibre will enable us to bring out the general principles upon which other similar though more complicated processes are based. Jute, in common with the other bast fibres which are applied to textile purposes, is obtained from a dicotyledonous annual. The corchorus species which yield it are cultivated chiefly in India by the Ryots, or native tenant farmers. The stems are slender and bear leaves, but are seldom branched except at the summit. The sowing takes place usually in the rainy season (March-April), the harvest about August, when the flowering is over, the stems having attained a height of 10 to 12 feet, and a thickness of 7 or 8 inches. Having been cut down close to the ground, and freed from leaves and seed-pods, the stems are tied into loose bundles, weighted with stones, and submerged in stagnant or slowly running water. After the lapse of five to ten days the "retting" is complete: the bast and cortical tissue are stripped together from the wood, and separated from one another by a rapid to and fro motion beneath the water. The bast, thus freed from cellular elements, is dried in the sun, made into bales, and in this form exported. From the specimen on the table you will see that in the aggregate condition the fibre is about 7 feet long, and of a colour varying from a dark brown (towards the root as well as throughout the fibre in the lower qualities) to silver-grey. The fibres are matted together by a resinous constituent, and in order to fit them for the operations of combing they are subjected to the preliminary treatment of "batching." This consists in moistening the fibre with a mixture of oil (whale oil together with petroleum) and water, applied separately and carefully distributed; the solvent action of

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[*] Read before the Owens College Chemical Society, April 16, 1880.

this mixture on the resinous matters being aided by passing the moistened fibre through a long series of pairs of heavy fluted rollers (" breaking " or " crushing "), or, in the case of finer qualities, stacking it and allowing it to remain for two or three days. The fibre is then ready to undergo the usual textile processes of combing, carding, spinning, and weaving. These are explained sufficiently for our purpose by the specimens on the table. The processes adopted in the case of other fibres differ from the above in detail only ; we shall therefore pass over them to the consideration of the constitution of these interesting plant elements, keeping jute before us as the type, a selection which we shall find justified by many subsequent considerations. Examining with the microscope a preparation of the raw fibre, previously boiled in water and mounted in glycerine, we find that we have a number of cylindrical threads, showing parallel, longitudinal, and transverse markings ; further, that adhering to these threads, which we shall presently see are bundles of the ultimate fibres, are a number of yellowish, resinous masses. Taking now a fresh portion similarly mounted after boiling in strong alcohol, we observe no change as regards the fibres, but the masses which we described as resinous appear now as colourless parenchymatous tissue. The alcohol has removed the resin, which, adhering previously to the fibres on the one hand and parenchyma on the other, prevented the complete separation of the latter in the original retting process. In this observation we have the *rationale* of the batching process before described. This resinous parenchyma we may dismiss therefore as a foreign body.

To isolate the pure bast we have always adopted the plan of boiling for a few minutes with dilute ammonia, which also dissolves the resin, the fibre sustaining a loss of 1 per cent in weight. Taking the fibre thus treated, mounted after teasing, we shall find that the bundles are now disintegrated, and the structure of the ultimate " fibrils " is apparent. They are cylindrical for the most part, tapering to a fine point at either extremity (this is discovered by tracing the fibres to their termination). The central lumen is well defined ; in some cases its diameter is four-fifths of that of the whole fibre, and its outline regular. In other cases we find that this regularity gives place to an indented outline, in consequence of irregular thickening of the cell walls, the lumen being often obliterated by the cylinder becoming solid at intervals. This feature is more prevalent in the fibres of the early growths (root ends), but is often observable in the later. Variations from limit to limit are not infrequently met with in the contiguous fibres of the same bundle. In no portion of the length, therefore, can jute be said to present a constant structure of its constituent fibrils.

From structure we pass to chemical composition, which we shall consider first in the aggregate. The percentage of inorganic (ash) constituents is small, vary from 0 6 in the finer qualities to 2·0 per cent in the less fine. It is almost entirely soluble in hydrochloric acid. We have not made complete analyses, as our attention has been taken up by the more absorbing questions involved on the organic side. The following results of partial analyses are nevertheless worth recording, as they exhibit the main features of the inorganic skeleton of the fibre :—

SiO_2	33·73	34·43	37·66
Fe_2O_3	5·53	6·67	8·33
Mn_3O_4..	0·75	0·43	0 88
CaO	16·67	17·82	13·84
MgO	5·81	—	2·98
Al_2O_3	6·15	5·14	5·24
P_2O_5	13·87	8·39	8·85
SO_3	—	5·35	1·23
Cl	—	0·87	—

The ash contains in addition about 10 per cent carbonic anhydride, formed during combustion, leaving a balance of 5-10 per cent for the alkaline constituents.

The presence of manganese is noteworthy. Some varieties of flax exhibit the same peculiarity. A remarkable feature of the ash of this, as indeed of all fibres, is that in it the structural peculiarities of the original are accurately preserved, suggesting an essential relationship of this skeleton to the organic portion of the structure. An inorganic constituent of considerable importance is the water of hydration of the fibre. This is constant at from 10 to 12 per cent of its weight, varying within these limits with the temperature and hygroscopic condition of the air. Its relations to the fibre have been the subject of some experiment and speculation. Sir William Thompson is inclined to regard the phenomenon as involving the laws of capillarity only, as dependent therefore upon the tubular structure of the fibre ; but he advances no satisfactory evidence for neglecting the chemical nature of the fibre substance, which seems to us the essential cause of its hydration. Union with water of hydration, and the capacity of removing bodies such as colouring matters from solution, are properties which exist in colloid substances in a close and general connection, especially marked in those substances which play a prominent part in the economy of living nature, and that whether they are organised or not. This correlation we have every reason to regard as essential. Finding, moreover, that the structureless modification of the fibre substance, obtained by precipitation from its solution in ammonio-cupric oxide, has the same tendency to take up water, and that the microscopic investigation of the fibre, dyed in various ways, gives no evidence of variation of tinctorial capacity with variations in structure, we prefer to class this, together with the capacity for hydration of the fibre, with the group of phenomena included in the term chemical adhesion.

We now pass to the consideration of the most interesting problem of the nature and constitution of the organic portion of the fibre. In laying before you the results of our researches, I may say that we do so with much pleasure ; we all of us owe a great deal to this College, some of us to this Society, and it is a satisfaction to make a return, however small, in presenting the results of work undertaken in a new and rich field of research ; but we do this not without some hesitation, for they are necessarily incomplete, and when each day's work throws fresh light on the points involved it is difficult to break off at a given stage of imperfection and make them the subject of a paper. However, we are more confident in doing this before a young society of fellow students, especially as the discussion which will follow will doubtless be of much help in bringing our results to the stage in which they may be laid before an older society. In order to give point to our treatment of the subject we shall speak from and to a theoretical view of the chemistry of the jute fibre, which, although it generalises our results, we do not regard as yet established. First, as to the aggregate elementary composition of the fibre. In this, as in all similar determinations, it was necessary to take specimens representing an entire length of the plant. These were purified by boiling in ammonia, washing, and drying at 105° C. previous to the combustions. The following results were obtained :—

(*a*) 0·4105 gave 0·7035 CO_2 and 0·2155 H_2O (together with 0·003 ash).
(*b*) 0·385 gave 0·6425 CO_2 and 0·1995 H_2O (0·003 ash).
(*c*) 0·3665 gave 0·6 ·4 CO_2 and 0·1905 H_2O (0·006 ash).
(*d*) 0·375 gave 0·6185 CO_2 and 0·1955 H_2O (0·006 ash).

After deducting the ash the calculated numbers are—

	(*ı*)	(*b*)	(*c*)	(*d*)	Cellulose.
C ..	47·09	45·86	46·4	45·44	44·44
H ..	5·86	5·80	5·7	5 84	6·17

The mean percentage cf carbon is 46·3. Associated with the cellulose basis of the fibre there is therefore a body or group of bodies rich in carbon. These amounting, as we shall presently see,* to about 30 per cent of the

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* The percentage of nitrogen is so small that it may be neglected. Thus, 1337 grains of the fibre, after boiling in dilute sodium carbonate and drying, yielded, after ignition of platino-chloride, 0·0052 pt. corresponding to 0·053 per cent nitrogen—a quantity just appreciable.

weight of the fibre, we calculate them to contain in the aggregate about 50 per cent of carbon. Still keeping to the question of aggregate composition, we come to treat of the methods adopted for the isolation of the cellulose. Of these there are several, all dependent, however, upon the solubility of the bodies with which it is associated, after previous treatment with an oxidising agent, in boiling alkaline liquids. Schulze's method, which up till quite recently was exclusively employed, consists in digesting the fibre (1 part) for twelve to fourteen days, at 15° C., with a solution of 8/10 part $KClO_3$ in 12 parts of HNO_3 (sp. gr. 1·1), washing with cold and then with hot water, boiling in dilute ammonia (1 in 50), and lastly washing with water, alcohol, and ether. This method is imperfect, the product being often discoloured, and by no means pure, and is now generally superseded by that proposed by Hugo Müller, viz., treating the fibre with aqueous bromine in the cold, and subsequently boiling in dilute ammonia, repeating the process until bromine ceases to be taken up. Dr. Müller found that a very pure cellulose was thus obtained; and as, moreover, he observed that specimens of cellulose left for months in contact with aqueous bromine underwent no change, the method appeared in every way reliable. In investigating the *rationale* of the process in the care of the jute fibre, we found that there occurred not only an oxidation of the intercellular substance, but also bromination; the halogen product being decomposed by ammonia, with formation of products soluble in the alkaline liquid. The direct action of a halogen alone, of which we shall treat hereafter, especially in regard to the products formed, suggested itself as a much more rapid and satisfactory method of acting upon these bodies than in solution in water; and in fact we found that by means of chlorine we were able in a few hours, and with once repeating, to arrive at the same result as by Müller's method, which involves four or five repetitions, occupying from two to three days.

We will now introduce a demonstration of the isolation of cellulose in this way. This specimen of jute has been boiled in a weak alkali, and exposed in the moist state to the action of chlorine. It now exhibits the deep orange yellow colour of the chlorinated compound which has been formed. Plunging it into boiling dilute alkali the compound is decomposed, the solution becoming of a dark brown colour through taking up the products. Throwing the fibre on to a muslin filter, and washing with hot water, we obtain a disintegrated soft mass of a transparent brown colour. A small portion plunged into acid is for the most part decolorised, and now exhibits an appearance not far different from that of the pure jute cellulose. We will repeat the process on the remainder, and thereby arrive at a practically pure product.

We will discuss the two latter methods somewhat more in detail.

(*a*) *Müller's Method.*—The percentage obtained by this method—*i.e.*, the alternate action of bromine water and boiling dilute ammonia—varies from 70 to 72 in the finer varieties of jute. The results obtained are very constant. The rough criterion of the completion of the process is the homogeneity of the disintegrated mass; the purity of the cellulose is ascertained by plunging it into a hot weak solution of aniline-blue, by which any residual intercellular matter will be dyed to a bright tint, easily distinguishable, after thorough washing, from the pale greyish shade which represents the low limit of the dyeing capacity of the pure cellulose. (It is needless to say that the weight of the mass is not sensibly affected by the test.) With regard to the manipulation of the fibre, the washings are best carried out on square filters of well-bleached (printer's) calico. For digesting with the bromine water, the most convenient vessel is a common china mug (300 c.c.) indelibly lettered. For all weighings, before and after treatment, the substance is previously dried at 105°.

(*b*) *Chlorine Method.*—The results obtained are equally constant and identical with the above, *e.g.*—

4·374 grns., previously boiled in water, yielded 3·127 = 71·3 per cent cellulose.
4·363 grns., previously boiled in dilute Na_2CO_3 yielded 3·195 = 72·2 per cent cellulose.
4·346 grns., previously boiled in dilute Na_2CO_3 yielded 3·161 = 72·7 per cent cellulose.

A more rapid result is in both methods attained by previously boiling the fibre with dilute alkali (1 per cent NaOH); the yield of cellulose does not appear to be affected. The following results were obtained with the process thus modified:—

Bromine Method.—

4·430 yielded 3·108 = 70·1 p.c. (boiled 10 min. in alkali).
4·420 ,, 3·153 = 71·3 ,, (,, 60 ,, ,,).

Chlorine Method.—

4·150 yielded 2·889 = 70 per cent.
3·900 ,, 2·748 = 70 ,,
4·332 ,, 3·069 = 70·8 ,,

The effect of boiling in a stronger alkali (5 per cent NaOH) is still further to aid the subsequent chlorination without otherwise affecting the results unless the boiling be prolonged.

Bromine Method.—

5·504 yielded 4·014 = 72·9 per cent (boiled 5 min.)
4·544 ,, 3·265 = 71·8 ,, (,, 60 ,,)
6·519 ,, 4·577 = 70·2 ,, (,, 4 hrs.)

It is advantageous to boil in a weak solution of caustic soda (1 per cent) after chlorination, this alkali being a better solvent for the products of decomposition than ammonia; and the results do not appear to be otherwise influenced by the substitution of the former.

The yield of cellulose resulting from the above treatment is, it will be seen, fairly constant; but this is incidental to the fact that the process of disintegration has been uniformly arrested at the point when the product satisfied the conditions of purity above mentioned; but at this point it is by no means an end product. The following results may be selected to show that the "72 per cent cellulose," as we may call it, is further considerably acted upon by the alternate action of bromine water and ammonia:—

Original weighings—

A. $\dfrac{3{\cdot}400\times100}{4{\cdot}689}=72{\cdot}5$ p.c.   B. $\dfrac{4{\cdot}06\times100}{5{\cdot}673}=71{\cdot}6$ p.c.

After twice repeating bromine treatment—

A. $\dfrac{3{\cdot}224\times100}{4{\cdot}689}=68{\cdot}7$ p.c.   B. $\dfrac{3{\cdot}884\times100}{5{\cdot}673}=68{\cdot}4$ p.c.

After two further repetitions—

A. $\dfrac{3{\cdot}025\times100}{4{\cdot}689}=64{\cdot}5$ p.c.   B. $\dfrac{3{\cdot}780\times100}{5{\cdot}673}=66{\cdot}6$ p.c.

We have not investigated the limit of this conversion into soluble products, but another process for isolating cellulose will throw light on this point. When the fibre is digested for some hours with dilute nitric acid (5 per cent) at 70°, a portion of the intercellular substance, both aromatic and carbohydrate, is dissolved, the solution acquiring a bright yellow colour and an odour resembling that of vanille: the remainder is modified in such a way that it is dissolved on boiling the fibre with a dilute alkali, and pure cellulose remains. The following percentages have been in this way obtained:—61·4, 60·8, 61·6, 62·7, 63·7, 62·8, 61·6. These results will be subsequently developed and discussed. We may therefore infer the presence of a constituent in jute cellulose, readily susceptible of conversion into a soluble modification, and that the cellulose isolated by the more usual quantitative methods is a chemical aggregate, defined by the process of which it is the product. These observations are extended and confirmed by a modification of our chlorination process which we have only recently adopted. This consists in plunging the chlorinated fibre into a boiling solution of sodium sulphite: its colour changes from orange to purple, and a considerable quantity of its substance is dissolved. It is then boiled in a weak alkali (1 per cent $NaOH$), and pure cellulose is obtained thus with a single chlorination. The yield of cellulose is in this case higher than by any of the previous methods; *e.g.*, we have obtained 75·2 and 76·4 per cent, the product moreover possessing a much greater length of fibre. On twice treating the resulting cellulose according to Müller's method these quantities were diminished to 72·6 and 73 per cent respectively, numbers which are still above the average of former methods. Even when the process, modified as above, was supplemented *before weighing* by the latter treatment, the percentage yield was still above the average, *e.g.*, 74·7, 75·4, 76·9.* The inference from these results is that the intercellular substance contains a body of the nature of cellulose which may, in the process of re-solution, be separated partially in the insoluble form as cellulose, and partly as a soluble modification, the ratio of these being dependent upon the process employed. We may mention here, in support of this view, that on exhausting the chlorinated fibre with water, a body is dissolved which reduces Fehling's solution; and, on subsequently boiling with an alkali, the solution obtained has the same property. We shall discuss these points more fully hereafter, and show that there is reason for regarding the bodies to which the reducing action is due as carbohydrates. In concluding this portion of our subject we would point out the necessity of "defining" the cellulose obtained in the proximate analyses of plant structures, by a statement of the method of separation, and we would open the next by pointing out a similar want of clearness which attaches to the terms by which the intercellular and encrusting substances are chemically defined. In giving the results of such analyses it is usual to sum these constituents under the term "pectous substances." This seems to us an *a priori* assumption not necessarily to be verified by a special examination into their chemical nature.

(To be continued.)

UNIFORMITY IN CHEMICAL NOMENCLATURE.

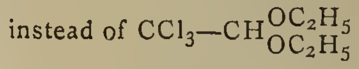
With the object of securing a nearer approach to uniformity in chemical nomenclature and the symbolic representation of formulæ and decompositions, the Council of the Chemical Society some time ago drew up certain rules and recommendations for the use of their Journal staff. We gladly accede to the request that we should publish the following extracts from them in our columns, in the hope that they may induce our readers in general to adopt a system which, coming from such a source, may be considered authoritative wherever chemistry is studied:—

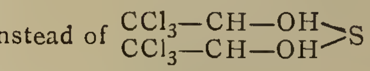
"Equations, as a rule, should *not* be written on a separate line, but should 'run on' with the text. Simple reactions involving a mere interchange of radicles, or the withdrawal or addition of a particular element or group of elements, need not generally be expressed by equations.

"Employ names such as sodium chloride, potassium sulphate, ethyl acetate, and only use the terminals *ous* and *ic* in distinguishing compounds of different orders derived from the same elementary radicle, *e.g.*, mercurous and mercuric chloride, sulphurous and sulphuric acid.

"Apply the term *acid* only to compounds of hydrogen with negative radicles, such as HNO_3, H_2SO_4, H_3PO_4, and denote the oxides which form acids by names such as sulphuric anhydride or sulphur trioxide. Term salts containing an amount of metal equivalent to the displaceable hydrogen of the acid *normal* and not neutral salts, and assign names such as hydrogen-sodium sulphate, hydrogen-disodium phosphate, &c., to the acid salts. As a rule, it is best merely to give the *formulæ* of basic salts.

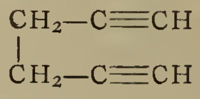
"To economise space, it is desirable, 1, that *dots* should be used *instead of dashes* in connecting contiguous symbols or radicles whenever this does not interfere with the clearness of the formula; 2, that formulæ should be shortened by the judicious employment of the symbols Me for CH_3, Et for C_2H_5, Pr^{α} for $CH_2.CH_2.CH_3$, Pr^{β} for $CH(CH_3)_2$, Ph for C_6H_5, $\bar{A}c$ for $CO.CH_3$, and $\overline{Bz}$ for $CO.C_6H_5$; and 3, that formulæ should be written *in one line* whenever this can be done without obscuring their meaning. For example:—

$CCl_3.CH(OEt)_2$ instead of 
$CCl_3{-}CH\genfrac{}{}{0pt}{}{OC_2H_5}{OC_2H_5}$

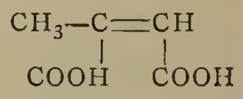
$(CCl_3.CH.OH)_2S$ instead of 
$\left.\begin{matrix}CCl_3{-}CH{-}OH\\CCl_3{-}CH{-}OH\end{matrix}\right>S$

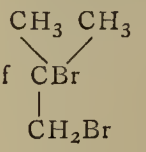
$CH_2 : CH_2$ instead of $CH_2{=}CH_2$

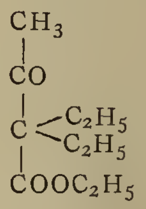
$CH : CH$ instead of $CH{\equiv}CH$

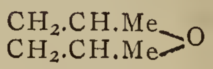
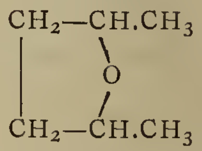
$CH : C.CH_2.CH_2.C : CH$ instead of 
$\begin{matrix}CH_2{-}C{\equiv}CH\\|\\CH_2{-}C{\equiv}CH\end{matrix}$

$MeCH : CHPr^{\alpha}$ instead of $CH_3.CH{=}CH.CH_2.CH_2.CH_3$

$COOH.CH : CMe.COOH$ instead of 
$\begin{array}{lll}CH_3{-}&C{=}&CH\\&|&|\\&COOH&COOH\end{array}$

$CH_2Br.CBrMe_2$ instead of 
$\begin{matrix}CH_3\ \ CH_3\\\diagdown\ \diagup\\CBr\\|\\CH_2Br\end{matrix}$

$\bar{A}c.CEt_2.COOEt$ instead of 
$\begin{matrix}CH_3\\|\\CO\\|\\C\genfrac{}{}{0pt}{}{\diagup C_2H_5}{\diagdown C_2H_5}\\|\\COOC_2H_5\end{matrix}$


$\left.\begin{matrix}CH_2.CH.Me\\CH_2.CH.Me\end{matrix}\right>O$ instead of 

$C_6H_5CH(\bar{O}Bz).CH(\bar{O}Bz).COOEt$ instead of $C_6H_5.CH(O.CO.C_6H_5).CH(O.CO.C_6H_5).COOC_2H_5$.

"Use names such as methane, ethane, &c., for the normal paraffins or hydrocarbons of the C_nH_{2n+2} series of the form $CH_3.(CH_2)_x.CH_3$. The isomeric hydrocarbons are usually most conveniently represented by names indicating their relation to methane: for example, $CH_3.CH_2.CH_2.CH_3$ = propylmethane; $CH_3.CH(CH_3)_2$ = isopropylmethane or trimethylmethane; or, although less frequently, by names such as diisopropyl.

"Term the hydrocarbons C_2H_4 and C_2H_2 ethylene and acetylene respectively (not ethene and ethine). Distinguish the homologues of ethylene whenever possible by names indicating their relation to it, such as methylethylene, dimethylethylene, &c., denoting the di-derivatives of the form $C_nH_{2n+1}.HC:CH.C_nH_{2n+1}$ as α-, and those of the form $CH_2:C(C_nH_{2n+1})_2$ as β- compounds: thus, $CH_3.CH:CH.CH_3$ = α-dimethylethylene; $CH_2:C(CH_3)_2$ = β-dimethylethylene. Similarly, use names such as methylacetylene and dimethylacetylene for the homologues of acetylene of the form $CH : C.C_nH_{2n+1}$ and $C_nH_{2n+1}.C : C.C_nH_{2n+1}$. Adopt the name *allene* for

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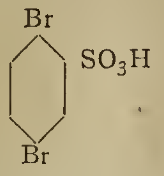
* Several subsequent applications of the bromine and ammonia treatment caused in these, as in previous similar cases, a relatively small decrease in weight, viz., from 74·7 to 72·3, from 76·9 to 74·5, and from 75·4 to 74 per cent, proving a marked influence of the sulphite treatment on the stability as well as on the yield of the cellulose.

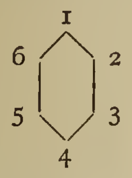
the hydrocarbon $CH_2{:}C{:}CH_2$, and indicate the relation which its homologues bear to it in the same manner as pointed out for acetylene.

"In representing the constitution of benzene derivatives, as a rule, merely indicate the relative positions of the radicles in the symbol of benzene by figures, instead of by means of the hexagon symbols, as in the following example:—

Paradibromobenzenesulphonic acid,—
$C_6H_3Br_2.SO_3H[Br : SO_3H : Br = 1 : 2 : 4]$ instead of—



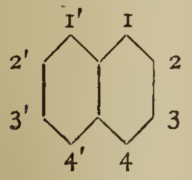
the figures always being used in the order—



Relatively to the position 1, the positions 2 and 6 should always be spoken of as *ortho*-positions, 3 and 5 as *meta*-positions, and 4 as the *para*-position. It is better, however, in speaking of the derivatives of benzene, to express their constitution by giving them names such as 1 : 2 dibromobenzene, 1 : 3 dibromobenzene, &c., rather than by terming them ortho- or metadibromobenzene, &c.

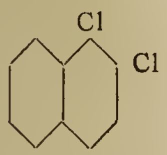
"In representing the constitution of derivatives of other 'closed chain' hydrocarbons also, as rule, do not employ graphic formulæ, but merely indicate the position of the radicles introduced in the following manner:—

"*In the case of naphthalene*, express the position of the radicles introduced in place of hydrogen relatively to the carbon atoms common to the two 'rings,' and number the positions in the one ring 1, 2, 3, 4, and those in the other 1′, 2′, 3′, 4′ in the order shown by the annexed symbol:—

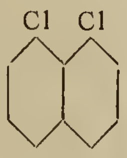


"The dichloronaphthalenes, for example, are spoken of simply as 1 : 2 dichloronaphthalene, or dichloronaphthalene [Cl : Cl = 1 : 2], &c., thus—

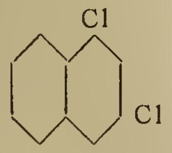
1 : 2 dichloronaphthalene =



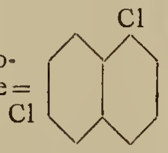
1 : 1′ dichloronaphthalene =



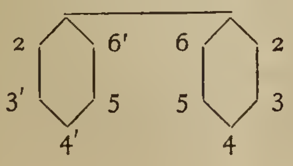
1 : 3 dichloronaphthalene =



1 : 3′ dichloronaphthalene =



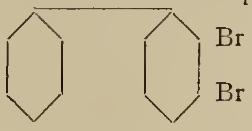
"*In the case of diphenyl*, indicate the position of the radicles relatively to the carbon atom of the one C_6 group which is associated with the other C_6 group, and number the positions in the one group by the figures 2, 3, 4, 5, 6, and the corresponding positions in the other group by the figures 2′, 3′, 4′, 5′, 6′, as shown by the following symbol:—



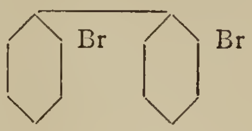
Thus the mono-derivatives, the bromodiphenyls, for example, are represented as—

Bromodiphenyl [Br = 2]
" [Br = 3]
" [Br = 4]

and the two dibromodiphenyls—

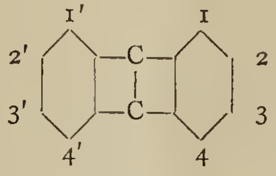


and



are respectively dibromodiphenyl [Br : Br = 2 : 3] and dibromodiphenyl [Br : Br = 2 : 6′].

"*In the case of anthracene*, employ the following symbol, and indicate the position of the radicles relatively to the central C_2— group:—



Examples:—
Alizarin, $C_6H_4{:}C_2O_2{:}C_6H_2(OH)_2[OH : OH = 1 : 2]$.
Quinizarin, $C_6H_4{:}C_2O_2{:}C_6H_2(OH)_2[OH : OH = 1 : 4]$.
Anthraflavic acid,—
$C_6H_3(OH){:}C_2O_2{:}C_6H_3(OH)[OH : OH = 2 : 3']$.
Purpurin, $C_6H_4{:}C_2O_2{:}C_6H(OH)_3[OH : OH : OH = 1 : 2 : 4]$.

"In speaking of compounds such as these, their constitution may be represented by the names—

1 : 2 Dihydroxyanthraquinone = Alizarin.
1 : 4 " = Quinizarin.
2 : 3′ " = Anthraflavic acid.
1 : 2 : 4 Trihydroxyanthraquinone = Purpurin.

Always include the letters and figures indicating the constitution of derivatives of closed chain hydrocarbons in square brackets.

"Distinguish all alcohols, *i.e.*, hydroxyl-derivatives of hydrocarbons, by names ending in *ol*, *e.g.*, quinol, catechol, resorcinol, saligenol, glycerol, erythrol, mannitol, instead of hydroquinone, pyrocatechin, resorcin, saligenin, glycerin, erythrite, mannite. Compounds which are not alcohols, but which are at present distinguished by names ending in *ol*, may be represented by names ending in *ole*, if a systematic name cannot be given. For example, write indole instead of indol; furfuraldehyde instead of furfurol; fucusaldehyde instead of fucusol. Ethers derived from phenols, such as $C_6H_5.OCH_3$, &c., hitherto called anisol, anethol, &c., may be distinguished by names ending in *oïl*, as anisoïl, anethoïl.

"Alcohols should be spoken of as mono-, di-, tri-, or n-*hydric*, according to the number of OH groups.

"Bodies such as the acids of the lactic series containing the group (OH) should be termed *hydroxy*-, and not oxy-derivatives, *e.g.*, hydroxyacetic and not oxyacetic acid. Compounds containing the analogous groups C_2H_5O, C_6H_5O, $CH_3.COO$, &c., should in like manner be termed ethoxy-, phenoxy-, acetoxy- derivatives. Thus, ethoxy-propionic acid instead of ethyl-lactic acid; 3 : 4 diethoxybenzoic acid instead of diethylprotocatechuic acid; and acetoxypropionic acid instead of acetyllactic acid. Terms such as diethylprotocatechuic acid should be understood to mean a compound formed by the displacement of hydrogen atoms in the hydrocarbon radicle of protocatechuic acid by ethyl, viz., $C_6H(C_2H_5)_2(OH)_2.COOH$, and not $C_6H_3(OC_2H_5)_2.COOH$, just as dibromoprotocatechuic acid is understood to be the name of a compound of the formula $C_6HBr_2(OH)_2.COOH$.

The term *ether* should be restricted to the oxides of hydrocarbon radicles, and the so-called compound ethers should be represented by names similar to those given to the analogously constituted metallic salts.

"Compounds of the radicle SO_3H should, whenever possible, be termed sulphonic acids, or, failing this, sulpho-compounds: as benzenesulphonic acid, sulphobenzoic acid, and not sulfi-compounds. Compounds of the radicle $SO_2.NH_2$ should be termed sulphonamides.

"Basic substances should invariably be indicated by names ending in *ine*, as aniline, instead of anilin, the termination *in* being restricted to certain neutral compounds, viz., glycerides, glucosides, bitter principles, and proteids, *e.g*,. palmitin, amygdalin, albumin. The compounds of basic substances with hydrogen chloride, bromide, or iodide, should always receive names ending in *ide* and not *ate*, as morphine hydrochloride and not morphine hydrochlorate."

NOTICES OF BOOKS.

A Manual of Quantitative Chemical Analysis, for the Use of Students. By F. A. Cairns, A.M., Instructor in Analytical Chemistry in the School of Mines, Columbia College. New York: H. Holt and Co.

This book consists substantially of the notes and instructions given by the late F. A. Cairns to the students at the School of Mines. At the time of his sudden and lamented death, he was collecting the subject-matter for the present work, which has been completed by his friend and colleague, Mr. Elwyn Waller. The art of quantitative chemical analysis is here taught, not by taking the pupil through the series of simple bodies and their compounds in the order usually met with in chemical treatises, but it is illustrated upon a number of commercial products and natural minerals so arranged that each may prepare the way for the determination of the succeeding bodies. Thus the first substance which the author puts into the hands of the student is barium chloride, upon which he illustrates the determination of baryta and of chlorine.

In the analysis of potable waters, the author recommends for the determination of organic matter that the permanganate process, as elaborated by Dr. Tidy, be superadded to the ammonic process of Wanklyn and Chapman. For the determination of phosphoric acid in superphosphates, he prescribes the ammonium molybdate method, making use of the volumetric uranium process only in the absence of iron and alumina. Reverted phosphoric acid he dissolves out in the usual way with ammonium citrate.

For the examination of white-lead ground up in oil,—a somewhat puzzling case for the inexperienced,—he removes the oil by digestion at a steam-heat with excessive and repeated doses of ether or gasoline. The oxides and metallic salts are then determined in the residue by ordinary methods.

In the analysis of milk Mr. Cairns determines moisture, or total solids, by simple evaporation on the water-bath without the previous addition of sand or plaster-of-Paris.

After closely examining the directions here given for the analysis of the various substances, as well as the more general instructions in the introduction, we recognise that the author was a careful and conscientious analyst, accustomed to lay due weight upon certain precautions necessary for the attainment of accurate results, but which are, nevertheless, often slurred over in the text-books. If there is any point upon which we should differ from him, it is in reference to his directions that filters and their contents should be thoroughly dried before ignition. This, save in a few exceptional cases, is a mere waste of time.

The Retrospect of Medicine. Edited by W. Braithwaite, M.D., and James Braithwaite, M.D. Vol. lxxxi., January—June, 1880. London: Simpkin, Marshall, and Co.

This half-yearly summary contains not a little matter which will be found interesting beyond the limits of the medical profession. As such we may particularly mention a paper on "Contagion, its Nature and Mode of Action," by J. Simon, F.R S. The author points out that the self-multiplying power of all contagia is strongly suggestive of their being not merely organic, but organised and endowed with life. He draws the following distinction between parasites and true, or metabolic, contagia. Each of the latter, in proportion as it multiplies in the body, transforms the bodily material with which it comes in contact in a specific way, different from the ways of other contagia. The parasite, on the contrary, however much it multiplies in the body, produces no qualitative effect specific to itself, but only such effects as are common to it and all other parasites. As one of the modes in which parasitic disease is spread, Mr. Simon mentions that in Iceland the very great sufferings of men and cattle from *Echino coccus* have been traced to the agency of dogs. These animals eat hydatidised offal from the slaughter-houses, and then void the eggs of the tape-worm in kitchen-gardens, water-courses, and pastures.

Another memoir, entitled "A Contribution to the Therapeutics of Starch Digestion," by Dr. W. Roberts, F.R.S., is also of general interest. The author finds that the pancreatic secretion of suckling calves does not act as a ferment of starch, and considers it probable that the same rule will hold good in young children, whose saliva is well known to have but a very feeble diastatic power. The practical conclusion from these facts is the unsuitableness of farinacea of all kinds,—unless accompanied by some ferment, such as extract of malt—for the nutriment of infants. Dr. Roberts recommends that a simple infusion of malt in cold water should be added to gruel or other farinaceous preparations.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Note.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 2, July 12, 1880.

Observations on the Density of the Vapour of Iodine.—M. Berthelot.—It is admitted in the abstract theory of gases that the simple gases experience at once the same increase of total energy and the same increase of the *vis viva* of translation when they undergo one and the same change of temperature. This conception explains the experiments of physicists on the specific heat of gases (the law of Dulong and Petit), on their expansion by heat (the law of Gay-Lussac), and on their compressibility (Mariotte's law). It may further be inferred from the two latter laws that the density of a gas, that is to say, the relation between the weight of a given volume of this gas and the weight of the same volume of air, taken at the same temperature and at the same pressure, is constant. The variations hitherto observed have been ascribed to secondary perturbations. These three laws have never been really demonstrated except for three elements (oxygen, hydrogen, and nitrogen); they constitute the sole scientific basis upon which is founded the physical determination of molecular weights, and consequently the numeration of atoms in actual theories. If these laws should cease to be true for certain elements, the physical definition of the molecular weights of these elements and that of the number of their atoms would become mere conventions. The author has already observed that the experiments of Kundt and Warburg on the velocity of sound in mercurial vapour cannot be reconciled with the totality of the three laws just referred to (*Annales de Chimie et de Physique*, 5th series, ix., p. 427). The experiments of M. V. Meyer on the decrease of the gaseous density of iodine and of the other halogens under a constant pressure, but at temperatures remote from each other, are still more at variance with admitted laws. These experiments are, moreover, confirmed and extended by those which M. Troost has just performed with his usual precision on the same body at a constant temperature, but at varying and slight pressures. Thus the variation of the *vis viva* of the molecular translation of gaseous iodine under the influence of very high temperatures or of very slight pressures far surpasses the same variation in the molecules of air, observed under the same conditions. The laws of Mariotte and of Gay-Lussac, founded merely on three simple gases, are applicable neither to iodine nor to the other halogens. We may here call to mind that the law of specific heats is not applicable to this group of elements, as the specific heats of chlorine and gaseous bromine exceed by one-fourth those of the other simple gases, even between the ordinary temperature and 200°, at which no dissociation is admissible. Hence it results that the increment of the total energy of the halogens with the rise of temperature exceeds that of the three other gases hitherto studied (nitrogen, hydrogen, and oxygen) as well as the increment of the *vis viva* of translation: these two orders of effects appear correlative. The decrease of the density of gaseous iodine being, moreover, progressive, it is the same with the increase of the *vis viva* of translation, and it is not permissible, as M. Troost has very judiciously remarked, to draw from it any correct conclusion as regards the variation of the number of the molecules. This kind of reasoning becomes arbitrary the moment that the molecular weight of iodine, either at a high temperature or at a low pressure, escapes from the old definitions. A single law remains applicable to the elements with an absolute and universal character: the invariability of the relations of weight according to which the elements combine with each other, that is to say, the notion of equivalents. It is at present the sole immoveable foundation of chemical science.

Formation-heat of Hydrocyanic Acid and of the Cyanides.—M. Berthelot.—The author has rectified his determinations, published in 1871, in conformity with the amended formation-heat of ammonia, formerly taken as +35·15 but now found to be +21·0. The corrected formation-heats of the cyanogen compounds are given in the form of a table.

Vapour-densities of Selenium and Tellurium.—MM. Sainte-Claire Deville and Troost.—The authors communicate the details of the experiments which they performed in 1860. For selenium the vapour-density found was 5·68, or by calculation 5·54. For tellurium the experimental result was 9·08, or by calculation 8·93.

Etiology of Rinder-Pest.—M. Pasteur, with the assistance of MM. Chamberland and Roux.—The author considers that the buried bodies of animals which have perished may still propagate the infection, as the disease-germs are brought to the surface by earth-worms. Hence he argues that animals should never be buried in lands used for pasturage or for the cultivation of forage plants. Such carcases should if possible be buried in poor, dry, sandy, or calcareous soils where earth-worms do not prosper. The cattle disease is unknown in the chalky "Savarts" district of Champagne.

Ammonia in the Air and the Waters.—Albert Lévy.—The author has made during the years 1875-79 inclusive, regular analyses of the rain-waters collected at a number of points in and around Paris. The yearly mean proportion of ammonia, expressed in milligrammes per litre, has been 1·98, 1·54, 1·91, and 1·20. The water is poorest in ammonia in July and August, whilst the maximum proportion appears in December. As regards the atmosphere the case is reversed. The proportions of ammonia found in 100 cubic metres of air were, in 1876-77, 3·0 m.grms.; in 1877-78, 2·3, and in 1878-79, 1·9.

Fluorine Compounds of Uranium.—A. Ditte.—The author has obtained the hydrofluorate of uranium sesquifluoride, U_2F_34HF, in yellow transparent crystals. He has formed also an uranyle fluoride, $(U_2O_2)F$. This latter compound if heated is decomposed into a volatile oxyfluoride and a sesquioxide, which is very unstable and loses a part of its oxygen and becomes transformed into a protoxide, which crystallises in the fluoric atmosphere.

The Constitution of Matter and the Ultra-Gaseous State.—Extract from a letter addressed by Mr. Crookes to M. Dumas.

Mono-chromatic Lamps.—L. Laurent.—This paper cannot be reproduced without the accompanying illustration.

Telephonic Effects resulting from the Shock of Magnetic Bodies.—M. Ader.—The author draws the general conclusion that every mechanical action having the effect of disturbing the state of molecular equilibrium of a magnetic nucleus, develops, at the moment when such nucleus returns abruptly to its conditions of equilibrium, an electric current capable of acting on the telephone.

Atomic Weight of Scandium, and on Certain of its Characteristic Salts.—L. F. Nilson.—The author has operated upon scandia extracted from euxenite. The properties by means of which it is possible to separate scandia from ytterbia are:—Scandium nitrate is decomposed by heat more readily than ytterbium nitrate: scandium sulphate added to a saturated solution of potassium sulphate produces a double salt, which is entirely insoluble. The atomic weight, assuming scandia Sc_2O_3, is 44·03. Scandia is a very light infusible white powder, resembling magnesia or glucina, and of the sp. gr. 3·864. The author describes the nitrate, sulphate, double potassium sulphate, selenites, and oxalate. He considers that scandium is the element which Prof. Mendelejeff predicted under the name of ekabor.

Ultimate Action of Bromine upon Malonic Acid: Bromoform.—E. Bourgoin.—Malonic acid is in great part attacked by bromine with the formation of bromated acids of little stability, so that the final reaction is expressed by the following equation:—

$$C_6H_4O_8+3Br_2=2C_2O_4+3HBr+C_2HBr_3,$$

a reaction comparable to that which yields tetrabromated ethane by means of succinic acid.

Etherification of Sulphuric Acid.—A. Villiers.—Sulphuric acid in presence of alcohol may give rise to an acid and a neutral ether, but the quantity of the latter is exceedingly small. As in the case of the hydracids, etherification ceases entirely on reaching a certain degree of dilution, but with sulphuric acid the same solutions cease to become etherified at common temperatures and at 100°, contrary to what takes place with the hydracids.

Biedermann's Central-Blatt für Agrikultur-Chemie.
Part 3, March, 1880.

Observations on Local Fluctuations of the Proportion of Carbonic Acid in the Atmosphere.—Drs P. Hässelbarth and J. Fittbogen.—At Rostock the atmospheric carbonic acid decreased with a south-west wind, and increased with a north easterly current. In Dahme, on the contrary, the proportion was greatest with a westerly wind. Rain and dew was found to produce a slight diminution; fog, in some cases, a decrease, and in others an increase. No influence was traced to sunshine and shade.

Comparative Meteorological Observations in Fields and Forests.—A. Matthieu.—On an average of eleven years the rain in two stations of the woods as compared with the open country was found to be in quantity as 100 and 97 to 81, and in spite of the screen of leaves the proportion which reaches the soil is as large, or larger than in the fields. In the fields the average evaporation is three times greater than in the forests. In the forests the extremes of temperature are smaller and the fluctuations less

Injurious Effect of Water from Peat Moors and Alder Swamps upon Meadows.—Dr. Klien.—These waters contain large proportions of soluble humic compounds, which, among other things, reduce all iron compounds to ferrous salts, which exert a poisonous influence upon the soil.

"Separation-Mud" as Manure.—Prof. E. v. Wolff.—The mud in question is formed on clarifying beet juice with lime. It contains 1 to 2 per cent of phosphoric acid and about ¼ per cent of nitrogen.

Cultivation of an Estate for Thirty-eight Years without Farm-yard Manure.—M. Stecker.—This estate has been exclusively treated with artificial manures, and no cattle have been kept. The yield of rye and oats is better than on the neighbouring estate of Bräunsdorf, which is abundantly dressed both with dung and artificial manures. In potatoes the returns are lower, and clover and flax have failed.

Manurial Experiments with Nitrate of Soda on Sugar-Beets.—Prof. Max Märcker.—The yield of sugar was found inferior in value.

Manurial Experiments on Rye, Wheat, and Oats.—Dr. A. Pagel and H. Meyer.—Not suitable for abstraction.

Experiments on Feeding Swine.—Prof. E. v. Wolff.—It has been ascertained that the nutritive effects of animal and vegetable albumen are substantially identical.

Chemical Researches on the Activity of Bees.—Dr. E. Erlenmeyer and A. v. Planta-Reichenau.—Wax is produced, not from nitrogenous, but from non-nitrogenous matter, especially sugar. The food of bees should not be very rich in albuminous compounds. They refused food containing peptons. Carbo-hydrates are expended in producing the tissue-fat of bees.

Determination of the Non-albuminoid Nitrogenous Compounds of Plants.—Dr. O. Kellner.—In clover, lucern, sorghum, &c., peptons neither exist pre-formed nor are produced by the treatment used by the experimentalists. During the germination of such plants, however, peptons make their appearance.

Germination of Beet-seed.—P. van de Putte.—Warm and insular climates are unsuitable for the sugar-beet, which is richest where early cold sets in after a hot summer.

Seed-production of Red Clover.—Dr. G. Haberlandt.—The author recommends the protection of humble bees and the diligent destruction of their enemies.

Researches on the Damage to Peas by Bruchus Pisi.—Prof. G. Marck.—Pea seed damaged by this weevil cannot be recommended for sowing. The author proposes to treat the peas with the vapour of carbon disulphide.

Tartar and Tartaric Acid in Must and Wine.—E. Mach, C. Portele, and C. v. Babo.—On the average as the percentage of alcohol (in volume) rises from 5 to 19, the tartar falls from 3·3 to 1·2. The free tartaric acid varies from 0 to 1·9. The absence of tartar in the presence of free tartaric acid is a proof that the wine has been plastered.

Tannin in Wines.—Ippolito Macagno.—Where alcohol is deficient tannin has not a preservative action. As wines grow older the glycerin increases and the tannin diminishes.

The Chemical Changes in Nitrogenous Matter due to Fermentation, and their Relation to the Growth and the Efficiency of Yeast.—Dr. M. Delbrück.—The absolute quantity of the yeast formed does not depend on the dissolved sugar, and is affected merely by the quantity of dissolved nitrogen. The formation of yeast is not connected with its fermentative efficiency.

Action of Products of Putrefaction upon Schizomycetes.—M. Wernich.—The bacteria of putrefaction appear to prepare materials for their own destruction. Phenol, cresol, scatol, indol, &c., all of them products of the putrefaction of albuminoid substances, interfere with the development of bacteria.

The Raw Material for Pressed (so-called German) Yeast.—Dr. M. Delbrück.—The author recommends the coarsest quality of rye.

Influence of the Galvanic Current upon Bacteria in a Mineral Nutritive Solution.—Prof. C. Cohn and Dr. B. Mendelsohn.—A battery of five elements destroys bacteria completely, and if fresh living bacteria are subsequently introduced they do not increase. Induction currents have no influence upon the multiplication of bacteria.

Justus Liebig's Annalen der Chemie,
Band 202, Heft 2.

Derivatives of Myristic Acid.—F. Masino.—The author finds the melting-point of myristin between 53° and 54°, which accords with theory and differs totally from Playfair's determination (31°). The derivatives examined are myristamide, myristanilide, and myristolic acid.

Crystalline Constituents of Coralline.—C. Zulkowsky.—The author criticises the memoir on Aurin (*Annalen*, 194, 75) by R. S. Dale and C. Schorlemmer. He remarks that after these chemists had for years upheld the formula $C_{20}H_{14}O_3$ for aurin, they now put the matter in such a light as if the researches of E and O. Fischer were in certain respects merely a confirmation of views, which they have in the meantime corrected. He states that Dale and Schorlemmer's method of preparing coralline requires more time than his own, yield much smaller returns, and produces, not pure aurin, but a mixture of several bodies, crystalline and amorphous. He adds the following recent observations on the manufacture of coralline:—It is not advantageous to heat the mixture of sulphuric acid and phenol in order to complete the formation of sulpho-phenol. The proportion of ⅔ part of sulphuric acid given in the author's earlier prescription should be reduced to ½ part, and the oxalic acid should be used in a dehydrated state. Good results were obtained by mixing 1 part phenol with ½ part sulphuric acid of 66° B., adding 0·6 to 0·7 part dehydrated oxalic acid, and heating to 120° to 130° until the escape of gas has become decidedly weaker, and the contents of the flask on cooling become a tough mass. About twenty-four hours are required for the completion of the process and the yield ranges from 60 to 70 per cent. Among the compounds specially examined are oxidised aurin, $C_{19}H_{16}O_6$, in violet needles; leucaurin, $C_{19}H_{16}O_3$; aurin, $C_{19}H_{14}O_3$; methyl-aurin, $C_{20}H_{16}O_3$; and its leuco-product, $C_{20}H_{18}O_3$.

Constitution of Certain Naphthalin Derivatives.—F. Beilstein and A. Kurbatow.—The two nitro-groups in α-dinitro-naphthalin are distributed to both the benzol-nuclei, whilst in β-dinitro-naphthalin they are in one benzol-ring.

Communications from the Laboratory of Owens College, Manchester. 1. Examination of Certain Cases of Isomorphism.—H. Baker.—After the examination of a number of double fluorides and oxy-fluorides, the author remarks that among all these salts there is a very close analogy of form, with two exceptions. On optical examination a difference appears, some of them being singly and others doubly refractive. If certain chemically analogous bodies, belonging to the same crystalline system, distinguished merely by small differences in their angles, and therefore regarded as amorphous, have very different optical properties, should not other bodies of mutually analogous composition also rank as isomorphs, though they have been placed in different systems on account of small differences in the magnitude of their angles or in their optical behaviour?

2. The Atomic Weight of Tellurium.—W. L. Wills. The atomic weight of tellurium is 126·83 with a probable error of 0·198. Hence it does not fall between iodine and antimony, but exceeds that of iodine, which therefore ought to rank before tellurium in Mendelejeff's classification.

3. **On Magnesium Vanadiates.**—S. Suguira and H. Baker.—The authors describe the brown salt,—

$$3MgO5V_2O_5,28H_2O,$$

and a red salt of the same composition.

4. **Certain Fluorine Compounds of Vanadium.**—H. Baker.—The author describes the potassium fluoroxy-vanadiate in a foliaceous and an acicular form; a corresponding ammonium salt, foliaceous, pyramidal, and acicular; a zinc fluoroxy-vanadiate, and an ammonium fluoroxy-hypo-vanadiate.

Communication from the Laboratory of the University of Halle.—This consists of a paper by W. Heintz on two compounds of urea with gold chloride.

Archives Néerlandaises des Sciences Exactes et Naturelles.
Tome xv., livraison 1.

The Diamond Mine of South Africa.—J. A. Roorda Smit.—The author summarises his observations as follows:—In the mines of South Africa the diamond is found in a primitive gangue, of volcanic origin, and which has merely experienced secondary modifications. The presence of the double carboniferous silicate is characteristic for these diamond mines, which are the extinct craters of volcanoes. In consequence of the plutonic phenomena the diamond has been formed at the expense of organic matter, under the influence of great pressure and a high temperature.

Tome xv., livraison 2.

Researches on Conquinamine.—A. C. Oudemans, Jr.—Already noticed.

The Phenomenon of Flashing in the Assay of Gold, and the Influence Exerted upon this Phenomenon by the Metals of the Platinum Group.—A. D. van Riemsdijk.—Already inserted.

Correspondenz-Blatt des Vereines Analytischer Chemiker.
No. 6, March 15, 1880.

Determination of Carbohydrates in Infants' Foods.—Dr. N. Gerber.—The author proposes the following method:—In a capsule of platinum or porcelain, holding 50 c.c., are put 2 or 3 grms. of the sample, taken from the middle of the package, and mixed with ten times the weight of water. (1.) In foods containing diastase proceed according to the directions for mixing given upon the package, making use of the thermometer. The mass is allowed to cool, placed in a tall beaker, holding 150 to 200 c.c., and this capsule is then washed with 100 c.c. of alcohol at 50° Tralles. Let stand over night, when the clear supernatant liquid can be easily drawn off with an aspirator. When the liquid has passed through, the precipitate is thrown upon the filter, and the beaker is washed upon it with 100 c.c. of alcohol at 50° Tralles. (2.) In ordinary foods, 1 part is also mixed up with 10 parts of water, and kept at a boil for exactly five minutes with constant stirring, and further treated as above. In order to determine the soluble carbohydrates the filtrate is evaporated down to quarter of its volume, separated by filtration from the albuminates and fatty matters, and then evaporated first in the water-bath, and then in the air-bath, at 100° to 110° till the weight becomes constant. The residue is lastly carefully incinerated. Then capsule + ash is subtracted from capsule + solid residue, the difference = soluble carbohydrates.

Microscopic Examination of Water.—This method of examining waters is strongly urged upon the attention of public analysts, and many of the more characteristic animal and vegetable forms are figured in the accompanying plate.

Occurrence of Indium in Norwegian Minerals.—S. Wleugel.—The author has examined many specimens of blende from different localities in the hope of finding indium. The minerals were as a rule either free from this element or showed very faint indications.

Proportion of Phosphates in the Moselle Wines.—R. Kayser.—These wines may contain 0·040 grm. P_2O_5 in 100 c.c.

No. 7, April 1, 1880.

Determination of the Decolourising Power of Different Samples of Animal Charcoal by Quantitative Spectrum Analysis.—C. H. Wolff.—Not suitable for abstraction.

Detection of Traces of Carbonic Oxide in Atmospheric Air.—C. H. Wolff.—This memoir requires the accompanying illustration. The apparatus described is made by Muenke Bros., Louisenstrasse, Berlin.

Extraction-Apparatus for the Determination of Fats, Alkaloids, &c.—F. Gantter.—This paper cannot be usefully reproduced without the accompanying three figures.

Phosphoric Acid in Wine.—R. Kayser.—The author is not of opinion that the total phosphoric acid present in wine is in combination with soda.

Die Chemische Industrie.
No. 4, April, 1880.

Testing Petroleum for its Inflammability.—R. Haas.—The flashing is ascertained by the aid of electric sparks. The apparatus cannot be intelligibly described without the accompanying cuts.

Disposal of Furnace Smoke.—Prof. C. Winckler.—The author describes the methods adopted for dealing with the very sooty and sulphurous smoke of the ultramarine works at Bockau. Absorption in milk of lime and in coke-towers, down which water was allowed to trickle proved impracticable. The most satisfactory results were obtained by allowing the gases to pass through granulated limestone plentifully moistened with water.

Composition and Analysis of the Manganese Peroxide Regenerated According to Weldon's Process.—G. Lunge.—See Chemical News, vol. xli., pp. 129 and 141.

Manufacture of Sulphuric Acid.—A. Houzé.—The author brings sulphurous acid, watery vapour, and air in contact at high temperatures, but below redness, and condenses the product in a series of stone-ware vessels.

Technical Extraction of Phosphoric Acid from Mineral Phosphates.—A. Houzé.—After mechanical purification the phosphates are treated with hydrochloric acid, the solution is diluted with water, decanted or filtered, and the solution treated with sodium sulphate as long as any lime is precipitated. The filtrate contains the phosphoric acid along with sodium chloride, which is removed by crystallisation.

DIE LICHT-MÜHLE.

In einem leeren Runde
  Da geht ein Licht-Mühl' Rad
Die Luft ist d'raus verschwunden
  Die dort gewohnet hat.

Durch Pumpen und durch Kochen
  Bleibt fast nichts mehr darin.
Ach weh! Er ist gebrochen!
  Mein Glück ist nunmehr hin.

Ich möcht' als närriger Hatter
  Wohl ziehn von Ort zu Ort
Betrachtend Radiant Matter
  Or any other sort.

Seh' ich 'ne Licht-Mühl' gehen
  Ich weiss' nicht was sie will.
Ich kann es nicht verstehen
  Warum sie bleibt nicht still.

A. v. Nudeln.

THE CHEMICAL NEWS.

VOL. XLII. No. 1082.

ON PHOSPHORUS ESTIMATION IN IRON AND IRON ORES.

By Professor v. EGGERTZ.

THE *Verhandlungen des Vereins zur Beforderung des Gewerbfleisses*, 1877, p. 267, contained a prize paper on a process for the estimation of phosphorus, by O. Korschelt, Professor in the Medical School of Yeddo, in Japan (translated in *The Metallurgical Review*, No. 5, p. 469; also communicated to *Berg. und Huttenm. Zeitung*, 1877, p. 195).

In this process the author makes some observations against my note on phosphorus estimation, published in 1859, and has proposed several improvements on it.

Prof. Korschelt thus expresses himself on my method:—"This process suffers still from some imperfections which cause much trouble in its technical application," and chief among such are the following:—

1. "The evaporation of the iron solution before precipitation." For iron *ores* this evaporation is unavoidable. For iron this is neglected here, and is done only with cast-iron containing much silicon. The dissolving of the iron must always be performed with the greatest care.

2. "For precipitation it requires two or three hours." Where speed is necessary, the iron as well as the molybdate solutions may be previously warmed to 40°; the time required for precipitation can thus be shortened to one or two hours. The solution may be allowed to stand covered for six hours at 40°, but not longer with safety, from the danger of some free molybdic acid precipitating.

3. "Drying at 95° demands much time." In my process I mentioned that the yellow phospho-molybdate precipitate can be dried for several hours at 120° to 140°, without undergoing special change in composition. The alteration is so unimportant, that after drying at 95° to 1·63 per cent of phosphorus, then drying at 120°, it will be 1·64 per cent. This tells only on the third or fourth decimal in its percentage in iron, if the amount of phosphorus is not very great. With me the drying is very often at a higher temperature than the water-bath.

4. "As phospho-molybdate of ammonia has the property of losing ammonia in drying at a constant temperature of under 100°, I (Prof. Korschelt) have therefore, instead of molybdate of ammonia, used a solution of molybdate of potassa for precipitation of phosphoric acid, and thereby completely avoided the tediousness of Eggertz's process in precipitation and drying." If the variation in the composition of the phospho-molybdate of ammonia precipitate, and its drying, is only as that given above, what more is there to say about it?

5. "The process has a source of error which certainly may be overcome by long practice, but which causes every one who has worked out the composition of the phospho-molybdate of ammonia to obtain a different figure for its percentage than his predecessors."

In my published process complete and detailed directions are given for its practice, and deviations therefrom are not allowed. Working by it is so easy, and gives such good results, that the students of the Mining School—where this method for over twenty years has been exclusively used—after executing two or three phosphorus estimations on only 1 grm. of iron, seldom obtain greater differences than 0·01 per cent, and only variations of 0·002 per cent to 0·003 per cent for less amounts of phosphorus.

The amount of phosphoric acid given by me—3·74 per cent (1·63 per cent phosphorus)—as in the yellow precipitate obtained by exactly the same means as in phosphorus estimations in iron, is the average of a great number of estimations, with 3·7 per cent as the minimum, and 3·75 per cent as the maximum. Prof. Korschelt, with molybdate of potassa as the precipitant, has obtained on average 3·96 per cent phosphoric acid (1·73 per cent phosphorus).

Prof. Rammelsberg undertook estimations according to Prof. Korschelt's method, and by three trials arrives at the conclusion that the amount of phosphorus in the precipitate of phospho-molybdate of potassa must be taken at 1·768 per cent, instead of 1·73 per cent, if obtained from an iron solution, and at 1·847 per cent if from an iron free solution.

Prof. Korschelt prepares his molybdate solution from molybdic acid, caustic potassa, tartaric acid, and nitric acid. This mixture is boiled, by which some molybdic acid separates out. This is filtered off, and the filtrate preserved. Prof. Korschelt remarks, "that tartaric acid is subject to a slow decomposition; he therefore recommends that a molybdate solution which has been kept a long time, should have a small quantity of tartaric acid added before using."

The ammonia molybdate solution used by me commonly maintains its stability; only a little free molybdic acid separates after a long time. The use of tartaric acid as an addition cannot come into question, because this impedes the precipitation of the phosphorus, at least under the usual time of one or two hours, and also causes a great evolution of gas.

Prof. Korschelt heats his molydate solution on the water-bath before using, and, further, warms "a short time" after precipitating. When arsenic is present, heat only to 50°. The precipitate is taken on a weighed filter, and washed first with water containing 3 per cent nitric acid, and then with alcohol, dried afterwards at 120° to 130°, and weighed. So much nitric acid in the wash-water acts sensibly as a solvent on the precipitate, and this is also the case with alcohol. I add only 1 per cent nitric acid to the wash-water, and continue the washing only so long as an evaporated drop of the wash-water leaves a scarcely noticeable ring.

For the estimation of phosphorus in iron Prof. Korschelt prescribes as follows:—"Take about as much iron as that the yellow precipitate will weigh 0·5 to 0·75 grm. Therefore if the iron contains 1 per cent phosphorus about 1 grm. is required."

Besides the great inconvenience of requiring beforehand to know the probable amount of phosphorus present in the iron, it follows also when—as is often the case in Sweden—the iron has phosphorus only to the amount of about 0·01 per cent, 100 grammes must be taken for estimation instead of 1 gramme. To dissolve 100 grammes iron requires about 1000 c.c. nitric acid. But here Prof. Korschelt prescribes that "You ought never to allow the iron solution to exceed 50 c.c."!

Concerning the process Prof. Korschelt says further:—"If the heating is not continued until free molybdic acid begins to separate, there is thus found remaining in solution only a very small quantity of phosphoric acid. I have considered it unnecessary to estimate this amount; the loss thus resulting in every case cannot be of greater influence in the analyses made by this method than some hundreds per cent."

Such phosphorus estimations are, at least in relation to Swedish experiments, altogether unsatisfactory, because here the amount of phosphorus is seldom found higher than 0·1 per cent. The most common amount lies between 0·01 and 0·05 per cent.

It is now easy to see from the above that my method does not demand more labour and little more time than Prof. Korschelt's. By collecting the precipitate in a funnel tube, pressing it together, and measuring, as described in my process, *small* amounts of phosphorus can be estimated in much shorter time.

Prof. R. Finkener gives in *Berichte der Deutschen Chemischen Gesellschaft*, 1878, p. 1638, directions to dissolve the phospho-molybdate precipitate upon the filter, evaporate the solution, and then heat so strongly that the nitrate of ammonia is removed (at about 200°). The greater part of the water is therewith expelled, and the composition of the salt will be as follows:—

$3P_2O_5+72MoO_3+8NH_4O+H_2O$, or

Phosphoric acid	3·794 (P=1·66)
Molybdic acid	92·341
Ammonia	3·705
Water	0·160
	100·000

Prof. Finkener thus expresses himself upon the Korschelt process:—"It follows from what Prof. Korschelt prescribes that in an iron containing 0·1 per cent phosphorus, none will be found present."

In the *Journal of the Chemical Society*, March, 1878, Mr. Ed. Riley communicates the magnesia method used by him for the estimation of phosphorus in iron. In the hands of Mr. Riley it is without doubt executed with great accuracy. In comparing it with my process we find:—

I. That with reference to precision in weighing the precipitates, we require to use seventeen times greater amount of iron (17 grms.) because the ignited pyrophosphate of magnesia contains 27·95 per cent phosphorus, but the phospho-molybdate of ammonia only 1·63 per cent phosphorus.

II. That the magnesia method demands much more time, labour, and skill.

III. That besides nitric acid I use no other reagent which can contain phosphoric acid, while Mr. Riley uses a magnesia salt, citric acid, also acetate of soda, and sulphite of soda. These last-named reagents are exceedingly difficult to procure completely free from phosphorus; I have not been so fortunate.

In this circumstance lies, perhaps, the explanation that Mr. Riley, as he says, often obtains about 0·02 per cent higher phosphorus in iron than several other chemists.—*Jern Kontorets Annaler* (1880, part 6, p. 355).

ON THE

VARYING CONDITION OF CARBON IN STEEL, AND ITS INFLUENCE ON EGGERTZ'S COLOURATION PROCESS.

By J. SPEAR PARKER, F.I.C.

The ease and rapidity with which results can be obtained by means of Eggertz's colouration process for the estimation of carbon, have rendered its adoption almost universal in the laboratories of steel works. I believe, however, that most chemists using the process have occasionally noticed anomalous results, which have aroused suspicions as to its reliability, and some I know have used it while regarding the results obtained as only roughly approximate. For my own part, exercising minute care in every stage, I used it for many years in the belief that its results could be fully depended upon; and although my suspicions had been once or twice excited, I was not at all prepared for the very serious variations to which after a thorough investigation I discovered it was liable. The standard I at that time habitually employed was of crucible steel, and agreed in its colour indications with others in use in several large works in this country, and with one or two test-bars from Sweden with which I had compared it. But I first became convinced that something must be seriously wrong with the process on the receipt from my friend, Mr. A. H. Allen, of drillings from a sample of hard Bessemer steel in which he had carefully estimated the carbon by combustion, and which gave, when compared with my crucible steel standard colouration, results fully one-sixth too low. After a number of experiments with different samples of crucible and Bessemer steel of various tempers, by carefully observing the phenomena when the cold nitric acid was added to the steel drillings, and by numerous check estimations by combustion or chromic acid oxidation, I became convinced that carbon was capable of existing in steel in different conditions, and that steel containing the same percentage of carbon might give, when dissolved in nitric acid, a greater or less degree of colouration, according as the carbon was in one condition or another. Fortunately also I was able to apply a method of correction, which, although entailing additional trouble, rendered the process in all ordinary cases susceptible of a high degree of accuracy.

In most samples of Bessemer steel containing over 0·60 per cent of carbon taken from the ingot, the carbon exists in a form agreeing with the sample received from Mr. Allen, already alluded to; while, on the other hand, most samples of crucible steel contain it in a form which gives a higher colouration for the same amount of carbon—nearly in the ratio of 6 to 5. If to a decigramme of each of two such steels containing a similar percentage of carbon, an equal amount of nitric acid be added, the difference of appearance as they dissolve side by side is most striking. In the first case the colour of the liquid while the first rapid action is proceeding appears of an orange-*yellow*; in the second a dark Vandyke-*brown*, sometimes approaching black; and when the effervescence has subsided, the appearance of the residue, as yet undissolved, is equally distinctive—consisting, in the first case, almost entirely of small skeletons, retaining the shape of the fragments of steel from which they are derived, while, in the second, it is entirely of a light flocculent character, and very much darker in colour than the former. For the sake of simplicity I shall call the first the "yellow," the second the "brown" form of carbon. Were there only these two extremes there would be no difficulty about the matter; but any grade intermediate between the two may occur, and in rare cases even greater differences are noticeable. The following are examples of the difference in colouration results obtained in a few typical samples:—

By colouration ..	0·88	By chromic acid ..	0·873
,, ,, ..	0·73	,, ,, ..	0·733
,, ,, ..	0·61	,, ,, ..	0·615
,, ,, ..	0·605	,, ,, ..	0·595

In the above the carbon appeared in the "yellow" form, as was the case with the standard used throughout; but in the following samples it was in the "brown" form.

By colouration ..	1·26 (1·05)	By chromic acid ..	1·059
,, ,, ..	0·95 (0·79)	,, ,, ..	0·773
,, ,, ..	0·875 (0·73)	,, ,, ..	0·695

The figures in parentheses are obtained by deducting one-sixth from the figures given by colour test, and it will be noticed that in the last case an even greater deduction is required to obtain the true amount of carbon: so again, in a few cases a slight addition is necessary to the result obtained from the standard here employed, which does not represent quite the extreme limit of "yellow"ness. Taking the two most extreme points I have hitherto met with in actual practice (the steel being unworked, hammered, or rolled, but *not* hardened), the ratio of colour obtained from the same amount of carbon would be nearly as 3 to 4; but with very rare exceptions the variation does not exceed the limit of 5 to 6. By very careful observation of the indications when dissolving, followed by numerous check tests by gravimetric analysis, I was able to work out a scale of fractional additions and subtractions, by the application of which I was enabled to obtain results of a high degree of accuracy. This system I have employed during nearly two and a half years with most satisfactory results, and only when a high percentage of carbon is indicated from which a large deduction has to be made do I feel any hesitation in fully relying upon the exactness of the estimation thus obtained. It is impossible of course by mere description to enable other workers to apply such a scale; each observer must work one out for himself. This will require time and trouble, and its application considerably complicates the colouration process, but such a system is absolutely necessary if reliable results are required.

The carbon from the same piece of steel may be readily changed from the one modification to the other. If, for instance, an ingot of hard Bessemer steel, with its carbon in the "yellow" condition, be strongly heated and allowed to cool slowly, it will generally be more or less completely converted into the "brown" form; by the uncorrected colouration method it would appear to contain a higher percentage of carbon than before, but apply combustion and its amount will be found to be unaltered (except, of course, near the surface, where it may be sensibly diminished owing to superficial oxidation in the heating). On the other hand, a steel containing carbon in the "brown" form, if hardened, even though subsequently annealed so as to admit of drilling, will probably have it converted into the "yellow"; judging by the uncorrected colouration results the percentage of carbon would appear to have diminished, although the same amount is still actually present. As steel when hardened cannot be filed or drilled, no method occurred to me for a long time by which I could test quantitatively the effect upon the colouration results of the change in condition of carbon which takes place on hardening. The following device, however, enabled me to obtain satisfactory results. Drillings of steel, not too large to be subsequently dissolved in nitric acid for the colouration test, were closely packed in the fine powder from the same sample, and tightly wrapped up in platinum foil. The whole was then heated to a bright red and plunged into cold water. Although where air had penetrated the usual dark blue temper colour was shown, a large portion of the sample was so effectually protected from even superficial oxidation as to retain its original colour and lustre. The sample was immediately pressed between blotting-paper, and thoroughly dried in the water-bath. Fragments were then carefully picked out which retained their metallic lustre and natural colour (or at most a faint straw), and a decigramme was dissolved in nitric acid in the usual way. The first sample operated upon contained its carbon nearly in the brown condition, and the result of hardening was to convert it into the extreme yellow form.

By colouration—
Unhardened steel.. 1·00 }
Hardened ,, .. 0·815 } Actual carbon.. 0·86

But when I operated in a similar manner upon steel containing its carbon already in the "yellow" condition, I found that the colouration results after hardening were still nearly one-fifth lower (*e.g.*, 0·74 and 0·60 respectively), the carbon being evidently further modified in the same direction. In this case the colour of the solution, after action of cold nitric acid, appeared of a peculiar deep orange colour, while, instead of the very decided skeletons left undissolved in the unhardened sample, nearly the whole went into solution before heating, the very minute residue consisting of a few diminutive orange filaments. It is fortunately impossible to drill hardened steel, so that in practice one has not to contend against this further distinction; steel which is to be tested by the colouration process should never be hardened, as even if subsequently annealed I consider the results so obtained untrustworthy.

The action of nitric acid upon steel, although the most conclusive, is not the only proof that there is a real difference in condition of the carbon in various samples. Many observers had previously noticed that hydrochloric acid acts in a different manner upon steel in the hardened and unhardened state,* their results being well summarised by Prof. Akerman (*Journal of the Iron and Steel Institute*, 1879, pp. 504-6) who remarks:—"It thus appears that the carbon commonly called combined ought properly to be divided into two kinds, viz., first the carbon most intimately combined with the iron, which we, in accordance with Rinman's proposal, shall call *hardening carbon*, inasmuch as it characterises the well hardened steel; and, further, the carbon incompletely combined with the iron, which may be said to be in a sort of passage to graphite, and which Rinman called *cement carbon*, because it occurs in largest proportion in the undrawn blister or cement steel."

I have found, moreover, that when the steels have been treated with cupric salts (as a preliminary to combustion) the residue of carbon, &c., left undissolved, presents a totally different appearance, according as the carbon existed in the "brown" or "yellow" form. In the former case it remains as a powder in an extremely fine state of division, with a great tendency to penetrate through the asbestos filter when washed; in the latter it is much more bulky, and apparently consists of porous carbonaceous skeletons of the original drillings of steel.

What is the exact nature of the difference in condition of the carbon which gives rise to these various phenomena? It is because I have as yet been unable satisfactorily to solve this problem that I have so long delayed publication of my observations. The carbon in white cast-iron and steel is usually spoken of as "combined." Mr. Snelus considers that it rather exists in a state of solution. Now experiments made by MM. Troost and Hautefeuille showed that a pure white carburetted cast-iron gave a greater amount of heat when treated with mercuric chloride in the calorimeter than pure iron itself; hence the absorption of the carbon by the iron had been accompanied by absorption of heat, and its condition was physically analogous to a state of solution. But a carbide of iron and manganese, on the other hand, gave very much less evolution of heat with mercuric chloride, and indicated the existence of a true *compound* of carbon and metal. Is it not then possible that in what I have called the brown form the carbon is in a state physically analogous to solution, whereas in the yellow form it is really chemically combined with the iron? This appears to me the most probable explanation. As yet, however, it is only conjectural; but I intend shortly to examine microscopically the etched polished surfaces of a series of specimens I have been collecting containing the carbon in extreme forms, a method of investigation which I hope may enable me to solve the problem, or at any rate throw considerable light upon the subject. I thought it right, however, without further delay to draw the attention of chemists to a source of variation in the colouration process, which, if not duly corrected, most seriously affects its accuracy, a source of error which so far as I can ascertain has hitherto escaped detection.

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* I may take this opportunity of remarking that the statement so frequently reproduced that unhardened steel contains *graphite* which passes into the combined state on hardening is quite erroneous. I have never observed true graphite in the enormous numbers of hard steels which have passed through my hands, and I have made special search for it by treating large quantities of highly carburetted crucible steel with hydrochloric acid, but always with negative results.

VOLUMETRIC DETERMINATION OF LEAD.

By W. DIEHL.

The author employs a 1-20th normal solution of potassium bichromate containing 7·38 grms. per litre, each c.c. representing 0·01035 grm. lead, and a solution of sodium hyposulphite, containing 4 to 5 grms. per litre. To determine the relations between these two solutions, 20 to 30 c.c. of the bichromate are run into a flask, diluted with 300 c.c. water, and mixed with 20 to 25 dilute sulphuric acid (1 vol. monohydrated acid and 2 vols. of water). A certain excess of sulphuric acid is indispensable, hydrochloric acid being less convenient. The liquid is brought to a boil, and the solution of hyposulphite is added drop by drop. The solution becomes gradually paler in colour. Towards the end, after the addition of a few drops, it is let boil up again. The end of the reaction may generally be distinguished by the liquid turning perfectly colourless, a result occasioned by a single drop. In order to judge of the colour, the flask, towards the end of the operation, may be set in a porcelain capsule. When very large quantities of bichromate are used the liquid does not become perfectly colourless, but slightly greenish.

In determining ores in this manner they are dissolved in *aqua regia* and dilute sulphuric acid, the solution concentrated till the sulphuric acid begins to evaporate, diluted with water, boiled to dissolve ferric sulphate, let cool, and filtered through a smooth filter, washing with water containing sulphuric acid. To the residue in the flask—as little as possible of which is thrown upon the filter—is added about 15 c.c. of a solution of neutral ammonium acetate, and about 50 c.c. of water. The whole is then boiled, and filtered through the same filter into which a drop of ammonia has been put, into a flask. The same operation is then repeated with 5 c.c. ammonium acetate, and the residue is finally well washed with boiling water to which a little of the same salt has been added. Thorough washing is necessary, since filters have been found to retain ammonium lead acetate and tartrate with considerable obstinacy. It is then advisable further to wash the filter from its margin downwards with a little boiling dilute hydrochloric acid (1 part hydrochloric acid at sp. gr. 1·12 with 10 parts of water), and then to wash again with hot water. In this manner every trace of lead is removed from the filter. A thin filter-paper should be used, and should be washed previously. The solution of lead sulphate in ammonium tartrate thus obtained is then titrated in the cold with potassium bichromate; with the aid of heat ammonium acetate dissolves a little lead chromate. The precipitate settles readily if the flask is shaken, and the end of the reaction can be observed to within 0·2 to 0·4 c.c.

An excess of at least 2 c.c. of the chromate solution should be added in order to obviate the solubility of the lead salt. It is advisable in every experiment to take as closely as possible an equal quantity. After thoroughly shaking, it is allowed to stand for half an hour and filtered. If the liquid passes through turbid, a few drops of a solution of sodium acetate acidulated with acetic acid are added. If, after all, a little lead chromate passes through the filter, the filtration is repeated. The precipitate is washed four times with cold water, and the solution is acidulated with sulphuric acid and titrated as above.

Ammonium acetate is preferable to all other ammonium salts as a solvent for lead sulphate. It is applied in a neutral or faintly acid state. Free ammonia renders the solution turbid. 1 grm. lead sulphate requires 15 c.c. of the liquid acetate for solution. Ammonium tartrate cannot be used.—*Zeitschrift für Analyt. Chemie.*

FURTHER COMMUNICATIONS ON THE REVERSION OF FERRIFEROUS AND ALUMINIFEROUS SUPERPHOSPHATES.

By Dr. C. F. MEYER.

CONTINUED investigations on the reversion of superphosphates containing iron and alumina have shown the author that the conclusions drawn from his former observations are incorrect. If a concentrated solution of monobasic calcium phosphate is mixed with neutral ferric sulphate, an abundant precipitate of calcium sulphate is determined. This precipitate is formed slowly and is much impeded by the presence of free acids, so as to escape observation in the author's first experiments. Hence he assumed that the double decomposition of monobasic calcium phosphate and ferric sulphate must be preceded by a conversion of the former into bibasic calcium phosphate, with separation of phosphoric acid, effected by the application of heat or by diluting the solution. Hence, he concluded, that in superphosphates, monobasic calcium phosphate must first form a bibasic salt by the reaction of the tribasic salt before the double decomposition with ferric sulphate can take place. This assumption is incorrect, since monobasic calcium phosphate and ferric sulphate very soon form gypsum and acid iron phosphate, and the reversion of superphosphates is therefore due to the action of this acid phosphate upon other constituents of the superphosphate. It was therefore of importance to acertain whether acid iron phosphate reacts upon tribasic calcium phosphate in such a manner as to explain the reversion, or if a reaction of the acid iron phosphate upon other constituents of the superphosphate must be taken into account. If the action of the acid iron phosphate upon tribasic calcium phosphate is of such a nature that two molecules of the former withdraw two atoms of calcium from one molecule of tribasic calcium phosphate, the transformation of monobasic calcium phospate with ferric sulphate into gypsum and acid iron phospate, and the action of the latter upon tribasic calcium phosphate, will be unattended by a loss of soluble phosphoric acid.

It seemed more probable that acid iron phosphate withdraws only one atom of calcium from tribasic calcium phosphate, and that thereby a third part of the phosphoric acid, which was originally dissolved, is rendered insoluble. For the decision of this question, the following experiments were performed:—

1. To 6 c.c. phosphoric acid, containing 1·42 grms. P_2O_5, iron phosphate was added till a portion remained undissolved. To this solution were added 5 grms. tribasic calcium posphate, and after thorough stirring the mass was let stand for half an hour. The analysis showed 1·44 grms. phosphoric anhydride soluble in water.

2. The first experiment was modified in such a manner that only 2 grms. iron phosphate were employed, the other proportions being unaltered. (For the formation of acid iron phosphate, 1·88 grms. of the iron phosphate employed were required.) The analysis showed 1·56 grms. P_2O_5, against 1·42.

3. 1 grm. neutral iron phosphate was used, the proportions being otherwise similar. The result was 1·80 P_2O_5, in place of 1·42.

These experiments support the view just expressed, that acid iron phosphate withdraws from tribasic calcium phosphate only 1 atom of calcium, and thereby renders a third of the dissolved phosphoric acid insoluble; for in case of an action of 2 molecules acid iron phosphate upon 1 molecule tribasic calcium phosphate, the above experiment should have yielded 2·13 grms. P_2O_5 soluble in water. The author does not consider this action of the acid iron phosphate dependent on a modification of the acid properties of the phosphoric acid, but on the circumstance that after the first action of the acid phosphate upon tribasic calcium phosphate, neutral iron phosphate separates out, which coats the bibasic calcium phosphate and protects it against a further action of the phosphoric acid.

The result of this investigation may be thus briefly summed up: by the reaction of ferric sulphate upon monobasic calcium phosphate, there is formed an acid iron phosphate, and by its action upon tribasic calcium phosphate about one-third of the soluble phosphoric acid is rendered insoluble. By this process, the reversion of the ferriferous and aluminiferous superphosphates can be explained. It is still possible, however, that the acid iron phosphate may also react upon other ingredients of the manure, and that thereby the reversion of the phosphoric acid may be promoted and increased.—*Zeitschrift f. Analyt. Chemie.*

The Chemical Society.—We are happy to state that the Worshipful Company of Drapers have intimated their intention of continuing, at all events for the present, their annual subscription of 100 guineas to the Research Fund of the Chemical Society.

CONTRIBUTIONS TO THE CHEMISTRY OF BAST FIBRES.*

By E. J. BEVAN and C. F. CROSS.

(Concluded from page 80.)

Our attention is now called to the results of such an investigation of the character and constitution of the bodies associated with the celluloid basis of the jute fibre. Their relations to its structure are essential, for with their removal the continuity of the fibre is dissolved *pari passu*, until when completely removed it is resolved into a mass of its minute ultimate elements. Not only is this the case, but its remarkable affinity for colouring matters is entirely conditioned by the same group of substances.

Both of these important characteristics are demonstrated by the specimens on the table, showing the gradual loss of continuity and of dyeing capacity as we proceed from the raw fibre to the pure cellulose. The distribution of the intercellular body is remarkably uniform throughout the fibre. On the table are specimens of entire lengths treated in one case with a solution of aniline sulphate, which colours this fibre constituent a bright yellow, and in the other with a hot solution of an aniline-blue. The perfect uniformity of colour throughout in each case is an index of the corresponding distribution of intercellular matters, and this is fully corroborated by determinations of the percentage of carbon in the fibre at different points in its length. The technical bearings of these properties are clearly very important. They are essential conditions of the interesting developments which are now being made in the applications of the fibre, and at the same time serve to place it on the limits of the possibility of such textile application; moreover, they explain its typical character and peculiar fitness to serve as the medium through which to approach the chemistry of bast fibres generally.

The action of chlorine, which we have already witnessed, shall be our first pioneer. It is accompanied by a change of colour to bright orange, a marked evolution of heat, and the formation of a body soluble in alcohol and in glacial acetic acid. On throwing either solution into water we observe a copious precipitation of a flocculent resinous substance.

Our investigation of this body has shown it to be a definite compound. The following analytical results will prove this. (The compound was previously dried at 100°):—

I.

Chlorine—0·6145 gave 0·6575 AgCl+0·0068 Ag

Combustions (in stream of oxygen with usual precautions).

(*a*) 0·2645 gave 0·4115 CO_2 and 0·83 H_2O
(*b*) 0·3645 ,, 0·579 ,, ,, 0·1115 H_2O
(*c*) 0·3385 ,, 0·5305 ,, ,, 0·1015 ,,

	(*a*)	(*b*)	(*c*)
C	42·42	43·32	42·74
H	3·48	3·40	3·33

	Mean.	Calc. $C_{19}H_{18}Cl_4O_9$.
C	42·82	42·85
H	3·40	3·38
Cl.. ..	26·83	26·69

II. The product was then dissolved in glacial acetic acid, precipitated by water, washed, and dried; then dissolved in alcohol, precipitated by water, washed, dried, and analysed:—

Chlorine.. .. 0·2755 gave 0·2895 AgCl+0·0072 Ag
Combustion .. 0·336 ,, 0·5295 CO_2 and 0·105H_2O

C	42·97
H	3·48
Cl	26·82

III. A fresh specimen of jute was chlorinated and the product isolated as before. It was then exposed to an atmosphere of chlorine for some hours, purified, and analysed.

Chlorine.. .. 0·721 gave 0 7705 AgCl and 0·0082 Ag
Combustion .. 0·2923 ,, 0·4625 CO_2 and 0·09 H_2O

C	42·99
H	3·40
Cl	26·81

IV. A specimen of jute was chlorinated after exposure for some hours to the action of sulphuric acid (5 per cent) at 60° to 80°. (This latter will be discussed from another point of view hereafter.) The product was further treated as in III.

Chlorine (*a*) 0·3045 yielded 0·324 AgCl+0·008 Ag
(27·25 per cent).
(*b*) 0·3038 yielded 0·3312 AgCl+0·0056 Ag
(27·15 per cent).

Combustion—0·299 gave 0·4675 CO_2 and 0·095 H_2O.

C	42·64
H	3·52
Cl	27·2

These results sufficiently establish the definite nature of this body. The formula by which the numerical results in respect of the ratio of the elements are expressed shows the general agreement, which might be anticipated, with the interesting, but as yet unclassified, group of plant constituents, of which we may mention xantho-rhamnin, $C_{23}H_{24}O_{14}$, luteolin, $C_{20}H_{14}O_8$, rubian, $C_{28}H_{34}O_{15}$, aloine, $C_{17}H_{18}O_7$.

The presence of 4 atoms of chlorine is again in harmony with the tendency to form tetra substitution products which characterises certain of these bodies.

The reactions of this body are quite as striking as its composition. We have already mentioned its solubility in alcohol and glacial acetic acid; it is, further, slightly soluble in water. It is dissolved by concentrated sulphuric acid without separation of carbon, but is not re-precipitated on dilution. All these solutions possess the characteristic smell of tetrachloroquinone. Moreover, they are coloured violet on addition of ammonia, similarly to chloranil. Bearing in mind the presence of Cl_4 in the molecule of both, and a number of *a priori* considerations which will suggest themselves to those who have worked at vegetable chemistry, there is ground for regarding this chlorine derivative, or rather the body from which it is obtained, as a quinone.

But we have yet to demonstrate its most striking reaction. Plunging this piece of the chlorinated fibre into a solution of sodium sulphite, the orange-yellow slowly gives place to a magnificent magenta of great purity. This reaction is suggestive of very much that remains to be investigated: being but a recent observation we must content ourselves at present with this bare fact. We may, however, mention that it is a valuable point in the diagnosis of the chemical characteristics of fibres; it is observable with the encrusting matters of other bast fibres, flax, hemp, nettle, &c., in which they are present in far too small quantity to allow of quantitative investigations such as the jute fibre permits. Hence by means of our type we are enabled to infer the general chemical identity of bast fibre constituents.

The technological application of the reaction in the identification of bast in paper and textile fabrics we need not dwell upon.

We turn to consider a branch of this portion of our subject which establishes still further the general character of the previous results.

The material known as Esparto, so largely used by the paper manufacturer, consists of the leaves of a species of grass (*Macrochloa tenacissima*). In this case, therefore, we are dealing with a monocotyledonous plant. The first process to which it is subjected in the paper factory is that of heating under pressure with a solution of caustic soda. The brown liquors obtained in this way yield, when neu-

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* Read before the Owens College Chemical Society, April 16, 1880.

tralised, a fine flocculent precipitate. This we collected, purified by dissolving in glacial acetic acid, and precipitating by the addition of water, repeating this process once or twice, obtaining finally a brownish black resinous body, free from silica, and from a fatty acid (wax),* originally present in some quantity, and having the following ultimate composition :—

			Calc. $C_{21}H_{24}O_8$.
C	61·5 per cent	C	62·37
H	5·86 ,,	H	5·94
(N	1·2) ,,		

This body was chlorinated by the action of potassium chlorate and hydrochloric acid: the product was purified by solution in acetic acid and precipitation by dilution with water. After repeating this process once or twice, the product was washed, dried, and analysed :—

Chlorine (*a*) 0·336 gave 0·314 $AgCl+$0·0046 Ag (23·54 per cent).
(*b*) After further exposure to chlorine gas, and subsequent purification, 0·264 gave 0·2455 $AgCl+$0·0096 Ag (24·16 per cent).
Combustion—0·2768 gave 0·455 CO_2 and 0·096 H_2O.

		Calc. $C_{22}H_{23}Cl_4O_{10}$.
C	44·79	44·2
H	3·83	3·9
Cl	23·8	24·1

This body is not to be distinguished, qualitatively, from the chlorine product from jute ; its composition, moreover, is closely similar, the ratio C : H : O being practically the same, and four Cl atoms appearing in the formula. It is to be observed that, though formed under different conditions, both as regards previous preparation, chlorination, and oxidation, and, further, derived from a plant of very different botanical characteristics, it yet exhibits all the essential features of the chlorine body previously described ; and we may therefore predicate of these natural quinones conformity to a general type, considerable stability, and wide diffusion in plant structures. The bromine derivatives of these bodies we have not yet been able to investigate quantitatively: the qualitative resemblance to the chlorine products is extremely close. In concluding this part of our subject, we would mention that the presence of these quinone-like bodies in solution—as, for instance, in alkaline liquids in which vegetable matters have been boiled—is easily ascertained by adding bromine water (after previously acidifying), which precipitates the brominated compounds in yellow flocks. On bringing them in contact with ammonia, which may be added without filtering, the characteristic bright purple colouration is developed.

The next division of our subject is connected with the preceding by the observation that on exhausting the ichlorinated jute fibre with water there is dissolved, in addition to a small quantity of the chloro body, a substance which reduces Fehling's solution. The quantity of CuO reduced was determined in two instances, with the following results :—

(*a*) 0·3582 grm. jute. Washings after chlorination reduced, 0·1555 grm. CuO.
(*b*) 3·693 grms. jute. Washings ditto, 0·1607 grm. CuO.

which show that this body must be present in some quantity. Two questions at once present themselves touching its identity. Is it a sugar ? Is it metapeptic acid ? To these probable *a priori* solutions we must give an answer. We will not ask you, however, to follow us through all the steps of an argument which has exacted many months of work, and is still in progress, though shortly, we hope, to be completed. The latter probability entailed much work, leading only to negative results, through finding a paper of Kolb's in the *Bull. Soc. Ind. Mulhouse*, June, 1868, in which he claims to have established the presence of pectose in the linen fibre. Guided by this light we have searched closely for members of this group of bodies, of which pectose is the first member, amongst the products of decomposition of the intercellular substance of bast, but as yet with no results. The decompositions of the jute fibre to which we look for the satisfactory solution of this problem are the action of dilute acids and alkalies, and a curious decay or fermentation which is induced under certain conditions.

(1) *Action of Acids.*—By the action of sulphuric acid (5 per cent) at 60° to 80° a remarkable change is wrought in the fibre: the colour is changed from silver-grey to dark brown, the fibre is entirely disintegrated, becoming at the same time so brittle that it may be rubbed in a mortar to a fine powder. (It is a remarkable circumstance that the fracture of the fibre, which is thus brought about, takes place across the whole bundle of ultimate fibres, the union of these into bundles being unaffected by the acid.) But the most important result is the formation and solution of a body reducing Fehling's solution. This occurs at a temperature as low as 50° to 60°, but much more abundantly at 80° to 90°. To isolate the body formed, the solution was neutralised with chalk, filtered from calcium sulphate, and precipitated in fractions by basic lead acetate. These precipitates were decomposed by sulphuric acid, filtered, and the solutions digested on barium carbonate. On filtering and adding to a portion of the filtrate an equal volume of alcohol no precipitation occurred ; the absence of metapectic acid was therefore established. The filtrates were severally evaporated to dryness ; the residue was in each case dark coloured, viscous when hot, but becoming brittle on cooling. It was broken up and exhausted with alcohol ; a large proportion of the mass was dissolved ; the solution was filtered, and after distilling the alcohol a viscous body was obtained, brown-coloured, smelling strongly of burnt sugar, containing 2·6 per cent of ash constituents, but no trace of barium. We have made analyses of this body, with the following results :—

* (*a*) 0·292 gave 0·155 H_2O+0·5075 CO_2
(*b*) 0·3322 ,, 0·178 H_2O+0·5615 CO_2

	(*a*)	(*b*)	Calc. $C_{12}H_{18}O_9$.
C	46·29	46·08	47·05
H	5·75	5·95	5·88

* Deducting ash.

The formula to which these numbers approximate is that of caramel ; but as the product is still under investigation we defer any final statement as to its composition. Still there can be little doubt that it is a carbohydrate, and it is certainly a neutral body. To what portion of the fibre does it owe its origin ? On this point we submit the following results :—

A specimen of raw jute digested with the dilute acid for forty-two hours at 60° to 90° lost 23 per cent in weight. Two specimens of the same portion of fibre, after undergoing the same treatment, were treated by Müller's method for the isolation of the cellulose, and they were found to yield 58·9 and 57·9 per cent respectively (we have a number of similar determinations giving identical results, but these it is unnecessary to detail). As the quantity of "aggregate" cellulose obtained directly from the raw fibre, by the same method, was 70 to 71 per cent, 12 per cent of the loss sustained under the acid treatment is accounted for, *i.e.*, is due to the conversion of a portion of the cellulose itself into a soluble modification. Direct experiments on cellulose supported this conclusion. We append the results of a comparative investigation of this point :—

(*a*) Digested 18 hours at 60°—80° with 7 per cent H_2SO_4.
(*b*) ,, 16 ,, ,, ,, ,, ,,
(*c*) ,, 12 ,, 80°—90° , ,,

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* A specimen of this wax, after purification, was found to contain—

			Calc. $C_{17}H_{34}O_2$.
C	75·38	C	75·5
H	12·3	H	12·5

	Loss per cent.		
	(*a*)	(*b*)	(*c*)
Rhea (China grass), cellulose ..	2·8	—	—
Cotton	2·7	3·6	—
Jute	12·0	(1) 8·4	(1) 9·9
		(2) 6·8	(2) 9·5

The cellulose in each case underwent a structural modification similar to that which we have described in the case of the raw jute fibre, *i.e.*, it was entirely disintegrated, breaking up under pressure, with a brittle fracture, into a fine powder. An investigation of the acid solution after digestion upon the raw fibre shows that traces only of the aromatic constituents of the fibre have been dissolved; so also after prolonged digestion at 110° in a sealed tube. Moreover, it will be remembered that on chlorinating the fibre after the action of the acid a product was isolated identical with that obtained from the normal fibre. The modifying action of sulphuric and hydrochloric acids we regard, then, as chiefly confined to the carbo-hydrate constituents of the fibre. But, reverting for a moment to the numbers expressing the loss of weight by the raw fibre under the action of the acid (25 per cent), and a consequent diminution of 12 per cent in the yield of cellulose, the residual 11 per cent, if a carbo-hydrate, must have been originally present in what we have termed the intercellular substance. Can this be of the nature of the glucosides? That a quinone should be associated with a sugar is by no means improbable—the chemistry of the madder root affords a typical instance of such a union; but that the integrity of the jute fibre should be bound up with that of a glucoside is a hypothesis which its practical stability tends to negative. (2) There is nevertheless an interesting species of *fermentation*, a case of which has recently come under our notice, which takes place when masses of jute are allowed to lie in the damp state, in consequence of which the fibre undergoes a similar dissolution to that which is induced by hot dilute acids, and which, together with what has gone before, will, we think, eventually lead to an adoption of a modification of the glucoside hypothesis as representing the actual chemical condition of the intercellular body. In examining a specimen of jute which had become entirely disintegrated in consequence of this fermentation, we found that it contained as much as 10 per cent of matter soluble in water, made up of both aromatic and carbohydrate bodies, the solution reducing CuO copiously. On exhausting with a boiling solution of caustic soda (1 per cent) as much as 23 per cent of the weight of the fibre was removed in solution. Further, the percentage of cellulose determined by Müller's method, was found in two specimens to be 60·4 and 58·8 respectively. The decomposition which had taken place resembled, therefore, that which is effected by hot dilute acids in every particular, with the exception that in the latter case traces only of the aromatic constituent are rendered soluble. Not having yet completed the more special investigation of the products of this natural resolution of the intercellular substance, we can apply it in this discussion only in its general features; these are such as to indicate an intimate association of the aromatic constituent with some carbohydrate. This view is still further supported (3) by a study of the *action of alkalies* on the fibre. The loss of weight experienced by the fibre when treated with a solution of caustic soda of course varies with its strength and temperature, and the duration of its action. A cold solution (1 per cent) removes 1 to 4 per cent: with boiling solutions the following results have been obtained:—

Loss of Weight.	Duration of Boiling.	Strength of Solution.
6 per cent	5 minutes	1 per cent
7·5 ,,	1 hour	1 ,,
8·7 ,,	4 hours	1 ,,
12·8 ,,	10 minutes	5 ,,
12·8 ,,	1 hour	5 ,,
14·6 ,,	5 hours	5 ,,

The solutions acquire a brown tint. On neutralising with an acid they give a flocculent precipitate soluble in alcohol, and therefore not pectic acid, which, moreover, on treatment with chlorine and sodium sulphite gives the reaction characteristic of the aromatic constituent. The quantity of this dissolved does not, however, appear to be large. The alkaline liquid does not reduce CuO directly, but after acidifying and boiling a copious reduction occurs. This is additional evidence of the existence of a carbohydrate in the fibre other than the more stable modification of cellulose. We will briefly summarise the points of this discussion before passing to the conclusion which is to be drawn from them.

(*a*) Chlorination of the aromatic constituent is accompanied by liberation of a body which reduces CuO, which does not decompose $BaCO_3$, and which is soluble in absolute alcohol.

(*b*) After the re-solution of the intercellular basis of the fibre by fermentation, water removes a soluble modification of the aromatic constituent together with a body reducing CuO.

(*c*) By the action of the dilute mineral acids (HCl, H_2SO_4), the fibre is disintegrated as in (*b*), and there is dissolved a carbohydrate resembling caramel in its properties, and having nearly the same percentage composition. The formation of this body occurs as low as 60° C., and takes place copiously at 80° to 90°; it cannot, therefore, be formed from cellulose as ordinarily understood. The examination of the acid solution, after digesting upon the fibre, by means of fractional precipitation, showed the presence of an aromatic body in small quantity (first ppt.), but with this exception, of carbohydrate products only.

(*d*) Under the action of boiling dilute alkalies the aromatic constituent is dissolved to some extent, and at the same time a body which, after boiling with a dilute mineral acid, reduces CuO.

(*e*) The average of our determinations of carbon in the raw fibre is 46·5 per cent. From the examination of the resinous body obtained from the intercellular substance of the Esparto fibre, and comparison of the chlorine product which it yields with that obtained from jute, it is evident that the formula of the aromatic constituent of the latter must approximate to $C_{20}H_{23}O_9$. Now the percentage of isolable cellulose in the fibre is 70—75; if the residual 30—25 per cent were entirely aromatic, the percentage of carbon should be at least 49. Finding that it is less than this by 2·5 per cent, and also that the re-solution of this portion of the fibre is attended by the formation of a body having the properties of a carbohydrate, the source of this product is, we conclude, first, a body of comparatively low carbon percentage, and secondly, a carbohydrate.

(*f*) The cellulose obtained from the fibre, by the action of halogens and alkalies, is not chemically homogeneous; moreover, the percentage yield varies with the several methods by which the intercellular substances may be removed. We have, therefore, as before stated, reason for believing them to contain a body allied to cellulose. These facts are explained by the hypothesis that the intercellular substance consists of a complicated quinone which is intimately associated with a carbohydrate. The stability of this union, in consequence of which it is only slowly acted upon by boiling acids, even resisting to a large extent the prolonged action of 5 per cent H_2SO_4 at 110° (for on subsequent chlorination a fresh quantity of the body which reduces CuO is liberated), is explained by the supposition that the carbohydrate is more closely allied to cellulose than to glucose. There are, moreover, evident *a priori* grounds for believing in the existence of a group of *cellulides* analogous to the glucosides, of which *celluloquinones* would be a subdivision; and the evidence afforded by our investigation leads us to such a view of the constitution of the intercellular substance of bast fibres. The results of the technical application of our researches to the treatment of jute for the production of the beautiful fabric called Kalameit are singularly in harmony with this view. To enable this fibre to withstand the oxidising action of the colour-mixtures, and of the superheated steam (105°) employed by the printer will suggest itself as a somewhat curious problem. That this can and has deen done the specimens on the table will show. As, however, the process is kept strictly secret we are not in a position to lay before you the additional evidence which it affords. Assuming that our hypothesis, as far as it goes, is confirmed by the further issues of our investigations, we doubt not that it will need to be considerably extended before it embraces the whole truth. For in addition to the observations forming the subject of what has gone before, there are others which we have made and not yet further investigated, and which cannot be explained by the knowledge we have thus far obtained. *E.g.*, (1) On boiling the fibre with water (or, better, dilute H_2SO_4) a small quantity of a solid body distils, resembling wax in appearance, and having the curious penetrating odour which is characteristic of ordinary jute goods. It is, moreover, highly probable that the distillate contains furfurol. To investigate these points it is our intention to operate with large quantities of material. (2) On exhausting the fibre, previously purified by boiling in ammonia, with hot alcohol, a bright green solution is obtained, resembling the alcoholic solution of chlorophyll. On evaporating the solution, small masses of a bright green resinous body are left, but the quantity is so minute (the loss of weight sustained by the fibre being scarcely perceptible) that we have not been able to submit it to further investigation. The spectroscopic examination of the solution we have had to defer.

With all the reservation, therefore, proper to inferences from work which is in progress, we are of opinion that the main portion of the intercellular substance of the jute fibre is a *celluloquinone*. Even should our future work fail to confirm this view, it has both given definiteness to our investigations and been productive of results; it therefore fulfils at least the demands of a good working hypothesis.

There still remains to bring before you the results of the study of the action of nitric acid on the constituents of fibres. The conversion of jute-cellulose into nitro-cellulose was undertaken for me by Mr. Brown, of the Government Gun-cotton Factory. He reported that it yielded a perfectly satisfactory nitro compound, but was not prepared to pronounce an opinion on the influence of its state of aggregation upon the combustibility of the product.

The action of the acid in dilute condition (5 per cent at 60° C.) upon the fibre is to effect the removal of the intercellular and encrusting matters, dissolving them to a bright yellow solution, which reduces Fehling's solution. We have several times attempted the separation of the products, but without success: we have therefore studied the action of the acid upon the resinous body obtained from Esparto, which we have shown to be closely similar to the aromatic portion of the jute intercellular; and as regards the carbohydrate portion, we assume that it will undergo a similar modification by this acid (in the dilute form) as by the other mineral acids.

The resinous body ($C_{21}H_{24}O_8$) was heated with nitric acid (50 per cent) until it was entirely converted into soluble products. The solution was diluted and filtered from a brown resinous body thereby precipitated, and evaporated to a small bulk. On cooling, some quantity of oxalic acid crystallised: the solution was decanted, neutralised with lime, and after filtering precipitated by basic lead acetate. This was decomposed by sulphuric acid, and the solution, filtered from lead sulphate, was boiled with barium carbonate. On filtering from the excess of the latter, concentrating and adding alcohol, a bright yellow body was precipitated. This body, the barium compound of the chief product of the action of the nitric acid on the resin, was washed with alcohol; dissolved in water, and reprecipitated by alcohol. Carrying this out in five fractions, we found the percentage of barium invariable. They were united, and the process once more repeated. The compound, dried at 100°, was analysed, with the following results:—

Ba 0·428 gave 0·299 $BaSO_4$.
N 0·960 gave after ignition platino-chloride 0·0772 pt.

Combustions—

(*a*) 0·133 $H_2O + (0·463 + 0·084^*)CO_2$ from 0·624
(*b*) 0·171 $H_2O + (0·588 + 0·107^*)CO_2$ from 0·8044

	(*a*)	(*b*)	Calc. $C_{25}H_{31}NO_{25}Ba_4$.	
C ..	23·91	23·58	C ..	23·20
H ..	2·36	2·36	H ..	2·40
Ba..		41·47	Ba ..	42·36
N ..		1·13	N ..	1·08

* Determined in ash.

The calcium compound was similarly prepared, and on analysis gave results in accordance with the above.

Ca (*a*) 0·339 gave 0·199 $CaSO_4$ (*b*) 0·235 gave 0·138 $CaSO_4$.
N 1·075 gave after ignition of platino-chloride 0·1101 pt.
CH 0·6735 gave $(680 + 0·126)\ CO_2 + 0·213\ H_2O$.

	(*a*)	(*b*)	Calc. $C_{25}H_{21}NO_{25}Ca_4$.
Ca.. ..	17·25	17·27	17·67
N	1·42	—	1·54
C	32·63		33·14
H	3·50		3·42

The free acid is, after drying at 100°, a dark-coloured gummy body, easily soluble in water to a bright yellow solution, which is bitter to the taste and dyes animal fibres a rich gold. It is evidently an aromatic compound, and possesses the general features of the group of quinones, of which it is a derived member: but beyond this we have not yet been able to investigate it.

With this we conclude the account of our investigations. They are as yet exceedingly imperfect, as is also our handling of them in relation to the theoretical questions involved; but in endeavouring to lay them before you we feel that we have undergone a discipline which will bear fruit in our future efforts.

It only remains for us to say a few words upon the existing literature bearing upon our subject. A brief notice must here suffice; we reserve the critical discussion which it invites for a more suitable occasion.

To Dr. Müller's "Pflanzenfaser," an invaluable work, and the only one which gives a survey of the general chemistry of plant fibres, we are much indebted. To this chemist, also, we owe valuable suggestions as to the nature and manipulation of the products through which the chemistry of these fibres is to be studied. Amongst these we may mention the observation (unpublished) of the presence of a quinone in bast fibres.

On the special chemistry of the jute fibre nothing appears to have been published. Our attention has been directed by Prof. Schorlemmer to certain papers of Erdmann's in the *Ann. Chem. Pharm.* (138-1 and Sup. 5-223), on "The Stony Concretions of Pears," and "Glycolignose" a constituent of pine wood), in which the author arrives at conclusions somewhat similar to those which we have advanced in the case of jute. The researches of Fremy, Girard, and others (*Comptes Rendus*) on cellulose and its modifications; the observed cases of conversion of carbohydrates into members of the pectic group (Divers, *Journ. Chem. Soc.*, Schoonbroodt, *Bull. Soc. Chim.*, &c.), we are investigating in their special relations to our subject. Some years will probably elapse before we shall have made out the chemical characteristics of these structures with definiteness. It is needless to remind our fellow students of the deep interest attaching to our work, far more than compensating for the failures and difficulties which we have had in turn to encounter.

We desire to express our thanks to the Barrow Flax and Jute Company, Messrs. Cowan and Co., of Musselburgh, and Mr. Bonsor, of the China Grass Company, Wakefield, for the loan of specimens in illustration of this paper. Also to Dr. Forbes Watson for a quantity of Rhea, to which fibre we hope soon to be able to turn our attention.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Note.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 3, July 19, 1880.

Researches on the Organic Alkalies.—M. Berthelot.—The author has determined the formation-heat of such organic alkalies as are capable of becoming gaseous at common temperatures. He finds that if ammonia and trimethylamine are brought in contact with equal equivalents of dilute hydrochloric acid, one-third of the trimethylamine is displaced by ammonia with disengagement of heat, whilst reciprocally two-thirds of the ammonia are displaced by trimethylamine with absorption of heat.

Atmospheric Electricity.—M. Mascart.—There is only one minimum during the day, and one maximum, which is almost constant during a great part of the night; that is to say, a single diurnal period. The amplitude of the diurnal oscillation is notably slighter during the winter.

Alternating Currents and the Electromotor Force of the Electric Arc.—J. Joubert.—The resistance of the arc is very feeble; it varies with the temperature and diminishes when the temperature rises. The difference of the potential between the two charcoal points is due chiefly to an electromotor force independent of the intensity, and which may be valued at 30 volts. The two points act towards each other like the two electrodes of a galvanometer.

A New Air Thermometer.—A. Witz.—The description of this instrument does not admit of useful abridgment.

Certain Fluorised Compounds of Uranium with the Alkaline Metals.—A. Ditte.—The action of a neutral fluoride of the alkaline metals upon the green oxide of uranium gives beautiful insoluble and anhydrous crystals of the formula $U_2O_2F,2MF$; whilst the action of the acid fluoride of the same metals produces soluble, hydrated salts, the composition of which may be expressed by the formula $U_2OF_2,2MF,nHO$.

Atomic Weight and Principal Properties of Glucinum.—L. F. Nilson and O. Pettersson.—The authors give for the atomic weight of glucinum (O=7·98) 4·542 as the mean of four determinations, former experimentalists having made it from 4·6 to 4·9. If glucina is Gl_2O_3 the atomic weight of the metal will be 13·65, in which case the atomic heat becomes completely normal.

Certain Compounds belonging to the Group of the Creatines and Creatinines.—E. Duvillier.—The compounds described are α-oxy-butyro-cyamine, α-oxy-butyro-cyamidine, iso-oxy-valero-cyamine, and iso-oxy-valero-cyamidine.

Action of Ethyl-chloride upon the Ethyl-amines.—E. Duvillier and A. Buisine.—The action of ethyl-chloride upon ammonia, studied by Hofmann, and upon the ethylamines formed during that reaction, renders it possible to obtain the four ethylated bases in large quantity. The reaction is more regular than that of the methylic ethers upon ammonia and upon the resulting methyl-amines.

Action of Electrolysis upon Benzol.—A. Rénard.—The author has obtained a secondary glycol, $C_6H_6(OH)$, which he names iso-benzo-glycol. It is soluble in water, alcohol, and ether; melts at 171°, and sublimes, with decomposition at 200°. Its solution is reduced by cupro-potassic liquor.

A Peculiar Alteration of Butcher's Meat.—M. Poincaré.—The author in examining a series of meat, condemned at the abattoir of Nancy, has detected heterogeneous elements, which appear to him to be parasites not yet classified, and which he has figured and described. He considers that they present a certain analogy with gregarines, and asks whether they may not be one of the phases or metamorphoses of the tænioids, and if it is not through their mediation that raw beef gives rise to tænia in human subjects.

Biedermann's Central-Blatt für Agrikultur-Chemie. Part 4, April, 1880.

Influence of Forests upon Rain-fall and Attraction of Watery Vapour by Pine Trees.—M. Fautrat.—The atmospheric strata over forests, and especially over pine trees, is distinctly richer in watery vapour than are strata at the same elevation over the bare ground.

Behaviour of the Natural Soil and of the Plants Rooted in it towards Water.—Dr. G. Havenstein.—Not suitable for abstraction.

Action of Fallows.—The fluctuations of temperature are much greater in fallow-land than in fields under crop. The amount of moisture is higher in the former even if frequently turned over.

Manurial Experiments on Rye.—Prof. E. Heiden.—Nitrogen in the form of nitric acid, gave a much better result than in the form of ammonia.

Cultivation of the Yellow Lupin.—Dr. Ernst Wein-—The lupin, without receiving nitrogenous manure, is capable of producing a large amount of nitrogenous tissues.

Should Potatoes be Manured with Bone-dust?—Dr. Meyer.—The total yield of the crop was found to be increased.

Chemical Researches on Woody Papilionaceous Plants.—Prof. P. Fliche and Prof. L. Grandeau.—From analyses of the ash of the laburnum, furze, acacia, and *Sarothamus vulgaris*, the author finds that plants belonging to one and the same natural family, and growing on the same soil, may differ greatly both in the quantity and distribution of starch, the proportion of ash and of nitrogen. The laburnum, though preferring lime soils, does not show a decidedly greater proportion of lime than the acacia.

Free Tartaric acid in Wine.—Prof. J. Nessler and H. Wachter.—The authors consider that genuine wines do not contain uncombined tartaric acid.

Determination of the Fatty Matter of Milk in Dairies.—Dr. P. Vieth.—The author rejects the cremometer and recommends the lacto-butyrometer.

Experiment with Feser's Lactoscope.—Ph. du Roi.—The author, after a series of eighty comparative experiments, finds the results of the lactoscope too low. In 45 cases, the fat was given as too little by 1 per cent; in 2 cases by 1½ per cent or upwards; in 26 cases by 1½ to 1, and only in 9 cases did the deficiency fall below ½ per cent. Considerable differences were found when two operators examined the same sample.

Part 5, May, 1880.

Culture of the Sugar Beet.—A. Ladureau.—Taken from the *Annales Agronomiques*.

Nutritive Value of Acorns.—H. Czubata.—The proteine compounds are small in proportion, and the dry matter contains nearly one-half of woody fibre.

Digestibility of Oat-straw, Pea-straw, and Meadow-hay.—Prof. E. v. Wolff, Prof. W. v. Funke, and Dr. C. Kreuzhage.—The nutritive value of pea-straw appeared equal to that of clover-hay. Oat-straw is inferior both in the proportion of nutritive matter and in digestibility.

Some Investigations on Hay.—W. Dircks.—Analysis of Norwegian samples of hay.

Transformations of Starch and Glycogen by Diastase, the Ferments of the Pancreatic Saliva and of the Liver, as in the Animal Organism.—F. Musculus, J. von Mering, and E. H. Bimmermann.—Saliva and pancreatic ferment yield with starch the same products as diastase. With glycogen the same result is produced by the same ferments. The action of diastase upon glycogen is less energetic than that of saliva. In the blood, soluble starch is transformed; among the products of decomposition, grape-sugar and dextrine were distinctly recognised in the urine. The presence of maltose was doubtful.

Formation of Albumen in Plants.—Dr. A. Emmerling.—It appears that the green leaves are especially the place where the nitric acid is converted into nitrogenous organic compounds. Ammoniacal salts play in comparison a subordinate part.

Beet-root Jelly.—Ph. von Tieghem.—Mention is made of a plant, *Leuconostoc mesenteroides*, which is developed in beet-root juice and inverts the sugar.

Culture Experiments with Varieties of Potatoes.—Dr. P. Wagner and Dr. W. Rohn.

Injury to Peas and Beans by the "Seed Beetle."—E. Wollny, A. Meyer, H. Grosjean, and Beal.—The insect in question is not named, but it is probably *Bruchus pisi.*

Sorghum Saccharatum.—Dr. A. v. Wachtel.—An account of the cultivation of sorghum in America and of some experiments tried with it in Bohemia.

Combustibility of Certain Kinds of Tobacco.—Prof. E. Quajat.—In the more combustible growths of tobacco, the potash salts predominate over the others. The proportion of nicotine had no influence.

Malto-dextrine.—Dr. A. Herzfeld.—The composition of malto-dextrin is $C_{18}H_{32}O_{16}$. It is capable of combustion and contains 12 acetyl groups to 18 atoms of carbon.

Methods of Determining the Dry Matter and the Fat in Milk, based upon Calculation.—Dr. Behrend, Dr. Morgen, F. Clausnitzer, and Prof. A. Meyer.—The two former chemists determine fat by the lacto-butyrometer, take the specific gravity, and find total solids by means of certain algebraic formulæ. The two latter authorities determine specific gravity and dry matter according to Schulze and Krämer's method and calculate the fat by a formula.

The "Stall-test" in the Supervision of Milk.—Ph. du Roi and Prof. Kirchner.—In prosecutions for the adulteration of milk, if a difference of opinion appears, the cow or cows alleged to have yielded the article are milked again in presence of an official, and the sample thus obtained is analysed by the same method as the first sample. If the second milk turns out decidedly better, it is considered evidence of adulteration.

Experiments on Skimming Milk by Swart's Process.—Dr. M. Schrodt.

A New Skimming Process.—Prof. Ad. Meyer.

Experiments with Levol's Separator.—Dr. N. Engström.

(The nature of these three papers appears sufficiently from the titles.)

On Condensed Milk.—Dr. E. Wein.—The author finds that, on account of the great proportion of sugar, the fat in such milks cannot be determined in the usual way. He stirs up 5 grms. of the condensed milk in a capsule with continually repeated doses of ether, which are rinsed into a flask through the same filter. He then adds to the milk sea-sand and extracts repeatedly with ether, breaking up the clots. The ethereal solution is evaporated to dryness, and the residue re-dissolved in ether to get rid of any milk sugar which may have been dissolved.

Experiments on Churning Milk and Cream.—Dr. M. Schrodt and Ph. du Roi.

Albumen of Milk and the Formation of Curd.—G. Musso and Cl. Menozzi.—The albumen present in milk is from 0·572 to 0·604 per cent. In composition it is almost completely identical with the albumen of blood. In its behaviour with precipitants it holds an intermediate place between blood albumen and casein. The acid required for the precipitation of casein decreases with the rise of temperature. At 130° to 150° no acid is required.

Preparation of Cheese, with Analyses of Danish Cheese for Exportation.—V. Storch.—The lower the temperature of coagulation the more watery is the curd.

Gluten Bread.—Prof. K. Birnbaum.—Results of the analysis of samples from Paris and Munich.

(We regret to learn from a notice inserted by the publisher, the death of Dr. Biedermann, the founder and editor of the *Central-Blatt.*)

Justus Liebig's Annalen der Chemie,
Band 202, Heft 3.

Phlobaphene, Oak-red, and Bark-tanning.—Dr. Carl Böttinger.—Phlobaphene and oak-red are identical. The substance, when isolated, is a reddish brown powder, insoluble in boiling water, cold alcohol, and boiling benzol; slightly soluble in boiling absolute alcohol; and tolerably soluble in a solution of the tannin of the oak, and in the aqueous solutions of the alkalies. By melting potassa, it is converted into protocatechuic acid and phloroglucine. In tannin with bark, phlobaphene is the true active principle, the tannin serving merely as a medium for its incorporation with the hide.

New Synthesis of the Acids of Carbon.—A. Geuther.—By the action of carbonic acid upon a mixture of sodium ethylate and sodium valerinate at 190°, there are formed, in addition to ketones, the following acids:—Ethyl-iso-propyl-acetic acid, ethyl-diethenyl-iso-propyl-acetic acid, ethyl-triethenyl-iso-propyl-acetic and probably ethyl-octaethenyl-iso-propyl-acetic acid.

Communications from the Chemical Laboratory at Greifswald.—These consist of a memoir on an azo-sulpho-benzolic acid, by Dr. Mahrenholtz and Dr. Gilbert, a paper on azo-oxysulpho-benzolic acid by Dr. C. Brunnmann; reseaches by W. Balentine on the diazo-compound of hydrazo-sulpho-benzolic acid; and a memoir on dibrom- and tetrabrom hydrazo-sulpho-benzolic acid.

Communication from the Laboratory of the University of Halle.—These communications include a paper by Max Barth, on the combination of alumina with carbonic acid and ammonia, and a notice on diethyliden-lactamidic acid, by W. Heintz. Herr Barth considers that the existence of an aluminium carbonate is not yet demonstrated.

Journal für Praktische Chemie.
Nos. 7 and 8, 1880.

The Behaviour of Sugars with Alkaline Solutions of Copper and Mercury (Conclusion).—Dr. Soxhlet.—If the reductive power of a sugar with Fehling's solution is smaller than that of grape sugar this applies also to Knapp's and Sachsse's solutions. The contrary holds good with inverted sugar and levulose. The proportion found for one of the three solutions does not apply to the others. Milk sugar, however, shows an equally diminished reductive power with the solutions of Fehling and of Sachsse. The two mercurial methods have no advantage over Fehling's method as regards accuracy, certainty, and convenience. The author gives the preference to Fehling's process, since it admits of the use of larger quantities of sugar, and since the point when the reduction is completed is most distinctly marked. The mercury methods are, however, of use when it is required to ascertain the identity of a sugar, or to determine two sugars occurring together.

Nitro-ortho- and Nitro-para-azo-phenetols.—Dr. Hans Andreæ.—During the nitration of ortho-azo-phenetol there are formed two isomeric dinitro-azo-phenetols, whilst from parazo-phenetol there may be obtained two isomeric trinitro-azoxy-phenetols and a dinitro-phenetol. The nitro groups introduced into ortho-azo-phenetol effect the conversion of the azo into the hydrazo compound, but by reason of their negative character they prevent the conversion into the isomeric diphenyl base.

On Ethylated Ortho amido-phenetols and Ortho-amido-phenols.—Dr. Förster.—This paper does not admit of useful abstraction.

Chloro-sulphonic Ether.—P. Clæsson.—A reply to the strictures of Dr. Behrend (vol. xx., p. 382).

Antiseptic Action of Salicylic Acid.—A. Schultz.—The author gives tables showing the what kinds of substances and in what proportion are able to combine with salicylic acid.

Correspondenz-Blatt des Vereines Analytischer Chemiker.

No. 8, April 15, 1880.

Apparatus for Testing Petroleum.—Prof. C. Engler.—This memoir cannot be usefully abstracted without the accompanying illustrations.

Universal Spectroscope for Chemical and Physical Laboratories.—C. H. Wolff.—Also an illustrated paper.

Examination of Soil for Coal-gas Products.—E. Königs.—An examination of earth which had become saturated with coal-gas. The author worked up samples of soil into a paste acidulated with sulphuric acid and passed a current of steam into the mixture. The gases and vapours given off were collected in glass receivers. The bulk of the product consisted of naphthalin. Carbolic acid was not obtained in sensible quantity.

Die Chemische Industrie.

No. 5, May, 1880.

Technical Analysis and Valuation of Mond's Sulphur Lyes.—Dr. K. Jurisch.—The three following tests are recommended:—(1.) Take 3·2 c.c. of the sulphur lye, add a solution of sodium acetate, and then an excess of solution of zinc sulphate; dilute to 200 c.c., filter off 100 c.c., add a little starch mucilage, and titrate with a decinormal solution of iodine, and multiply the number of c.c. of the iodine solution required by 2. (2.) Take 3·2 c.c. of the same sulphur lye, add starch mucilage, and titrate with decinormal solution of iodine. (3.) Decolourise the liquid obtained in the last test by adding a drop of a solution of sodium dithionite; add some tincture of litmus and titrate with decinormal caustic potassa. In order to find the precipitable sulphur multiply the c.c. of the iodine solution of the first test by 5. If this product is greater than the number of c.c. of the iodine solution of the second test the lye is over-blown, *i.e.*, oxidised too much. If this product is smaller than the number of the c.c. of the second test the lye is under-blown, *i.e.*, not oxidised enough. Subtract the smaller number from the larger and divide the difference in the former case by 40, and in the latter case by 20. The quotient thus obtained is subtracted from the total quantity of sulphur present.

No. 6, June, 1880.

Testing Weldon Mud.—Dr. Jurisch.—The author gives an account of a method which has been in use since 1878 at the works of James Muspratt and Sons, at Widnes.

Determination of the Proportion of Anhydride in Fuming Sulphuric Acid.—Clemens Winckler.—This memoir will be inserted at length.

Verhandlungen des Vereins zur Beforderung des Gewerbfleisses. Nos. 4 and 5, April and May, 1880.

These issues contain no chemical matter.

No. 6, June, 1880.

Machines for the Separation of Magnetic and Non-magnetic Ores.—Dr. Siemens.

Apparatus for Measuring the Rapidity of a Gaseous Current—Dr. O. Braun.—These two papers cannot be usefully abstracted without the accompanying woodcuts.

Weighting Textile Fabrics.—Dr. Grothe.—The author gives an account of the sizing of cotton tissues as at present carried on in England. Dr. Franke pointed out that this custom is by no means peculiar to England, and remarked that silks are weighted in a far larger proportion than cottons—sometimes up to 400 per cent.

Berichte der Deutschen Chemischen Gesellschaft zu Berlin.

No. 3, 1880.

Methylated Dioxethylen-amines.—H. F. Morley.—An account of the preparation and properties of mono-methyl-dioxethylen-amine and the corresponding di compound.

Oxidation of Substituted Phenols.—G. Magatti.—The author has obtained tetra-brom-diphenol, and on oxidising it with a small quantity of nitric acid after solution in glacial acetic acid, he produces a crystalline substance, $C_{12}H_4Br_4O_2$, which is dark red-brown by transmitted, but steel-blue by reflected light, and which is tetra-brom-diphenol-quinon, analogous to cedriret. Corresponding chlorine compounds have also been examined.

Certain Derivatives of Para-brom-aniline.—M. Dennstedt.—The author has prepared and examined brom-phenyl-urethan, brom-phenyl-cyanate, brom-phenyl-methyl-urethan, brom-phenyl-dicyanate, and dibrom-phenyl-biuret. From the bromo-phenylated oil of mustard he has obtained mono-brom-phenyl-sulphurea, phenyl-brom-phenyl-sulphurea, brom-phenyl-urethan-hemi-sulphide, sulpho-brom-phenyl-urethan, iso-cyan-mono-brom-phenyl-chloride, ethenyl-dibrom-phenyl-diamine, formo-brom-anilide, thio-formo-brom-anilide, brom-phenyl-glycocoll, and its brom-anilide (dibrom-phenyl-oxethylen-urea and brom-phenyl-glycocoll-ethyl-ether.

Crystalline Form of Ortho-thio-formic Benzyl-ether.—M. Dennstedt.—This substance crystallises in the rhombic system. The measurements of the angles are given.

Question of the Origin of Hypoxanthine from Albuminoid Bodies.—E. Drechsel.—A critique on the theory of Salomon, Krause, and Chittenden, that the xanthine compounds do not exist pre-formed in albuminoid bodies.

Vesbium and Norwegium.—C. Rammelsberg.—Already inserted.

NOTES AND QUERIES.

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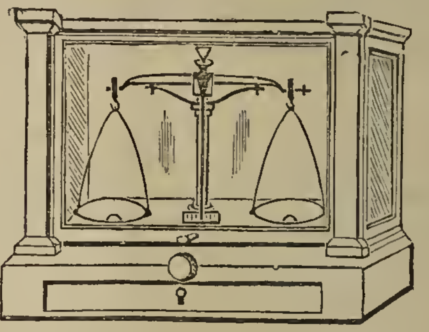

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THE CHEMICAL NEWS.

Vol. XLII. No. 1083.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

Swansea Meeting, August 25, 1879.

INAUGURAL ADDRESS OF THE PRESIDENT,

Andrew Crombie Ramsay, LL.D., F.R.S., V.P.G.S.,
Director-General of the Geological Survey of the United Kingdom' and of the Museum of Practical Geology.

On the Recurrence of Certain Phenomena in Geological Time.

In this address I propose to consider the recurrence of the same kind of incidents throughout all geological time, as exhibited in the various formations and groups of formations that now form the known parts of the external crust of the earth. This kind of investigation has for many years forced itself on my attention, and the method I adopt has not heretofore been attempted in all its branches. In older times, Hutton and Playfair, in a broad and general manner, clearly pointed the way to the doctrine of uniformity of action and results, throughout all known geological epochs down to the present day; but after a time, like the prophets of old, they obtained but slight attention, and were almost forgotten, and the wilder cosmical theories of Werner more generally ruled the opinions of the geologists of the time. Later still, Lyell followed in the steps of Playfair, with all the advantages that the discoveries of William Smith afforded, and aided by the labours of that band of distinguished geologists, Sedgwick, Buckland, Mantell, De la Beche, Murchison, and others, all of whom some of us knew. Notwithstanding this new light, even now there still lingers the relics of the belief (which some of these geologists also maintained), that the physical phenomena which produced the older strata were not only different in kind, but also in degree from those which now rule the external world. Oceans, the waters of which attained a high temperature, attended the formation of the *primitive* crystalline rocks. Volcanic eruptions, with which those of modern times are comparatively insignificant, the sudden upheaval of great mountain-chains, the far more rapid decomposition and degradation of rocks, and, as a consequence, the more rapid deposition of strata formed from their waste—all these were assumed as certainties, and still linger in some parts of the world among living geologists of deservedly high reputation. The chief object of this address is, therefore, to attempt to show, that whatever may have been the state of the world long before geological history began, as now written in the rocks, all known formations are comparatively so recent in geological time, that there is no reason to believe that they were produced under physical circumstances differing either in kind or degree from those with which we are now more or less familiar.

It is unnecessary for my present purpose to enter into details connected with the recurrence of marine formations, since all geologists know that the greater part of the stratified rocks were deposited in the sea, as proved by the molluscs and other fossils which they contain, and the order of their deposition and the occasional stratigraphical breaks in succession are also familiar subjects. What I have partly to deal with now, are exceptions to true marine stratified formations, and after some other important questions have been considered, I shall proceed to discuss the origin of various non-marine deposits from nearly the earliest known time down to what by comparison may almost be termed the present day.

Metamorphism.

All, or nearly all, stratified formations have been in a sense metamorphosed, since, excepting certain limestones, the fact of loose incoherent sediments having been by pressure and other agencies turned into solid rocks constitutes a kind of metamorphism. This, however, is only a first step toward the kind of metamorphism, the frequent recurrence of which in geological time I have now to insist upon, and which implies that consolidated strata have undergone subsequent changes of a kind much more remarkable.

Common stratified rocks chiefly consist of marls, shales, slates, sandstones, conglomerates, and limestones, generally distinct and definite; but not infrequently a stratum, or strata, may partake of the characters in varied proportions of two or more of the above-named species. It is from such strata that metamorphic rocks have been produced, exclusive of the metamorphism of igneous rocks, on which I will not enter. These may be looked for in every manual of geology, and usually they may be found in them.

As a general rule, metamorphic rocks are apt to be much contorted, not only on a large scale, but also that the individual layers of mica quartz and felspar in gneiss are bent and folded in a great number of minute convolutions, so small that they may be counted by the hundred in a foot or two of rock. Such metamorphic rocks are often associated with masses of granite both in bosses and in interstratified beds or layers, and where the metamorphism becomes extreme it is often impossible to draw a boundary line between the gneiss and the granite; while, on the other hand, it is often impossible to draw any true boundary between gneiss (or other metamorphic rocks) and the ordinary strata that have undergone metamorphism. Under these circumstances it is not surprising that when chemically analysed, there is often little difference in the constituents of the unmetamorphosed and the metamorphosed rock. This is a point of some importance in relation to the origin and non-primitive character of gneiss and other varieties of foliated strata, and also of some quartzites and crystalline limestones.

I am aware that in North America formations consisting of metamorphic rocks have been stated to exist of older date than the Laurentian gneiss, and under any circumstances it is obvious that vast tracts of pre-Laurentian land must have existed in all regions, by the degradation of which, sediments were derived wherewith to provide materials for the deposition of the originally unaltered Laurentian strata. In England, Wales, and Scotland attempts have also been made to prove the presence of more ancient formations, but I do not consider the data provided sufficient to warrant any such conclusion. In the Highlands of Scotland, and in some of the Western Isles, there are gneissic rocks of pre-Cambrian age, which, since they were first described by Sir Roderick Murchison in the North-west Highlands, have been, I think justly-considered to belong to the Laurentian series, unconformably underlying Cambrian and Lower Silurian rocks, and as yet there are no sufficient grounds for dissenting from his conclusion that they form the oldest known rocks in the British Islands.

It is unnecessary here to discuss the theory of the causes that produced the metamorphism of stratified rocks, and it may be sufficient to say, that under the influence of deep underground heat, aided by moisture, sandstones have been converted into quartzites, limestones have become crystalline, and in shaly, slaty, and schistose rocks, under like circumstances, there is little or no development of new material, but rather, in the main, a re-arrangement of constituents according to their chemical affinities in rudely crystalline layers, which have very often been more or less developed in pre-existing planes of bedding. The materials of the whole are approximately the same as those of the unaltered rock, but have been re-arranged in layers, for example, of quartz, felspar, and mica, or of hornblende, &c., while other minerals, such as schorl and garnets, are of not infrequent occurrence.

It has for years been an established fact that nearly the whole of the mountain masses of the Highlands of Scotland (exclusive of the Laurentian, Cambrian, and Old Red Sandstone formations), mostly consist of gneissic rocks of many varieties, and of quartzites and a few bands of crystalline limestone, which, from the north shore to the edge of the Old Red Sandstone, are repeated again and again in stratigraphical convolutions great and small. Many large bosses, veins, and dykes of granite are associated with these rocks, and, as already stated, it sometimes happens that it is hard to draw a geological line between granite and gneiss and *vice versâ*. These rocks, once called Primary or Primitive, were first proved by Sir Roderick Murchison to be of Lower Silurian age, thus revolutionising the geology of nearly one-half of Scotland. To the same age belongs by far the greater part of the broad hilly region of the south of Scotland that lies between St. Abb's Head on the east and the coast of Ayrshire and Wigtonshire on the west. In the south-west part of this district, several great masses of granite rise amid the Lower Silurian rocks, which in their neighbourhood pass into mica-schist and even into fine-grained gneiss.

In Cornwall the occurrence of Silurian rocks is now well known. They are of metamorphic character, and partly associated with granite; and at Start Point, in South Devonshire, the Silurian strata have been metamorphosed into quartzites.

In parts of the Cambrian areas, Silurian rocks in contact with granite have been changed into crystalline hornblendic gneiss, and in Anglesey there are large tracts of presumed Cambrian strata, great part of which have been metamorphosed into chlorite and mica-schist and gneiss, and the same is partly the case with the Lower Silurian rocks of the centre of the island, where it is almost impossible to disentangle them from the associated granite.

In Ireland similar metamorphic rocks are common, and, on the authority of Prof. Hull, who knows them well, the following statements are founded:—"Metamorphism in Ireland has been geographical and not stratigraphical, and seems to have ceased before the Upper Silurian period.

"The epoch of greatest metamorphism appears to have been that which intervened between the close of the Lower Silurian period and the commencement of the Upper Silurian, taking the formations in ascending order.

"It is as yet undecided whether Laurentian rocks occur in Ireland. There are rocks in north-west Mayo very like those in Sutherlandshire, but if they are of Laurentian age they come directly under the metamorphosed Lower Silurian rocks, and it may be very difficult to separate them.

"Cambrian purple and green grits are not metamorphosed in the counties of Wicklow and Dublin, but the same beds at the southern extremity of County Wexford, near Carnsore Point, have been metamorphosed into mica-schist and gneiss.

"In the east of Ireland the Lower Silurian grits and slates have not been metamorphosed, except where in proximity to granite, into which they insensibly pass in the counties of Wicklow, Dublin, Westmeath, Cavan, Longford, and Down; but in the west and north-west of Ireland they have been metamorphosed into several varieties of schists, hornblende-rock, *and gneiss, or foliated granite.*"

It would be easy to multiply cases of the metamorphism of Silurian rocks on the continent of Europe, as, for example, in Scandinavia and in the Ural Mountains, where, according to Murchison, "by following its masses upon their strike, we are assured that the same zone which in one tract has a mechanical aspect and is fossiliferous, graduates in another parallel of latitude into a metamorphic crystalline condition, whereby not only the organic remains, but even the original impress of sedimentary origin, are to a great degree obliterated." The same kind of phenomena are common in Canada and the United States; and Medlicott and Blanford, in "The Geology of India," have described the thorough metamorphism of Lower Silurian strata into gneiss and syenitic and hornblende schists.

In Britain, none of the Upper Silurian rocks have undergone any serious change beyond that of ordinary consolidation, but in the Eastern Alps at Gratz, Sir Roderick Murchison has described both Upper Silurian and Devonian strata interstratified with separate courses of metamorphic chloritic schist.

Enough has now been said to prove the frequent occurrence of metamorphic action among Cambrian and Lower and Upper Silurian strata.

If we now turn to the Devonian and Old Red Sandstone strata of England and Scotland, we find that metamorphic action has also been at work, but in a much smaller degree. In Cornwall and Devon, five great bosses of granite stand out amid the stratified Silurian, Devonian, and Carboniferous formations. Adjoining or near these bosses the late Sir Henry De la Beche remarks that "in numerous localities we find the coarser slates converted into rocks resembling mica-slate and gneiss, a fact particularly well exhibited in the neighbourhood of Meavy, on the south-east of Tavistock," and "near Camelford we observed a fine arenaceous and micaceous grauwacke turned into a rock resembling mica-slate near the granite." Other cases are given by the same author, of slaty strata turned into mica-schist and gneiss in rocks now generally considered to be of Devonian age.

The Devonian rocks and Old Red Sandstone are of the same geological age, though they were deposited under different conditions, the first being of marine, and the latter of fresh-water, origin. The Old Red Sandstone of Wales, England, and Scotland has not, as far as I know, suffered any metamorphism, excepting in one case in the north-east of Ayrshire, near the sources of the Avon Water, where a large boss of granite rises through the sandstone, which all round has been rendered crystalline with well-developed crystals of felspar.

On the Continent of Europe, a broad area of Devonian strata lies on both banks of the Rhine and the Moselle. Forty years ago, Sedgwick and Murchison described the crystalline quartzites, chlorite, and micaceous slates of the Hundsruck and the Taunus, and from personal observation I know that the rocks in the country on either side of the Moselle are, in places, of a foliated or semi-foliated metamorphic character. In the Alps also, as already noticed, metamorphic Devonian strata occur interstratified with beds of metamorphic schists, and, Sir Roderick adds, "we have ample data to affirm that large portions of the Eastern Alps . . . are occupied by rocks of true palæozoic age, which in many parts have passed into a crystalline state."

I know of no case in Britain where the Carboniferous strata have been thoroughly metamorphosed, excepting that in South Wales, beds of coal, in the west of Caermarthenshire, and in South Pembrokeshire, gradually pass from so-called bituminous coal into anthracite. The same is the case in the United States, in both instances the Carboniferous strata being exceedingly disturbed and contorted. In the Alps, however, Sir Roderick Murchison seems to have believed that Carboniferous rocks may have been metamorphosed: a circumstance since undoubtedly proved by the occurrence of a coal-measure calamite, well preserved, but otherwise partaking of the thoroughly crystalline character of the gneiss in which it is imbedded and which was shown to me by the late Prof. Gastaldi, at Turin.

I am well acquainted with all the Permian strata of the British Islands and of various parts of continental Europe, and nowhere, that I have seen, have they suffered from metamorphic action, and strata of this age are, I believe, as yet unknown in the Alps. This closes the list of metamorphism of palæozoic strata.

I will not attempt (they are so numerous) to mention all the regions of the world in which Mesozoic or Secondary formations have undergone metamorphic action. In Britain and the non-mountainous parts of France, they are generally quite unaltered, but in the Alps it is different. There, as everyone knows who is familiar with that region, the crystalline rocks in the middle of the chain have the same general strike as the various flanking stratified formations. As expressed by Murchison, "as we follow the chain from N.E. to S.W. we pass from the clearest types of sedimentary rocks, and, at length, in the Savoy Alps, are immersed in the highly altered mountains of Secondary limestone," while "the metamorphism of the rocks is greatest as we approach the centre of the chain," and, indeed, any one familiar with the Alps of Switzerland and Savoy knows that a process of metamorphism has been undergone *by all the Jurassic rocks* (Lias and Oolites) of the great mountain chain. Whether or not any strata of Neocomian and Cretaceous age have been well metamorphosed in this region I am unable to say; but it seems to be certain that the Eocene or Lower Tertiary Alpine formation, known as the Flysch, contains beds of black schists which pass into Lydian stone, and also that in the Grisons it has been converted into gneiss and mica-schist, a fact mentioned by Studer and Murchison. I also have seen in the country north of the Oldenhorn, nummulitic rocks so far foliated that they formed an imperfect gneiss.

In Tierra del Fuego, as described by Darwin, clay slates of early cretaceous date pass into gneiss and mica-slate with garnets, and in Chonos Islands, and all along the great Cordillera of the Andes of Chili, rocks of Cretaceous or Cretaceo-oolitic age have been metamorphosed into foliated mica-slate and gneiss, accompanied by the presence of granite, syenite, and greenstone.

This ends my list, for I have never seen, or heard, of metamorphic rocks of later date than those that belong to the Eocene series. Enough, however, has been said to prove, that from the Laurentian epoch onward, the phenomenon of extreme metamorphism of strata has been of frequent recurrence all through Palæozoic and Mesozoic times, and extends even to a part of the Eocene series equivalent to the soft unaltered strata of the formations of the London and Paris basins, which excepting for their fossil contents, and sometimes highly inclined positions, look as if they had only been recently deposited.

Volcanoes.

The oldest volcanic products of which I have personal knowledge are of Lower Silurian age. These in Wales consist of two distinct series, the oldest of which, chiefly formed of felspathic lavas and volcanic ashes, lie in and near the base of the Llandeilo beds, and the second, after a long interval of repose, were ejected and intermingled with the strata forming the middle part of the Bala beds. The Lower Silurian rocks of Montgomeryshire, Shropshire, Radnorshire, Pembrokeshire, Cumberland, and Westmoreland are to a great extent also the result of volcanic eruptions, and the same kind of volcanic rocks occur in the Lower Silurian strata of Ireland. I know of no true volcanic rocks in the Upper Silurian series.

In the Old Red Sandstone of Scotland lavas and volcanic ashes are of frequent occurrence, interstratified with the ordinary lacustrine sedimentary strata. Volcanic rocks are also intercalated among the Devonian strata of Devonshire. I know of none in America or on the Continent of Europe.

In Scotland volcanic products are common throughout nearly the whole of the Carboniferous sub-formations, and they are found also associated with Permian strata.

I now come to the Mesozoic or Secondary epochs. Of Jurassic age (Lias and Oolites), it is stated by Lyell with some doubt, that true volcanic products occur in the Morea and also in the Apennines, and it seems probable, as stated by Medlicott and Blanford, that the Rajmahal traps may also be of Jurassic age.

In the Cordillera of South America, Darwin has described a great series of volcanic rocks intercalated among the Cretaceo-oolitic strata that forms so much of the chain; and the same author in his "Geological Observations in South America," states that the Cordillera has been, probably with some quiescent periods, a source of volcanic matter from an epoch anterior to our Cretaceo-oolitic formation to the present day. In the Deccan volcanic traps rest on Cretaceous beds, and are overlaid by Nummulitic strata, and according to Medlicott and Blanford, these were poured out in the interval between Middle Cretaceous and Lower Eocene times.

In Europe the only instance I know of a volcano of Eocene age is that of Monte Bolca, near Verona, where the volcanic products are associated with the fissile limestone of that area.

The well-preserved relics of Miocene volcanoes are prevalent over many parts of Europe, such as Auvergne and The Velay, where the volcanic action began in Lower Miocene times, and was continued into the Pliocene epoch. The volcanoes of the Eifel are also of the same general age, together with the ancient Miocene volcanoes of Hungary.

The volcanic rocks of the Azores, Canaries, and Madeira are of Miocene age, while in Tuscany there are extinct volcanoes that began in late Miocene, and lasted into times contemporaneous with the English Coralline Crag. In the north of Spain also, at Olot, in Catalonia, there are perfect craters and cones remaining of volcanoes that began to act in newer Pliocene times and continued in action to a later geological date. To these I must add the great *coulées* of Miocene lava, so well known in the Inner Hebrides, on the mainland, near Oban, &c., in Antrim in the north of Ireland, in the Faroe Islands, Greenland, and Franz-Joseph Land. It is needless, and would be tiresome, further to multiply instances, for enough has been said to show that in nearly all geological ages volcanoes have played an important part, now in one region, now in another, from very early Palæozoic times down to the present day; and, as far as my knowledge extends, at no period of geological history is there any sign of their having played a more important part than they do in the epoch in which we live.

Mountain-Chains.

The mountain-chains of the world are of different geological ages, some of them of great antiquity, and some of them comparatively modern.

It is well known that in North America the Lower Silurian rocks lie uncomformably on the Laurentian strata, and also that the latter had undergone a thorough metamorphism and been thrown into great anticlinal and synclinal folds, accompanied by intense minor convolutions, before the deposition of the oldest Silurian formation, that of the Potsdam Sandstone. Disturbances of the nature alluded to imply beyond a doubt that the Laurentian rocks formed a high mountain-chain of pre-Silurian date, which has since constantly been worn away and degraded by sub-aërial denudation.

In Shropshire, and in parts of North Wales, and in Cumberland and Westmoreland, the Lower Silurian rocks by upheaval formed hilly land before the beginning of the Upper Silurian epoch; and it is probable that the Lower Silurian gneiss of Scotland formed mountains at the same time, probably very much higher than now. However that may be, it is certain that these mountains formed high land before and during the deposition of the old Red Sandstone, and the upheaval of the great Scandinavian chain (of which the Highlands may be said to form an outlying portion) also preceded the deposition of the Old Red Strata. In both of these mountain regions the rocks have since undergone considerable movements, which in the main seem to have been movements of elevation, accompanied undoubtedly by that constant atmospheric degradation to which all high land is especially subject.

The next great European chain in point of age is that of the Ural, which according to Murchison is of pre-

Permian age, a fact proved by the Permian conglomerates which were formed from the waste of the older strata. On these they lie quite unconformably and nearly undisturbed on the western flank of the mountains.

In North America the great chain of the Alleghany Mountains underwent several disturbances, the last (a great one) having taken place after the deposition of the Carboniferous rocks, and before that of the New Red Sandstone. The vast mountainous region included under the name of the Rocky Mountains, after several successive disturbances of upheaval, did not attain its present development till after the Miocene or Middle Tertiary epoch.

In South America, notwithstanding many oscillations of level recorded by Darwin, the main great disturbance of the strata that form the chain of the Andes took place apparently *in post-cretaceous times.*

The Alps, the rudiments of which began in more ancient times, received their greatest disturbance and upheaval in post-Eocene days, and were again raised at least 5000 feet (I believe much more) at the close of the Miocene epoch. The Apennines, the Pyrenees, the Carpathians, and the great mountain region on the east of the Adriatic and southward into Greece, are of the same general age, and this is also the case in regard to the Atlas in North Africa and the Caucasus on the borders of Europe and Asia. In the north of India the history of the Great Himalayan range closely coincides with that of the Alps, for while the most powerful known disturbance and elevation of the range took place after the close of the Eocene epoch, a subsequent elevation occurred in post-Miocene times closely resembling and at least equal to that sustained by the Alps at the same period.

It would probably not be difficult by help of extra research to add other cases to this notice of recurrences of the upheaval and origin of special mountain chains, some of which I have spoken of from personal knowledge; but enough has been given to show the bearing of this question on the argument I have in view, namely, that of epetition of the same kind of events throughout all known geological time.

Salt and Salt Lakes.

I now come to the discussion of the circumstances that produced numerous recurrences of the development of beds of various salts (chiefly common rock-salt) in many formations, which it will be seen are to a great extent connected with continental or inland conditions. In comparatively rainless countries salts are often deposited on the surface of the ground by the effect of solar evaporation of moisture from the soil. Water dissolves certain salts in combination with the ingredients of the underlying rocks and soils, and brings it to the surface, and when solar evaporation ensues the salt or salts are deposited on the ground. This is well known to be the case in and near the region of the Great Salt Lake in North America, and in South America in some of the nearly rainless districts of the Cordillera, extensive surface-deposits of salts of various kinds are common. The surface of the ground around the Dead Sea is also in extra dry seasons covered with salt, the result of evaporation, and in the upper provinces of India (mentioned by Medlicott and Blanford) "many tracts of land in the Indo-Gangetic alluvial plain are rendered worthless for cultivation by an efflorescence of salt known in the North-West Provinces as *Reh*," while every geographer knows that in Central Asia, from the western shore of the Caspian Sea to the Kinshan Mountains of Mongolia, with rare exceptions nearly every lake is salt in an area at least 3500 miles in length. This circumstance is due to the fact that all so-called fresh-water springs, and therefore all rivers, contain small quantities of salt in solution only appreciable to the chemist, and by the constant evaporation of pure water from the lakes, in the course of time it necessarily happens that these salts get concentrated in the water by the effect of solar heat, and, if not already begun, precipitation of solid salts must ensue.

The earliest deposits of rock-salt that I know about have been described by Mr. A. B. Wynne, of the Geological Survey of India, in his memoir "On the Geology of the Salt Range in the Punjab."* The beds of salt are of great thickness, and along with gypsum and dolomitic layers occur in marl of a *red colour* like our Keuper Marl. This colour I have for many years considered to be, in certain cases, apt to indicate deposition of sediments in inland lakes, salt or fresh, as the case may be; and with respect to these strata in the Punjab Salt Range, authors seem to be in doubt whether they were formed in inland lakes or in lagoons near the seaboard, which at intervals were liable to be flooded by the sea, and in which in the hot seasons salts were deposited by evaporation caused by solar heat. For my argument it matters but little which of these was the true physical condition of the land at the time, though I incline to think the inland lake theory most probable. The age of the strata associated with this salt is not yet certainly ascertained. In "The Geology of India" Medlicott and Blanford incline to consider them of Lower Silurian age, and Mr. Wynne, in his "Geology of the Salt Range," places the salt and gypsum beds doubtfully on the same geological horizon.

The next salt-bearing formation that I shall notice is the Salina or Onondaga Salt Group of North America, which forms part of the Upper Silurian rocks, and lies immediately above the Niagara Limestone. It is rich in gypsum and in salt-brine, often of a very concentrated character, "which can only be derived from original depositions of salt," and it is also supposed by Dr. T. Sterry Hunt to contain solid rock-salt 115 feet in thickness at the depth of 2085 feet, near Saginaw Bay, in Michigan.

In the Lower Devonian strata of Russia, near Lake Ilmen, Sir R. Murchison describes salt springs at Starai Russa. Sinkings "made in the hope of penetrating to the source of these salt springs" reached a depth of 600 feet without the discovery of rock-salt, "and we are left in doubt whether the real source of the salt is in the lowest beds of the Devonian rocks or even in the Silurian system."

In the United States brine springs also occur in Ohio, Pennsylvania, and Virginia, in Devonian rocks.

In Michigan, salts are found from the Carboniferous down to the Devonian series; and in other parts of the United States, Western Pennsylvania, Virginia, Ohio, Illinois, and Kentucky, from the lower Coal-measures salts are derived which must have been deposited in inland areas, since even in the depths of inland seas that communicate with the great ocean, such as the Mediterranean and the Red Sea, no great beds of salt can be deposited. Before such strata of salt can be formed supersaturation must have taken place.

In the North of England, at and near Middlesborough, two deep bore-holes were made some years ago in the hope of reaching the Coal-measures of the Durham coalfield. One of them at Salthome was sunk to a depth of 1355 feet. First they passed through 74 feet of superficial clay and gravel, next through about 1175 feet of red sandstones and marls, with beds of rock-salt and gypsum. The whole of these strata (excepting the clay and gravel) evidently belong to the Keuper marls and sandstones of the upper part of our New Red series. Beneath these they passed through 67 feet of dolomitic limestone, which in this neighbourhood forms the upper part of the Permian series, and beneath the limestone the strata consist of 27 feet of gypsum and rock-salt and marls, one of the beds of rock-salt having a thickness of 14 feet. This bed of Permian salt is of some importance, since I have been convinced for long that the British Permian strata were deposited, not in the sea, but in salt lakes comparable in some respects to the great salt lake of Utah, and in its restricted fauna to the far greater salt-lake of the Caspian Sea. The gypsum, the dolomite or magnesian limestone, the red marls covered with rain-pittings, the sun-cracks, and the impressions of footprints of reptiles made in the

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* Many earlier notices and descriptions of the Salt Range might be quoted, but Mr. Wynne's is enough for my purpose.

soft sandy marls when the water was temporarily lowered by the solar evaporation of successive summers, all point to the fact that our Permian strata were not deposited in the sea, but in a salt lake or lakes once for a time connected with the sea. The same may be said of other Permian ares in the central parts of the Continent of Europe, such as Stassfurt and Anhalt, Halle and Altern, in Thuringia, and Sperenberg, near Berlin, and also in India.[*]

Neither do I think that the Permian strata of Russia, as described by Sir Roderick Murchison, were necessarily, as he implies, deposited in a wide ocean. According to his view all marine life universally declined to a minimum after the close of the Carboniferous period, that decline beginning with the Permian and ending with the Triassic epoch. Those who believe in the doctrine of evolution will find it hard to accept the idea which this implies, namely, that all the prolific forms of the Jurassic series sprang from the scanty faunas of the Permian and Triassic epochs. On the contrary, it seems to me more rational to attribute the poverty of the faunas of these epochs to accidental abnormal conditions in certain areas, that for a time partially disappeared during the deposition of the continental Muschelkalk which is absent in the British Triassic series.

In the whole of the Russian Permian strata only fifty-three species were known at the time of the publication of "Russia and the Ural Mountains," and I have not heard that this scanty list has been subsequenly increased. I am therefore inclined to believe that the red marls, grits, sandstones, conglomerates, and great masses of gypsum and rock-salt were all formed in a flat inland area which was occasionally liable to be invaded by the sea during intermittent intervals of minor depression, sometimes in one area, sometimes in another, and the fauna small in size and poor in numbers is one of the results, while the deposition of beds of salt and gypsum is another. If so, then in the area now called Russia, in sheets of inland Permian water, deposits were formed strictly analogous to those of Central Europe and of Britain, but on a larger scale.

Other deposits of salt deep beneath overlying younger strata are stated to occur at Bromberg, in Prussia, and many more might be named as lying in the same formation in Northern Germany.

If we now turn to the Triassic series it is known that it consists of only two chief members in Britain, the Bunter Sandstones and the Keuper or New Red Marls, the Muschelkalk of the Continent being absent in our islands. No salt is found in the Bunter sandstones of England, but it occurs in these strata at Schöningen, in Brunswick, and also near Hanover. In the lower part of the Keuper series deposits of rock-salt are common in England and in Ireland. At Almersleben, near Calbe, rock-salt is found in the Muschelkalk, and also at Erfurt and Slottenheim, in Thuringia, and at Wilhelmsglück, in Wurtemberg. In other Triassic areas it is known at Honigsen, in Hanover, in middle Keuper beds. In the red shales at Sperenberg and Lieth on the Lower Elbe, salt was found at the depth of 3000 feet, and at Stassfurth the salt is said to be "several hundred yards thick."

In Central Spain rock-salt is known, and at Tarragona, Taen. and also at Santander, in the north of Spain, all in Triassic strata. Other localities may be named in the Upper Trias, such as the Salzkammergut, Aussee, Hallstatt, Ischl, Hallein in Salzburg, Halle in the Tyrol, and Berchesgaden in Bavaria.

In the Salt Range of mountains in Northern India saliferous strata are referred with some doubt by Medlicott and Blanford to the Triassic strata.

In the Jurassic series (Lias and Oolites) salt and gypsum are not uncommon. One well-known instance occurs at Berg, in the valley of the Rhone, in Switzerland, where salt is derived from the Lias. Salt and gypsum are also found in Jurassic rocks at Burgos, in Spain. At Gap, in France, there is gypsum, and salt is found in the Austrian Alps in Oolitic limestone.

In the Cretaceous rocks salt occurs, according to Lartet, at Jebel Usdom by the Red Sea, and other authorities state that it occurs in the Pyrenees and at Biskra, in Africa, where "mountains of salt" are mentioned as of Cretaceous age. The two last-named localities are possibly uncertain; but whether or not this is the case, it is not the less certain that salt has been deposited in Cretaceous rocks, and, judging by analogy, probably in inland areas of that epoch.

In the Eocene or Older Tertiary formations, rock-salt is found at Cardona, in Spain, and at Kohat, in the Punjab, it occurs at the base of Nummulitic beds. It is also known at Mandi, in India, in strata supposed to be of Nummulitic Eocene age.

The record does not end here, for a zone of rock-salt lies in Sicily at the top of the Salina clays in Lower Miocene beds, and in Miocene strata gypsum is found at several places in Spain, while salt also occurs in beds that are doubtfully of Miocene age (but may be later) at Wielitzka in Poland, Kalusz in Galicia, Bukowina, and also in Transylvania.

In Pliocene or Later Tertiary formation, thick beds of gypsum are known in Zante, and strata of salt occur in Roumania and Galicia, while in Pliocene rocks, according to Dana, or in Post-Tertiary beds, according to others, a thick bed of pure salt was penetrated at a depth of 38 feet at Petit Anse, in Louisiana. This ends my list, though I have no doubt that, by further research, many more localities might be given. Enough, however, has been done to show that rock-salt (and other salts) are of frequent *recurrence* throughout all geological time, and as in my opinion it is impossible that common salt can be deposited in the open ocean, it follows that this and other salts must have been precipitated from solutions, which, by the effect of solar evaporation, became at length supersaturated, like those of the Dead Sea, the great salt lake of Utah, and in other places which it is superfluous to name.

Fresh-water. Lakes and Estuaries.

I now come to the subject of recurrences of fresh-water conditions both in lakes and estuaries. In the introduction to the "Geology of India," by Messrs. Medlicott and Blanford, mention is made of the Blaini and Krol rocks as probably occupying "hollows formed by denudation in the old gneissic rocks," and the inference is drawn that "if this be a correct view, it is probable that the cis-Himalayan palæozoic rocks are in great part of fresh-water origin, and that the present crystalline axis of the Western Himalayas approximately coincides with the shore of the ancient palæozoic continent, of which the Indian peninsula formed a portion." The Krol rocks are classed broadly with "Permian and Carboniferous" deposits, but the Blaini beds are doubtfully considered to belong to Upper Silurian strata. If this point be by-and-bye established, this is the earliest known occurrence of fresh-water strata in any of the more ancient palæozoic formations.

It is a fact worthy of notice that the colour of the strata formed in old lakes (whether fresh or salt) of palæozoic and mesozoic age is apt to be red: a circumstance due to the fact that each little grain of sand or mud is usually coated with a very thin pellicle of peroxide of iron. Whether or not the red and purple Cambrian rocks[*] may not be *partly* of fresh-water origin, is a question that I think no one but myself has raised.[†]

There is however, in my opinion, no doubt with regard to the fresh-water origin of the Old Red Sandstone, as distinct from the contemporaneous marine deposits of the

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[*] See "Physical Geology and Geography of Great Britain,' fifth edition, where the question is treated in more detail.

[*] By Cambrian I mean only the *red and purple* rocks of Wales, England, Scotland, and Ireland, older than the Menevian beds, or any later division of the Silurian, strata, that may chance to rest upon them.

[†] "On the Red Rocks of England of Older Date than the Trias, *Jour. Geol. Soc.*, 1871 vol. xxviii.

Devonian strata. This idea was first started by that distinguished geologist, Doctor Fleming, of Edinburgh, followed by Mr. Godwin-Austen, who, from the absence of marine shells and the nature of the fossil fishes in these strata, inferred that they were deposited, not in the sea, as had always been asserted, but in a great fresh-water lake or in a series of lakes. In this opinion I have for many years agreed, for the nearest analogies of the fish are, according to Huxley, the Polypterus of African rivers, the Ceratodus of Australia, and in less degree the Lepidosteus of North America. The truth of the supposition that the Old Red Sandstone was deposited in fresh water is further borne out by the occurrence of a fresh-water shell, *Anodonta Jukasii*, and of ferns in the Upper Old Red Sandstone in Ireland; and the same shell is found at Dura Den in Scotland, while in Caithness, along with numerous fishes, there occurs the small bivalve crustacean Estheria Murchisoniæ.

I think it more than probable that the red series of rocks that form the Catskill Mountains of North America (and with which I am personally acquainted) were formed in the same manner as the Old Red Sandstones of Britain; for, excepting in one or two minor interstratifications, they contain no relics of marine life, while "the fossil fishes of the Catskill beds, according to Dr. Newberry, appear to represent closely those of the British Old Red Sandstone." (Dana.)

The Devonian rocks of Russia, according to the late Sir Roderick Murchison, consist of two distinct types, viz., Devonian strata identical in general character with those in Devonshire and in various parts of the continent of Europe. These are exclusively of a marine character, while the remainder corresponds to the Old Red Sandstone of Wales, England, and Scotland.

At Tchudora, about 105 miles S.E. of St. Petersburg, the lowest members of the series consist of flag-like compact limestones accumulated in a tranquil sea and containing fucoids and encrinites, together with shells of Devonian age, such as Spirifers, Terebratulæ, Orthis, Leptænas, Avicula, Modiola, Natica, Bellerophon, &c., while the upper division graduates into the Carboniferous series as it often does in Britain, and, like the Old Red Sandstone of Scotland, contains only fish-remains, and in both countries they are of the same species. "Proceeding from the Valdai Hills on the north," the geologist "quits *a Devonian Zone with a true 'Old Red' type* dipping under the Carboniferous rocks of Moscow, and having passed through the latter, he finds himself suddenly in a yellow-coloured region, entirely dissimilar in structure to what he had seen in any of the northern governments, which, of a different type as regards fossils, is the true stratigraphical *equivalent of the Old Red System*." This seems to me, as regards the Russian strata, to mean, that just as the Devonian strata of Devonshire are the true equivalents of the Old Red Sandstone of Wales and Scotland, they were deposited under very different conditions, the first in the sea and the others in inland fresh-water lakes. At the time Sir Roderick Murchison's work was completed, the almost universal opinion was that the Old Red Sandstone was a marine formation. In the year 1830 the Rev. Dr. Fleming of Edinburgh, read a paper before the Wernerian Society, in which he boldly stated that the "*Old Red Sandstone* is a *fresh-water formation*" of older date than the Carboniferous Limestone. This statement, however, seems to have made no impression on geologists till it was revived by Godwin-Austen in a memoir "On the Extension of the Coal-measures," &c., in the *Journal of the Geological Society*, 1856. Even this made no converts to what was then considered a heretical opinion. I have long held Dr. Fleming's view, and unfortunately published it in the third edition of "The Physical Geology and Geography of Great Britain," without at the time being aware that I had been forestalled by Dr. Fleming and Mr. Godwin-Austen.

To give anything like a detailed account of all the fresh-water formations deposited in estuaries and lakes from the close of the Old Red Sandstone times down to late Tertiary epochs is only fitted for a manual of geology, and would too much expand this Address, and I will therefore give little more than a catalogue of these deposits in ascending order.

In the Coal-measure parts of the Carboniferous series a great proportion of the shales and sandstones are of fresh-water origin. This is proved all over the British Islands by the shells they contain, while here and there marine interstratifications occur, generally of no great thickness. There is no doubt among geologists that these Coal-measure strata were chiefly deposited under estuarine conditions, and sometimes in lagoons or in lakes; while numerous beds of coal formed by the life and death of land plants, each underlaid by the soil on which the plants grew, evince the constant recurrence of terrestrial conditions. The same kind of phenomena are characteristic of the Coal-measures all through North America, and in every country on the continent of Europe, from France and Spain on the west, to Russia in the east, and the same is the case in China and in other areas.

In Scotland, according to Prof. Judd, fresh-water conditions occur more or less all through the Jurassic series, from the Lias to the Upper Oolites. In England, fresh-water strata, with thin beds of coal, are found in the Inferior Oolite of Yorkshire, and in the middle of England and elsewhere in the Great Oolite. The Purbeck and Wealden strata, which, in a sense, fill the interval between the Jurassic and Cretaceous series, are almost entirely formed of fresh-water strata, with occasional thin marine interstratifications. By some the Wealden beds are considered to have been formed in and near the estuary of a great river, while others, with as good a show of reason, believe them to have been deposited in a large lake subject to the occasional influx of the sea.

In the eastern part of South Russia the Lias consists chiefly of fresh-water strata, as stated by Neumayr.

The Godwana rocks of Central India range from Upper Palæozoic times well into the Jurassic strata, and there all these formations are of fresh-water origin. Fresh-water beds with shells are also interstratified with the Deccan traps of Cretaceous and Tertiary (Eocene) age, while 2000 feet of fresh-water sands overlie them.

In South-western Sweden, as stated by Mr. Bauermann, "the three coalfields of Hoganas, Stabbarp, and Rodingé, lie in the uppermost Triassic or Rhætic series." In Africa, the Karoo beds, which it is surmised may be of the age of the New Red Sandstone, contain beds of coal. In North America, certain fresh-water strata, with beds of lignite, apparently belong to the Cretaceous and Eocene epochs, and in the north of Spain and south of France there are fresh-water lacustrine formations in the highest Cretaceous strata.

In England the lower and upper Eocene strata are chiefly of fresh-water origin, and the same is the case in France and other parts of the Continent. Certain fresh-water formations in Central Spain extend from the Eocene to the upper Miocene strata.

There is only one small patch of Miocene beds in England, at Bovey Tracey, near Dartmoor, formed of fresh-water deposits with interstratified beds of lignite or Miocene coal. On the continent of Europe Miocene strata occupy immense independent areas, extending from France and Spain to the Black Sea. In places too numerous to name, they contain beds of "brown coal," as lignite is sometimes called. These coal-beds are often of great thickness and solidity. In one of the pits which I descended near Teplitz, in Bohemia, the coal, which lies in a true basin, is 40 feet thick, and underneath it there is a bed of clay, with rootlets, quite comparable to the underclay which is found beneath almost every bed of coal in the British and other coal-fields of the Carboniferous epoch. The Miocene rocks of Switzerland are familiar to all geologists who have traversed the country between the Jura and the Alps. Sometimes they are soft and incoherent, sometimes formed of sandstones, and sometimes of conglomerates, as on the Righi. They chiefly consist of fresh-water lacustrine strata, with some minor marine interstratifications which mark the influx of the sea during occasional partial submergences of portions of the area. These fresh-water strata, of great extent and thickness, contain beds of lignite, and are remarkable for the relics of numerous trees and other plants, which have been described by Prof. Heer, of Zurich, with his accustomed skill. The Miocene fresh-water strata, of the Sewalik Hills in India, are well known to most students of geology, and I have already stated that they bear the same relation to the more ancient Himalayan Mountains that the Miocene strata of Switzerland and the north of Italy do to the pre-existing range of the Alps. In fact, it may be safely inferred that something far more than the rudiments of our present continents existed long before Miocene times, and this accounts for the large areas on those continents which are frequently occupied by Miocene fresh-water strata. With the marine formations of Miocene age this address is in no way concerned, nor is it essential to my argument to deal with those later tertiary phenomena, which in their upper stages so easily merge into the existing state of the world.

Glacial Phenomena.

I now come to the last special subject for discussion in this address, viz., the Recurrence of Glacial Epochs, a subject still considered by many to be heretical, and which was generally looked upon as an absurd crotchet when, in 1855, I first described to the Geological Society, boulder-beds, containing ice-scratched stones, and erratic blocks in the Permian strata of England. The same idea I afterwards applied to some of the Old Red Sandstone conglomerates, and of late years it has become so familiar, that the effects of glaciers have at length been noted by geologists from older Palæozoic epochs down to the present day.

In the middle of last July I received a letter from Prof. Geikie, in which he informed me that he had discovered mammillated *moutonnée* surfaces of Laurentian rocks, passing underneath the Cambrian sandstones of the north-west of Scotland at intervals, all the way from Cape Wrath to Loch Torridon, for a distance of about 90 miles. The mammillated rocks are, says Prof. Geikie, "*as well rounded off as any recent roche moutonnée*," and, "in one place these bosses are covered by a huge angular breccia of this old gneiss (Laurentian) with blocks sometimes five or six feet long." This breccia, where it occurs, forms the base of the Cambrian strata of Sutherland, Ross, and Cromarty, and while the higher strata are always well stratified, where they approach the underlying Laurentian gneiss, "they become pebbly, passing into coarse unstratified agglomerates or boulder-beds." In the Gairloch district, "it is utterly unstratified, the angular fragments standing on end and at all angles," just as they do in many modern moraine mounds wherever large glaciers are found. The general subject of Palæozoic glaciers has long been familiar to me, and this account of more ancient glaciers of Cambrian age is peculiarly acceptable.

The next sign of ice in Britain is found in the Lower Silurian rocks of Wigtonshire and Ayrshire. In the year 1865 Mr. John Carrick Moore took me to see the Lower Silurian graptolitic rocks at Corswall Point in Wigtonshire, in which great blocks of gneiss, granite, &c., are imbedded, and in the same year many similar erratic blocks were pointed out to me by Mr. James Geikie in the Silurian strata of Carrick in Ayrshire. One of the blocks at Corswall, as measured by myself, is nine feet in length, and the rest are of all sizes, from an inch or two up to several feet in diameter. There is no gneiss or granite in this region nearer than those of Kirkcudbrightshire and Arran, *and these are of later geological date than the strata amid which the erratic blocks are imbedded.* It is therefore not improbable that they may have been derived from some high land formed of Laurentian rocks of which the outer Hebrides and parts of the mainland of Scotland form surviving portions. If so, then I can conceive of no agent capable of transporting large boulders and dropping them into the Lower Silurian mud of the seas of the time save that of icebergs or other floating ice, and the same view with regard to the neighbouring boulder-beds of Ayrshire is held by Mr. James Geikie. If, however, anyone will point out any other natural cause still in action by which such results are at present brought about, I should be very glad to hear of it.

I must now turn to India for further evidence of the action of palæozoic ice. In the Himalayas of Pangi, S.E. of Kashmir, according to Medlicott and Blanford, "old slates, supposed to be Silurian, contain boulders in great numbers," which they believe to be of glacial origin. Another case is mentioned as occurring in "transition beds of unknown relations," but in another passage they are stated to be "very ancient, but no idea can be formed of their geological position." The *underlying rocks are marked by distinct glacial striations.*

The next case of glacial boulder-beds with which I am acquainted is found in Scotland, and in some places in the north of England, where they contain what seem to be indistinctly ice-scratched stones. I first observed these rocks on the Lammermuir Hills, south of Dunbar, lying unconformably on Lower Silurian strata, and soon inferred them to be of glacial origin, a circumstance that was subsequently confirmed by my colleagues, Prof. and Mr. James Geikie, and is now familiar to other officers of the Geological Survey of Scotland.

I know of no boulder formations in the Carboniferous series, but they are well known as occurring on a large scale in the Permian brecciated conglomerates, where they consist "of pebbles and large blocks of stone, generally angular, imbedded in a marly paste . . . . the fragments have mostly travelled from a distance, apparently from the borders of Wales, and some of them are three feet in diameter." Some of the stones are as well scratched as those found in modern moraines or in the ordinary boulder-clay of what is commonly called the Glacial Epoch. In 1855 the old idea was still not unprevalent that during the Permian Epoch, and for long after, the globe had not yet cooled sufficiently to allow of the climates of the external world being universally affected by the constant radiation of heat from its interior. For a long time, however, this idea has almost entirely vanished, and now, in Britain at all events, it is little if at all attended to, and other glacial episodes in the history of the world have continued to be brought forward and are no longer looked upon as mere ill-judged conjectures.

The same kind of brecciated boulder-beds that are found in our Permian strata occur in the Rotheliegende of Germany, which I have visited in several places, and I believe them to have had a like glacial origin.

Mr. G. W. Stow, of the Orange Free State, has of late years given most elaborate accounts of similar Permian boulder-beds in South Africa. There, great masses of moraine matter not only contain ice-scratched stones, but on the banks of rivers where the Permian rock has been removed by aqueous denudation, the underlying rocks, well rounded and mammillated, *are covered by deeply incised glacier grooves* pointing in a direction which at length leads the observer to the pre-Permian mountains from whence the stones were derived that formed these ancient moraines.*

Messrs. Blanford and Medlicott have also given in "The Geology of India" an account of boulder-beds in what they believe to be Permian strata, and which they compare with those described by me in England many years before. There the Godwana group of the Talchir strata contains numerous boulders, many of them six feet in diameter, and "in one instance *some of the blocks were found to be polished and striated, and the underlying Vindhyan rocks*

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* Mr. Stow's last memoir on this subject is still in manuscript. It is so exceedingly long, and the sections that accompany it are of such unusual size, that the Geological Society could not afford their publication. It was thought that the Government of the Orange Free State might undertake this duty, but the late troubles in South Africa have probably hindered this work—it is to be hoped only for a time.

were similarly marked." The authors also correlate these glacial phenomena with those found in similar deposits in South Africa, discovered and described by Mr. Stow.

In the Olive group of the Salt range, described by the same authors, there is a curious resemblance between a certain conglomerate "and that of the Talchir group of the Godwana system." This "Olive conglomerate" belongs to the Cretaceous series, and contains ice-transported erratic bolders derived from unknown rocks, one of which of red granite "is polished and striated on three faces in so characteristic a manner that very little doubt can exist of its having been transported by ice." One block of red granite at the Mayo Salt Mines of Khewra "is 7 feet high and 19 feet in circumference." In the "Transition beds" of the same authors, which are supposed to be of Upper Cretaceous age, there also are boulder beds with erratic blocks of great size.

I know of no evidence of glacial phenomena in Eocene strata excepting the occurrence of huge masses of included gneiss in the strata known as Flysch in Switzerland. On this question, however, Swiss geologists are by no means agreed, and I attach little or no importance to it as affording evidence of glacier ice.

Neither do I know of any Miocene glacier-deposits excepting those in the north of Italy near Turin, described by the late eminent geologist, Gastaldi, and which I saw under his guidance. These contain many large erratic boulders derived from the distant Alps, which, in my opinion, were then at least as lofty or even higher than they are now, especially if we consider the immense amount of denudation which they underwent during Miocene, later Tertiary, and post-tertiary times.

At a still later date there took place in the north of Europe and America what is usually misnamed "*The* Glacial Epoch," when a vast glacial mass covered all Scandinavia, and distributed its boulders across the north of Germany, as far south as the country around Leipzig, when Ireland also was shrouded in glacier ice, and when a great glacier covered the larger part of Britain, and stretched southward, perhaps nearly as far as the Thames on the one side, and certainly covered the whole of Anglesey, and probably the whole, or nearly the whole, of South Wales. This was after the advent of man.

Lastly, there is still a minor Glacial Epoch in progress on the large and almost unknown Antarctic continent, from the high land of which in latitudes which partly lie as far north as 60° and 62°, a vast sheet of glacier-ice of great thickness extends far out to sea and sends fleets of icebergs to the north, there to melt in warmer latitudes. If in accordance with the theory of Mr. Croll, founded on astronomical data, a similar climate were transferred to the northern hemisphere, the whole of Scandinavia and the Baltic would apparently be covered with glacier-ice, and the same would probably be the case with the Faroe Islands and great part of Sibera, while even the mountain tracts of Britain might again maintain their minor systems of glaciers.

Conclusions.

In opening this address, I began with the subject of the oldest metamorphic rocks that I have seen—the Laurentian strata. It is evident to every person who thinks on the subject that their deposition took place *far from the beginning of recognised geological time.* For there must have been older rocks by the degradation of which they were formed. And if, as some American geologists affirm, there are on that continent metamorphic rocks of more ancient dates than the Laurentian strata, there must have been rocks more ancient still to afford materials for the deposition of these pre-Laurentian strata.

Starting with the Laurentian rocks, I have shown that the phenomena of *metamorphism* of strata have been continued from that date all through the later formations, or groups of formations, down to and including part of the Eocene strata in some parts of the world.

In like manner I have shown that ordinary volcanic rocks have been ejected in Silurian, Devonian, Carboniferous, Jurassic, Cretaceo-oolitic, Cretaceous, Eocene, Miocene, and Pliocene times, and from all that I have seen or read of these ancient volcanoes, I have no reason to believe that volcanic forces played a more important part in any period of geological time than they do in this our modern epoch.

So, also, mountain chains existed before the deposition of the Silurian rocks, others of later date before the Old Red Sandstone strata were formed, and the chain of the Ural before the deposition of the Permian beds. The last great upheaval of the Alleghany Mountains took place between the close of the formation of the Carboniferous strata of that region and the deposition of the New Red Sandstone.

According to Darwin, after various oscillations of level, the Cordillera underwent its chief upheaval after the Cretaceous epoch, and all geologists know that the Alps, the Pyrenees, the Carpathians, the Himalayas, and other mountain-chains (which I have named) underwent what seems to have been their chief great upheaval after the deposition of the Eocene strata, while some of them were again lifted up several thousands of feet after the close of the Miocene epoch.

The deposition of salts from aqueous solutions in inland lakes and lagoons appears to have taken place through all time—through Silurian, Devonian, Carboniferous, Permian, Triassic, Jurassic, Cretaceous, Eocene, Miocene, and Pliocene epochs—and it is going on now.

In like manner fresh-water and estuarine conditions are found now in one region, now in another, throughout all the formations or groups of formations possibly from Silurian times onward; and glacial phenomena, so far from being confined to what was and is generally still termed *the* Glacial Epoch, are now boldly declared, by independent witnesses of known high reputation, to begin with the Cambrian epoch, and to have occurred somewhere, at intervals, in various formations, from almost the earliest Palæozoic times down to our last post-Pliocene "Glacial Epoch."

If the nebular hypothesis of astronomers be true (and I know of no reason why it should be doubted), the earth was at one time in a purely gaseous state, and afterwards in a fluid condition, attended by intense heat. By-and-bye, consolidation, due to partial cooling, took place on the surface, and as radiation of heat went on, the outer shell thickened. Radiation still going on, the interior fluid matter decreased in bulk, and, by force of gravitation, the outer shell being drawn towards the interior, gave way, and, in parts, got crinkled up, and this, according to cosmogonists, was the origin of the earliest mountain-chains. I make no objection to the hypothesis, which, to say the least, seems to be the best that can be offered and looks highly probable. But, assuming that it is true, these hypothetical events took place so long before authentic geological history began, as written in the rocks, that the earliest of the physical events to which I have drawn your attention in this address was, to all human apprehension of time, so enormously removed from these early assumed cosmical phenomena, *that they appear to me to have been of comparatively quite modern occurrence, and to indicate that from the Laurentian epoch down to the present day, all the physical events in the history of the earth have varied neither in kind nor in intensity from those of which we now have experience.* Perhaps many of our British geologists hold similar opinions, but, if it be so, it may not be altogether useless to have considered the various subjects separately on which I depend to prove the point I had in view.

Action of Phosphorus Perchloride and Oxychloride upon Cinchonine Hydrochlorate.—W. Königs.—These two substances taken singly have little effect upon cinchonine, but if applied jointly a substance is produced which may be regarded as cinchonine where chlorine has taken the place of hydroxyl.—*Berichte der Deutschen.*

THF THALLEIOQUIN TEST.*

By CHARLES FREDERICK ZELLER.

This test, which is one of the most beautiful of the alkaloidal tests, was accidentally discovered by M. J. J. Andre, in 1835. He presented a memoir† to the College of Pharmacy of Paris, "On the Action of Acid on Quinia," in which he endeavours to explain the cause of the fluorescence of quinia solutions, and then the action of chlorine on these solutions. He considered quinia to be a sort of resinate of ammonia, and in attempting to precipitate the resin, which he supposed to have been acted upon by the chlorine, by means of water of ammonia, he obtained a green precipitate, which was immediately re-dissolved by the liquid, and communicated to it a rich emerald colour.

The name was probably derived from the Greek word "Thallos," meaning green, and quinia or chinin, the English and German words indicating the source from which obtained. As this action of chlorine water and ammonia upon quinia had often been spoken of on account of its extreme delicacy, and also of the influence of the presence of hydrochloric acid upon the delicacy of the test, the following experiments were entered into to determine these points. As both quinia and quinidia are similarly acted upon, solutions of both were used.

A. *Solution of Sulphate of Quinia and Fresh Chlorine Water.*

1. Solution of sulphate of quinia (1 in 1000) 100 grs., chlorine water 25 drops = 23 grs., liquor ammonia 1 drop = 0·8 gr. produced green colour at once.

2. Solution of sulphate of quinia 100 grs., chlorine water 15 drops, dilute ammonia 1 drop, produced first a green colour, then, by carefully shaking up the liquid, there was produced in its stead a beautiful rose colour, which in the course of two hours assumed a brownish colour, which changed in one week to light yellow.

3. Same as No. 2, and after rose colour was produced a second drop of dilute ammonia deepened the colour, third drop changed to dark green, and fourth drop a distinct emeral green.

4. Solution of sulphate of quinia 100 grs., chlorine water 10 drops, dilute ammonia 1 drop; same effect as in No. 2.

5. Solution of sulphate of quinia 100 grs, chlorine water 10 drops, dilute ammonia 1 drop, and then 3 more drops were added, which had the same effect as in No. 3.

6. Solution of sulphate of quinia 100 grs., chlorine water 5 drops, dilute ammonia 1 drop, produced green colour, light at first, but deepens by standing.

7. Solution of sulphate of quinia (1 in 2000) 100 grs., chlorine water 5 drops, diluted ammonia 1 drop, gives green colour well, but does not change to rose colour.

8. Solution of sulphate of quinia (1 in 4000) 100 grs., chlorine water 5 drops, dilute ammonia 1 drop, gives a light green tint, which fades away in about twelve hours.

B. *Solution of Sulphate of Quinidia and Fresh Chlorine Water.*

1. Solution of sulphate of quinidia (1 part in 1000) 100 grs., chlorine water 25 drops = 23 grs., liquor ammonia 1 drop = 0 8 gr.; same result as A 1.

2 and 3. Solution of sulphate of quinidia, chlorine water and dilute ammonia, same quantities and same results as A 2 and A 3.

4. Solution of sulphate of quinidia 100 grs., chlorine water 10 drops, dilute ammonia 1 drop, gave green colour, did not change to rose.

5. Solution of sulphate of quinidia 100 grs., chlorine water 5 drops, dilute ammonia 1 drop, gave a distinct green colour at once.

6. Solution of sulphate of quinidia (1 in 2000) 100 grs., chlorine water 5 drops, dilute ammonia 1 drop, distinct green, lighter than B 5.

7. Solution of sulphate of quinidia (1 in 4000) 100 grs., chlorine water 5 drops, dilute ammonia 1 drop, gave only a very light greenish yellow tint.

The above experiments were performed in order to obtain the smallest amounts of chlorine water and ammonia which would take part in the reaction; the proportions used by Brande* were: 1 gr. sulphate of quinia in 100 grs. of water, 200 drops of the aqueous solution of chlorine, recently prepared, and 10 or 20 drops of the solution of ammonia. This colour is stated and was proven by experiment to be so intense that it can be diluted with twenty thousand parts of water and it will still retain an appreciable green tint. The proportions recommended in the Pharmacographia are:—Solution of quinia, or one of its salts, 10 volumes, mixed with 1 volume of chlorine water, and then 1 drop of solution of ammonia is added; in solutions containing *less* $\frac{1}{1000}$ of quinia no precipitate is formed. The last-mentioned proportions were tried, but did not produce a perfectly clear liquid. The method of performing the test was as follows: the alkaloidal solution was first poured into the test-tube, the chlorine water then dropped into it, care being taken that none ran along on the side of the test-tube, the drop of ammonia was then dropped into the centre of the liquid; the result was a slight reaction, with the production of white vapours, and a green layer on the top of the liquid, which was then shaken up. The first experiment with both sulphate of quinia and quinidia solutions cause the best and most permanent results, although much smaller amounts of chlorine water and ammonia gave the same results, as may be seen by experiment A 7: the colour is not of the same intensity, becoming lighter as the alkaloidal solution decreases in strength. The liquid as thus obtained is of a beautiful emerald-green colour, has a slight somewhat ammoniacal odour; by standing, an amorphus dark green substance is deposited, which is insoluble in ether, chloroform, and benzine, but soluble in absolute alcohol; when heated it fuses and is decomposed, giving off white vapours having a disagreeable slightly ammoniacal odour. The thalleioquin solution when heated first darkens in colour, and is then decomposed, becoming dark brown.

The experiments which now follow were performed in order to ascertain the effect of hydrochloric acid in chlorine water, tables C and D being given first as representing the most natural way in which acid would be present in chlorine water (by decomposition), the others being given to show the amount of acid which would prevent or modifiy the test. The chlorine water used in tables C and D was over one year old, had been kept in a dark place, in an amber-coloured bottle, tightly corked with a rubber stopper, was nearly colourless, but still had the odour of chlorine; examined by the mercury test before mentioned, it gave an acid reaction with litmus solution.

C. *Solution of Sulphate of Quinia and Old Chlorine Water, Specific Gravity* 1·0012.

1. Solution of sulphate of quinia (1 in 1000) 100 grs., chlorine water 25 drops, solution ammonia 1 drop, gave the emerald-green colour.

2. Solution of sulphate of quinia 100 grs., chlorine water 15 drops, diluted ammonia 1st drop produced a green tint, and in a few seconds the solution was almost colourless and required 3 drops more dilute ammonia to bring back a pale green colour. (No rose colour was produced.)

3. Solution of sulphate of quinia 100 grs., chlorine water 10 drops dilute ammonia 1 drop, gave a very slight yellowish green; 3 drops of dilute ammonia were required to bring out the emerald colour.

4. Solution of sulphate of quinia (1 in 2000) 100 grs., chlorine water 5 drops, dilute ammonia 1st drop a very pale green; 4 drops were required to produce a marked green colour.

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* From an Inaugural Essay.

† *Amer. Jour. Pharm.*, 1836, p. 212.

* *Amer. Jour. Pharm.*, 1839, p. 37.

5. Solution of sulphate of quinia (1 in 4000) 100 grs., chlorine water 5 drops, dilute ammonia 1 drop, and then same as C 4, with same result.

D. *Solution of Sulphate of Quinidia and Old Chlorine Water, Specific Gravity 1·0012.*

1. Solution of sulphate of quinidia (1 in 1000) 100 grs., &c.; same as C 1, with same result.
2. Solution of sulphate of quinidia 100 grs., chlorine water 15 drops, dilute ammonia 1 drop, produced first green, then a light rose tint, which deepened on standing.
3. Same as D 2; a second drop of dilute ammonia brought back green colour.
4. Solution of sulphate of quinidia 100 grs., chlorine water 10 drops, dilute ammonia 1 drop, gave green colour.
5. Solution of sulphate of quinidia 100 grs., chlorine water 5 drops, dilute ammonia 1 drop produced a white precipitate.
6. Solution of sulphate of quinidia (1 in 2000) 100 grs., chlorine water 5 drops, dilute ammonia 1 drop; same effect as C 4.
7. Solution of sulphate of quinidia (1 in 4000) in same manner as C 4, with same result, the green which was produced being lighter than by D 6.

It will be seen, by looking over these tables, that as the amount of hydrochloric acid was increased the ammonia was proportionately increased also; for instance, fresh chlorine water without acid required 1 drop solution ammonia to produce the emerald-green colour and this seems to prove that after a certain amount of acid has been neutralised the excess of chlorine bleaches out the colour, forming a yellow solution, which turns to an amber-brown colour if the acid be not present in too large a quantity.

Amount of Solution of Ammonia Required.

	Old Chlorine Water.	5 p. c. Acid.	10 p. c. Acid.	15 p. c. Acid.	25 p. c. Acid.	50 per cent Acid.
Quinia solution—	2 drops	2 drops	3 drops	3 drops	5 drops	scarcely shows ppt.
Quinidia—	2 drops	2 drops	3 drops	3 drops	5 drops	scarcely shows ppt

As a matter of interest the bromine water-test was tried, having been recommended as being more delicate than chlorine water. A saturated solution of bromine was made by dissolving 12 drops of bromine in 1 fluid ounce of distilled water. It was found by experiment that 100 grs. of sulphate of quinia or quinidia solution required but 5 drops of bromine water and 1 drop solution ammonia to produce a clear emerald-green colour.

To sum up these experiments it may be stated:—1st. That the chlorine water for performing this test should be freshly prepared is not absolutely necessary, *provided* it is preserved in amber-coloured glass bottles, tightly corked, and kept in a dark place. Chlorine water which had been kept in this manner over a year gave test colour well, as seen in tables C and D. 2nd. That hydrochloric acid, when added to *fresh* chlorine water, in quantities not exceeding 25 per cent, does not prevent its giving the test colour, but will require a proportionately large amount of ammonia to neutralise the acid so added. 3rd. That when hydrochloric acid is present in *old* chlorine water (the result of decomposition) it is due to the loss of chlorine so incurred that the solution loses its value as a test liquid. 4th. It has been shown that bromine water is about four times more delicate than chlorine water, requiring but one-fifth to one-twelfth as much to produce the same results. 5th. On account of the simplicity and ease with which bromine water can be made, it is very much to be preferred as a means of performing the thalleioquin test.—*American Journal of Pharmacy.*

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Berichte der Deutschen Chemischen Gesellschaft zu Berlin. No. 3, 1880.

Behaviour of Mono-chlor-tetracrylic Acid on Melting.—A. Geuther.—The author remarks that he has been misunderstood by G. W. A. Kahlbaum (*Berichte*, xii., p. 2337).

Dinitro-para-toluidine.—F. Beilstein.—Dinitro-toluidine obtained from para-toluidine may be regarded as proximate meta-dinitro-para-toluidine, whilst that prepared from trinitro-toluol is symmetrical dinitro-toluidine.

Ethylen-iodo-picrate.—L. W. Andrews.—This compound melts at 69·5°, is insoluble in water, sparingly soluble in cold alcohol and in ether, but readily in chloroform. With potassium cyanide it yields a colouring-matter analogous to potassium picro-cyaminate. Ethylen-bromide does not act upon potassium picrate at 130°.

A New Kind of Ammonium Compounds (Memoir 1).—P. Griess.—In this paper the author describes the tri-methyl-phenol-ammonium bases, obtained by the action of an excess of methyl-iodide upon the isomeric amido-phenols. He describes ortho-trimethyl-phenol-ammonium, its hydriodate, hemi-hydriodate, hydrochlorate, hydro-nitro-prusside; the platinum double salt and the periodide. Ortho-trimethyl-phenol-ammonium yields on dry distillation ortho-dimethyl-amido-anisol, which greatly resembles dimethyl-aniline, and like it yields peculiar colouring-matters, which the author is examining.

Contribution to the Knowledge of the Protein Bodies.—A. Stutzer.—The copper hydroxide used by Ritthausen for the precipitation of dissolved protein compounds may be advantageously used to separate such bodies from other nitrogenous bodies occurring in vegetable juices. All the protein compounds examined by the author can be resolved into two bodies or two groups by the action of pepsine with hydrochloric acid. On the one hand are formed soluble peptones, acid albuminates, &c., whilst a well-defined portion remains undigested. The latter group contains phosphorus along with nitrogen, and seems to resemble nucleine.

Tropidine.—A. Ladenburg.—Tropidine may also be formed by heating tropine with dilute sulphuric acid.

Hydroscyamine.—A. Ladenburg.—The author regards hydroscyamine as isomeric with atropine. The formula of Höhn and Reichardt is rejected.

Duboisine.—A. Ladenburg.—The object of this paper is a demonstration of the identity of duboisine and hyoscyamine.

The History of Periodic Atomistics.—Lothar Meyer. Noticed elsewhere.

Acetylisation of Certain Carbohydrates on Liebermann's Process.—A. Herzfeld.—Not suitable for abstraction.

Rouge Francaise.—W. v. Miller.—This colour is a mixture of the sodium salts of two tinctorial acids,—a yellow and a red. Both form acicular crystals and yield sparingly soluble barium salts.

Oxyacrylic Acid.—P. Melikoff.—The composition of the potassium salt of this acid is $C_3H_3KO_3+\frac{1}{2}H_2O$.

New Hydrogen Lines and the Dissociation of Calcium.—H. W. Vogel.—Already noticed.

Phenyl-glycolide of Mustard Oil.—C. Liebermann and M. Voeltzkow.—Not suitable for abstraction.

Aromatic Products of the Animal Body.—E. Baumann.—Hydro-para-cumaric acid is readily formed from tyrosine, which compound may, in the author's opinion, very probably undergo a similar change in the organism. He has succeeded in isolating paroxy-phenyl-acetic acid from human urine.

Preparation of Anthranilic Acid from Ortho-nitro-toluol.—P. Greiff.—Dibrom-nitro-toluol being isomeric with dibrom-anthranilic acid, yielded anthranilic acid on treatment with sodium amalgam.

No. 4, 1880.

Phenyl-lactic Acids.—Emil Erlenmeyer.—The author points out the distinctions between his phenyl-lactic acid and that discovered by Glaser.

Phenyl-brom-lactic Acid. — E. Erlenmeyer. — Not suitable for abstraction.

Ortho-nitro-benzaldehyd and its Behaviour on Treatment with Nascent Hydrogen.—C. Rudolph.—By the action of stannous chloride and hydrochloric acid upon ortho-nitro-benzaldehyd the author has obtained relatively large quantities of a base, C_7H_4ClN, melting at 82° to 84°.

Amides and Anilides of β-Oxybutyric Acid.—L. Balbiano.—The β-amido-butyric amide is a syrupy liquid readily soluble in water and hot alcohol, and very sparingly in ether. The hydro-chlorate of the β-amido-butyric anilide is insoluble in ether, sparingly soluble in boiling water, and melts at 206° to 207°. The free base was not examined.

Disulpho-phenanthrenic Acid and some of its Derivatives.—E. Fischer.—The author forms this acid by gradually adding, with brisk agitation, 1 part of phenanthrene to 4 parts of commercial pyrosulphuric acid, heating finally for quarter to half an hour on the water-bath. The free acid after purification is a brownish yellow syrup, of a very sour and bitter taste, and little disposed to crystallise. Its salts are readily soluble in water and insoluble in alcohol and ether.

A Series of New Colouring-matters.—E. Fischer.—Disulpho-phenanthrenic acid, if heated with phenols, especially resorcin, yields richly coloured condensation-products, closely resembling the phthaleines discovered by Baeyer. If 1 mol. of the acid is heated with 2 mols. resorcin on the water-bath, and the temperature then gradually raised to 195° to 200°, the reaction closely resembles that of phthalic acid and resorcin. The product is a brittle green mass, which, when pulverised, yields a dark brown-red powder, and its solutions are even more fluorescent than the corresponding solutions of fluorescein. The author names this substance phenanthren-sulphein-resorcin. It dissolves with difficulty in water, and the solution dyes silk a yellow. The substance takes up bromine, but its tinctorial power is lost by the action of an excess of this halogen. The rosaniline salts of the sulpheine are remarkably beautiful. If the sulpheine is covered with alcohol and mixed with more than its weight of rosaniline, there is formed a splendid cherry-red solution, which dyes the same shade upon silks. The author proposes the following method for the preparation of disulpho-phenanthrenic acid as commercially available. The phenanthrene is sulphated as above described, poured into water, neutralised with lime, separated by pressure from calcium sulphate, the solution mixed with milk of lime in excess, and the lime precipitated by neutralisation with sulphuric acid. The calcium carbonate thrown down carries with it the impurities.

Iso-phthalo-phenon.—E. Ador.—The author has recognised the symmetric constitution of this body.

Action of Carbonic Oxide upon the Alkali Hydrates at Higher Temperatures.—A. Geuther.—Referring to a paper on the formation of formic acid during the action of carbonic oxide on the hydrates of potassium and sodium at temperatures above 100° (*Berichte*, xiii., p. 23), the author points out that this result was long ago obtained and described by Fröhlich and himself.

Synthesis of Formic Acid.—O. Loew.—As far back as 1864 the author observed that when carbon disulphide and water mixed with iron filings are heated for a length of time to 100° in a closed tube, the final products of the reaction are iron sulphide, ferrous formiate, carbon dioxide, and two bodies consisting of carbon, hydrogen, and sulphur.

Detection and Determination of Chlorine in Presence of Bromine and Iodine.—G. Vortmann.—Inserted at length.

Cinnamic Aldehyd as a Scission-Product in the Pancreatic Digestion of Fibrin.—J. Ossikovsky.—The author detects cinnamic aldehyd among the products by its odour. He considers that this result readily explains the presence of phenyl-propionic and phenyl-acetic acids in the putrefaction of the albuminoid bodies, as discovered by the brothers Salkowski.

Contribution to a Knowledge of the Chemical Constitution of Tyrosine and Skatol.—J. Ossikovsky.—Not adapted for useful abridgment.

Speed of Reactions.—B. Pawlewski.—The author concludes that the speed of reactions as regards hydrochloric and nitric acids of known concentration and pure, recently precipitated barium, strontium, and calcium carbonates, is inversely proportional, not to the molecular weights, but to the atomic weights of the metals of the carbonates.

Occurrence of Vanilline in Certain Raw Beet-root Sugars.—C. Scheibler.—The author has obtained from crude sugars vanilline in quantity sufficient for identification by its odour, its behaviour on sublimation, and its crystalline character.

Certain Dichromates.—K. Preis and B. Rayman.—A description of barium, strontium, and lead dichromates.

Action of Iodine upon Aromatic Compounds with long Lateral Chains.—B. Rayman and K. Preis.—The substances experimented upon are cymol, amyl-benzol, and camphor.

Oxidation of Sulphamin-meta-toluylic Acid.—Ira Remsen.—A controversial paper in reply to a memoir by Jacobsen (*Berichte*, xii., p. 2316).

Splitting up of Inactive Malic Acid.—G. J. W. Bremer.—The author proposes to call the inactive acid, capable of being resolved into two active modications, paramalic acid, and the dextro-rotatory form antimalic acid.

Dinitro-naphthaline.—F. Beilstein and A. Kurbatow.—The authors have submitted the two isomeric dinitro-naphthalines to oxidation in order to throw a light upon their structure.

Dinitro-benzoic Acid.—F. Beilstein and A. Kurbatow.—The authors have obtained dinitro-benzoic acid by the oxidation both of α- and β-dinitro-naphthaline.

Action of Dehydrating Agents upon Acids.—B. Vangel.—The author concludes that the mono-basic acids, under the action of dehydrating agents, yield carbonic acid or carbonic oxide; the bibasic acids evolve both gases in the proportion 1 : 1, and the tribasic the same gases in the proportion 1 : 2.

Condensation of Benzhydrol and Naphthaline.—A. Lehne.—By the condensation of these bodies the author obtained naphthyl-diphenyl-methan. He also describes naphthyl-phenyl-carbinol.

Gluten.—T. Weyl and M. Bischoff.—The author's experiments render it probable that myosine is concerned in the formation of gluten, and that gluten as such does not pre-exist in flour.

Daturine.—E. Schmidt.—The author has not been able to find any distinction, chemical or optical, between daturine and atropine.

Daturine.—A. Ladenburg and G. Meyer.—The authors maintain the identity of daturine, hyoscyamine, and duboisine.

Artificial Formation of Tropic Acid.—A. Ladenburg and L. Rügheimer.—The authors have succeeded in successively bringing hydratropasic acid, atrolactic acid, atropic acid, and tropic acid into genetic connection.

Cubic Alum and the Accommodation of the Chrome Alums.—Alfred Polis.—The author examines the conditions for obtaining cubic alum and its behaviour with other alums as regards overgrowth. The best results were obtained by dissolving 250 grms. alum in 800 c.c. water at 25° to 30°, adding 45 grms. sodium carbonate, and exposing the clear solution to spontaneous evaporation. Ten of the crystals first formed were suspended in the liquid, and after 74 to 92 days they had become pure shining permanent cubes. On suspending such cubic alum in a solution of potash-chrome alum the crystal increased as a cube appearing to be a cubic chrome alum crystal, but displaying small, subordinate octahedral surfaces.

Aromatic Amido-Acids.—F. Tiemann.—The author has prepared and examined phenyl-amido-acetic acid.

Sulphuretted Derivatives of Diphenyl.—S. Gabriel and A. Deutsch.—An account of diphenyl-mono-sulphon-chloride, diphenyl-mono-sulph-hydrate, diphenyl-sulphide, diphenyl-sulphon, diphenyl-disulphide, diphenyl-mono-sulphinic acid, diphenyl-mono-sulphonic-ethyl-ether, diphenyl-sulpho-cyanide, diphenyl-mono-sulphacetic acid, diphenyl-disulphon-chloride, diphenyl-disulphamid, and diphenyl-disulphacetic acid.

Experiments on the Vapour-densities of the Alkaline Metals.—Victor Meyer.—From the author's experiments it appears that the vapour-density of sodium cannot be determined in vessels of glass, porcelain, silver, and platinum. Prof. Meyer intends to procure vessels of graphite.

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Organic Chemistry Theoretical Chemistry	H. FRESENIUS, Ph.D.
Chemical Technology	E. BORGMANN, Ph.D.
Mineralogy	F. HENRICH.
Microscopy, with exercises in microscopic work	E. BORGMANN, Ph.D.
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THE CHEMICAL NEWS.

Vol. XLII. No. 1084.

ADDRESS TO THE CHEMICAL SECTION OF THE BRITISH ASSOCIATION.

By J. H. GILBERT, Ph.D., F.R.S., V.P.C.S., F.L.S.,
President of the Section.

Some of my predecessors in this Chair, whose duties as teachers of chemistry lead them to traverse a wide range of the subject every year, have appropriately and usefully presented to the Section a *résumé* of the then recent progress in the manifold branches of the science which have now such far-reaching ramifications. Such a course has, however, come to be of much less importance and interest of late years since the systematic publication by the Chemical Society of abstracts of chemical papers in home and foreign journals as soon as possible after their appearance. Some, on the other hand, have confined attention to some department with which their own enquiries have more specially connected them. And, when the Council of the Association request a specialist like myself to undertake the Presidency of the Section, it is to be supposed that they take it for granted that he will select for his opening address some branch of the subject with which he is known to be mainly associated.

But it seems to me that there is a special reason why I should bring the subject of Agricultural Chemistry before you on the present occasion. Not only is the application of chemistry to agriculture included in the title of this Section; but in 1837 the Committee of the Section requested the late Baron Liebig to prepare a report upon the then condition of Organic Chemistry, and it is now exactly forty years since Liebig presented to the British Association the first part of his report, which was entitled "Organic Chemistry in its Applications to Agriculture and Physiology"; and the second part was presented two years later, in 1842, under the title of "Animal Chemistry, or Organic Chemistry in its Application to Physiology and Pathology." Yet, so far as I am aware, no President of the Section has, from that time to the present, taken as the subject of his address the Application of Chemistry to Agriculture.

Appropriate as, for these reasons, it would seem that I, who have devoted a very large portion of the interval since the publication of Liebig's works, above referred to, to agricultural enquiries, should occupy the short time that can be devoted to such a purpose in attempting to note progress on that important subject, it will be readily understood that it would be quite impossible to condense into the limits of an hour's discourse anything approaching to an adequate account, either of the progress made during the last forty years, or of the existing condition of agricultural chemistry.

For what is agricultural chemistry? It is the chemistry of the atmosphere; the chemistry of the soil; the chemistry of vegetation; and the chemistry of animal life and growth. And but a very imperfect indication of the amount of labour which has been devoted of recent years to the investigation of these various branches of what might at first sight seem a limited subject will suffice to convince you how hopeless a task it would be to seek to do more than direct attention to a few points of special interest. Indeed, devoting to the purpose such leisure as I have been able to command, the more I have attempted to become acquainted with the vast literature which has been accumulated on the subject, the more difficulty have I felt in making a selection of illustrations which should not convey an idea of the limits, rather than of the extent, of the labour which has been expended, and of the results which had been attained, in agricultural research.

The works of Liebig to which I have referred have, as you all know, been the subject of a very great deal of controversy. Agricultural chemists, vegetable physiologists, and animal physiologists have each vehemently opposed some of the conclusions of the author, bearing upon their respective branches. But if the part which has fallen to my own lot in these discussions qualifies me at all to speak for others as well as myself, I would say that those who, having themselves carefully investigated the points in question, have the most prominently dissented from any special views put forward in those works, will—whether they be agricultural chemists, vegetable physiologists, or animal physiologists—be the first to admit how vast has been the stimulus, and how important has been the direction, given to research in their own department, by the masterly review of then existing knowledge, and the bold, and frequently sagacious, generalisations of one of the most remarkable men of his time!

Confining attention to researches bearing upon agriculture, it will be well, before attempting to indicate either the position established by Liebig's first works, or the direction of the progress since made, to refer very briefly to the early history of the subject.

From what we now know of the composition and of the sources of the constituents of plants, it is obvious that a knowledge of the composition of the atmosphere and of water was essential to any true conception of the main features of the vegetative process; and it is of interest to observe that it was almost simultaneously with the establishment, towards the end of the last century, of definite knowledge as to the composition of the air and of water, that their mutual relations with vegetation were first pointed out. To the collective labours of Black, Scheele, Priestly, Lavoisier, Cavendish, and Watt, we owe the knowledge that common air consists chiefly of nitrogen and oxygen, with a little carbonic acid: that carbonic acid is composed of carbon and oxygen; and that water is composed of hydrogen and oxygen; whilst Priestly and Ingenhousz, Sennebier and Woodhouse, investigated the mutual relations of these bodies and vegetable growth. Priestly observed that plants possessed the faculty of purifying air vitiated by combustion or by the respiration of animals; and, he having discovered oxygen, is was found that the gaseous bubbles which Bonnet had shown to be emitted from the surface of leaves plunged in water consisted principally of that gas. Ingenhousz demonstrated that the action of light was essential to the development of these phenomena; and Sennebier proved that the oxygen emitted resulted from the decomposition of the carbonic acid taken up.

So far, however, attention seems to have been directed more prominently to the question of the influence of plants upon the media with which they were surrounded, than to that of the influence of those media in contributing to the increased substance of the plants themselves. Towards the end of the last century, and in the beginning of the present one, De Saussure followed up these enquiries; and in his work entitled "Recherches Chimiques sur la Végétation," published in 1804, he may be said to have indicated, if not indeed established, some of the most important facts with which we are yet acquainted regarding the sources of the constituents stored up by the growing plant. De Saussure illustrated experimentally, and even to some extent quantitatively, the fact that in sunlight plants increase in carbon, hydrogen, and oxygen, at the expense of carbonic acid and of water; and in the case of his main experiment on the point, he found the increase in carbon, and in the elements of water, to be very closely in the proportion in which these are known to exist in the carbohydrates. He further maintained the essentialness of the mineral or ash constituents of plants; he pointed out that they must be derived from the soil; and he called attention to the probability that the incombustible constituents so derived by plants from the soil were the source of those found in the animals fed upon them.

With regard to the nitrogen which plants had already been shown to contain, Priestly and Ingenhousz thought their experiments indicated that they absorbed free nitrogen from the atmosphere; but Sennebier and Woodhouse arrived at an opposite conclusion. De Saussure, again, thought that his experiments showed rather an evolution of nitrogen at the expense of the substance of the plant than any assimilation of it from gaseous media. He further concluded that the source of the nitrogen of plants was more probably the nitrogenous compounds in the soil, and the small amount of ammonia which he demonstrated to exist in the atmosphere.

Upon the whole, De Saussure concluded that air and water contributed a much larger proportion of the dry substance of plants than did the soils in which they grew. In his view a fertile soil was one which yielded liberally to the plant nitrogenous compounds, and the incombustible or mineral constituents; whilst the carbon, hydrogen, and oxygen, of which the greater proportion of the dry substance of the plant was made up, were at least mainly derived from the air and water.

Perhaps I ought not to omit to mention here that, each year for ten successive years, 1802 to 1812, Sir Humphry Davy delivered a course of lectures on the "Elements of Agricultural Chemistry," which were first published in 1813, were finally revised by the author for the Fourth Edition in 1827, but have gone through several editions since. In those Lectures, Sir Humphry Davy passed in review and correlated the then existing knowledge, both practical and scientific, bearing upon agriculture. He treated of the influences of heat and light; of the organisation of plants; of the difference, and the change, in the chemical composition of their different parts; of the sources, composition, and treatment of soils; of the composition of the atmosphere, and its influence on vegetation; of the composition and the action of manures; of fermentation and putrefaction; and, finally, of the principles involved in various recognised agricultural practices.

With the exception of these discourses of Sir Humphry Davy, the subject seems to have received comparatively little attention, nor was any important addition made to our knowledge in regard to it during the period of about thirty years from the date of the appearance of De Saussure's work in 1804 to that of the commencement of Boussingault's investigations.

About 1834, Boussingault became, by marriage, joint proprietor with his brother-in-law of the estate of Bechelbronn, in Alsace. His brother-in-law, M. Lebel, was both a chemical manufacturer and an intelligent practical farmer, accustomed to use the balance for the weighing of manures, crops, and cattle. Boussingault seems to have applied himself at once to chemico-agricultural research; and it was under these conditions of the association of "practice with science" that the first laboratory on a farm was established.

From this time forward, Boussingault generally spent about half the year in Paris, and the other half in Alsace; and he has continued his scientific labours, sometimes in the city, and sometimes in the country, up to the present time. His first important contribution to agricultural chemistry was made in 1836, when he published a paper on the amount of nitrogen in different foods, and on the equivalence of the foods, founded on the amounts of nitrogen they contained; and he compared the results so arrived at with the estimates of others founded on actual experience. Although his conclusions on the subject have doubtless undergone modification since that time, the work itself marked a great advance on previously existing knowledge, and modes of viewing the question.

In 1837, Boussingault published papers—on the amount of gluten in different kinds of wheat, on the influence of the clearing of forests on the diminution of the flow of rivers, and on the meteorological influences affecting the culture of the vine. In 1838 he published the results of an elaborate research on the principles underlying the value of a rotation of crops. He determined by analysis the composition, both organic and inorganic, of the manures applied to the land, and of the crops harvested. In his treatment of the subject he evinced a clear perception of the most important problems involved in such an enquiry; some of which, with the united labours of himself and many other workers, have scarcely yet reached an undisputed solution.

Thus, in the same year (1838), he published the results of an investigation on the question whether plants assimilate the free or uncombined nitrogen of the atmosphere; and although the analytical methods of the day were inadequate to the decisive settlement of the point, his conclusions were in the main those which much subsequent work of his own, and much of others also, has served to confirm.

As a further element of the question of the chemical statistics of a rotation of crops, Boussingault determined the amount and composition of the residues of various crops; also the amount of constituents consumed in the food of a cow and of a horse respectively, and yielded in the milk and excretions of the cow, and in the excretions of the horse. Here, again, the exigencies of the investigation he undertook were beyond the reach of the known methods of the time. Indeed, rude as the art of agriculture is generally considered to be, the scientific elucidation of its practices requires the most refined, and very varied, methods of research; and a characteristic of the work of Boussingault may be said to be that he has frequently had to devise methods suitable to his purpose before he could grapple with the problems before him.

In 1839, chiefly in recognition of his important contributions to agricultural chemistry, Boussingault was elected a member of the Institute; and in 1878, thirty-nine years later, the Council of the Royal Society awarded to him the Copley Medal, the highest honour at their disposal, for his numerous and varied contributions to science, but especially for those relating to agriculture.

The foregoing brief historical sketch is sufficient to indicate, though but in broad outline, the range of existing knowledge on the subject of agricultural chemistry prior to the appearance of Liebig's memorable work in 1840. It will be seen that some very important and indeed fundamental facts had already been established in regard to vegetation, and that Boussingault had not only extended enquiry on that subject, but he had brought his own and previous results to bear upon the elucidation of long recognised agricultural practices. There can be no doubt that the data supplied by his researches contributed important elements to the basis of established facts upon which Liebig founded his brilliant generalisations. Accordingly, in 1841, Dumas and Boussingault published, jointly, an essay which afterwards appeared in English under the title of "The Chemical and Physiological Balance of Organic Nature"; and, in 1843, Boussingault published a larger work, which embodied the results of many of his own previous original investigations.

But there can be no doubt that the appearance of Liebig's two works, which were contributions made in answer to a request submitted to him by the committee of this Section of the British Association, constituted a very marked epoch in the history of the progress of agricultural chemistry. In the treatment of his subject he not only called to his aid the previously existing knowledge directly bearing upon it, but he also turned to good account the more recent triumphs of organic chemistry, many of which had been won in his own laboratory. Further, a marked feature of his expositions was the adoption of what might be called the *statistical* method—I use the word statistical rather than quantitative, as the latter expression has its own technical meaning among chemists, which is not precisely what I wish to convey.

It seems that, notwithstanding the conclusive evidence afforded by the direct experiments of De Saussure and his predecessors, vegetable physiologists continued to hold the view that the humus of the soil was the source of the carbon of vegetation. Not only did Liebig give full weight to the evidence of the experiments of De Saussure and others, and illustrate the possible and probable transformations within the plant by facts already established in organic chemistry, but he demonstrated the utter impossibility of humus supplying the amount of carbon assimilated over a given area. He pointed out that humus itself was the product of previous vegetable growth, and that it could not therefore be an original source of carbon; and that, from the degree of its insolubility, either in pure water or in water containing alkaline or earthy bases, only a small fraction of the carbon assimilated by plants could be derived from the amount of humus that could possibly enter the plant in solution. He maintained that, so far as humus was beneficial to vegetation at all, it was only by its oxidation, and a consequent supply of carbonic acid within the soil; a source which he considered only of importance in the early stages of the life of a plant, and before it had developed and exposed a sufficient amount of green surface to the atmosphere to render it independent of soil supplies of carbonic acid.

With regard to the hydrogen of plants, at any rate that portion of it contained in their non-nitrogenous products, he maintained that its source must be water; and that the source of the oxygen was either that contained in carbonic acid or that in water.

With regard to the nitrogen of vegetation, both from the known characters of free nitrogen, and as he considered a legitimate deduction from direct experiments, he argued that plants did not take up free or uncombined nitrogen, either from the atmosphere, or dissolved in water and so absorbed by the roots. The source of the nitrogen of vegetation was, he maintained, ammonia; the product of the putrefaction of one generation of plants and animals supplying the ammonia for the next. He pointed out that, in the case of a farm receiving nothing from external sources, and selling off certain products, the amount of nitrogen in the manure derived by the consumption of some of the vegetable produce on the farm itself, together with that due to the refuse of the crops, must always be less than was contained in the crops grown; and he concluded that though the quantity so returned to the land was important, a main source of the nitrogen assimilated over a given area was that brought down from the atmosphere in rain.

There can be no doubt that, owing to the limited and defective experimental evidence then at command on the point, Liebig at that time (as he has since) greatly overestimated the amount of ammonia available to vegetation from that source. In Boussingault's "*réclamation*" already referred to, he gave much more prominence to the importance of the nitrogen of manures. In Liebig's next edition (in 1843) he combatted the notion of the relative importance of the nitrogen of manures; maintained, in opposition to the view put forward in his former edition, that the atmosphere afforded a sufficient supply of nitrogen for cultivated as well as for uncultivated plants; that the supply was sufficient for the cereals as well as for leguminous plants; that it was not necessary to supply nitrogen to the former; and he insisted very much more strongly than formerly on the relative importance of the supply of the incombustible, or, as he designated them, the "inorganic" or "mineral" constituents.

As to the incombustible or mineral constituents themselves, Liebig adduced many illustrations in proof of their essentialness. He called attention to the variation in the composition of the ash of plants grown on different soils; and he assumed a greater degree of mutual replaceability of one base by another, or of one acid by another, than could be now admitted. He traced the difference in the mineral composition of different soils to that of the rocks which had been their source; and he seems to have been led by the consideration of the gradual action of "weathering," in rendering available the otherwise locked-up stores, to attribute the benefits of fallow exclusively to the increased supply of the incombustible constituents which would by its agency be brought into a condition in which they could be taken up by plants.

The benefits of an alteration of crops Liebig considered to be in part explained by the influence of the excreted matters from one description of crop upon the growth of another. He did not attach weight to the assumption that such matters would be directly injurious to the same description of crop; but he supposed rather that the matters excreted were those which the plant did not need, and would therefore be of no avail to the same description of plant, but would be of no use to another. He, however, attributed much of the benefits of a rotation to different mineral constituents being required from the soil by the respective crops.

Treating of manure, he laid the greatest stress on the return by it of the potass and the phosphates removed by the crops. But he also insisted on the importance of the nitrogen, especially that in the liquid excretions of animals, and condemned the methods of treatment of animal manures by which the ammonia was allowed to be lost by evaporation. It is curious and significant, however, that some of the passages in his first edition, in which he the most forcibly urges the value of the nitrogen of animal manures, are omitted in the third and fourth editions.

The discussion of the processes of fermentation, decay, and putrefaction, and that of poisons, contagions, and miasms, constituted a remarkable and important part of Liebig's first report. It was the portion relating to poisons, contagions, and miasms, that he presented to this Section as an instalment, at the meeting of the Association held at Glasgow in 1840. It was in the chapters relating to the several subjects here enumerated that he developed so prominently his views on the influence of contact in inducing chemical changes. He cited many known transformations, other than those coming under either of the heads in question, in illustration of his subject: and he discussed with great clearness the different conditions occurring, and the different results obtained, in various processes—such as the different modes of fermenting beer, the fermentation of wine from different kinds of grapes, the production of acetic acid, &c. As is well known, he claimed a purely chemical explanation for the phenomena involved in fermentation. He further maintained that the action of contagions was precisely similar. In his latest writings on the subject (in 1870), he admits some change of view; but it is by no means easy to decide exactly how much or how little of modification he would wish to imply.

Liebig's second report, presented at the meeting of this Association in 1842, and published under the title of "Animal Chemistry, or Organic Chemistry in its applications to Physiology and Pathology," perhaps excited even more attention than his first; and, probably from the manner as much as from the matter, aroused a great deal of controversy, especially among physiologists and physicians. Liebig was severe upon what he considered to be a too exclusive attention to morphological characters in physiological research, and at any rate too little attention to chemical phenomena, and, so far as these were investigated, an inadequate treatment of the subject according to strictly quantitative methods.

He combatted the view that nervous action, as such, could be a source of any of the heat of the body; and he adduced numerous illustrations and calculations in support of the view that the combustion of carbon and hydrogen in the system was sufficient to account for, and was the only source of, animal heat.

He compared and contrasted the general composition of plants and animals. In accordance with Mulder, he pointed out that whilst plants formed the nitrogenous bodies which they contain from carbonic acid, water, and ammonia, animals did not produce them, but received them ready-formed in their vegetable food; that, in fact, the animal begins only where the plant ends. But, going beyond Mulder, and beyond what had then, or has since, been established, he maintained the identity in composition of the admittedly analogous nitrogenous compounds in plants and in the blood of animals.

Omitting the fat which the carnivora might receive in the animals they consumed, he stated the characteristic difference between the food of carnivora and herbivora to be, that the former obtained the main proportion of their respiratory material from the waste of tissue; whilst the latter obtained a large amount from starch, sugar, &c. These different conditions of life accounted for the comparative leanness of carnivora and fatness of herbivora.

He maintained that the vegetable food consumed by herbivora did not contain anything like the amount of fat which they stored up in their bodies; and he showed how nearly the composition of fat was obtained by the simple elimination of so much oxygen, or of oxygen and a little carbonic acid, from the various carbohydrates. Much less oxygen would be required to be eliminated from a quantity of fibrine, &c., containing a given amount of carbon than from a quantity of carbohydrates containing an equal amount of carbon. The formation of fatty matter in plants was of the same kind; it was the result of a secondary action, starch being first formed from carbonic acid and water.

He concluded from the facts adduced that the food of man might be divided into the *nitrogenised* and the *non-nitrogenised* elements. The former were capable of conversion into blood, the latter incapable of such transformation. The former might be called the *plastic elements of nutrition*, the latter *elements of respiration*. From the plastic elements, the membranes and cellular tissue, the nerves and brain, cartilage, and the organic part of bones, could be formed; but the plastic substance must be received ready-made. Whilst gelatine or chondrine was derived from fibrine or albumen, fibrine or albumen could not be re-produced from gelatine or chondrine. The gelatinous tissues suffer progressive alteration under the influence of oxygen, and the materials for their re-formation must be restored from the blood. It might, however, be a question whether gelatine taken in food might not again be converted into cellular tissue, membrane, and cartilage, in the body.

At that time, adopting and attaching great importance to Mulder's views in regard to proteine, he says:—"All the organic nitrogenised constituents of the body, how different soever they may be in composition, are derived from proteine. They are formed from it by the addition or subtraction of the element of water or of oxygen, and by re-solution into two or more compounds."

He seeks to trace the changes occurring in the conversion of the constituents of food into blood, of those of blood into the various tissues, and of these into the secretions and excretions.

He states that the process of chymification takes place in virtue of a purely chemical action, exactly similar to those processes of decomposition or transformation which are known as putrefaction, fermentation, or decay. Thus, the clear gastric juice contains a substance in a state of transformation, by the contact of which with the insoluble constituents of the food they are rendered soluble, no other element taking any share in the action excepting oxygen and the elements of water. All substances which can arrest the phenomena of fermentation and putrefaction in liquids also arrest digestion when taken into the stomach. Putrefying blood, white of egg, flesh, and cheese, produce the same effects in a solution of sugar as yeast or ferment; the explanation being that ferment, or yeast, is nothing but vegetable fibrine, albumen, or caseine, in a state of decomposition.

Referring to the derivation of the animal tissues, he says they all contain, for a given amount of carbon, more oxygen than the nitrogenous constituents of blood. In hair and gelatinous membrane there is also an excess of nitrogen and hydrogen, and in the proportions to form ammonia. We may suppose an addition of these elements, or a subtraction of carbon, the amount of nitrogen remaining the same. The gelatinous substance is not a compound of proteine; it contains no sulphur, no phosphorus; and it contains more nitrogen, or less carbon, than proteine.

He next, as he says, attempts to develop analytically the principal metamorphoses which occur in the animal body. He adds that the results have surprised himself no less than they will others, and have excited in his own mind the same doubts as others will conceive. He nevertheless gives them, because he is convinced that the method by which they have been obtained is the only one by which we can hope to acquire an insight into the nature of organic processes.

Referring to the animal secretions, he argues that they must contain the products of the metamorphosis of the tissues. He says a starving man with severe exertion secretes more urea than the most highly fed individual in a state of rest; and he combats the idea that the nitrogen of the food can pass into urea without having previously become part of an organised tissue.

Having shown the chemical relations of bile and urine to the proteine bodies, he illustrates, by formulæ, the connection between allantoine and the constituents of the urine of animals that respire. He insists that in the herbivora the carbohydrates must take part in the formation of bile; and he calculates the number of equivalents of proteine, starch, oxygen, and water, which would yield a given number of equivalents of urea, choleic acid, ammonia, and carbonic acid. The non-nitrogenous constituents in the food of the herbivora retard the the metamorphosis of the nitrogenous bodies, rendering this less rapid than in the carnivora. It may be said that proteine, starch, and oxygen give the secretions and excretions, carbonic acid by the lungs, urea and carbonate of ammonia by the kidneys, choleic acid by the liver. It is the study of the phenomena which accompany the metamorphoses of the food in the organism, the discovery of the share which the atmosphere and the elements of water take in these changes, by which we shall learn the conditions necessary for the production of a secretion or of an organised part.

He traces the possible formation of taurine from caffeine or asparagine by their assumption of oxygen and of the elements of water. And from the composition of the vegetable alkaloids he suggests the possibility of their taking a share in the formation of new, or the transformation of existing, brain and nervous matter.

Finally, in reference to these various illustrations and considerations, he says however hypothetical they may appear, they deserve attention in so far as they point out the way which chemistry must pursue if she would really be of service to physiology and pathology. Chemistry, he says, relates to the conversion of food into the various tissues and secretions, and to their subsequent metamorphosis into lifeless compounds.

After this lapse of time, it will certainly be granted that, quite irrespectively of the admissibility or otherwise of the particular illustrations adduced, or of the truth or error of and of the conclusions drawn—and some at least are so true that they seem to us now all but truisms, and you may be disposed to ask me why I should tell you over again a story so often told before—there is no doubt that Liebig's manner of treating the subject did exert an immense influence by stimulating investigation, by fixing attention on the points to be investigated and on the methods that must be followed, and thus by leading to the establishment or the correction of any special views he put forward, and to a vast extension of our knowledge on the complicated questions involved.

In the third part of Liebig's second volume he treats of the phenomena of motion in the animal organism. It is to his views in regard to one aspect only of this very wide and very complicated subject that I propose to call your attention here, as it is chiefly in so far as that aspect is concerned that the question is of interest from the point of view of the agricultural chemist. He says:—

"We observe in animals that the conversion of food into blood, and the contact of the blood with the living tissues, are determined by a mechanical force, whose manifestation proceeds from distinct organs, and is effected by a distinct system of organs, possessing the property of communicating and extending the motion which they receive. We find the power of the animal to change its place and to produce mechanical effects by means of its limbs dependent on a second similar system of organs or apparatus."

He points out that the motion of the animal fluids proceeds from distinct organs (as, for example, that of the blood from the heart), which do not generate the force in themselves, but receive it from other parts by means of the nerves: the limbs also receive their moving force in the same way. He adds :—" Where nerves are not found motion does not occur." Again :—

"As an immediate effect of the manifestation of mechanical force, we see that a part of the muscular substance loses its vital properties, its character of life; that this portion separates from the living part, and loses its capacity of growth and its power of resistance. We find that this change of properties is accompanied by the entrance of a foreign body (oxygen) into the composition of the muscular fibre . . .; and all experience proves that this conversion of living muscular fibre into compounds destitute of vitality is accelerated or retarded according to the amount of force employed to produce motion." He adds that a rapid transformation of muscular fibre determines a greater amount of mechanical force, and that conversely a greater amount of mechanical motion determines a more rapid change of matter.

"The change of matter, the manifestation of mechanical force, and the absorption of oxygen, are, in the animal body, so closely connected with each other that we may consider the amount of motion and the quantity of living tissue transformed as proportional to the quantity of oxygen inspired and consumed in a given time by the animal." Again :—

"The production of heat and the change of matter are closely related to each other; but although heat can be produced in the body without any change of matter in living tissues, yet the change of matter cannot be supposed to take place without the co-operation of oxygen."

Further, on the same point :—"The sum of force available for mechanical purposes must be equal to the sum of the vital forces of all tissues adapted to the change of matter. If, in equal times, unequal quantities of oxygen are consumed, the result is obvious in an unequal amount of heat liberated, and of mechanical force. When unequal amounts of mechanical force are expended, this determines the absorption of corresponding and unequal quantities of oxygen."

Then, more definitely still, referring to the changes which take place coincidently with the exercise of force, and to the demands of the system for repair accordingly, he says :—

"The amount of azotised food necessary to restore the equilibrium between waste and supply is directly proportional to the amount of tissues metamorphosed. The amount of living matter, which in the body loses the condition of life, is, in equal temperatures, directly proportional to the mechanical effects produced in a given time. The amount of tissue metamorphosed in a given time may be measured by the quantity of nitrogen in the urine. The sum of the mechanical effects produced in two individuals, in the same temperature, is proportional to the amount of nitrogen in their urine, whether the mechanical force has been employed in the voluntary or involuntary motions, whether it has been consumed by the limbs, or by the heart and other viscera."

Thus, apparently influenced by the physiological considerations which have been adduced, and notwithstanding in some passages he seemed to recognise a connection between the total quantity of oxygen inspired and consumed and the quantity of mechanical force developed, Liebig nevertheless very prominently insisted that the amount of muscular tissue transformed—the amount of nitrogenous substance oxidated—was the measure of the force generated. He accordingly distinctly draws the conclusion that the requirement for the azotised constituents of food will be increased in proportion to the increase in the amount of force expended.

It will be obvious that the question whether in the feeding of animals for the exercise of mechanical force, that is, for their labour, the demands of the system will be proportionally the greater for an increased supply of the nitrogenous or of the non-nitrogenous constituents of food, is one of considerable interest and practical importance. To this point I shall have to refer further on.

So far, I have endeavoured to convey some idea of the state of knowledge on the subject of the chemistry of agriculture prior to the appearance of Liebig's first two works bearing upon it, and also briefly to summarise the views he then enunciated in regard to some points of chief importance. Let us next try to ascertain something of the influence of his teaching.

Confining attention to agricultural research, it may be observed that about the year 1843, that is, very soon after the appearance of the works in question, there was established the Chemico-Agricultural Society of Scotland, which was, I believe, broken up, after it had existed about five years, because its able chemist, the late Professor Johnston, was unable to find a remedy for the potato disease. Shortly after this, the Highland and Agricultural Society of Scotland appointed a consulting chemist; somewhat later the Royal Agricultural Society of England did the same; and later still followed the Chemico-Agricultural Society of Ulster. Lastly, the very numerous "Agricultural Experimental Stations" which have been established, not only in Germany, but in most Continental States, owe their origin directly to the writings, the teachings, and the influence of Liebig. The movement seems to have originated in Saxony, where Stöckhardt had already stimulated interest in the subject by his lectures and his writings. After some correspondence, in 1850-1, between the late Dr. Crucius and others on the one side, and the Government on the other, the first so-called Agricultural Experimental Station was established at Möchern, near Leipzig, 1851-2. In 1877, the twenty-fifth anniversary of the foundation of that institution was celebrated at Leipzig, when an account (which has since been published) was given of the number of stations then existing, of the number of chemists engaged, and of the subjects which had been investigated. From that statistical statement we learn that in 1877 the number of stations was :—

In the various German States ..	74
In Austria	16
In Italy	10
In Sweden	7
In Denmark	1
In Russia	3
In Belgium	3
In Holland..	2
In France	2
In Switzerland	3
In Spain	1
Total	122

Besides these 122 stations on the Continent of Europe, the United States are credited with 1, and Scotland also with 1.

Each of these stations is under the direction of a chemist, frequently with one or more assistants. One special duty of most of them is what is called manure-, or seed-, or feeding-stuff-control; that is, to examine or analyse, and report upon such substances in the market, and it seems to have been found the interest of dealers in these commodities to submit their proceedings to a certain degree of supervision by the chemist of the station of their district.

But agricultural research has also been a characteristic feature of these institutions. It is stated that the investigation of soils has been the prominent object at 16 of them; experiments with manures at 24; vegetable physiology at 28; animal physiology and feeding experiments at 20; vine culture and wine making at 13; forest culture at 9; and milk production at 11. Others, according to their locality, have devoted special attention to fruit culture, olive culture, the cultivation of moor, bog, and peat land, the production of silk, the manufacture of spirit, and other products.

Nor does this enumeration of the institutions established as the direct result of Liebig's influence, and of the subjects investigated under their auspices, complete the list either of the workers engaged, or of the work accomplished in agricultural research. To say nothing of the labours of Boussingault, which commenced some years prior to the appearance of Liebig's first work, and which are fortunately still at the service of agriculture, important contributions have been made by the late Professors Johnston and Anderson in Scotland, and in this country both by Mr. Way and Dr. Voelcker, each alike in his private capacity, and in fulfilment of his duties as Chemist to the Royal Agricultural Society of England. Nor would it be fair to Mr. Lawes (who commenced experimenting first with plants in pots, and afterwards in the field, soon after entering into possession of his property in 1834, and with whom I have myself been associated since 1843), were I to omit in this place any mention of the investigations which have been so many years in progress at Rothamsted.

So much for the machinery; but what of the results achieved by all this activity in the application of chemistry to agriculture?

As I have already intimated, and as the foregoing brief statistical statement will have convinced you must be the case, it will be utterly impossible to give, on such an occasion as this, anything approaching to an adequate review of the progress achieved. Indeed, I have to confess that the more I have looked at the subject with the hope of treating it comprehensively, the more I have been impelled to substitute a very limited plan for the much more extended scheme which I had at first hoped to be able to fill up. I propose then to confine attention to a few special points, which have either some connection with one another, or to which recent results or discussions lend some special interest.

First as to the sources and the assimilation of the carbon, the hydrogen, and the oxygen of vegetation. From the point of view of the agricultural chemist, the hydrogen and the oxygen may be left out of view. For, if the cultivator provide to the plant the conditions for the accumulation of sufficient nitrogen and carbon, he may leave it to take care of itself in the matter of hydrogen and oxygen. That the hydrogen of the carbo-hydrates is exclusively obtained from water is, to say the least, probable: and whether part of their oxygen is derived from carbonic acid, and part from water, or the whole from either of these, will not affect his agricultural practice.

With regard to the carbon, the whole tendency of subsequent observations is to confirm the opinion put forward by De Saussure about the commencement of the century, and so forcibly insisted upon by Liebig forty years later—that the greater part, if not the whole of it, is derived from the carbonic acid of the atmosphere. Indeed, direct experiments are not wanting—those of Moll, for example—from which it has been concluded that plants do not even utilise the carbonic acid which they may take up from the soil by their roots. However this may be, we may safely conclude that practically the whole of the carbon which it is the object of the cultivator to force the plants he grows to take up is derived from the atmosphere, in which it exists in such extremely small proportion, but nevertheless large actual, and constantly renewed amount.

Judging from the more recent researches on the point, it would seem probable that the estimate of one part of carbon, as carbonic acid, in 10,000 of air, is more probably too high than too low as an estimate of the average quantity in the atmosphere of our globe. And, although this corresponds to several times more in the column of air resting over an acre of land than the vegetation of that area can annually take up, it represents an extremely small amount at any one time in contact with the growing plants, and could only suffice on the supposition of a very rapid renewal, accomplished as the result, on the one hand, of a constant return of carbonic acid to the atmosphere by combustion and the respiration of animals, and, on the other, of a constant interchange and equalisation among the constituents of the atmosphere.

It will convey a more definite idea of what is accomplished by vegetation in the assimilation of carbon from the atmosphere if I give, in round numbers, the results of some direct experiments made at Rothamsted, instead of making general statements merely.

In a field which has now grown wheat for thirty-seven years in succession, there are some plots to which not an ounce of carbon has been returned during the whole of that period. Yet, with purely mineral manure, an average of about 1000 pounds of carbon is annually removed from the land; and where a given amount of nitrogenous manure is employed with the mineral manure, an average of about 1500 pounds per acre per annum more is obtained; in all, an average of about 2500 pounds of carbon annually assimilated over an acre of land without any return of carbonaceous manure to it.

In a field in which barley has been grown for twenty-nine years in succession, quite accordant results have been obtained. There, smaller amounts of nitrogenous manure have been employed with the mineral manure than in the experiments with wheat above cited; but the increase in the assimilation of carbon for a given amount of nitrogen supplied in the manure is greater in the case of the barley than of the wheat.

With sugar-beet, again, larger amounts of carbon have been annually accumulated without the supply of any to the soil, but under the influence of a liberal provision of both nitrogenous and mineral manure, than by either wheat or barley.

Lastly, with grass, still larger amounts of carbon have been annually accumulated, without any supply of it by manure.

Many experiments have been made, in Germany and elsewhere, to determine the amount of the different constituents taken up at different periods of the growth of various plants. But we may refer to some made at Rothamsted long ago to illustrate the rapidity with which the carbon of our crops may be withdrawn from the atmosphere.

In 1847 we carefully took samples from a growing wheat crop, at different stages of its progress, commencing on June 21st, and in these samples the dry matter, the mineral matter, the nitrogen, &c., were determined. On each occasion the produce of two separate eighths or sixteenths of an acre was cut and weighed, so that the data were provided to calculate the amounts of the several constituents which had been accumulated per acre at each period. The result was that, whilst during little more than five weeks from June 21st, there was comparatively little increase in the amount of nitrogen accumulated over a given area, more than half the total carbon of the crop was accumulated during that period.

Numerous experiments of a somewhat similar kind, made in another season (1856)), concurred in showing that, whilst the carbon of the crop was more than doubled after the middle of June, its nitrogen increased in a much less degree over the same period.

Similar experiments were also made, in 1854 and in 1856, with beans. The general result was that a smaller proportion of both the total nitrogen and the total carbon was accumulated by the middle or end of June than in the case of the wheat; though the actual amount of nitrogen taken up by the beans was much greater, both before and after that date. The nitrogen of this leguminous crop increased in a much greater proportion during the subsequent stages of growth than did that of the gramineous crop; but the carbon increased in a larger proportion still, three-fourths or more of the total amount of it being accumulated after the middle of June.

I should say that determinations of carbon, made in samples of soil taken from the wheat field at different periods during recent years, indicate some decline in the percentage of carbon in the soils, but not such as to lead to the supposition that the soils have contributed to the carbon of the crops. Besides the amount of carbon annually removed, there will, of course, be a further accumulation in the stubble and roots of the crops; and the reduction in the total carbon of the soil, if such have really taken place, would show that the annual oxidation within the soil is greater than the annual gain by the residue of the crops.

Large as is the annual accumulation of carbon from the atmosphere over a given area in the cases cited, it is obvious that the quantity must vary exceedingly with variation of climatal conditions. It is, in fact, several times as great in the case of tropical vegetation—that of the sugar-cane, for example. And not only is the greater part of the assimilation accomplished within a comparatively small portion of the year (varying of course according to the region), but the action is limited to the hours of daylight, whilst during darkness there is rather loss than gain.

But it is remarkable that whilst the accumulation of carbon, the chief gain of solid material, takes place under the influence of light, cell-division, cell-multiplication, increase in the structure of the plant; in other words, what, as distinguished from assimilation, vegetable physiologists designate as *growth*, takes place, at any rate chiefly, during the night, and is accompanied not with the taking up of carbonic acid and the yielding up of oxygen, but with the taking up of oxygen and the giving up of carbonic acid. This evolution of carbonic acid during darkness must obviously be extremely small, compared with the converse action during daylight, coincidentally with which practically the whole of the accumulation of solid substance is accomplished. But, as the product of the night action is the same as in the respiration of animals, this is distinguished by vegetable physiologists as the respiration of plants.

I suppose I shall be considered a heretic if I venture to suggest that it seems in a sense inappropriate to apply the term *growth* to that which is associated with actual loss of material, and that the term *respiration* should be applied to so secondary an action as that as the result of which carbonic acid is given off from the plant. It may, I think, be a question whether there is any advantage in thus attempting to establish a parallelism between animal and vegetable processes; rather would it seem advantageous to keep prominently in view their contrasted, or at any rate complementary characteristics, especially in the matter of the taking up of carbonic acid and the giving up of oxygen on the one hand, and the taking in of oxygen and the giving up of carbonic acid on the other.

But it is obvious that in latitudes where there is comparatively continuous daylight during the periods of vegetation, the two actions—designated respectively assimilation and growth—must go on much more simultaneously than where there is a more marked alternation of daylight and darkness. In parts of Norway and Sweden, for example, where, during the summer, there is almost continuous daylight, crops of barley are grown with only from six to eight weeks intervening from seed time to harvest. And Prof. Schübeler, of Christiania, after making observations on the subject for nearly thirty years, has recently described the characteristics of the vegetation developed under the influence of short summers with almost continuous light. He states that, after acclimatisation, many garden flowers increase in size and depth of colour; that there is a prevailing tinge of red in the plants of the fjelds; that the aroma of fruits is increased, and their colour well developed, but that they are deficient in sweetness; and that the development of essential oils in certain plants is greater than in the same plants grown in other latitudes. Indeed, he considers it to be an established fact that light bears the same relation to aroma as heat does to sweetness.

In connection with this question of the characters of growth under the influence of continuous light, compared with those developed with alternate light and darkness, the recent experiments of Dr. Siemens on the influence of electric light on vegetation are of considerable interest.

In one series of experiments he kept one set of plants entirely in the dark; a second he exposed to electric light only; a third to daylight only; and a fourth to daylight, and afterwards to electric light from 5 to 11 p.m. Those kept in the dark acquired a pale yellow colour, and died; those exposed to electric light only maintained a light green colour, and survived; those exposed to daylight were of a darker green colour, and were more vigorous; and, lastly, those submitted to alternate daylight and electric light, and but a few hours of darkness, showed decidedly greater vigour, and, as he says, the green of the leaf was of a dark rich hue. He concluded that daylight was twice as effective as electric light; but that nevertheless, "electric light was clearly sufficiently powerful to form chlorophyll and its derivatives in the plants."

In a second series of experiments one group of plants was exposed to daylight alone; a second to electric light during eleven hours of the night, and was kept in the dark during the day; and a third to eleven hours a day, and eleven hours electric light. The plants in daylight showed the usual healthy appearance; those in alternate electric light and darkness were for the most part of a lighter colour; and those in alternate daylight and electric light far surpassed the others in darkness of green and vigorous appearance generally.

I have carefully considered these general descriptions with a view to their bearing on the question whether the characters developed under the influence of electric light, and especially those under the influence of almost continuous light, are more prominently those of assimilation or of growth; but I have not been able to come to a decisive opinion on the point. From some conversation I had with Dr. Siemens on the subject, I gather that the characteristics were more those of dark colour and vigour than of tendency to great extension in size. The dark green colour we may suppose to indicate a liberal production of chlorophyll; but if the depth of colour was more than normal, it might be concluded that the chlorophyll had not performed its due amount of assimilation work. In regard to this point, attention may be called to the fact that Dr. Siemens refers to the abundance of the blue or actinic rays in the electric arc, conditions which would not be supposed specially to favour assimilation. On the other hand, the vigour, rather than characteristic extension in size, would seem to indicate a limitation of what is technically called growth, under the influence of the almost continuous light.

Among the numerous field experiments made at Rothamsted, we have many examples of great variation in depth of green colour of the vegetation growing on plots side by side under known differences as to manuring; and we have abundant evidence of difference of composition, and of rate of carbon-assimilation, coincidently with those different shades of colour. One or two instances will strikingly illustrate the point under consideration.

There are two plots side by side in the series of experiments on permanent grass land, each of which received during six consecutive years precisely the same amount of a mixed mineral manure, including potass, and the same amount of nitrogen in the form of ammonia salts. After those six years, one of the two plots was still manured in exactly the same way each year; whilst the other was so, with one exception—namely, the potass was now excluded from the manure. Calculation shows that there was a great excess of potass applied during the first six years; and there was no marked diminution of produce during the five or six years succeeding the cessation of the application. But each year subsequently, up to the present time, now a period of fourteen years, or of nineteen since the exclusion of the potass, the falling off in produce has been very great.

The point of special interest is, however, that all but identically the same amount of nitrogen has been taken up by the herbage growing with the deficiency of potass as by that with the continued supply of it. The colour of the vegetation with the deficiency of potass has been very much darker green than that with the full supply of it. Nevertheless, taking the average of the eight years succeeding the first six of the exclusion of the potass, there has been nearly 400 lbs. less carbon assimilated per acre per annum; and in some of the still later years the deficiency has been very much greater than this.

We have here, then, the significant fact that an equal amount of nitrogen was taken up in both cases, that chlorophyll was abundantly produced, but that the full amount of carbon was not assimilated. In other words, the nitrogen was there, the chlorophyll was there, there was the same sunlight for both plots; but the assimilation-work was not done where there was not a due supply of potass.

Again, in the field in which barley has now been grown for twenty-nine years in succession, there are two plots which have annually received the same amount of nitrogen—the one in conjunction with salts of potass, soda, and magnesia; and the other with the same, and superphosphate of lime in addition. The plot without the superphosphate of lime always maintains a darker green colour. At any given period of growth the dry substance of the produce would undoubtedly contain a higher percentage of nitrogen; but there has been a deficient assimilation of carbon, amounting to more than 500 lbs. per acre per annum, over a period of twenty-eight years. Here again, then, the nitrogen was there, the chlorophyll was there, the sunlight was there; but the work was not done.

It may be stated generally that, in comparable cases, depth of green colour, if not beyond a certain limit, may be taken to indicate corresponding activity of carbon assimilation; but the two instances cited are sufficient to show that we may, so far as the nitrogen, the chlorophyll, and the light are concerned, have the necessary conditions for full assimilation, but not corresponding actual assimilation.

It cannot, I think, fail to be recognised that in these considerations we have opened up to view a very wide field of research, and some of the points involved we may hope will receive elucidation from the further prosecution of Dr. Siemens's experiments. He will himself, I am sure, be the first to admit that what he has already accomplished has done more in raising than in settling important questions. I understand that he proposes to submit plants to the action of the separated rays of his artificial light, and the results obtained cannot fail to be of much interest. But it is obvious that the investigation should now pass from its present initiative character to that of a strictly quantitative enquiry. We ought to know not only that, under given conditions as to light, plants acquire a deeper green colour, and attain maturity much earlier than under others, but how much matter is assimilated in each case, and something also of the comparative chemical characters of the products. As between the action of one description of light and another, and as between the greater or less continuity of exposure, we ought to be able to form a judgment whether the proper balance between assimilation on the one hand, and growth and proper maturation on the other, has been attained; whether the plants have taken up nitrogen and mineral matter, and produced chlorophyll, in a greater degree than the quantity and the quality of the light have been able to turn to account; or whether the conditions as to light have been such that the processes of transformation and growth from the reserve material provided by assimilation have not been normal, or have not kept pace with the production of that material.

(To be continued.)

A LECTURE EXPERIMENT.

By HENRY LEFFMANN, M.D.

The decomposition of steam by red-hot iron is sometimes shown as a lecture experiment, but it is quite a troublesome one. By substituting magnesium for iron, the operation can be conducted in a glass tube, and will be much more satisfactory.

A yard or so of common magnesium ribbon is placed, in folds, in a hard-glass tube, in such a way that the metal touches the glass in a number of points. One end of the tube is drawn into a pretty wide jet, the other is attached to a flask of water. Steam is produced, and allowed to flow until air is expelled, and the apparatus heated sufficiently to prevent condensation at the mouth of the jet. The metal is then strongly heated at the extreme end. After a few moments, it takes fire, burning brilliantly, and the escaping hydrogen may be lighted at the jet. It is best to keep the metal quite hot throughout. The magnesia produced is dense and crisp. It may possibly be partly hydrated, but this has not been determined. The experiment, besides being a striking one, is interesting as showing a body acting as a supporter of combustion, and becoming itself converted into a combustible.

920, Walnut Street, Philadelphia,
August 14, 1880.

ON REVERTED PHOSPHORIC ACID.

By Dr. C. F. MEYER.

The author examines Wagner's view that the reversion of phosphoric acid in ferruginous and aluminous superphosphates is occasioned by the transmutation of ferric sulphate and aluminium sulphate with monobasic calcium phosphate into calcium sulphate and acid iron and aluminium phosphates. These two compounds form in the superphosphate an insoluble precipitate consisting of ferric oxide, alumina, lime, and phosphoric acid. Dr. Meyer observed that a solution made from 10 grms. Mejillones-Guano superphosphate at 20 per cent and 1 litre water gave at once an abundant precipitate with neutral ferric sulphate. A stronger solution made from 40 grms. of the same superphosphate and 1 litre water was precipitated much more slowly and to a less extent. On the addition of water a copious precipitation of ferric phosphate soon followed.

A third experiment with a solution of 40 grms. superphosphate in 200 c.c. water was not precipitated by ferric sulphate unless diluted or heated. The action of solutions of aluminium sulphate with solutions of superphosphate was precisely similar. The author concludes that reversion begins during the manufacture of superphosphate in consequence of the reaction of ferric sulphate and aluminium sulphate with monobasic lime phosphate, aided by the heat which is evolved. Hence the manufacturer on the first examination of the product does not find the proportion of soluble phosphoric acid which he expects according to the quantity of sulphuric acid used. The subsequent reversion after the completion of the product Dr. Meyer ascribes to the simultaneous action of tribasic and monobasic calcium phosphates and ferric sulphate.

The author also examines the separation of ortho- and pyro-phosphoric acid, He converted bibasic calcium phosphate into pyro-phosphate by ignition with the aid of a blast till the weight became constant. The salt thus obtained was decomposed with sulphuric acid, the lime removed by means of alcohol, and the solution made up to 200 c.c. Of this solution 10 c.c., on neutralisation with ammonia, yielded a perfectly clear solution on heating with ammonium acetate and uranium nitrate. On cooling, the salt which had been formed partly separated out. On heating with sodium acetate and uranium nitrate the precipitate first formed was re-dissolved. If 10 c.c. of this solution were diluted with 120 c.c. water, no precipitate was obtained with ammonium chloride, magnesium chloride, and ammonia.

One grm. of the above-mentioned biphosphate was ignited, dissolved in hydrochloric acid, the excess of acid neutralised with ammonia, and the solution made up to 200 c.c. On heating 50 c.c. of the liquid with ammonium acetate, the calcium pyrophosphate was almost entirely separated out, so that ammonia scarcely occasioned a turbidity in the filtrate. Another 50 c.c. mixed with ammonium citrate and chloride yielded no precipitation with magnesium chloride and ammonia. On heating with ammonium acetate and uranium nitrate the precipitate formed was re-dissolved with difficulty. The author will give his opinion, on a future occasion, if a quantitative separation of ortho- and pyro-phosphoric acids can be based upon these methods.—*Zeitschrift für Analytische Chemie.*

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Note.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 4, July 26, 1880.

Apparatus for Measuring the Combustion-heat of Gases by Detonation.—M. Berthelot.—The apparatus cannot be intelligibly described without the eight accompanying illustrations.

Solution of Chlorine in Water.—M. Berthelot.—At one and the same temperature water dissolves proportions of chlorine, which vary according to the duration of contact and the intensity of the light. The variation in the heat evolved during the solution of one and the same weight of chlorine in water are considerable, as it may range from +1·50 to 3·77 cals., the first figure answering to a simple solution, and a second to a decomposition of the water by the chlorine. The displacement of oxygen by chlorine is effected much more distinctly with hydrogen peroxide, a reaction which is explained very simply on thermo-chemical principles. The solubility of chlorine in concentrated solutions of earthy chlorides is less than in water, but increases with dilution. In concentrated hydrochloric acid chlorine dissolves much more plentifully than in water, and with a greater liberation of heat, facts which render probable the formation of a peculiar combination between chlorine and hydrochloric acid.

Successive Transformations of the Photographic Image by the Prolongation of the Luminous Action.—J. Janssen.—The author has obtained the following successive transformations:—(1) The ordinary negative image; (2) a first neutral state in which the plate becomes uniformly obscure under the action of the developing agent; (3) a positive image; (4) a second neutral state opposed to the first, in which the plate becomes uniformly light; (5) a second negative image, similar to the ordinary one, but differing from it by the intervening states through which it has passed, and by the enormous difference of luminous intensity necessary for its production; (6) a third neutral state in which the negative image of the second order has disappeared, and is succeeded by a uniform sombre tint.

Causes of Terrestrial Magnetism.—Selim Lemström.—A mathematical paper, not susceptible of useful abstraction.

An Electro-dynamic Paradox.—M. Gérard-Lescuyer.—Dynamo-electric machines, of which the Gramme machine is the most common type, are reversible, that is to say, if they are traversed by a current they produce motion, and may serve for the transmission of force. Under similar conditions magneto-electric machines with constant currents possess the same properties. But if the current produced by a dynamo-electric machine is passed into a magneto-electric machine, a novel phenomenon is produced. As soon as the circuit is closed the electro-magnetic machine sets itself in motion, and takes a regular speed in proportion to the intensity of the current which excites it; suddenly it slackens, stops, and begins to move in the opposite direction, then stops anew, and turns in the same direction as at first. It is animated with a regular alternative movement, which continues as long as does the exciting current. The author explains these phenomena by supposing that the electro-magnetic machine gives rise to a special current, which in turn traverses the dynamo-electric machine in the inverse direction, reversing the polarities of the inductors and generating a new current.

Researches on Ozone.—P. Hautefeuille and J. Chappuis.—At very elevated temperatures the transformation of oxygen into ozone and of ozone into oxygen probably obey the laws of the decomposition of homogeneous systems. At mean temperatures the decomposition of ozone is considered always complete, slowly at common temperatures, but quickly at 250°. The instability of this body is therefore comparable to that of hypochlorous acid or nitrogen chloride. But whilst the heat necessary to constitute these explosive bodies can only be acquired by a secondary simultaneous reaction, the allotropic transformation of oxygen may be determined by the electric effluve alone. The act of electrisation places momentarily oxygen in conditions analogous to those of bodies which possess the property of combining directly, or of becoming polymerised under the action of heat.

A New Isomeric Modification of Aluminium Hydroxide.—Dr. D. Tommasi.—Ordinary aluminium hydroxide, as obtained by precipitating a solution of alum with ammonia, if left to itself in presence of water for about three months, undergoes a molecular change, becoming as little soluble in acids and alkalies as ignited alumina, though still retaining its 3 mols. of water. The author designates this hydrate by the letter δ, to distinguish it from the normal hydrate, α, from gibbsite, β, and from the colloidal alumina of Graham, γ. Concentrated hydrochloric and nitric acids in excess require sixty hours to dissolve the hydrate, δ. Caustic potash, highly concentrated, requires about the same time; glacial acetic acid has scarcely any appreciable action; concentrated sulphuric acid dissolves the hydrate δ at once, but if diluted to 4 per cent it requires six days to take up even a small quantity. Normal alumina forms with its chloride an oxychloride, whilst alumina δ does not.

Observations on M. E. Bourgoin's Note on the Ultimate Action of Bromine upon Malonic Acid.—B. Petrieff.—The author points out that he obtained five years ago the results which M. Bourgoin describes on p. 121 of the present volume (see *Berichte der Deutsch. Chem. Gesells.*, viii., 730).

Molecular Heats and Volumes of the Rare Earths and of their Sulphates.—L. F. Nilson and O. Petterson.—The determinations given in this paper have been made under like conditions with chemically pure compounds, the molecular weight of which is determined in each case, and they are consequently mutually comparable. The specific heat is determined from 0° to 100° with Bunsen's calorimeter; as the aqueous sulphates cannot bear a temperature of 100° without decomposition their specific heat has been determined at 46° to 47° in the vapour of carbon disulphide. The specific gravities have been determined by a method specially adapted to avoid the errors springing from the adhesion of air to pulverulent bodies (see Otto Petterssen "Molekular volumina," *Nov. Act. R. Soc. Scien. Upsal.*, iii., 1878). The figures given are the mean of two closely concordant determinations. M. Lecoq de Boisbaudran placed at the disposal of the authors 0·138 grm. of gallium, and M. Cleve supplied them with the purest erbia which he has hitherto obtained. The authors then give tables showing the molecular weights, specific gravities, specific heats, molecular heats, and volumes of the following oxides, glucina, alumina sapphire, chrysoberyl, scandia, gallium oxide, yttria, indium oxide, erbia, ytterbia, lanthanum oxide, didymium oxide, zirconia, cerium oxide, and thoria; the anhydrous sulphates of glucinum, aluminium, scandium, chromium, iron (ferric), gallium, yttrium, indium, lanthanum, cerium, didymium, erbium, ytterbium, and thorium, and the aqueous sulphates of glucinum, yttrium, lanthanum, cerium, didymium, erbium, and ytterbium. On subtracting from the molecular heats and volumes found for the aqueous sulphates the same values for the anhydrous sulphates, there is found a remainder expressing the molecular heat and volume of the water united to the sulphates. For 1 mol. of free water these are equal to 18, whilst in the highest case determined the molecular heat = 13·02, and the molecular volume = 12·04. The molecular heat and volume of the water combined with these sulphates are therefore diminished considerably, showing a minimum value hitherto unknown. If the isomorphous compounds, *i.e.*, those of yttrium, erbium, and ytterbium, or of lanthanum and didymium, are placed in juxtaposition we see readily that the molecular heat of compounds intimately connected by isomorphism increases simultaneously with the atomic weight of the metal, whilst the molecular volume decreases. As regards the atomicity of glucinum the numbers found are particularly important. The atomic heat of the oxygen is perfectly normal in G_2O_3. The molecular heat and volume of glucina and alumina are almost identical, so that alumina examined either as crystalline sapphire, or as an amorphous powder, and chrysoberyl, considered not as an aluminate, but as—

$$\left.\begin{matrix}Al_{\frac{3}{2}}\\G_{\frac{1}{2}}\end{matrix}\right\}O_3$$

give values completely identical. The molecular heat and volume of glucinum sulphate, compared with the same numbers for the sulphates of aluminium, scandium, gallium, yttrium, &c., argue also in favour of the formula G_2O_3. All the physical and chemical properties of the metal, the oxide and the sulphate, confirm this opinion. Glucinum by its double sulphate, $3K_2O_2SO_2+G_2O_6{,}3SO_2$, typical for all the metals of gadolinite and cerite appears to belong to the series of these elements, collateral with but distinct from another series of elements which form alums.

A New Fermentation of Glucose.—L. Boutroux.—The author points out that the acid described in his memoir (*Comptes Rendus*, lxxxvi., p. 605) is not the lactic, but the gluconic.

Journal für Praktische Chemie.
No. 9, 1880.

Reply to H. W. Hempel.—By H. Kolbe and E. von Meyer.*—A controversial paper relating to the comparative value of salicylic, carbolic, and benzoic acids as antizymotics, the authors giving the preference to the first-mentioned of these bodies. It is somewhat singular that the memoir of Herrn Hempel, here criticised, appears as far back as 1875 (*Berichte der Chem. Gesell.*, vol. viii., p. 1657).

On the Oxidation of Haloid Salts.—Dr. Hans Schulze.—In this paper the author successively examines the behaviour of oxygen with the chlorides, bromides, iodides, and fluorides; the action of nascent oxygen upon the same bodies; the behaviour of oxygen with haloid salts in presence of acid anhydrides, and the reaction of such anhydrides upon haloid salts in the absence of oxygen.

Destructive Action of Woody Fibre upon Salicylic Acid.—H. Kolbe.—The attempt has been made to preserve the water-supply on board ship, by a small addition of salicylic acid. It appeared that the acid was entirely destroyed and that the water then became offensive. On further examination it was found that the salicylic acid is not absorbed by the wood, but destroyed. The result is similar if salicylic acid is added to wine in the wood.

No. 10, 1880.

Thermo-chemical Researches (Part 32).—Julius Thomsen.—In this memoir, which does not admit of useful abstraction, and which contains several illustrations of apparatus, the author investigates the formation-heat of nitrous and nitric acids, nitrogen dioxide, nitric and nitrous oxides, ammonia, the ammoniacal salts and the nitrates.

Berichte der Deutschen Chemischen Gesellschaft zu Berlin.
No. 3, 1880.

Behaviour of Iodine at High Temperatures.—V. Meyer.—Already noticed.

Chlorine.—V. Meyer and H. Züblin.—Already noticed.

Observations on Vapour-densities.—V. Meyer.—Already noticed.

Platinum Bromide.—V. Meyer and H. Züblin.—In preparing Pt_2Br_4 the authors succeeded in obtaining $PtBr_4$, which had not hitherto been produced. It is a black powder, not in the slightest deliquescent, moderately soluble in water, but much more freely in alcohol and ether.

Density of Bromine Vapour at a Yellow Heat.—V. Meyer and H. Züblin.—Already inserted.

Calorimetric Determination of Temperature.—V. Meyer.—The author concludes from his own experiments and those of Roscoe that his method for the determination of high temperatures is approximately accurate.

Potassium Leuco-sulphin-digotate and Potassium Indoxyl-sulphate.—E. Baumann and F. Tiemann.—Not suitable for abstraction.

Fittica's Fourth Nitro-phenol.—S. Natanson.—The author finds that the product obtained by Fittica's process is chiefly pure ortho-nitro-phenol.

Constitution of Phthalyl Chloride.—E. von Gerichten. Not capable of useful abstraction.

MISCELLANEOUS.

British Association for the Advancement of Science.—The following are the names of the Officers and Committee of Section B (Chemical Science Section) of the Swansea Meeting of the British Association:—

President.—Joseph Henry Gilbert, Ph.D., F.R.S.

Vice-Presidents.—I. Lowthian Bell, F.R.S.; William Crookes, F.R.S.; W. Chandler Roberts, F.R.S.; Prof. Abel, F.R.S.; Dr. J. H. Gladstone, F.R.S; A. G. Vernon Harcourt, F.R.S.; Prof. A. W. Williamson, F.R.S.

Secretaries.—Harold B. Dixon, M.A.; Dr. W. R. Eaton Hodgkinson; P. Phillips-Bedson, D.Sc.; J. M. Thomson, F.R.S E. (Recorder).

Committee.—Prof. Attfield, F.R.S.; A. H. Allen; P. Braham; R. C. Clapham; J. M. Cameron; W. Fairley F.R.S.E.; C. Le Neve Foster, D.Sc.; Prof. Carey Foster,

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* The word which we render "reply," is equivalent to "dressing down," "polishing off," &c., and other strong colloquialisms.

F.R.S.; A. E. Fletcher; G. Gladstone; Prof. A. K. Huntington; Dr. Morgan; Dr. Macadam, F.R.S.E.; Dr. Morrison; J. A. Phillips; Dr. Ronalds; Dr. W. Ramsay· M. R. D. Silva; John Spiller; Peter Spence; A. Schuster, F.R.S.; Prof. Silvanus P. Thompson; W. Thomson, F.R.S.E.; W. Weldon, F.R.S.E.

The papers brought before the Section were as follows:—

Dr. J. Pattinson.—Report of Committee on the best means for the Development of Light from Coal-gas of Different Qualities. Part II.

Walter Weldon, F.R.S.E.—On some Relations between the Atomic Volumes of Certain Elements, and the Heats of Formation of some of their Compounds.

Harold B. Dixon, M.A.—On the Influence of Water on the Union of Carbonic Oxide with Oxygen at a High Temperature.

J. Sakurai.—On Metallic Compounds containing Divalent Organic Radicals. Part I.

Prof. C. Bolton.—On the Action of Organic Acids on Minerals.

Dr. A. Schuster, F.R.S.—Report on Spectrum Analysis (Spectra of Metalloids).

Prof. A. K. Huntington.—Report on Spectrum Analysis (Ultra-violet Spectra).

J. W. Starling.—An Improved Volumetric Apparatus.

J. W. Thomas.—On the Coal Seams of the Eastern portion of the South Wales Basin and their Chemical Composition.

P. Spence.—On a New Mode for Purification of Sewage.

J. M. Cameron.—Comparison of the State of Agricultural Chemistry in England and on the Continent.

Dr. J H. Gladstone, F.R.S.—On the Refraction Equivalents of the Diamond and various Carbon Compounds.

A. H. Allen.—On the Specific Rotatory Power of Cane and Grape Sugar.

John Spiller.—On the Identification of the Coal Tar Colours.

W. Chandler Roberts, F.R.S., and T. Wrightson.—On the Fluid Density of Bismuth.

P. Braham.—On some Crystals of Mercury Sulphate.

E. A. Parnell.—On a New Process for the Metallurgic Treatment of Complex Ores containing Zinc.

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W. Henderson.—A New Process for Separating Silver from Copper, in Copper Ore and Regulus.

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William Thomson, F.R.S.E.—On the Identification of the Ink used in Writing Letters and Documents, as Evidence in Cases of Libel, Forgery, &c.

P. Braham.—Note on Silver Sulphate.

Major-General Scott, C.B., F.R.S.—On the Influence of Magnesia on Vegetation.

W. H. Watson.—On the Action of Oils upon Copper and Iron. Part II.

F. W. Hodges.—On Bleaching-powder Residues.

Mineral Leather.—The Eglinton Chemical Company, of 29, St. Vincent Place, Glasgow, have erected an Exhibition Tannery, in order to bring under the notice of those concerned a new method of tanning, effected not by means of tannin in any form, but of chrome. The process in question, invented and patented by Dr. Heinzerling, is said to be cheaper and more expeditious than the traditional methods of tanning, and to produce leather stronger, more durable, and pliant, and especially less pervious to moisture. About 300 chrome-tanned hides are being exhibited, some of them made into belting, harness, boots, &c.

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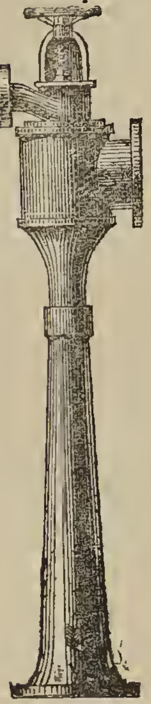

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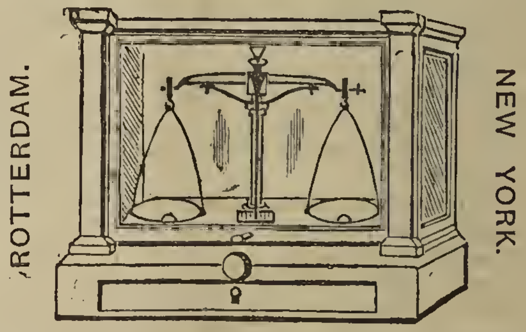

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THE CHEMICAL NEWS.

Vol. XLII. No. 1085.

ADDRESS TO THE CHEMICAL SECTION OF THE BRITISH ASSOCIATION.

By J. H. GILBERT, Ph.D., F.R.S., V.P.C.S., F.L.S., President of the Section.

(Concluded from page 118.)

But one word more in reference to Dr. Siemens's results and proposed extension of his enquiries. Even supposing that by submitting growing crops to continuous light by the aid of the electric light during the night they could be brought to maturity within a period shorter than at present approximately in proportion to the increased number of hours of exposure, the estimates of the cost of illuminating the vegetation of an acre of land certainly do not seem to hold out any hope that agriculture is likely to derive benefit from such an application of science to its needs. If, however, the characters of growth and of maturation should prove to be suitable for the requirements of horticultural products of luxury and high value, it may possibly be otherwise with such productions.

The above considerations obviously suggest the question —What is the office of chlorophyll in the processes of vegetation? Is it, as has generally been assumed, confined to effecting, in some way not yet clearly understood, carbon assimilation, and, this done, its function ended? Or is it, as Pringsheim has recently suggested, chiefly of avail in protecting the subjacent cells and their contents from those rays of light which would be adverse to the secondary processes which have been distinguised as growth?

Appropriate as it would seem that I should attempt to lay before you a *résumé* of results bearing upon the points herein involved, so numerous and so varied have been the investigations which have been undertaken on the several branches of the question in recent years, that adequately to discuss them would occupy the whole time and space at my disposal. I must therefore be content thus to direct attention to the subject and pass on to other points.

It has been shown that the plant may receive abundance of nitrogen, may produce abundance of chlorophyll, and may be subject to the influence of sufficient light, and yet not assimilate a due amount of carbon. On the other hand, it has been seen that the mineral constituents may be liberally provided, and yet, in the absence of a sufficient supply of nitrogen in an available condition, the deficiency in the assimilation of carbon will be still greater. In fact, assuming all the other necessary conditions to be provided, it was seen that the amount of carbon assimilated depended on the available supply of nitrogen.

In a certain general sense it may be said that the success of the cultivator may be measured by the amount of carbon he succeeds in accumulating in his crops. And as, other conditions being provided, the amount of carbon assimilated depends on the supply of nitrogen in an available form within the reach of the plants, it is obvious that the question of the sources of the nitrogen of vegetation is one of first importance. Are they the same for all descriptions of plants? Are they to be sought entirely in the soil, or entirely in the atmosphere, or partly in the one and partly in the other?

These are the questions which Mr. Lawes and myself have discussed so frequently that it might seem some apology was due for recurring to the subject here, especially as I considered it in some of its aspects before this Section at the Sheffield Meeting last year. But the subject still remains one of first importance to agriculture, and it could not be omitted from consideration in such a review as I have undertaken to give. Moreover, there are some points connected with it still unsettled, and some still disputed.

It will be remembered that De Saussure's conclusion was that plants did not assimilate the free or uncombined nitrogen of the atmosphere, and that they derived their nitrogen from the compounds of it existing in the atmosphere, and especially in the soil. Liebig, too, concluded that plants do not assimilate nitrogen from the store of it existing in the free or uncombined state, but that ammonia was their main source, and he assumed the amount of it annually coming down in rain to be much more than we now know to be the case.

Referring to our previous papers for full details respecting most of the points in question, I will state, as briefly as I can, the main facts known—first in regard to the amount of the measurable, or as yet measured, annual deposition of combined nitrogen from the atmosphere; and, secondly, as to the amount of nitrogen annually assimilated over a given area by different crops—so that some judgment may be formed as to whether the measured atmospheric sources are sufficient for the requirements of agricultural production, or whether, or where, we must look for other supplies?

First, as to the amount of combined nitrogen coming down as ammonia and nitric acid in the measured aqueous deposits from the atmosphere.

Judging from the results of determinations made many years ago, partly by Mr. Way and partly by ourselves, in the rain, &c., collected at Rothamsted; from the results of numerous determinations made much more recently by Professor Frankland in the deposits collected at Rothamsted, and also in rain collected elsewhere; from the results obtained by Boussingault, in Alsace; from those of Marié-Davy, at the Meteorological Observatory at Montsouris, Paris; and from those of many others made in France and Germany—we concluded, some years ago, that the amount of combined nitrogen annually so coming down from the atmosphere would not exceed 8 or 10 lbs. per acre per annum in the open country in Western Europe. Subsequent records would lead to the conclusion that this estimate is more probably too high than too low. And here it may be mentioned, in passing, that numerous determinations of the nitric acid in the drainage water collected from land at Rothamsted, which had been many years unmanured, indicate that there may be a considerable annual loss by the soil in that way; indeed, probably sometimes much more than the amount estimated to be annually available from the measured aqueous deposits from the atmosphere.

It should be observed, however, that the amount of combined nitrogen, especially of ammonia, is very much greater in a given volume of the minor aqueous deposits than it is in rain; and there can be no doubt that there would be more deposited within the pores of a given area of soil than on an equal area of the non-porous even surface of a rain-gauge. How much, however, might thus be available beyond that determined in the collected and measured aqueous deposits the existing evidence does not afford the means of estimating with any certainty.

The next point to consider is—What is the amount of nitrogen annually obtained over a given area, in different crops, when they are grown without any supply of it in manure? The field experiments at Rothamsted supply important data relating to this subject.

Thus, over a period of 32 years (up to 1875 inclusive), wheat yielded an average of 20·7 lbs. of nitrogen per acre per annum, without any manure; but the annual yield has declined from an average of more than 25 lbs. over the first 8, to less than 16 lbs. over the last 12, of those 32 years; and the yield (it is true with several bad seasons) has been still less since.

Over a period of 24 years, barley yielded 18·3 lbs. of nitrogen per acre per annum, without any manure; with a decline from 22 lbs. over the first 12 to only 14·6 lbs. over the next 12 years.

With neither wheat nor barley did a complex mineral manure at all materially increase the yield of nitrogen in the crops.

A succession of so-called "root crops"—common turnips, Swedish turnips, and sugar beet (with 3 years of barley intervening after the first eight years)—yielded, with a complex mineral manure, an average of 26·8 lbs. of nitrogen per acre per annum over a period of 31 years. The yield declined from an average of 42 lbs. over the first 8 years to only 13·1 lbs. (in sugar beet) over the last five of the 31 years; but it has risen somewhat during the subsequent 4 years, with a change of crops to mangolds.

With the leguminous crops, beans, there was obtained, over a period of 24 years, 31·3 lbs. of nitrogen per acre per annum without any manure, and 45·5 lbs. with a complex mineral manure, including potass (but without nitrogen). Without manure the yield declined from 48·1 lbs. over the first 12 years to only 14·6 lbs. over the last 12; and with the complex mineral manure it declined from 61·5 lbs. over the first 12, to 29·5 lbs. over the last 12, years of the 24.

Again, an ordinary rotation of crops of turnips, barley, clover or beans, and wheat, gave over a period of 28 years an average of 36·8 lbs. of nitrogen per acre per annum without any manure, and of 45·2 lbs. with superphosphate of lime alone, applied once every four years, that is for the root crop. Both without manure, and with superphosphate of lime alone, there was a considerable decline in the later courses.

A very remarkable instance of nitrogen yield is the following—in which the results obtained when barley succeeds barley, that is when one gramineous crop succeeds another, are contrasted with those when a leguminous crop, clover, intervenes between the two cereal crops. Thus, after the growth of six grain crops in succession by artificial manures alone, the field so treated was divided, and, in 1873, on one half barley and on the other half clover was grown. The barley yielded 37·3 lbs. of nitrogen per acre, but the three cuttings of clover yielded 151·3 lbs. In the next year, 1874, barley succeeded on both the barley and the clover portions of the field. Where barley had previously been grown, and had yielded 37·3 lbs. of nitrogen per acre, it now yielded 39·1 lbs.; but where the clover had previously been grown, and had yielded 151·3 lbs. of nitrogen, the barley succeeding it gave 69·4 lbs., or 30·3 lbs. more after the removal of 151·3 lbs. in clover, than after the removal of only 37·3 in barley.

Nor was this curious result in any way accidental. It is quite consistent with agricultural experience that the growth and removal of a highly nitrogenous leguminous crop should leave the land in high condition for the growth of a gramineous corn crop, which characteristically requires nitrogenous manuring; and the determinations of nitrogen in numerous samples of the soil taken from the two separate portions of the field, after the removal of the barley and the clover respectively, concurred in showing considerably more nitrogen, especially in the first 9 inches of depth, in the samples from the portion where the clover had been grown than in those from the portion whence the barley had been taken. Here, then, the surface soil at any rate had been considerably enriched in nitrogen by the growth and removal of a very highly nitrogenous crop.

Lastly, clover has now been grown for twenty-seven years in succession on a small plot of garden ground which had been under ordinary garden cultivation for probably two or three centuries. In the fourth year after the commencement of the experiment the soil was found to contain, in its upper layers, about four times as much nitrogen as the farm-arable land surrounding it; and it would doubtless be correspondingly rich in other constituents. It is estimated that an amount of nitrogen has been removed in the clover crops grown corresponding to an average of not far short of two hundred pounds per acre per annum, or about ten times as much as in the cereal crops, and several times as much as in any of the other crops, growing on ordinary arable land; and, although the yield continues to be very large, there has been a marked decline over the second half of the period compared with the first. Of course calculations of the produce of a few square yards into quantities per acre can only be approximately correct. But there can at any rate be no doubt whatever that the amount of nitrogen annually removed has been very great, and very far beyond what it would be possible to attain on ordinary arable land; where, indeed, we have not succeeded in getting even a moderate growth of clover for more than a very few years in succession.

One other illustration should be given of the amounts of nitrogen removed from a given area of land by different descriptions of crop, namely, of the results obtained when plants of the gramineous, the leguminous, and other families, are growing together, as in the mixed herbage of grass-land.

It is necessary here to remind you that gramineous crops grown separately on arable land—such as wheat, barley, or oats—contain a comparatively small percentage of nitrogen, and assimilate a comparatively small amount of it over a given area. Yet nitrogenous manures have generally a very striking effect in increasing the growth of such crops. The highly nitrogenous leguminous crops (such as beans and clover), on the other hand, yield, as has been seen, very much more nitrogen over a given area, and yet they are by no means characteristically benefited by direct nitrogenous manuring; whilst, as has been shown, their growth is considerably increased, and they yield considerably more nitrogen over a given area, under the influence of purely chemical manures, and especially of potass manures. Bearing these facts in mind, the following results, obtained on the mixed herbage of grass-land, will be seen to be quite consistent.

A plot of such mixed herbage, left entirely unmanured, gave over twenty years an average of 33 pounds of nitrogen per acre per annum. Over the same period, another plot, which received annually a complex mineral manure, including potass, during the first six years, but excluding it during the last fourteen years, yielded 46·3 lbs. of nitrogen; whilst another, which received the mixed mineral manure, including potass, every year of the twenty, yielded 55·6 lbs. of nitrogen per acre per annum. Without manure there was some decline of yield in the later years; with the partial mineral manuring there was a greater decline; but with the complete mineral manuring throughout the whole period, there was even some increase in the yield of nitrogen in the later years.

Now, the herbage growing without manure comprised about fifty species, representing about twenty natural families; that growing with the limited supply of potass comprised fewer species, but a larger amount of the produce, especially in the earlier years, consisted of leguminous species, and the yield of nitrogen was greater. Lastly, the plot receiving potass every year yielded still more leguminous herbage, and, accordingly, still more nitrogen.

The most striking points brought out by the foregoing illustrations are the following:—

First. Without nitrogenous manure, the gramineous crops annually yielded, for many years in succession, much more nitrogen over a given area than is accounted for by the amount of combined nitrogen annually coming down in the measured aqueous deposits from the atmosphere.

Second. The root crops yielded more nitrogen than the cereal crops, and the leguminous crops very much more still.

Third. In all cases—whether of cereal crops, root crops, leguminous crops, or a rotation of crops—*the decline in the annual yield of nitrogen, when none was supplied, was very great.*

How are these results to be explained? Whence comes the nitrogen? and especially whence comes the much larger amount taken up by plants of the leguminous and some other families than by Gramineæ? And, lastly, what is the significance of the great decline in the yield of nitrogen in all the crops when none is supplied in the manure?

Many explanations have been offered. It has been assumed that the combined nitrogen annually coming down from the atmosphere is very much larger than we have estimated it, and that it is sufficient for all the requirements of annual growth. It has been supposed that "broad-leaved plants" have the power of taking up nitrogen in some form from the atmosphere, in a degree, or in a manner, not possessed by the narrow-leaved Gramineæ. It has been argued that, in the last stages of the decomposition of organic matter in the soil, hydrogen is evolved, and that this nascent hydrogen combines with the free nitrogen of the atmosphere, and so forms ammonia. It has been suggested that ozone may be evolved in the oxidation of organic matter in the soil, and that, uniting with free hydrogen, nitric acid would be produced. Lastly, it has been by some concluded that plants assimilate the free nitrogen of the atmosphere, and that some descriptions are able to do this in a greater degree than others.

We have discussed these various points on more than one occasion; and we have given our reasons for concluding that none of the explanations enumerated can be taken as accounting for the facts of growth.

Confining attention here to the question of the assimilation of free nitrogen by plants, it is obvious that, if this were established, most of our difficulties would vanish. This question has been the subject of a great deal of experimental enquiry, from the time that Boussingault entered upon it, about the year 1837, nearly up to the present time. About twenty years ago it was elaborately investigated at Rothamsted. In publishing the results of that enquiry those of others relating to it were fully discussed; and although the recorded evidence is admittedly very conflicting, we then came to the conclusion, and still adhere to it, that the balance of the direct evidence on the point is decidedly against the supposition of the assimilation of free nitrogen by plants. Indeed, the strongest argument we know of in its favour is that some such explanation is wanted.

Not only is the balance of direct experimental evidence against the assumption that plants assimilate free or uncombined nitrogen, but it seems to us that the balance of existing indirect evidence is also in favour of another explanation of our difficulties.

I have asked what is the significance of the gradual decline of produce of all the different crops when continuously grown without nitrogenous manure? It cannot be that, in growing the same crop year after year on the same land, there is any residue left in the soil that is injurious to the subsequent growth of the same description of crop; for (excepting the beans) more of each description of crop has been grown year after year on the same land than the average yield of the country at large under ordinary rotation, and ordinary treatment—provided only that suitable soil conditions were supplied. Nor can the diminishing produce, and the diminishing yield of nitrogen, be accounted for on the supposition that there was a deficient supply of available mineral constituents in the soil. For it has been shown that the cereals yielded little more, and declined nearly as much as without manure, when a complex mineral manure was used, such as was proved to be adequate when available nitrogen was also supplied. So far as the root crops are concerned, the yield of nitrogen, though it declined very much, was greater at first, and on the average, than in the case of the cereals. As to the Leguminosæ, which require so much nitrogen from somewhere, it is to be observed that on ordinary arable land the yield has not been maintained under any conditions of manuring; and the decline was nearly as marked with mineral manures as without any manure. Compared with the growth of the Leguminosæ on arable land, the remarkable result with the garden clover would seem clearly to indicate that the question was one of soil, and not of atmospheric supply. And the fact that all the other crops will yield full agricultural results even on ordinary arable land, when proper manures are applied, is surely very strong evidence that it is with them, too, a question of soil, and not of atmospheric supply.

But we have other evidence leading to the same conclusion. Unfortunately we have not reliable samples of the soil of the different experimental fields taken at the commencement of each series of experiments, and subsequently at stated intervals. We have nevertheless, in some cases, evidence sufficient to show whether or not the nitrogen of the soil has suffered diminution by the continuous growth of the crop without nitrogenous manure.

Thus, we have determined the nitrogen in the soil of the continuously unmanured wheat plot at several successive periods, and the results prove that a gradual reduction in the nitrogen of the soil is going on; and, so far as we are able to form a judgment on the point, the diminution is approximately equal to the nitrogen taken out in crops, and the amount estimated to be received in the annual rainfall is approximately balanced by the amount lost by the land as nitrates in the drainage water.

In the case of the continuous root-crop soil, on which the decline in the yield of nitrogen in the crop was so marked, the percentage of nitrogen, after the experiment had been continued for twenty-seven years, was found to be lower where no nitrogen had been applied than in any other arable land on the farm which has been examined.

In the case of the experiments on the mixed herbage of grass land, the soil of the plot which, under the influence of a mixed mineral manure, including potass, had yielded such a large amount of leguminous herbage and such a large amount of nitrogen, showed, after twenty years, a considerably lower percentage of nitrogen than that of any other plot in the series.

Lastly, determinations of nitrogen in the garden soil which has yielded so much nitrogen in clover, made in samples collected in the fourth and the twenty-sixth years of the twenty-seven of the experiments, show a very large diminution in the percentage of nitrogen. The diminution, to the depth of 9 inches only, represents approximately three-fourths as much as the amount estimated to be taken out in the clover during the intervening period; and the indication is, that there has been a considerable reduction in the lower depths also. It is to be supposed, however, that there would be loss in other ways than by the crop alone.

I would ask, Have we not in these facts—that full amounts of the different crops can be grown, provided proper soil conditions are supplied; that without nitrogenous manure the yield of nitrogen in the crop rapidly declines; and that, coincidently with this, there is a decline in the percentage of nitrogen in the soil—have we not in these facts cumulative evidence pointing to the soil, rather than to the atmosphere, as the source of the nitrogen of our crops?

In reference to this point, I may mention that the ordinary arable soil at Rothamsted may be estimated to contain about 3000 lbs. of nitrogen per acre in the first 9 inches of depth, about 1700 lbs. in the second 9 inches, and about 1500 lbs. in the third 9 inches,—or a total of about 6200 lbs. per acre to the depth of 27 inches.

In this connection it is of interest to state that a sample of Oxford clay, obtained in the Sub-Wealden exploration boring, at a depth of between 500 and 600 feet (and which was kindly given to me by the President of the Association, Professor Ramsay, some years ago), showed, on analysis at Rothamsted, approximately the same percentage of nitrogen as the subsoil at Rothamsted taken to the depth of about 4 feet only.

Lastly, in a letter received from Boussingault some years ago, referring to the sources whence the nitrogen of vegetation is derived, he says:—

"From the atmosphere, because it furnishes ammonia in the form of carbonate, nitrates, or nitrites, and various kinds of dust. Theodore de Saussure was the first to demonstrate the presence of ammonia in the air, and consequently in meteoric waters. Liebig exaggerated the influence of this ammonia on vegetation, since he went so far as to deny the utility of the nitrogen which forms a part of farm-yard manure. This influence is nevertheless real, and comprised within limits which have quite recently been indicated in the remarkable investigations of M. Schlösing.

"From the soil, which, besides furnishing the crops with mineral alkaline substances, provides them with nitrogen, by ammonia, and by nitrates, which are formed in the soil at the expense of the nitrogenous matters contained in diluvium, which is the basis of vegetable earth; compounds in which nitrogen exists in stable combination, only becoming fertilising by the effect of time. If we take into account their immensity, the deposits of the last geological periods must be considered as an inexhaustible reserve of fertilising agents. Forests, prairies, and some vineyards have really no other manures than what are furnished by the atmosphere and by the soil. Since the basis of all cultivated land contains materials capable of giving rise to nitrogenous combinations, and to mineral substances, assimilable by plants, it is not necessary to suppose that in a system of cultivation the excess of nitrogen found in the crops is derived from the free nitrogen of the atmosphere. As for the absorption of the gaseous nitrogen of the air by vegetable earth, I am not acquainted with a single irreproachable observation that establishes it; not only does the earth not absorb gaseous nitrogen, but it gives it off, as you have observed in conjunction with Mr. Lawes, as Reiset has shown in the case of dung, as M. Schlösing and I have proved in our researches on nitrification.

"If there is one fact perfectly demonstrated in physiology, it is this of the non-assimilation of free nitrogen by plants; and I may add by plants of an inferior order, such as mycoderms and mushrooms."—(Translation.)

If, then, our soils are subject to a continual loss of nitrogen by drainage, probably in many cases more than they receive of combined nitrogen from the atmosphere—if the nitrogen of our crops is derived mainly from the soil, and not from the atmosphere—and if, when due return is not made from without, we are drawing upon what may be termed the store of nitrogen of the soil itself—is there not, in the case of many soils at any rate, as much danger of the exhaustion of their available nitrogen as there has been supposed to be of the exhaustion of their available mineral constituents?

I had hoped to say something more about soils, to advance our knowledge respecting which an immense amount of investigation has been devoted of late years, but in regard to which we have yet very much more to learn. I must, however, now turn to other matters.

I have thus far directed attention to some points of importance in connection with the sources of the constituents of our crops, and I must now briefly refer to some in connection with the composition, and to some relating to the uses, of the crops themselves.

As to composition, I must confine myself to indicating something of what is known of the condition of the nitrogen in our various crops; though I had intended to say something respecting the carbo-hydrates, and especially respecting the various members of the cellulose group.

As to the nitrogen—in our first experiments on the feeding of animals, made in 1847, 1848, and 1849, the results of which were published in the last-mentioned year—we found that, in the case of succulent roots used as food, not only were they not of value as food in proportion to their richness in nitrogen, but when the percentage of it was higher than a certain normal amount, indicating relative succulence and immaturity, they were positively injurious to the animals. So marked was the variation of result, according to the condition of maturity or otherwise of the foods employed, that, when reviewing the results of the experiments which had up to that time been conducted, in a paper read before this Section of the British Association at the Belfast Meeting in 1852 (and which was published in full in the annual volume*), we stated that the mode of estimating the amount of proteine compounds by multiplying the percentage of nitrogen by 6·3 was far from accurate, especially when applied to succulent vegetable foods, and that the individual compounds ought to be determined. The Rothamsted Laboratory staff was, however, much smaller then than it is now, and, with the pressure of many other subjects upon us, it was at that time quite impossible to follow up the enquiry in that direction.

It is, indeed, only within the last ten years or so that the question has been taken up at all systematically; but we are already indebted to E. Schulze, A. Urick, Church, Sachsse, Maercker, Kellner, Vines, Emmerling, and others, for important results relating to it.

Our knowledge in regard to the subject is, however, still very imperfect. But it is in progress of investigation from two distinctly different points of view—from that of the vegetable physiologist, and that of the agricultural chemist. The vegetable physiologist seeks to trace the changes that occur in the germination of the seed, and during the subsequent life-history of the plant, to the production of seed again. The agricultural chemist takes the various vegetable products in the condition in which they are used on the farm, or sold from it. And as a very large proportion of what is grown—such as grass, hay, roots, tubers, and various green crops—are not matured productions, it comes to be a matter of great importance to consider whether or not any large proportion of the nitrogenous contents of such products is in such condition as not to be of avail to the animals which consume them in their food?

We cannot say that the whole of the nitrogen in the seeds with which we have to deal exists as albuminoids. But we may safely assume that the nearer they approach to perfect ripeness, the less of non-albuminoid nitrogenous matters will they contain; and, in the case of the cereal grains at any rate, it is probable that if really perfectly ripe they will contain very nearly the whole of their nitrogen as albuminoids. With regard to some leguminous and other seeds, which contain peculiar nitrogenous bodies, the range may, however, be wider.

But whatever the condition of the nitrogenous bodies in the seeds we grow or sow, with germination begins a material change. Albuminoids are transformed into peptones, or peptone-like bodies, or degraded into various amido- or other compounds. Such change into more soluble and more diffusible bodies is, it is to be supposed, essential to their free migration and to their subserviency to the purposes of growth. In the case of the germination, especially of some leguminous seeds, asparagine has been found to be a very prominent product of such degradation of the albuminoids; but it would seem that this disappears as the green parts are developed. But now the plant begins to receive supplies of nitrogen from the soil, as nitrates or ammonia, and it would seem that amides constitute a considerable proportion of the produced nitrogenous bodies, apparently as an intermediate stage in the formation of albuminoids. At any rate such bodies are found to exist largely in the immature plant, whilst the amount of them diminishes as the plant or its various parts approach to maturity.

But not only have we thus, in unripened vegetable productions, a greater or less, and sometimes a very large, proportion of the nitrogenous bodies formed within the plant existing as amido-compounds, but we may have a large amount existing in the juices as nitric acid, and some as ammonia, &c. Thus, E. Schulze determined the nitric acid in various "roots," and he found that, in some mangels, more than one-third of the total nitrogen existed in that form, and about one-tenth as much as ammonia. In a considerable series at Rothamsted we have found an ex-

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* "On the Composition of Foods in relation to Respiration and the Feeding of Animals.

tremely variable proportion existing as nitric acid, according to the size, succulence, or degree of maturity, of the roots; the amount being, as a rule, the least with the ripest and less highly nitrogenous roots, and the most with the most succulent, unripe, and highly nitrogenous ones. In some cases it reached as much as from 20 to nearly 30 per cent of the total nitrogen. In many other immature vegetable products nitric acid and ammonia have been found, but, so far as I remember, in none in anything like so large a proportion as in the so-called "root-crops," especially mangels. In many, however, the quantity appears to be immaterial; and it is remarkable that whilst there is so much in the "roots," little or none is found in potatoes.

No wonder that, in the experiments already referred to, we found the feeding result to be the worse the more succulent and immature the roots, and the higher their percentage of nitrogen, accordingly.

But it is to the difference in amount of the albuminoid bodies themselves, in different descriptions of vegetable produce, that I wish specially to direct attention, making, however, some reference to what is known of the proportion of the nitrogen existing as amido-compounds.

In some mangels E. Schulze found only from about 20 to 22 per cent of their total nitrogen to exist as insoluble and soluble albumin. But he found in one case 32·5, and in the other 40·8, per cent of the total nitrogen as amides. In a large series of determinations at Rothamsted, by Church's method, we found a variation of from under 20 to over 40 per cent of the total nitrogen of mangels to exist as albuminoids; or, in other words, from nearly 60 to over 80 per cent of it in the non-albuminoid condition.

In potatoes Schulze found from under 50 to 65 per cent of the total nitrogen as soluble and insoluble albumin, and from 27·7 to 49·1 per cent as neutral and acid amides. In a series of potatoes grown at Rothamsted, under very various conditions as to manuring, and in two different seasons, we found the nitrogen as albuminoids to range from little over 50 to more than 71 per cent of the total nitrogen, leaving, of course, from less than 30 to nearly 50 per cent to be accounted for in other ways.

Kellner determined the amount of nitrogen as albuminoids, and as amido-compounds, in a considerable series of green foods, both leguminous and gramineous, cut at different stages of their growth. The proportion of the total nitrogen not as albuminoids was, upon the whole, greater in the Leguminosæ than in the Gramineæ. In both, however, the proportion as albuminoids increased as the plants approached to maturity. The proportion as albuminoids was in all these products very much larger than in roots, and generally larger than in potatoes. In the case of first-crop meadow hay, we found in the separated gramineous herbage 76·4, in the leguminous herbage 82, and in the miscellaneous herbage 80·3 per cent of the nitrogen as albuminoids; and in the second crop 86·2 per cent in the gramineous, 88·3 per cent in the leguminous, and 88·1 per cent in the miscellaneous herbage. How far the higher proportion of the nitrogen as albuminoids in the second crops is to be taken as any indication of the characteristics of the autumn growth, or how far it is to be attributed to the accidental condition of the weather, may be a question.

These illustrations are sufficient to give some idea of the range and proportion of the nitrogen in different feeding crops which does not exist as albuminoids; and they are sufficient to show that a very large proportion of the non-albuminoid matter exists as various amido-compounds. The question arises, therefore, whether these bodies contribute in any way to the nutrition of the animals which feed upon them? We have but little experimental evidence on this point. As green herbage is the natural food of many descriptions of animal, we might suppose that characteristic constituents of it would not be without some value as food; but the cultivated root crops are much more artificial productions, and it is in them that we find such a very large proportion of non-albuminoid nitrogen. With respect to some of the amido-compounds, at any rate, direct experiments seem to show that they are digested in the animal body, and increase the elimination of urea. Weiske and Schrodt found that rabbits receiving, as their only nitrogenous food, either asparagine or gelatin, wasted and died; but a rabbit receiving both asparagine and gelatin increased in weight and survived to the end of the experiment, which lasted seventy-two days. From the results of other experiments made with sheep, they concluded that both asparagine and gelatin protect the albuminoids of the body from oxidation.

These considerations lead me, in conclusion, to refer briefly, and I promise it shall be as briefly as is consistent with clearness, to the two very much disputed questions of *the origin of muscular power*, and *the sources of the fat of the animal body*. These subjects Mr. Lawes and myself have frequently discussed elsewhere; but as the controversy has assumed a new phase quite recently, it seems desirable and appropriate that I should recur to it on the present occasion.

With regard to the question of the sources in the food of the fat of the animal body, Liebig originally maintained that although fat might be formed from the nitrogenous compounds within the body, the main source of it in the herbivora was the carbo-hydrates. In his later writings, he sharply criticised the experiments and arguments of those who have maintained the formation of fat chiefly from the proteine compounds; but he at the same time seems to attach more importance to that source than he formerly did. He gives it as his opinion that the question cannot be settled by experiments with herbivora. He adds that what we know with certainty is that, with these animals, albuminates and carbohydrates work together to produce fat; but whether the non-nitrogenous product, fat, has its origin in the albumin or in the carbohydrate, he considers it not easy to determine.

At the time when we commenced our experiments on the feeding of animals in 1847, the question whether the fat of the animals fed for human food was mainly derived from albuminoids or from carbohydrates had been scarcely raised, or at least it was not prominent. The question then was rather—whether the herbivora received their fat ready formed in their food, or whether it was produced within the body—the latter view being that which Liebig had so forcibly urged, at the same time maintaining that at any rate its chief source was the carbohydrates. Accordingly, our experiments were not specially arranged to determine whether or not the whole of the fat produced could or could not be derived from the albuminoids.

For each description of animal, oxen, sheep, and pigs, such foods as had been established by common experience to be appropriate were selected. The general plan of the experiments was, to give to one set a fixed amount of a recognised good food, containing known quantities of nitrogen, fatty matter, &c., to another set the same amount of another food, of different characters in these respects; to other sets also fixed amounts of other foods in the same way: and then there was given to the whole series the same complementary food *ad libitum*. Or, to one set was supplied a uniform food rich in nitrogen, and to others uniform foods poorer in nitrogen, and so on, in each case *ad libitum*.

It will be seen that, in this way, a great variety of dietaries was arranged, and it will be observed that in each case the animals themselves fixed their consumption according to the requirements of the system.

As already indicated, the individual nitrogenous and non-nitrogenous compounds of the foods were not determined. As a rule, the constituents determined were—the total dry matter, the ash, the fatty matter, and the nitrogen; from which last the amount of nitrogenous compound it might represent was calculated by the usual factor. But, as already said, the results so obtained were only used with considerable reservation, especially in the case of all immature vegetable produce. Nor was the crude fibre determined; but, as in the case of the estimated nitrogenous substance, when interpreting the results, it was always considered whether or not the food contained much or little of probably indigestible woody matter.

The animals being periodically weighed, we were thus able to calculate the amounts of the so-estimated nitrogenous substance, and of the total non-nitrogenous substance, including and excluding fat, consumed—*for a given live-weight within a given time*, and *to produce a given amount of increase in live-weight*.

Experiments were made with a large number of sheep and a large number of pigs. And, even without making allowance for the different condition of the nitrogenous or of the non-nitrogenous constituents, in comparable foods, the results so uniformly indicated that, both the amount consumed by a given live-weight of animal within a given time, and that required to produce a given amount of increase, were determined much more by the amount of the non-nitrogenous than by that of the nitrogenous constituents which the food supplied. And when allowance was made for the different condition of the nitrogenous constituents, and for the greater or less amount of the non-nitrogenous ones which would probably be indigestible and effete, the indications were still more remarkable and conclusive.

In very many cases the animals were slaughtered, and carefully examined as to whether the tendency of development had been more that of growth in frame and flesh, or in fatness. Here, again, the evidence was clear, that the tendency to growth in frame and flesh was favoured by a high proportion of nitrogen in the food, and that to the production of fat by a high proportion of digestible non-nitrogenous constituents.

In a few cases the actual amount of fat in the animals in the lean, and in the fat condition, was determined; and the results admitted of no doubt whatever that a very large proportion of the stored-up fat could not have been derived from the fatty matter of the food, and must have been produced within the body.

So decisive and consistent were the very numerous and very varied results in regard to these points, that we had no hesitation in concluding—not only that much of the fat stored up was produced within the body, but that the source of much, at any rate, of the produced fat must have been the non-nitrogenous constituents of the food—in other words, the *carbohydrates*.

As already stated, however, as the question whether the source of the produced fat was the proteine compounds or the carbohydrates was not then prominent, we had not so arranged the experiments as to obtain the largest possible increase in fat with the smallest possible supply of nitrogenous compounds in the food, nor did we then even calculate whether or not there was sufficient nitrogenous matter consumed to be the source of the whole of the fat produced.

This question, indeed, excited very little interest, until, at a meeting of the Congress of Agricultural Chemists held at Munich, in 1865 (at which I happened to be present), Professor Voit, from the results of experiments made in Pettenkofer's respiration apparatus with dogs fed on flesh, announced his conclusion that fat must have been produced from the nitrogenous substance, and that this was probably the chief, if not the only, source of the fat, even of herbivora—an opinion which he subsequently urged much more positively.

In the discussion which followed the reading of Prof. Voit's paper, Baron Liebig forcibly called in question his conclusions; maintaining not only that it was inadmissible to form conclusions on such a point in regard to herbivora, from the results of experiments made with carnivora, but also that direct quantitative results obtained with herbivorous animals had afforded apparently conclusive evidence in favour of the opposite view.

Voit's paper excited considerable controversy, in which Mr. Lawes and myself joined. We maintained that experiments to determine such a question should be made not with carnivora or omnivora fed on flesh, but with herbivora fed on their appropriate fattening food, and on such herbivora as common experience showed to be pre-eminently fat-producers. We pointed out* that the pig comprised, for a given live-weight, a comparatively small proportion of alimentary organs and contents; that, compared with that of the ruminants, his food was of a high character, yielding, for a given weight of it, much more total increase, much more fat, and much less necessarily effete matter; that, in proportion to his weight, he consumes a larger amount of food, and yields a larger amount, both of total increase and of fat, within a given time; and, lastly, that he contains a larger proportion of fat, both in a given live-weight and in his increase whilst fattening.

It is obvious that, with these characteristics, there is much less probable range of error in calculating the amount and the composition of the increase in live-weight in relation to the amount and composition of the food consumed, than in the case of the ruminants; and that, therefore, the pig is very much more appropriate for the purpose of experiments to determine the sources in its food of the fat it produces.

Accordingly, we calculated a number of our early experiments made with pigs, to determine whether or not the nitrogenous substance they consumed was sufficient for the formation of the fat they produced. For simplicity of illustration, and to give every possible advantage to the view that nitrogenous substance might have been the source of the produced fat, we assumed the whole of the crude fat of the food to have been stored up in the animal—thus estimating a minimum amount to be produced. Then, again, we supposed the whole of the nitrogenous substance of the food to be perfectly digested, and to become available for the purposes of the system. Lastly, after deducting the amount of nitrogenous substance estimated to be stored up as such, the whole of the remainder was reckoned to be so broken up that no other carbon compounds than fat and urea would be produced.

The result was, that, even adopting these inadmissible assumptions, in all the cases in which, according to common experience, the food was admittedly the most appropriate for the fattening of the animal, the calculation showed that a large amount of fat had been produced which could not have been derived from the nitrogenous substance of the food, and must therefore have had its source in the carbohydrates. Such a result is, moreover, entirely accordant with experience in practical feeding.

Reviewing the whole subject in great detail in 1869, Professor Voit refers to these results and calculations. He confesses that he has not been able to get a general view of the experiments from the mass of figures recorded, and from his comments he shows that he has on some points misunderstood them. He admits, however, that, as the figures stand, it would appear that fat had, in some instances, been derived from the carbohydrates. Still, he says, he cannot allow himself to consider that a transformation of carbohydrates into fat has thus been proved.

Professor Emil von Wolff, again, in his *Landwirthschaftliche Fütterungslehre*, referring to the same experiments, admits that they are almost incomprehensible unless we assume the direct concurrence of the carbohydrates in the formation of fat. He nevertheless seems to consider that evidence of the kind in question is inconclusive; and he suggests that experiments with pigs should be made in a respiration apparatus to determine the point.

Mr. Lawes and myself entertained, however, the utmost confidence that the question was of easy settlement without any such apparatus, provided only suitable animals and suitable foods were selected. I accordingly gave a paper on the subject in the *Section für Landwirthschaft und Agricultur Chemie*, at the *Naturforscher Versammlung*,

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* "On the Sources of the Fat of the Animal Body," *Philosophical Magazine*, December, 1866.

held at Hamburg, in 1876.[*] The points which I particularly insisted upon were—that the pig should be the subject of experiment; that he should be allowed to take as much as he would eat of his most appropriate fattening food, so that his increase, and the fat he produced, should bear as large a proportion as possible to his weight, to the total food, and to the total nitrogenous substance consumed. Finally, it was maintained that, if these conditions were observed, and the constituents of the food determined, and those of the increase of the animal estimated according to recognised methods, the results could not fail to be perfectly conclusive, without the intervention, either of a respiration apparatus, or of the analysis of the solid and liquid matters voided.

Results so obtained were adduced in proof of the correctness of the conclusions arrived at. We at the same time admitted, that although, for reasons indicated, we had always assumed that fat was formed from the carbohydrates in the case of ruminants as well as of pigs, yet, as in our experiments with those animals we had supplied too large amounts of ready-formed fat, or of nitrogenous matter, or of both, it could not be shown so conclusively by the same mode of calculation in their case as in that of pigs.

In the discussion which followed, Professor Henneberg agreed that it seemed probable that fat could be formed from the carbohydrates in the case of pigs. In the case of experiments with other animals, however, the amount of fat produced was too nearly balanced by the amount of fat and albuminous matters available, to afford conclusive evidence on the point.

Quite recently, Professor Emil von Wolff (*Landwirthschaftliche Jahrbücher*, band viii., 1879, Supplement) has applied the same mode of calculation to results obtained by himself with pigs some years ago. He concluded that the whole of the body fat could not have been formed without the direct co-operation of the carbohydrates of the food. But what is of greater interest still is that he also calculated, in the same way, the results of some then quite recent experiments of Henneberg, Kern, and Wattenberg, with sheep. He thus found that, even including the whole of the estimated amides with the albumin, there must have been a considerable production of fat from the carbohydrates; and, excluding the amides, the amount reckoned to be derived from the carbohydrates was of course much greater.

I will only add, on this point, that, on re-calculating some of our early results with sheep, which did not afford sufficient conclusive evidence when the whole of the nitrogen of the food was reckoned as albumin, show a very considerable formation of fat from the carbohydrates, if deduction be made for the probable amount of non-albuminoid nitrogenous matter of the food.

We have now, then, the two agricultural chemists of perhaps the highest authority, both as experimenters and writers on this subject on the continent, giving in their adhesion to the view, that the fat of the herbivora, which we feed for human food, may be, and probably is, largely produced from the carbohydrates. I dare say, however, that some physiologists will not change their view until Voit gives them sanction by changing his, which, so far as I know, he has not yet done.

The question which has been currently entitled that of "The Origin of Muscular Power," or "The Sources of Muscular Power," has also been the subject of much investigation, and of much conflict of opinion, since the first publication of Liebig's views respecting it in 1842.

As I have already pointed out, he then maintained that the amount of muscular tissue transformed, the amount of nitrogenous substance oxidated, was the measure of the force generated in the body. He accordingly concluded that the requirement for the nitrogenous constituents of food would be increased in proportion to the increase of the source expended. In his more recent writings on the subject, he freely criticises those who take an opposite view. He nevertheless grants that the secretion of urea is not a measure of the force exerted; but, on the other hand, he does not commit himself to the admission that the oxidation of the carbohydrates is a source of muscular power.

The results of our own early and very numerous feeding experiments were, as has been said, extremely accordant in showing that, provided the nitrogenous constituents in the food were not below a certain rather limited amount, it was the quantity of the digestible and available non-nitrogenous constituents, and not that of nitrogenous substance, that determined—both *the amount consumed by a given live-weight within a given time*, and *the amount of increase in live-weight produced*. They also showed that one animal, or one set of animals, might consume two or three times as much nitrogenous substance in proportion to a given live-weight within a given time as others in precisely comparable conditions as to rest or exercise. It was further proved that they did not store up nitrogenous substance at all in proportion to the greater or less amount of it supplied in the food, but that the excess reappeared in the liquid and solid matters voided.

So striking were these results that we were led to turn our attention to human dietaries, and also to a consideration of the management of the animal body undergoing somewhat excessive labour, as, for instance, the hunter, the racer, the cab-horse, and the foxhound, and also pugilists and runners. Stated in a very words, the conclusion at which we arrived from these enquiries (which were summarised in our paper given at Belfast in 1852) was—that, unless the system were overtaxed, the demand induced by an increased exercise of force was more characterised by an increased requirement for the more specially respiratory, than for the nitrogenous, constituents of food.

Soon afterwards, in 1854, we found by direct experiments with two animals in exactly equal conditions as to exercise, both being in fact at rest, that the amount of urea passed by one feeding on highly nitrogenous food was more than twice as great as that fed on a food comparatively poor in nitrogen.

It was clear, therefore, that the rule which had been laid down by Liebig, and which has been assumed to be correct by so many writers, even up to the present time, did not hold good—namely, that "The sum of the mechanical effects produced in two individuals, in the same temperature, is proportional to the amount of nitrogen in their urine; whether the mechanical force has been employed in voluntary or involuntary motions, whether it has been consumed by the limbs or by the heart and other viscera"—unless, indeed, as has been assumed by some experimenters, there is, with increased nitrogen in the food, an increased amount of mechanical force employed in the "involuntary motions" sufficient to account for the increased amount of urea voided.

The question remained in this condition until 1860, when Bischoff and Voit published the results of a long series of experiments made with a dog. They found that, even when the animal was kept at rest, the amount of urea voided varied closely in proportion to the variation in the amount of nitrogenous substance given in the food—a fact which they explained on the assumption that there must have been a corresponding increase in the force exercised in the conduct of the actions proceeding within the body itself in connection with the disposal of the increased amount of nitrogenous substance consumed. Subsequently, however, they found that the amount of urea passed by the animal was, with equal conditions as to food, &c., no greater when he was subjected to labour than when at rest; whilst, on the other hand, the carbonic acid evolved was much increased by such exercise. They accordingly somewhat modified their views.

In 1866 appeared a paper by Professors Fick and Wislicenus, giving the results obtained in a mountain ascent. They found that practically the amount of urea voided

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[*] The substance of that communication is given in the *Journal of Anatomy and Physiology* vol. xi., part iv.

was scarcely increased by the labour thus undertaken. Professor Frankland gave an account of these experiments in a lecture at the Royal Institution in the same year; and he subsequently followed up the subject by an investigation of the heat developed in the combustion of various articles of food, applying the results in illustration of the phenomena of the exercise of force.

Lastly, Kellner has made some very interesting experiments with a horse at Hohenheim, the results of which were published last year. In one series, the experiment was divided into five periods, the same food being given throughout; but the animal accomplished different distances, and drew different weights, the draught being measured by a horse dynamometer. The changes in live-weight, the amount of water drank, the temperature, the amount of matters voided, and their contents in nitrogen, were also determined.

The result was, that with only moderate labour there was no marked increase in the nitrogen eliminated in the urine; but that with excessive labour the animal lost weight and eliminated more nitrogen. Kellner concluded, accordingly, that, under certain circumstances, muscular action can increase the transformation of albumin in the organism in a direct way; but that, nevertheless, in the first line is the oxidation of the non-nitrogenous matters —carbohydrates and fat, next comes in requisition the circulation albumin, and finally the organ albumin is attacked.

In reference to these conclusions from the most recent experiments relating to the subject, we may wind up this brief historical sketch of the changes of view respecting it, with the following quotation from our own paper published in 1866*:—" . . . . all the evidence at command tended to show that by an increased exercise of muscular power there was, with increased requirement for respirable material, probably no increased production and voidance of urea, unless, owing to excess of nitrogenous matter in the food, or a deficiency of available non-nitrogenous substance, or diseased action, the nitrogenous constituents of the fluids or solids of the body were drawn upon in an abnormal degree for the supply of respirable material."

In conclusion, although I fully agree with Voit, Zuntz, Wolff, and others, that there still remains much for both Chemistry and Physiology to settle in connection with these two questions of "The Sources of the Fat of the Animal Body," and "The Origin of Muscular Power," yet I think we may congratulate ourselves on the re-establishment of the true faith in regard to them, so far at least as the most important practical points are concerned.

THE CONDITIONS NECESSARY FOR THE EXISTENCE OF MATTER IN THE LIQUID STATE.

EXISTENCE OF ICE AT HIGH TEMPERATURES.

By THOMAS CARNELLEY.

Numerous experiments which I have made during the last few weeks on the boiling-points of substances under low pressures, the details of which will shortly be published, have led to the following conclusions in reference to the conditions necessary for the existence of any substance in the liquid state. These are two in number, viz.:—

1. In order to convert a gas into a liquid the *temperature* must be *below* a certain point (termed by Andrews the *critical temperature* of the substance), otherwise no amount of pressure is capable of liquefying the gas.
2. In order to convert a solid into a liquid the *pressure* must be *above* a certain point, which I propose to call the *critical pressure* of the substance, otherwise no amount of heat will melt the substance.

If the second of the above conditions be true, it follows that if the necessary temperature be attained the liquefaction of the substance depends solely on the superincumbent pressure; so that if by any means we can keep the pressure on the substance below its critical pressure no amount of heat will liquefy it, for in this case the solid substance passes directly into the state of gas, or, in other words, it sublimes without previous melting.

Having come to this conclusion it was easily foreseen that if these ideas were correct it would be possible to have *solid ice at temperatures far above the ordinary melting-point*. After several unsuccessful attempts I was so fortunate as to attain the most perfect success, and have obtained solid ice at temperatures so high that it was impossible to touch it without burning one's self. This result has been obtained many times and with the greatest ease, and not only so, but on one occasion a small quantity of water was frozen in a glass vessel which was so hot that it could not be touched by the hand without burning it. I have had ice for a considerable length of time at temperatures far above the ordinary boiling-point, and even then it only sublimed away without any previous melting. These results were obtained by *maintaining* the superincumbent pressure below 4·6 m.m. of mercury, *i.e.*, the tension of aqueous vapour at the freezing-point of water. Other substances also exhibit these same phenomena, the most notable of which is mercuric chloride, for which latter the pressure need only be reduced to about 420 m.m. On letting in the pressure the substance at once liquefies.

For the success of these experiments in the case of water one or two details of manipulation are necessary, but these will be subsequently described.

Firth College, Sheffield, September 6, 1880.

ON THE

CONDITION OF CARBON IN STEEL, AND THE EFFECT OF "HARDENING" UPON IT.

By THOMAS W. HOGG.

In connection with the observations published by Mr. Parker in the Chemical News, vol. xlii., p. 88, I beg to submit the following results which were obtained some time ago while trying a number of hardening experiments for Messrs. John Spencer and Sons. I may here remark that their publication was delayed with the view of making them more complete. I therefore, in consideration of Mr. Parker's prior publication, submit them as a more complete addition to the same. (See Table next page.)

The first three are samples of a peculiar kind of steel, and were selected for these experiments on account of the large amount of graphite present in them. No. 4 is a sample of B. Vaughan's steel, and No. 6 a sample of file steel.

All these samples were hardened in cold water with the exception of A. After hardening, the first three samples were kept in an annealing furnace three or four days, the other samples being softened by merely allowing them to cool in ashes after heating them to the required temperature.

In connection with the above analyses the following were the methods used:—

Colour test.—These were all made by means of a carefully-prepared standard of hammered crucible steel, which contained the merest trace of graphite. In the case of the hardened samples which could not be drilled, these were found to be easily broken into splinters on prolonged hammering with a hard hammer. The splinters obtained from the centre of the material, and weighing approxi-

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* "Food in its Relations to Various Exigencies of the Animal Body," *Phil. Mag.*, July, 1866.

HARDENING EXPERIMENTS.

No. 1 Sample—

	Before Hardening.	Hardened.	Annealed or Softened.
Combined carbon (colour test)	0·89	0·58	0·98
Graphitic carbon	0·29	traces	0·20
Total carbon (cl. test + graphite)	1·18	0·58	1·18
Total carbon (by combustion)	1·18	1·09	1·20

No. 2 Sample—

	Before Hardening.	Hardened.	Annealed or Softened.
Combined carbon	0·80	0·66	0·79
Graphitic carbon	0·34	0·07	0·44
Total carbon (cl. test + graphite)	1·14	0·73	1·23
Total carbon (by combustion)	1·21	1·10	1·19

No. 3 Samble—

	Before Hardening.	Hardened.	Annealed or Softened.
Combined carbon	0·93	0·60	0·80
Graphitic carbon	0·11	traces	0·25
Total carbon (cl. test + graphite)	1·04	0·60	1·05
Total carbon (by combustion)	1·00	0·97	1·00

No. 4 Sample—

	Before Hardening.	Hardened.	Annealed or Softened.
Combined carbon	0·38	0·22	0·38
Graphitic carbon	0·04	none	traces
Total carbon (cl. test + graphite)	0·42	0·22	0·38
Total carbon (by combustion)	0·39	0·34	0·38

No. 5 Sample—

	Before Hardening.	Hardened.	Annealed or Softened.
Combined carbon	0·45	0·21	0·39
Graphitic carbon	0·04	none	0·07
Total carbon (cl. test + graphite)	0·49	0·21	0·46
Total carbon (by combustion)	0·47	0·50	0·48

No. 6 Sample—

	Before Hardening.	Hardened.	Annealed or Softened.
Combined carbon	1·05	0·52	0·94
Graphitic carbon	traces	traces	0·06
Total carbon (cl. test + graphite)	1·05	0·52	1·00
Total carbon (by combustion)	0·99	1·02	0·98

Sample marked A.—

	Before Hardening.	Hardened in Water.	Hardened in Oil.
Combined carbon	0·50	0·23	0·33
Graphitic carbon	0·04	traces	0·04
Total carbon (cl. test + graphite)	0·54	0·23	0·37
Total carbon (by combustion)	0·56	0·54	0·53

mately 0·1 grm., were specially retained for colour test; the weight of standard being made exactly equal to that of the splinter, and the comparisons being made by a very simple method to be described hereafter.

Combustion.—The samples were treated with neutral cupric chloride, the carbon separated from the precipitated copper by means of cupric chloride, and hydrochloric acid, filtered on an asbestos filter, washed with hydrochloric acid until the washings were colourless, then with hot water, then dried, and mixed with black oxide copper, and burnt in a current of pure oxygen as usual.

Graphite.—Before making these determinations, I tried the following preliminary experiments, in order to see whether the strength of the acid used and its temperature had any effect upon the amount found:—

A. The sample was dissolved in hot strong hydrochloric acid (1·16 sp. gr.). Graphite = 0·24 per cent.

B. The sample was dissolved in hot dilute (the above *plus* an equal bulk of H_2O.) Graphite = 0·20 p.c.

C. The sample was dissolved in cold dilute (as in B). Graphite = 0·20 per cent.

I concluded from these and a great many other results that the amount found by using hydrochloric acid of varying strengths within ordinary limits would not differ (practically) if the air was carefully excluded and the solution filtered immediately after the last particle of steel had dissolved, as it is improbable that any graphite would be decomposed in the presence of the escaping nascent hydrogen. The graphite being here in a very minute state of division, hydrochloric acid in the presence of air and with the aid of heat attacks it very rapidly. I therefore (with the above precautions) used the method B, as the time required for perfect solution was very much less than C. The carbon was separated on an asbestos filter, and burnt in oxygen as usual, the accuracy of the apparatus being verified by blank experiments and burning very small quantities of sugar. This was necessary on account of the very small amounts of graphite present in some cases. Probably the process recently elaborated by Dr. Dupré for this purpose would here be of great advantage, but for want of time I was obliged to forego it.

Apart from the considerations to be applied to the various phenomena connected with hardening, the above results, together with Mr. Parker's, point out the probable cause of the discrepancies which frequently occur amongst chemists, for it is evident that two samples of steel may yield identical results by colour test and yet differ very considerably by combustion, or *vice versa*. In fact, by hardening the sample at different temperatures, or cooling in different fluids, it seems possible *to cause the sample to yield any required colour test within certain limits.* For instance, it would be possible to cause No. 6 sample above by a little manipulation to yield colour tests ranging from 0·52 to 1·02.

On examining the results detailed above it will be seen that the difference observed is much greater than those described by Mr. Parker. The reason for this is to be found in the fact that his samples were hardened under conditions not favourable to the production of the maximum hardness capable of being produced by cold water.

With regard to the peculiar reactions of the carbon shown above, it is evident that it therefore exists in three different modifications, as follows:—

(1.) As graphitic carbon: in this condition it is insoluble in hot nitric acid (1·2 sp. gr.).

(2.) As a peculiar modification of carbon, capable of dissolving in hot nitric acid (1·2), and communicating the peculiar colouration on which Eggertz's colour test depends.

(3.) As a peculiar modification of—or, what is more probable, a definite compound of—carbon and iron, which dissolves in, or is otherwise decomposed by, hot nitric acid (1·2) *without communicating any colour whatever.*

The first and second forms exist in ordinary unhardened and unhammered steel: the first may be entirely, or at least partially, eliminated by hammering.

The third is an essential constituent of steel *after* it has been hardened, *not before*: the second also always exists to a certain extent.

The following are the amounts of the carbon in this state after the steel has been hardened in the samples examined, as shown by the difference of the colour test *plus* graphite and the result found by combustion:—

No. 1	0·51 per cent	Of carbon neither graphitic nor in the ordinary state as shown by Eggertz's colour test.
2	0·37 ,,	
3	0·37 ,,	
4	0·12 ,,	
5	0·29 ,,	
6	0·50 ,,	
A	0·31 ,,	
A hardened in oil	0·16 ,,	

Of course the carbon has already been divided into three states, named graphitic, cement, and hardening carbon respectively; but the above third form of carbon differs most decidedly from the hardening carbon here mentioned, because it is shown not to exist until the steel is hardened, while the theory is that it exists before hardening. (For instance, Prof. Akermann says—"Hammering favours the conversion of cement carbon into hardening carbon.")

Taking these results as they stand, the conclusion I come to is that iron forms a definite compound with carbon at high temperatures, and that because of its instability it gradually decomposes when the material in which it exists is submitted to slow cooling; and if the heat be suddenly abstracted, the compound is prevented from decomposition. The more rapid the abstraction of heat the more of this compound there is left undecomposed, and the harder therefore will be the steel.

The theory which is, I believe, at present most favourably looked on as explaining the phenomena of hardening may be summed up as follows:—"The violent compression produced as a result of the sudden cooling causes the carbon to combine or enter into more intimate union with the iron." In opposition to this I place the following as one tending to a more satisfactory explanation:—"That the rapid abstraction of heat partially prevents the decomposition of an unknown unstable compound of iron and carbon, the presence of which compound communicates the peculiar properties of hardened steel."

Laboratory, Newburn Steel Works,
Newcastle-on-Tyne, August 23, 1880.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Note.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 5, August 2, 1880.

Preparation of Chlorine.—M. Berthelot.—The author has resumed the study of the reactions which ensue when chlorine is prepared in the ordinary manner. The facts observe prove that they are accompanied by phenomena of equilibrium in which the relative proportions of water, of hydrochloric acid, and of chlorine play an essential part. The formation of the brown soluble compound requires not merely the presence of chlorine and of manganese, but also of a decided excess of hydrochloric acid. The compound is perchlorinised hydrochlorate of manganese.

Heats of Combustion.—M. Berthelot.—The author considers that the researches of M. Thomsen, published in the *Berichte* of the German Chemical Society, confirm his own results.

Synthesis of Hexamethyl-benzine and of Mellic Acid.—C. Friedel and J. M. Crafts.—The authors have completed the oxidation of hexamethyl-benzine, thus obtaining mellic acid, and realising the synthesis of one of the most complex natural acids.

Researches on the Electric Effluve.—P. Hautefeuille and J. Chappuis.—The alternative discharge apparatus of M. Arn. Thénard are very suitable for showing the rain of electric fire in the different gases and under varied pressures. The phenomena in question are particularly distinct and brilliant in silicon fluoride. Next to this gas nitrogen presents the finest rain of fire. In oxygen the effluve is scarcely visible.

Researches on Batteries.—A. d'Arsonval.—The author has found two methods for obviating the inconvenience that chemical action in batteries is never entirely arrested when the circuit is open. One of these methods is based upon the absorbent power of animal charcoal, and is applicable to all the cases where the depolarising liquid is a metallic salt. He has constructed a sulphate of copper battery, in which the copper solution cannot be diffused through the zinc. He takes an ordinary Callaud element, at the bottom of which is placed a stratum of powdered copper sulphate, covered with bone-black, washed, and powdered. The zinc is placed in the upper part of the jar, and is separated from the copper sulphate by the bone-black. The element thus arranged resembles a Minotti battery, in which bone-black is substituted for sand. The zinc remains entirely unaffected. The second method, more general than the first, consists in taking as a depolarising body a liquid which gives a precipitate on mixture with the liquid which attacks the zinc. The diaphragm separating the two liquids is thus rendered completely impermeable. The precipitate formed in its pores must be a conductor of electricity and must be capable of electrolysis.

Researches on the Combustion-heats of Certain Bodies of the Fatty Series.—W. Louguinine.—A determination of the combustion-heats of allylic alcohol, ethyl-vinyl-carbinol, normal propylenic glycol, and isopropylenic glycol.

MISCELLANEOUS.

The British Association.—At the concluding meeting of the British Association at Swansea, Dr. Sclater, after having read the report of the General Committee, remarked that the Swansea meeting had not been successful, but then the greatest success was never expected in towns which were very far away from the great centres of population. So far as the excursions went they had laboured under an *embarras de richesses*, and several of the proposed parties had to be abandoned. The total number of tickets issued was 915, made up as follows:—Old life members, 144; new life members, 41; associates, 389; ladies, 147; and foreigners, 12. The total receipts were £899, as against £1736 received at Sheffield. The grants, amounting to £1010, were appropriated as follows:—Mathematics and Physics—Mr. G. H. Darwin, Lunar Disturbance of Gravity, £30; Professor Everett, Underground Temperature, £20; Professor Carey Foster, Electrical Standards, £100; Mr. James Glaisher, Luminous Meteors, £15; Dr. Joule, Mechanical Equivalent of Heat, £40; Dr. O. Lodge, High Insulation Key, £5; Professor Sylvester, Fundamental Invariations, £40; Sir William Thomson, Seismic Experiments, £30; Sir William Thomson, Tidal Observations, £10; Mr. J. M. Thomson, Inductive Capacity of Crystals and Paraffins, £10. Chemistry—Dr. Gladstone, Specific Refractions, £10; Lord Rayleigh, Spectrum Analysis, £10. Geology—Professor Duncan, Fossil Polyson, £10; Mr. J. Evans, Geological Record, £100; Professor E. Hull, Underground Waters, £10; Professor A. C. Ramsay, Earthquakes in Japan, £25; Dr. Sorby, Metamorphic Rocks, £10; Professor W. C. Williamson, Tertiary Flora, £20. Biology—Dr. M. Foster, Scottish Zoological Station, £50: Dr. M. Foster, Naples Zoological, £75; Lieutenant-Colonel Godwin Austen, Natural History of Socotra, £50; Mr. Gwyn Jeffreys, Exploration of Seabed North of the Hebrides, £50; General Pitt-Rivers, Anthropological Notes, £20; Dr. Pye-Smith, Elimination of Nitrogen during Bodily Exercise, £50; Professor Rolleston, Pre-Historic Remains in Dorsetshire, £25; Mr. Sclater, Natural History of Timorlant, £50; Mr. Stainton, Geological Record, £100. Economical Science and Statistics—Mr. F. Galton, Estimation of Weights and Heights of Human Beings, £30. Mechanics—Mr. Bramwell, Patent Laws, £5; Mr. James Glaisher, Wind Pressure on Buildings, £5; Professor Osborne Reynolds, Steering Steamships, £5. The meeting in 1881 will be held at York, where the first meeting, in 1831, was held, under the presidency of Sir John Lubbock, Bart., F.R.S.

NOTICE.

The STUDENTS' NUMBER of the Chemical News will be published on Friday, September 17th. Gentlemen holding official positions in the Universities, Colleges, Medical Schools, &c., of the United Kingdom, where Chemistry and Physical Science form a part of the education, who have not yet forwarded the necessary information to our Office for publication in that Number, will confer a favour by sending it with the least possible delay.

THE CHEMICAL NEWS.

VOL. XLII. No. 1086.

ADDRESS TO STUDENTS.

THE duty or the policy—and the two are here identical—of a man entering upon any course of serious study is not as simple as was the case in former days. We refer not merely to the vastly increased extent of every science, to the enhanced accuracy now expected in all investigations, or to the rapid progress made in every department, which frequently compels us to revise before the end of the year views which we had adopted at its beginning. There is another and a more perplexing difficulty. Formerly, and even at present in some countries, the student had to keep in view one paramount object only. To whatever science he had devoted himself he had to make himself thoroughly master of its principles and its methods. The purpose of study was not so much to acquire a mere summary of what had already been discovered by others as to become capable of continuing their work and of adding to the stores of truth which they had accumulated. The power of effecting such continuations and of making such additions is surely the best, the all-sufficient, proof that the student's time has not been misspent.

But in higher education as conducted in modern England this unity of purpose no longer exists, and this test of proficiency is no longer accepted. The student is required not merely to make himself, if possible, thoroughly acquainted with his subject, but to satisfy certain official persons that he has obtained such an acquaintance. If he does not succeed in the latter object his actual proficiency in the science in question will be of no avail. And if a due knowledge of such science be a part of the preparation required for some professional career his time will have been in one sense wasted. Hence this latter object assumes the preponderance;—the shadow outweighs the substance, and, in the never-to-be-forgotten words of Prof. Huxley, we study in these days not to know but to "pass," the consequence being that we pass and don't know. The difficulty, then, placed before the British student is how to combine the two purposes: how, whilst qualifying himself to take a good position at an examination, he may at the same time become fitted for a career of research hereafter. We cannot, unfortunately, give any neat formula or recipe for compassing this object. But there are some considerations which may profitably be kept in mind. Let it be remembered that examinations are only a means, real or supposed, towards a certain end, not the end itself. When a man has got so far as to define, even in his secret thoughts, Science as a mere something to be examined in he is intellectually dead. In conjunction with this caution we must make a demand upon the moral nature of the student. We must exhort him, at whatever cost of time and labour, to eschew cram, including under the term all the tricks and dodges by which a really undisciplined mind is made to put on a false appearance of mastership. It is not safe to argue that the English system of examinations being essentially a sham it may be legitimately evaded. He who wins degrees and diplomas by deceit will have acquired habits of dishonesty which will cling to him in after life, and which will manifest themselves in a propensity for trimming and cooking results, for suppressing inconvenient facts and forging evidences for a tottering theory. He who cheats examiners in his youth will in after life be apt to cheat scientific societies and the learned world at large for his own glorification, and may perhaps for a time suceeed.

If we might presume to address a word of advice to examiners we would recommend them, in place of reading up recondite matter in order to puzzle a student, to devise means for distinguishing true knowledge from mere cram, and intelligent comprehension of principles from the results of verbal memory.

UNIVERSITIES AND COLLEGES.

UNIVERSITY OF LONDON.

CANDIDATES for any Degree granted by this University are required to have passed the Matriculation Examination, to which no candidate is admitted unless he has produced a certificate showing that he has completed his sixteenth year.

The Fee for this examination is £2.

The Examination will be held on Monday, January 10th, 1881. It is conducted by means of Printed Papers; but the Examiners are not precluded from putting, for the purpose of ascertaining the competence of the Candidates to pass, *viva voce* questions to any Candidate in the subjects in which they are appointed to examine.

Candidates are not approved by the Examiners unless they have shown a competent knowledge in each of the following subjects:—Latin. Any two of the following Languages:—Greek, French, German, and either Sanskrit or Arabic. The English Language, English History, and Modern Geography. Mathematics. Natural Philosophy. Chemistry.

The Papers in Latin and Greek will contain passages to be translated into English, with questions in Grammar and History and Geography arising out of the subjects of the books selected. Short and easy passages will also be set for translation from other books not so selected. A separate paper will be set containing questions in Latin Grammar, with simple and easy sentences of English to be translated into Latin.

Candidates may substitute German for Greek.

The papers in French and German will contain passages for translation into English, and questions in Grammar, limited to the Accidence. The paper in Sanskrit or Arabic will contain passages for translation into English, and questions in Grammar.

The examination in the English Language, English History, and Modern Geography includes Orthography; Writing from Dictation; the Grammatical Structure of the Language. History of England to the end of the Seventeenth Century.

That in Mathematics includes the ordinary Rules of Arithmetic; Vulgar and Decimal Fractions; Extraction of the Square Root. Addition, Subtraction, Multiplication, and Division of Algebraical Quantities; Proportion; Arithmetical and Geometrical Progression; Simple Equations. The First Four Books of Euclid, or the subjects thereof.

The Questions in Natural Philosophy are of a strictly elementary character; they include Mechanics, Hydrostatics, Hydraulics, Pneumatics, Optics, and Heat.

The Examination in Chemistry is—Chemistry of the Non-metallic Elements; including their compounds—

their chief physical and chemical characters—their preparation—and their characteristic tests.

A Pass Certificate, signed by the Registrar, will be delivered to each Candidate who applies for it; after the Report of the Examiners has been approved by the Senate.

If in the opinion of the Examiners any Candidates in the Honours Division of not more than Twenty years of age at the commencement of the Examination possess sufficient merit, the first among such Candidates will receive an Exhibition of thirty pounds per annum for the next two years; the second among such Candidates will receive an Exhibition of twenty pounds per annum for the next two years; and the third will receive an Exhibition of fifteen pounds per annum for the next two years; such exhibitions are payable in quarterly instalments, provided that on receiving each instalment the Exhibitioner declares his intention of presenting himself either at the two Examinations for B.A., or at the two Examinations for B.Sc., or at the First LL.B. Examination, or at the Preliminary Scientific and First M.B. Examinations, within three academical years* from the time of his passing the Matriculation Examination.

Under the same circumstances, the fourth among such Candidates will receive a prize to the value of ten pounds in books, philosophical instruments, or money; and the fifth and sixth will each receive a prize to the value of five pounds in books, philosophical instruments, or money.

Any Candidate who may obtain a place in the Honours Division at the Matriculation Examination in January is admissible to the First B.A. or to the First B.Sc. Examination in the following July.

First B.Sc. Examination.

The First B.Sc. Examination will be held in July, 1881.

No Candidate (with the exception of such as have obtained Honours at the Matriculation Examination in the preceding January) is admitted to this Examination within one academical year of the time of his passing the Matriculation Examination.

The Fee for this Examination is £5.

The Examination embraces the following subjects:—Pure and Mixed Mathematics, Inorganic Chemistry, Experimental Physics, and General Biology.

Examination for Honours.

Any Candidate who has passed the First B.Sc. Examination in all its subjects may be examined at the Honours Examination next following the First B.Sc. Examination at which he has passed for Honours in (1) Mathematics, (2) Experimental Physics, (3) Chemistry, (4) Botany, and (5) Zoology; unless he has previously obtained the Exhibition in Pure and Mixed Mathematics at the First B.A. Examination, in which case he will not be admissible to the Examination for Honours in that subject; or unless he has previously obtained an Exhibition at the Preliminary Scientific (M.B.) Examination in either of the subjects which are common to it with the first B.Sc. Examination, in which case he will not be admissible to the Examination for Honours in that subject.

Candidates for Honours in Chemistry will be examined in Inorganic Chemistry, treated more fully than in the Pass Examination. In addition, they will be examined practically in Simple Qualitative Analysis. This Examination, which will consist of six hours' examination by printed papers and of six hours' practical work, will take place on Thursday and Friday in the same week with the Examination for Honours in Mathematics, commencing on each day at 10 a.m.

In the Examination for Honours, the Candidate, not being more than 22 years of age at the commencement of the Pass Examination, who most distinguishes himself in Mathematics, will receive an Exhibition of £40 per annum for the next two years.

Second B.Sc. Examination.

The Second B.Sc. Examination will be held in October, 1881.

Candidates for this Examination are required to have passed the First B.Sc. Examination at least one academical year previously.

The Fee for this Examination is £5.

The regulations are framed with the view of allowing the candidate to select *any three* of the following nine subjects:—

1. Pure Mathematics.
2. Mixed Mathematics.
3. Experimental Physics.
4. Chemistry.
5. Botany, including Vegetable Physiology.
6. Zoology.
7. Animal Physiology.
8. Physical Geography and Geology.
9. Mental and Moral Science.

Examination for Honours.

Any Candidate who has passed the Second B.Sc. Examination, and has not previously passed the Second B.A. Examination, may be examined at the Honours Examination next following the Second B.Sc. Examination at which he has passed, for Honours in (1) Mathematics, (2) Mental and Moral Science, (3) Experimental Physics, (4) Chemistry, (5) Botany, (6) Zoology, (7) Physiology, (8) Physical Geography and Geology; provided that he shall have gone through the Pass Examination in the corresponding subject or subjects immediately before. And any Bachelor of Arts who has passed the Second B.Sc. Examination may under the same conditions be examined for Honours in one or more of the above mentioned subjects, unless he have previously obtained a Scholarship at the Second B.A. Examination in either of the first two of those subjects, in which case he shall not be admissible to the Examination for Honours in that subject.

The examination for Honours in Chemistry will take place on Monday and Tuesday in the week following the Examination for Honours in Mathematics; on Monday by printed papers (chiefly on Organic Chemistry), and on Tuesday by practical exercises in Simple Qualitative and Quantitative Analysis.

The candidate, being not more than 23 years of age, who most distinguishes himself in Chemistry, will receive £50 per annum for the next two years, with the style of University Scholar.

Doctor of Science.

The examination for the Degree of Doctor of Science takes place annually within the first twenty-one days of June, and the examination in each branch occupies four days.

No candidate is admitted to the examination for the Degree of D.Sc. until after the expiration of two Academical Years from the time of his obtaining the Degree of B.Sc. in this University, unless he shall have passed the Second B.Sc. Examination in the First Division at least two Academical years subsequently to having passed the first B.Sc. Examination, in which case he shall be admitted to the examination for the Degree of Doctor of Science at the expiration of *one* Academical Year from the time of obtaining his B.Sc. Degree.

The Fee for this Examination is £10.

Every candidate for the degree of D.Sc is examined in some one or more of the various branches of Physical,

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* By the term "Academical Year" is ordinarily meant the period intervening between any Examination and an Examination of a higher grade in the following year; which period may be either more or less than a Calendar year. Thus the interval between the *First* Examinations in Arts, Science, and Medicine, and the *Second* Examinations of the next year in those Faculties respectively, is about sixteen months, whilst the interval between the Second B.A. Examination and the M.A. Examination of the next year, or between the Second B.Sc. Examination and the D.Sc. Examination of the next year, is less than eight months. Nevertheless, each of these intervals is counted as an "Academical Year."

Biological, or Mental Science, to be selected by himself; and no candidate is approved by the examiners unless he has shown a thorough practical knowledge of the principal subject and a general acquaintance with the subsidiary subject or subjects, specified as belonging to the branch so selected. He is expected to be so fully conversant with the principal subject he may select as to be able to go through any examinational test (whether theoretical or practical) of his acquirements in it that can be fairly applied. Candidates, when giving notice, must specify the branch or branches in which they desire to be examined.

BRANCH IV. OF PHYSICAL SCIENCE.
INORGANIC CHEMISTRY.

Principal Subject—Inorganic Chemistry.

Subsidiary Subject—Either Organic Chemistry; or Mineralogy, Crystallography, and Chemical Technology in its relations to Inorganic Chemistry.

BRANCH V., ORGANIC CHEMISTRY.

Principal Subject—Organic Chemistry.

Subsidiary Subjects—Either Inorganic Chemistry; or Chemical Technology in its relations to Organic Chemistry, and the Chemistry of Animal and Vegetable Life.

PRELIMINARY SCIENTIFIC (M.B.) EXAMINATION.

This examination will be held in July, 1881.

No Candidate is admitted to this examination until he has completed his seventeenth year, and has either passed the Matriculation Examination* or taken a Degree in Arts in one of the Universities of Sydney, Melbourne, Calcutta, or Madras (provided that Latin was one of the subjects in which he passed). The fee for this examination is five pounds.

Candidates for the degree of M.B. are strongly recommended by the Senate to pass the Preliminary Scientific Examination before commencing their regular medical studies; and to devote a preliminary year to preparation for it according to the following programme:—Winter Session: Experimental Physics; Chemistry (especially Inorganic); Zoology. Summer Session: Practical Chemistry (Inorganic); Botany.

Any candidate who has passed the Preliminary Scientific (M.B.) Examination, may be examined at the Honours Examination next following the Preliminary Scientific Examination at which he has passed, unless he has previously obtained an Exhibition in any one of the subjects at the First B.Sc. Examination, in which case he is not admissible to the Examination for Honours in that subject.

Candidates for Honours in Chemistry are examined in Inorganic Chemistry, treated more fully than in the Pass Examination. In addition they are examined practically in Simple Qualitative and Quantitative Analysis.

EXAMINATION IN SUBJECTS RELATING TO PUBLIC HEALTH.

A Special Examination will be held in December in subjects relating to public health.

No candidate is admitted to this Examination unless he has passed the Second Examination for the Degree of Bachelor of Medicine in ths University at least one year previously; nor unless he shall have given notice of his intention to the Registrar at least two calendar months before the commencement of the Examination.

The Fee for this Examination is £5.

Candidates are examined in the following subjects:—

Chemistry and Microscopy, in relation to the examination of Air, Water, and Food.

Meteorology and Geology, as far as they bear on the duties of Health Officers, viz.:—General knowledge of Meteorological Conditions; Reading and Correction of Instruments. General knowledge of Soils; their Conformation and Chemical Composition.

Vital Statistics, in reference to the methods employed for determining the Health of a Community; Birth-rate; Death-rate; Disease-rate; Duration and of Expectancy of Life. Present amount of Mortality, and its causes, in different Communities.

Hygiene.—General principles of Hygiene. Special topics:—Soil. Construction of Dwellings. Conservancy of Cities. Unhealthy Trades. Supply of Food to Cities, and Examination of Food. Disposal of Sewage. Water-supply.

Medicine, in reference to the origin, spread, and method of prevention of Diseases generally, but especially those of the Epidemic class.

Sanitary Engineering, as far as regards the arrangements connected with Water-supply, Sewerage, and Ventilation. A knowledge of the reading of Plans, Sections, Scales, &c.

Sanitary Law, as far as it relates to the duties of Officer of Health. A knowledge of the powers given under the various Sanitary Acts, as defined in the Instructions issued by the Local Government Board, and of the methods of procedure in special cases.

The Examination, which is both written and practical, extends over four days.

GILCHRIST SCHOLARSHIPS.

1. A Scholarship of the value of Fifty Pounds per annum, and tenable for three years, is annually awarded to the Candidate from the Royal Medical College, Epsom, who at the June Matriculation Examination stands highest among the Candidates approved by the Head Master of that Institution, and who passes either in the Honours List or in the First Division; on condition of his prosecuting his studies during the tenure of his Scholarship with a view to Graduation in one of the Faculties of the University of London.—Particulars may be obtained on application to the Secretary of the Royal Medical College, 37, Soho Square, W.

2. A similar amount is annually offered to Candidates intending to pursue, at Owens College, Manchester, their studies for Graduation in one of the four Faculties of the University of London; a single Scholarship of Fifty Pounds per annum for three years being awarded to the highest of those Candidates at the June Matriculation Examination who shall have been previously approved by the Principal of Owens College, provided that he pass in the Honours Division; or, in case no Candidate should so pass, two Scholarships, each of Twenty-five Pounds per annum, being awarded to the two Candidates as aforesaid who shall stand highest in the First Division.—Particulars may be obtained on application to the Principal of Owens College, Manchester.

Particulars of the Colonial and Indian Scholarships may be obtained on application to the Secretary of the Gilchrist Educational Trust, 4, Broad Sanctuary, Westminster, S.W.

SCIENCE AND ART DEPARTMENT OF THE COMMITTEE OF COUNCIL ON EDUCATION, SOUTH KENSINGTON.

A sum of money is voted annually by Parliament for scientific instruction in the United Kingdom. The object of the grant is to promote instruction in Science, especially among the industrial classes, by affording a limited and partial aid or stimulus towards the founding and maintenance of Science schools and classes.

In order to place a science school or class in connection with the Science and Art Department, an approved committee, consisting of at least five well known and responsible persons, must be formed.

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* Candidates who pass in all the subjects of the Preliminary Scientific (M.B.) Examination, and also pass *at the same time* in the Pure Mathematics of the First B.Sc. Examination, or who have previously passed the First B.A. Examination, are considered as having passed the First B.Sc. Examination. The attention of such candidates is directed to the fact that, under the new regulations for the B.Sc. Degree, this degree may be obtained by passing at the Second B.Sc. Examination in the three Biological subjects only.

The following are the sciences towards instruction in which aid is given by the Science and Art Department:—Practical, Plane, and Solid Geometry; Machine Construction and Drawing; Building Construction; Naval Architecture and Drawing; Pure Mathematics; Theoretical Mechanics; Applied Mechanics; Sound, Light, and Heat; Magnetism and Electricity; Inorganic Chemistry; Organic Chemistry; Geology; Mineralogy; Animal Physiology; Elementary Botany; Biology, including Animal and Vegetable Morphology and Physiology; Principles of Mining; Metallurgy; Navigation; Nautical Astronomy; Steam; Physiography; Principles of Agriculture.

The aid is given in the form of—1. Public examinations in which Queen's Prizes are awarded, held at all places, complying with certain conditions; 2. Payments on results as tested by these examinations; 3. Scholarships and Exhibitions; 4. Building grants; 5. Grants towards the purchase of fittings, apparatus, &c.; 6. Supplementary grants in certain subjects, and special aid to teachers and students.

The examinations are held about the month of May under the superintendence of the local committees and local officers. The examination papers are prepared by the professional examiners in London. An evening is set apart for one or more subjects, so that the examination in each subject is simultaneous over the whole kingdom.

A packet of examination papers is sent to each local examination secretary, who opens it in the presence of the committee and candidates. The committee is held responsible that no unfair means of any description are used in working the papers, and that the rules of the Department are strictly complied with.

The examinations are of two kinds, but held together, viz.—

a. The Class examinations, of which there are two grades or stages; the first stage or elementary examination, and the second stage or advanced examination. The successful candidates in both stages are divided into 1st and 2nd class.

b. The Honours examination of a highly advanced character. In this there are also two classes.

Any person however taught may sit at any one of these examinations.

Queen's prizes, consisting of books or instruments, are given to all candidates who are successful in obtaining a first class in either stage of the class examinations, and Bronze Medals to those who obtain a first class in Honours.

The payments on results are made only on account of the instruction of students of the industrial classes, or on account of the instruction of their children. They are—£2 for a first class, and £1 for a second class, in each stage of the class examinations, and £2 and £4 for a second or first class respectively in honours. Special extra payments are made in attendance in organised Science Schools. Special payments are also made for Chemistry.

There are also two forms of scholarship in connection with elementary schools.

a. In the Elementary School Scholarship £5 are granted to the managers of any elementary school for the support of a deserving pupil selected by competition, if they undertake to support him for a year and subscribe £5 for that purpose. The payment of £5 by the Science and Art Department is conditional on the scholar passing in some branch of science at the next May examination.

b. In the Science and Art Scholarship, which is of a more advanced character, a similar contribution of £5 is required on the part of the locality, and a grant of £10 is made by the Department towards the maintenance, for one year, of the most successful pupil or pupils in elementary schools who have passed certain examinations in science and in drawing.

In the first case the scholar must be from twelve to sixteen, and in the latter from twelve to seventeen years of age. One scholarship is allowed per 100 pupils in the school. Thus a school with 50 pupils may have one scholarship, a school with 150 pupils two scholarships.

There are also two forms of Exhibitions. These are:—

a. Local Exhibitions to enable students to complete their education at some college or school where scientific instruction of an advanced character may be obtained. Grants of £25 per annum, for one, two, or three years are made for this purpose when the locality raises a like sum by voluntary subscriptions. And if the student attend a State School, such as the Royal School of Mines in London, the Royal College of Chemistry in London, or Royal College of Science in Ireland, the fees are remitted. The exhibition must be awarded in competition.

b. Royal Exhibitions of the value of £50 per annum tenable for three years, to the Royal School of Mines, London, and the Royal College of Science, Dublin, are given in competition at the May examinations. Six are awarded each year—three to each institution. The exhibitions entitle the holder to free admissions to all the Lectures, and to the Chemical and Metallurgical Laboratories at those two institutions.

The competition for the Whitworth Scholarships, tenable for three years, is also in part determined by the results of the May examinations.

A grant in aid of a new building, or for the adaptation of an existing building, for a School of Science may be made at a rate not exceeding 2s. 6d. per square foot of internal area, up to a maximum of £500 for any one school, provided that certain conditions are complied with and that the school be built under the Public Libraries and Museums Act, or be built in connection with a School of Art, aided by a Department building grant.

A grant towards the purchase of fittings, apparatus, diagrams, &c., of 50 per cent of the cost of them is made to Science Schools.

Special extra grants in the form of capitation payments are made in fully organised Science Schools.

UNIVERSITY OF OXFORD.

Waynflete Professor of Chemistry.—W. Odling, M.A., F.R.S.

Professor of Mineralogy.—N. S. Maskelyne, M.A., F.R.S.

Every Student must reside in one or other of the Colleges or Halls, or in licensed lodgings, for a period of three years, passing at least two examinations in Arts, and one in either Mathematics, Natural Science, Law, Modern History, or Theology, when, if he obtain a first, second, or third class, he can take his B.A. Degree; if he do not gain such honour he has to pass a third examination in *Literis Humanioribus*.

The fee for students working in the Laboratory for three days in the week during the Term is £3; for students working every day, £5.

Scholarships of about the value of £75 are obtainable at Christ Church, Magdalen, and other colleges, by competitive examination in Natural Science.

More detailed information may be obtained from the University Calendar; from the professors; from E. Chapman, Esq., M.A., Frewin Hall; and from the Sub-Librarian in the Radcliffe Library or the Museum.

UNIVERSITY OF CAMBRIDGE.

Professor of Chemistry.—G. D. Liveing, M.A., F.R.S.

Jacksonian Professor of Natural and Experimental Philosophy.—J. Dewar, M.A., F.R.S.

Demonstrators.—J. W. Hicks, M.A., W. J. Sell, B.A., and H. J. H. Fenton, B.A.

The Student must enter at one of the Colleges, or as a Non-collegiate Student, and keep terms for three years by residence in the University. He must pass the previous examination in Classics and Mathematics, which may be done in the first or second term of residence, or, through the Oxford and Cambridge Schools Examination Board, or through the Senior Local Examinations, before commencing residence. He may then proceed to take a Degree in Arts, either continuing mathematical and classical study, and passing the ordinary examinations for B.A., or going out in one of the Honour Triposes.

The scholarships, ranging in value from £20 to £80 a year, are chiefly given for mathematical and classical proficiency. Scholarships are given for Natural Science in Trinity, St. John's, St. Peter's, Clare, Christ's, Sidney, Pembroke, Caius, and Downing Colleges; the examinations being at Easter, and in June and October.

The Chemical Laboratory of the University is open daily for the use of the Students. The Demonstrators attend daily to give instructions.

Non-collegiate Students are allowed to attend certain of the College Lectures and all the Professors' Lectures, and have the same University status and privileges as the other Students. They are under the superintendence of the Rev. R. B. Somerset, Orford House, Cambridge, from whom further information may be obtained.

The following are the Lectures on Chemistry for the ensuing Academical Year:—

Michaelmas Term, 1880.

General Course, by the Professor of Chemistry, on Mondays, Wednesdays, and Fridays, at 12 noon. Begin Oct. 13.

Spectroscopic Analysis, by the Professor of Chemistry, on Mondays, Wednesdays, and Fridays, at 1.30 p.m. Begin Oct. 18.

Analysis, by the Professor and the Demonstrators of Chemistry. Daily. Begin Oct. 13. Also at St. John's College, by Mr. Main. Begin Oct. 13. Also at Caius College, by Mr. Pattison Muir; begin Oct. 18. Also at Sidney College. Begin Oct. 12.

Metals, by Mr. Pattison Muir. Monday, Wednesday, and Friday, at 10. Begin Oct. 18.

Physical Chemistry, by the Jacksonian Professor, on Tuesdays, Thursdays, and Saturdays, at 11. Begin Oct. 14.

Elementary Organic Chemistry, by Mr. Main, at St. John's College, on Tuesdays, Thursdays, and Saturdays, at 10 a.m. Begin Oct. 14.

Volumetric Analysis, by a Demonstrator, on Tuesdays, Thursdays, and Saturdays, at 10 a.m. Begin Oct. 14.

Catechetical Lectures, by Mr. Lewis, at Downing College, on Mondays, Wednesdays, and Fridays, at 9 a.m. Begin Oct. 14.

Electricity and Electro-Magnetism, by the Professor of Experimental Physics, on Mondays, Wednesdays, and Fridays, at 1 p.m. Begin Oct. 13.

Lent Term, 1881.

General Course continued, by the Professor of Chemistry, on Mondays, Wednesdays, and Fridays, at 12 noon. Begin Feb. 2.

Analysis, by the Professor or Demonstrators of Chemistry. Daily. Begin Jan. 19. Also at St. John's College. Begin Jan. 31. Also at Caius College Laboratory. Begin Feb. 2. Also at Sidney College Laboratory. Begin Feb. 2.

Organic Chemistry, by the Jacksonian Professor, on Tuesdays, Thursdays, and Saturdays, at 12 noon. Begin Feb. 3.

General Course, begun by Mr. Main, at St. John's Laboratory, on Tuesdays, Thursdays, and Saturdays, at 11 a.m. Begin Feb. 1.

Non-metallic Elements, by Mr. Pattison Muir, at Caius Laboratory, on Mondays, Wednesdays, and Fridays, at 10 a.m. Begin Feb. 2.

Catechetical Lectures, by Mr. Lewis, at Downing College, on Mondays, Wednesdays, and Fridays, at 9 a.m. Begin Feb. 31.

Electricity and Magnetism, by Mr. Trotter, at Trinity College, on Mondays, Wednesdays, and Fridays, at 10 a.m. Begin Feb. 4.

Easter Term, 1881.

Some Special Department, by the Professor of Chemistry, on Tuesdays and Saturdays, at 12 noon. Begin May 3.

Analysis, by the Professor or Demonstrators of Chemistry. Daily. Begin April 25. Also at St. John's College. Begin May 2. Also at Caius College. Begin April 27. Also at Sidney College Laboratory. Begin April 27.

Elementary Chemistry, by a Demonstrator of Chemistry, on Mondays, Wednesdays, and Fridays, at 3 p.m. Begin May 2.

General Course concluded, by Mr. Main, at St. John's Laboratory, on Tuesdays, Thursdays, and Saturdays, at 12 noon. Begin April 27.

Elementary Organic Chemistry and Analysis, by Mr. Pattison Muir, at Caius Laboratory, on Mondays, Wednesdays, and Fridays, at 10 a.m. Begin April 27.

Catechetical Lectures, by Mr. Lewis, at Downing College, on Mondays, Wednesdays, and Fridays, at 9 a.m. Begin April 23.

Inorganic Chemistry, Catechetical Lectures, by Mr. Walker, on Mondays, Wednesdays, and Fridays, at 12 noon. Begin April 27.

Electricity and Magnetism, by the Professor of Physics, at Cavendish Laboratory, on Tuesdays, Thursdays, and Saturdays, at 12 noon. Begin in April.

Long Vacation (July and August), 1881.

Practical Physics, in the Cavendish Laboratory.

Practical Chemistry, in the University Laboratory.

UNIVERSITY OF DUBLIN.—TRINITY COLLEGE.

The Chemical Laboratory will re-open on October 1st, 1880, with complete working accommodation for upwards of 100 students.

The University Professor of Chemistry, Dr. Emerson Reynolds, F.R.S., assisted by the Demonstrator of Chemistry, Mr. Early, conducts the undermentioned Courses of Laboratory instruction:—

The First Course of Practical Chemistry.—Michaelmas Term:—Qualitative Analysis and the Use of the Spectroscope. Hilary Term: Volumetric and Simple Gravimetric Analysis. Trinity Term: Organic Preparations and Analysis.

Students can also attend the Professor's Lectures on General Chemistry, and repeat most of the experiments performed in the Theatre.

This Course terminates on the last day of June.

The Second or Advanced Course includes instruction in the higher branches of Experimental and Analytical Chemistry, and in Methods of Research. Students who take out this Course are free to devote their chief attention to the study of special departments of Chemistry as applied to Arts and Industries.

Summer Course of Practical Chemistry for Medical Students.—This Course commences on the first Monday in April and terminates on the 30th of June following. Students experiment in the Laboratory from 2 to 4 o'clock on Tuesdays, Thursdays, and Saturdays.

Special Courses.—Students can enter at any time throughout the academic year for short terms of Laboratory instruction in Medical or Pharmaceutical Chemistry, or in the Methods of Analyses of Water, Air, &c., for Sanitary Purposes.

Lectures in Medical and Pharmaceutical Chemistry.—Dr. Reynolds lectures at 2 o'clock on Tuesdays, Thursdays, and Saturdays, from the 1st of November to the 31st of March following.

All the classes are open to extra-Academic Students.

KING'S COLLEGE.

(Department of Engineering and Applied Science.)

Professor of Chemistry.—C. L. Bloxam, F.C.S.

Demonstrator of Practical Chemistry.—J. M. Thomson, F.C.S.

Assistant Demonstrator.—G. S. Johnson, F.C.S.

On Tuesday and Friday at 10.20 a.m. Students of the First Year are admitted to the Course of Theoretical and Applied Chemistry. The Course commences with a View of the Forces which concur to the production of Chemical Phenomena, after which the laws of Chemical Attraction are discussed, and the Non-metallic elements and their principal Compounds are described.

The Metals and their principal compounds are next examined, care being taken to point out the applications of the Science to the Arts; and the processes of the different Manufactures, of Metallurgy, and of Domestic Economy, are explained and illustrated.

Examinations of the Class, both *vivâ voce* and by written papers, are held at interval during the course at the usual Lecture hour.

Second Year.—Students attend in the Laboratory twice a week, on Tuesday and Friday, at 10.20, and they go through a course of Manipulation in the most important operations of Chemistry, including the first steps of Analysis.

Any Student of this Department may be admitted to this Class at any period of his study on payment of an extra fee.

Experimental and Analytical Chemistry in the Laboratory.—The object of this Class is to afford to Students who are desirous of acquiring a knowledge of analysis, or of prosecuting original research, an opportunity of doing so under the superintendence of the Professor and Demonstrator; Students may enter, upon payment of extra Fees, at any time except during the vacation, and for a period of one, three, six, or nine months, as may best suit their convenience. The laboratory hours are from ten till four daily, except Saturday, on which day the hours are from ten till one.

In addition to the Laboratory Fee, each Student defrays the expenses of his own Experiments. The amount of this expense, which is comparatively trifling, is entirely under his own control.

Special hours and fees are arranged for the convenience of such Third Year Students as wish to study Analytical Chemistry.

Fees.—Chemistry per term, £3 3s. od.; per ann., £8 8s. od.; Practical Chemistry per term, £4 4s. od.; per ann., £10 10s. od.; Experimental and Analytical Chemistry —One Month (daily attendance), £4 4s. od.; Three Months (daily attendance), £10 10s. od.; Six Months (daily attendance), £18 18s. od.; Nine Months (daily attendance), £26 5s. od. A student taking a month's ticket may attend daily during 1 month, or 3 days a week during 2 months, or 2 days a week during 3 months.

Rules as to Admission of Students.

I. The Academical Year consists of Three terms: Michaelmas Term, from beginning of October to the week before Christmas; Lent Term, from the middle of January to the week before Easter; Easter Term, from Easter to the beginning of July.

II. The days fixed for the Admission of New Students in the Academical Year 1880-81, are Tuesday, October 5, Wednesday, January 12, and Wednesday, April 27.

UNIVERSITY COLLEGE.

Faculty of Science.

Chemistry.—Professor Williamson, Ph.D., F.R.S.

I. General Course.

Lectures daily (except Saturday) from 11 to 12 a.m., up to the last week in March.

Exercises on Tuesdays, Wednesdays, Thursdays, and Fridays, from 9 to 10 a.m.

Fees for the Course, £7 7s.; Perpetual, £9 9s.; for the Half Course, £4 4s.; for the Organic Course alone, £2 2s.

Fee for the Exercise Class, £2 2s.

The instruction in this Class is of two kinds, consisting partly of Experimental Lectures by the Professor, partly of Exercises and personal instruction on the subject of the Lectures by an Assistant.

Attendance on the Exercise Class (conducted by Mr. Temple A. Orme, F.C.S.) enables Students to do their work more effectually and rapidly than they can do it by themselves.

A. *The first half* of the course, to Christmas, includes those parts of Chemistry which are required for the Matriculation Examination of the University of London.

B. *The second half* of the Course, from January to March, includes the following subjects:—

1. Preparation and properties of the chief *Metals*, including their characteristic reactions and most important salts. Detection of metallic poisons. Quantitative estimation of metals. Principles of classification. Monatomic, diatomic metals, &c.

Organic Chemistry commences in the second week in February, and occupies five Lectures weekly till about the end of March. It includes a study of the characteristics and metamorphoses of the chief organic acids, bases, alcohols, ethers, colouring-matters, &c. Methods of ultimate and proximate analysis. Determination of molecular weights. Theory of types; of compound radicals. Phenomena of fermentation, &c.

Training of Teachers.

Teachers of Chemistry are trained in the theory and practice of their profession. A two years' Course is absolutely requisite for this purpose; but Students will with advantage devote a longer period to it.

The first year is occupied with attendance on the Courses of Chemistry and of Analytical Chemistry. In the second year the Student again attends the Course of Chemistry, and is intrusted with teaching-work in conjunction with the Tutors of the class. At the same time he continues to work in the Laboratory at analysis and original research.

In order to qualify themselves for rising to the higher ranks of the Profession, gentlemen remain for a further period, in which case they may obtain remunerative work in teaching through the recommendation of the Professor.

It must not, however, be supposed that a study of Chemistry alone, however complete, is sufficient to qualify a man to teach the Science effectively. A competent knowledge of Physics, Mathematics, and either French or German must necessarily be acquired at some period of the Student's Course.

II.—Analytical and Practical Chemistry.

A. *Birkbeck Laboratory.*

Assistants.—C. A. Bell, B.A., M.B., and H. Forster Morley, M.A., B.Sc.

When accompanied or preceded by attendance on the lectures on Chemistry, the Laboratory Course qualifies Students in the application of Chemistry to the Manufacturing Arts, Metallurgy, Medicine, or Agriculture, &c. Instruction is given in the principles and processes of gas-analysis.

The Laboratory and offices are open daily from 9 a.m. to 4 p.m., from the 6th of October until the middle of July, with a short recess at Christmas and at Easter. Saturday, from 9 to 2.

Fees, for the Session, 25 guineas; six months, 18 guineas; three months, 10 guineas; one month, 4 guineas; exclusive of the expense of materials.

A Gold Medal and Certificates of Honour are competed for by students entered for the session.

B.—Summer Practical Courses.

Chemical Theatre.

1. *Elementary Course.* (*Men.*)—About Forty Lessons, of one hour each, commencing in the first week of May.

The first six weeks of the Course are occupied by the study of the chief non-metallic elements and their simple compounds. Metallic salts, &c., are subsequently studied

Fees—including the cost of materials and apparatus: for the Course, £5 5s.; for a Second Course, £3 3s.

2. *Senior Course.* (*Men.*)—This Course consists of Twenty Lessons of two hours each, commencing in the first week in May.

The first half of the Course includes tests for fixed and volatile organic acids, nitrogenised acids, sugars, glycerin, alkaloids, &c.

The second half of the Course includes tests for mineral poisons in organic mixtures; also tests for organic bodies, such as alkaloids, when mixed with other organic substances.

Volumetric methods of the quantitative analysis of sugar and urea, chlorides, phosphates, hardness of water, alkalimetry, are practised.

Analysis of milk and ashes of blood.

Fees—including cost of materials and apparatus: for the Course, £5 5s.; for a Second Course, £3 3s.

III.—SUMMER MATRICULATION COURSE. (*Men.*)

TEMPLE A. ORME, F.C.S.

This Course includes those parts of Chemistry which are required for the Matriculation Examination of the University of London.

The Course consists of about Twenty Lessons in Practical Chemistry, and of an equal number of oral lessons. These lessons will begin on March 30th, 1881, at 11.

Fee, including cost of materials and apparatus, £4 4s.

ELEMENTARY CHEMISTRY.

Chemical Theatre. (*Women*).

Lectures—Wednesday and Friday, from 4 to 5.

A Class of Elementary Chemistry, including the subjects required for Matriculation, will be given during the Winter Session by C. A. Bell, B.A., M.B., Chief Assistant in the Chemical Laboratory, commencing Wednesday, October 13.

The instruction will consist partly of Lectures, partly of Laboratory Experiments performed by the Students.

Fees for the Course, including use of apparatus and materials, £4 4s.

Chemical Technology.

Professor CHARLES GRAHAM, D.Sc., F.I.C.

Assistant.—C. J. Wilson, F.C.S.

The Course of instruction in this Department is designed to afford to Students who propose to devote themselves to industrial pursuits in which Chemistry plays an important part, or to prepare themselves for the profession of Consulting Chemist, the instruction essential for their success in their future line of work. It will also be found of great value in two of the branches (Organic and Inorganic Chemistry) in which the Degree of Doctor of Science can be taken at the University of London.

Assuming that the Student enters for a three years' study, the following will give an idea of the nature of the work during the period:—

In the first year the Student will attend Lectures on Theoretical Chemistry, and work at Analytical Chemistry in the Chemical Laboratory, and will also attend Lectures on Mathematics, Mechanics, and Physics. The Mechanical Drawing Class should also be attended during the first year by all Students.

In the second year, the Student will again attend the Lectures on Theoretical Chemistry, and will begin his study of Applied Chemistry by attendance on the Lectures in this subject, and by practical work in the Laboratory on the applications of Chemistry.

The third year will also be chiefly occupied with attendance on the Lectures on Chemical Technology, and in practical work connected therewith in the Laboratory.

In the second and third years the Student will, in addition to the foregoing subjects, which are common to all, attend Lectures and work at such other branches of Pure and Applied Science as may be deemed advisable after consultation with the Professor.

Students entering the College with more advanced scientific knowledge will be able to shorten the Course described to two years, or even one year, as may be found advisable.

For the convenience of those already engaged in business, and of those from other causes prevented from entering for a longer period of study, it is arranged that they can attend a Course of Lectures upon any one subject of Applied Chemistry without being required to attend any other lectures, either in Applied Chemistry or in other subjects.

In the Session 1881-82, it is proposed to treat of the following subjects:—

(1) The Chemistry of the Alkali trade.
(2) Soap, Glass, Pottery, Cements.
(3) Chemistry of Brewing.
(4) Agricultural Chemistry.

Should a sufficient number of Students desire a Course of Lectures on some subject of Applied Chemistry other than those above mentioned, the Professor will give such either in lieu of, or in addition to, those mentioned.

Students desirous of working at subjects not included in the foregoing Courses, such as Photography and Photographic materials, Paper-making, Gas-tar products, the products of the Distillation of Wood, Tanning, and other Chemical industries, will receive individual instruction in the Laboratory.

Fees—for each Course, £2 2s.; for the four Courses together, £5 5s.

Chemical Laboratory.

The instruction in the Laboratory in Chemical Technology, will consist of the examination and valuation of raw materials used, and of the final products obtained, in various manufacturing industries, and of experimental examination of the processes employed in the arts and manufactures.

The Laboratories are open daily from 9 a.m. to 4 p.m., from the 6th of October until the middle of July, with a short recess at Christmas and at Easter. Saturday, from 9 to 2.

Fees—for the Session, 25 guineas; six months, 18 guineas; three months, 10 guineas; one month, 4 guineas; exclusive of the expense of materials.

ROYAL SCHOOL OF MINES.

The mode of instruction at the Royal School of Mines is by systematic Courses of Lectures, by written and oral examinations, by practical teaching in the Laboratories, in the Drawing Office, and in the Field.

To become Associates at the Royal School of Mines it is necessary to pass through the following course of study:—

1st Year—Inorganic Chemistry, with practice in laboratory; Mechanical Drawing, both terms.

2nd Year—1st Term, Applied Mechanics and Physics, with practice in Laboratory. 2nd Term, Mineralogy; Mechanical Drawing, both terms.

3rd Year (Mining Division)—1st Term, Mining; Assaying. 2nd Term, Geology, with practice in laboratory and field. Metallurgical Division, 1st and 2nd Terms—Metallurgy, with practice in laboratory. Geological Division, 1st Term—Natural History, with practice in laboratory. 2nd Term—Geology, with practice in laboratory and field; Palæontological Demonstrations.

The courses of instruction are distributed over three years, but those students who possess sufficient knowledge may, if they think fit, pass through the whole in two years, by presenting themselves during the current year for examination in the subjects allotted to the first and second years.

Students desirous of obtaining the distinction of Associate of the Royal School of Mines, who have already acquired a knowledge of the subjects of the first two years, may proceed at once to the courses of the third year, by passing the final class examinations in those subjects before the Professors of the Royal School of Mines, and paying a fee of £1 for each examination.

During the Session 1880-81 the following courses of Lectures will be delivered:—

40 Lectures on Inorganic Chemistry, commencing Oct. 4, 1880; 30 Lectures on Organic Chemistry, commencing Jan. 17, 1881, by E. Frankland, F.R.S.

80 Lectures on Biology, by T. H. Huxley, LL.D., F.R.S., commencing Oct. 4, 1880.

36 Lectures on Applied Mechanics, by T. M. Goodeve, M.A., commencing Oct. 4, 1880.

60 Lectures on Physics, by F. Guthrie, F.R.S., commencing Nov. 23, 1880.

50 Lectures on Metallurgy, by W. Chandler Roberts, F.R.S., commencing Oct. 4, 1880.

60 Lectures on Mining, commencing Nov. 8, 1880, and 40 Lectures on Mineralogy, commencing Feb. 21, 1881, by W. W. Smyth, M.A., F.R.S.

50 Lectures on Geology, by John W. Judd, F.R.S., commencing Feb. 14, 1881.

20 Lectures on Mechanical Drawing, by J. H. Edgar, M.A., commencing Oct. 9, 1880.

The Lectures on Chemistry, Physics, Mechanics, Biology, Geology, Metallurgy, and Mechanical Drawing are delivered at the Science Schools, South Kensington, where instruction is also given in the Laboratories. The Lectures on Mineralogy and Mining are given at the School of Mines in Jermyn Street, where the Metallurgical Laboratory is situated.

The Laboratories for instruction in chemical manipulation, in qualitative and quantitative analysis, the technical application of analysis, and in the method of performing chemical researches, are under the direction of Dr. Frankland, and will be opened on Friday, October 1, 1880.

The charge for instruction in the Chemical Laboratory is £12 for three months, £9 for two months, and £5 for one month.

The Metallurgical Laboratory is conducted by the Instructor in Assaying, Mr. R. Smith, in consonance with the views of the Lecturer, and is devoted to practical instruction in Metallurgy, especially in Assaying. The nature of this instruction will be adapted to the special requirements of the Student. It comprises:—Assaying in all its branches, especially of the more important metals, such as iron, copper, lead, tin, alloys of silver and gold, &c.; and the examination of fuel, fire-clay, ores, and metallurgical products.

The ability of the Student to make trustworthy assays is in every case thoroughly tested; and no certificate of competency is given to a Student who has not furnished satisfactory proof that he is able to obtain trustworthy results.

The charge for instruction in the Metallurgical Laboratory is £12 for three months, £9 for two months, and £5 for one month.

Lectures to Working Men.—Short Courses of Lectures at suitable periods of the year are given in the evening to Working Men. These courses are systematic, and arranged so as to illustrate, within a period of two years, the principal subjects taught at the institution. Those for the ensuing Session include Chemistry, Mineralogy, Applied Mechanics.

The following Exhibitions, Scholarships, and Prizes are awarded in connection with the School:—

Nine Royal Exhibitions, each of £50 per annum, three of which are competed for annually at the May examinations of the Department of Science and Art.

Two Royal Scholarships of £15 each to the best students of the first year; and One Royal Scholarship of £25 to the best second year's student.

The Edward Forbes Medal and Prize for Biology and Palæontology; the De la Beche Medal for Mining; and the Murchison Medal and Prize for Geology.

The public will be admitted to the lectures on payment of £4 for each course of 40 or more lectures, and £3 for the course of 30 and under 40 lectures.

The fee for students desirous of becoming Associates is £30 in one sum on entrance, or two annual payments of £20 each. Students presenting themselves for re-examination must pay a fee of £1 for each subject.

Persons who have taken either a first or a second class certificate in the advanced stage in any subject in science at the examinations held by the Science and Art Department, and who show that they are *bona fide* Science Teachers, may attend the Day Lectures gratuitously, provided that they be examined in at least one subject, paying a fee for such examination of £1 per course.

Perpetual Tickets are issued, which entitle the holder to attend all present and future courses of lectures upon payment of £40.

Officers of the Army and Navy, Her Majesty's Consular and Diplomatic Officers, Officers of the Crown at home on furlough, and acting Mine Agents and Managers of Mines, are admitted to the Lectures at half the above charges.

UNIVERSITY OF ABERDEEN.

Professor of Chemistry.—J. S. Brazier, F.C.S.

I. *Systematic Course.*—The Lectures are delivered on the first five days of each week during the College session. They commence with the discussion of the General Principles of Chemical Philosophy, including the Atomic Theory and the Chemical Relations of Heat. The Nonmetallic and Metallic Elements and their Compounds are fully treated of, together with their more important applications to the Arts. The latter part of the course is devoted to the subject of Organic Chemistry. Examinations are held at fixed periods during the Session. The fee is £3 3s.

II. *Practical Course.*—This course is given during the Summer Session. It is chiefly devoted to practice in Qualitative Analysis, with the view of enabling the Student to test unknown substances, poisons, the animal secretions, &c. The fee is £3 3s.

III. *Laboratory Pupils.*—The Chemical Laboratory is open during the College Session on the first five days in each week, from 10 a.m. till 3 p.m. The course of instruction is under the direction of the Professor of Chemistry and of the Teaching Assistant.

UNIVERSITY COLLEGE, BRISTOL.

Professor of Chemistry.—W. Ramsay, Ph.D.
Lecturer.—D. Orme Masson, M.A., B.Sc.

Inorganic Chemistry.

This Course treats of the principles of Chemistry, and of the Chemistry of the Non-Metals and Metals.

Lectures will be given at 10 o'clock on Mondays, Wednesdays, Fridays, and Saturdays during the First and Second Terms; the last of these being a tutorial lecture, which will deal with points requiring detailed explanation.

The lectures will be illustrated with experiments and diagrams.

Examinations will be held from time to time during the courses.

Fee, £4 4s. for two Terms, £3 3s. for one Term.

Technical Chemistry.

Lectures will be given during the First and Second Terms on Thursdays at 11, on Manufacturing and Metallurgical Processes with which Chemistry has specially to deal.

Fee, £2 2s.

Organic Chemistry.

This Course will relate to the more important groups of the Compounds of Carbon.

Lectures will be given during the Second Term on Tuesdays and Thursdays at 10 o'clock; during the Third Term on Tuesdays, Thursdays, and Saturdays at 10 o'clock.

Fee, £3 3s.

Practical Chemistry.—Laboratory Instruction.

The Laboratory will be open daily from 10 a.m. to 5 p.m., except on Saturdays, when it will close at 1 p.m. Instruction will be given in the Laboratory in all branches of Practical Chemistry, including Qualitative and Quantitative Inorganic and Organic Analysis, the preparation of Chemical Products, and Inorganic and Organic Research. Special facilities will be afforded to those who desire to study Practical Chemistry as applied to the different processes employed in the Arts and Manufactures.

Fees in Guineas—

	Whole Day.			Half Day.	
	6 Days a Week.	3 Days a Week.	2 Days a Week.	6 Days a Week.	3 Days a Week.
Per Session	18	10	7½	12	7
,, Two Terms ..	13	7½	5½	8½	5
,, One Term ..	7	4	3	4½	3
,, Month	3	2	1½		

In order that Students may have an opportunity of acquiring some knowledge of Applied Chemistry, excursions to some of the Mines and Manufactories of the neighbourhood will be occasionally made. They will be conducted by the Professor or by the Lecturer.

Evening Lectures.

Lecturer.—D. Orme Masson, M.A., B.Sc.

Wednesday and Friday, 8 to 9.

This course will consist of Two Lectures a week during the First and Second Terms; they will be devoted to the consideration of the Principles of Chemistry and Chemical Physics and the Study of the chief Non-Metallic Elements. A few Lectures at the end of the Course will be devoted to the consideration of Metals. In treating of the various products under the latter heading special attention will be devoted to their applications in the Arts and Manufactures.

Fee, 10s. 6d. for Two Terms; 7s. for One Term.

Evening Course of Practical Chemistry.

Professor.—W. Ramsay, Ph.D.

Tuesday and Thursday, 7 to 9.

A Practical Class will be formed for instruction in Qualitative and Quantitative Analysis. The Fee for the course(extending over the first and second terms) will be £3 3s., and no additional charge will be made for reagents or apparatus, provided the latter be returned undamaged.

All particulars may be learned by application to the Secretary, University College, Bristol.

ROYAL AGRICULTURAL COLLEGE, CIRENCESTER.

Chemical Department.

Lectures and Laboratory Instruction are given in Organic, Inorganic, and Analytical Chemistry.

THE YORKSHIRE COLLEGE, LEEDS.

Professor of Chemistry.—T. E. Thorpe, Ph.D., F.R.S., F.C.S.

Lecture Courses.

1. General Course on Inorganic and Organic Chemistry—Monday, Tuesday, Wednesday, and Thursday, at 4 p.m., from October to the end of the second term. Fee for the Course, £4 4s.

2. Lectures on Laboratory Practice and Chemical Calculations—Thursday, at 10 a.m., during the First and Second Terms. Fee, £1 1s.

3. Lectures on the Chemistry of the Non-Metals—Saturday, at 12 a.m., during the First and Second Terms. Fee, 10s. 6d.

Laboratory Courses.

The College Laboratory will be open daily from 9 a.m. to 1 p.m., and from 2 to 5 p.m., except on Saturdays, when it will close at 1 p.m.

Fees for the Session—Students working six days per week, £17 17s.; four, £13 13s.; three £11 11s.; two, £8 8s.; one, £4 4s.

Class in Practical Chemistry, Saturday mornings, from 9 to 12, during First and Second Terms. Fee £1 11s. 6d.

Practical Chemistry for Medical Students.—On Tuesday and Thursday, from 10 to 12 a.m., from May to July.

Evening Classes.

A Course of twenty Lectures by Mr. C. H. Bothamley, on the Elements of Inorganic Chemistry (the Non-Metals) will begin during the first and second Terms, on Fridays, at 8 p.m., beginning October 15. Fee, 10s. 6d.

A Course of Twenty Lectures by Mr. C. H. Bothamley on the Metals will be given during the first and second Terms, on Mondays, at 8 p.m., beginning Oct. 11. Fee, 10s. 6d.

Dyeing Department.

Instructor.—J. J. Hummel, F.C.S.

Lecture Course, with practical work in the Dye-house, for Students who wish to receive, on leaving the College, a certificate of proficiency in Dyeing; such certificate to be obtained by special examination in the several subjects of the Course, the latter extending over a period of two years.

Scholarships.

For Associates.—The Cavendish Scholarship. Value £50 per annum, tenable for one year.

Advanced Scholarships.—

The Brown Scholarship. Value £35, tenable for two years.

The Akroyd Scholarship. Value £30 per annum, tenable for two years.

Entrance Scholarships.—

The Salt Scholarship. Value £20 per annum, tenable for two years.

The Akroyd Scholarships. One, value £25, and one, value £20, tenable for two years.

The Brown Scholarships. One, value £25, and one, value £20, tenable for two years.

The Clothworkers' Company Scholarships. For Textile Department four Scholarships, and for Dyeing Department two Scholarships, each of the value of £25 per annum, and tenable for one year.

UNIVERSITY OF DURHAM. COLLEGE OF PHYSICAL SCIENCE, NEWCASTLE.

Professor of Chemistry.—A. Freire-Marreco, M.A.
Demonstrator—J. T. Dunn, B.Sc.

Junior Division.—General Principles of Chemistry. History of the Non-Metallic Elements. History of the Metals and their more important Native and Artificial Compounds. Principles of Qualitative Analysis. Elements of Organic Chemistry. *Senior Division.*—Organic Chemistry. Elements of Applied Chemistry, including Chemical Mineralogy and Analysis.

Practical Chemistry.—The Laboratory is open from 10 a.m. to 1 p.m., and from 2 to 5 p.m., except on Saturdays, when it closes at 1 p.m. *Laboratory Fees.*—Students working six days per week, £5 5s. per term; alternate days, £3 3s.; one day per week, £1 1s.

Arrangements for Laboratory work in the evening and during vacation will be made.

The Session will commence on October 4.

Courses of Study.—Students will be distinguished into Regular and Occasional. Regular Students will be required to follow such a course of study in the subjects professed in the College as will enable them to pass the Examinations for the title of Associate in Physical Science. Occasional Students will attend such classes as they may select. Every candidate for admission as a regular student must pass an examination on entrance, in reading, writing from dictation, English or Latin Grammar, arithmetic (including decimals), and geography. Registered students in medicine are exempted from this examination, or students who produce a certificate of having passed either of the two following examinations:—

1. Durham Senior Examination of Persons not Members of the University, held in June.

2. Durham Examination for Students in Arts in their first year, or any examination of a similar nature that may be accepted by the Council.

Associateship in Physical Science.—Every candidate for the Associateship in Physical Science, will be required to satisfy the examiners in three, at least, of the four subjects,—Mathematics, Physics, Chemistry, and Geology,—in an examination, to be held at the beginning of his second year.

The examination in Chemistry comprises:—General Principles of Chemistry. Elements of Inorganic Chemistry. Elements of Qualitative Analysis, including a Practical Examination.

The examination in Chemistry for Candidates at the end of their second year comprises:—Elements of Organic Chemistry. Applied Chemistry. Advanced Qualitative Analysis, including a Practical Examination. Elements of Quantitative Analysis.

Exhibitions.—Three Exhibitions of £15 each will be awarded in October next, to Candidates desirous of attending the first year course of study in the College.

Candidates must pass the entrance examination, and will, in addition, be examined in the following subjects:—Algebra, up to Quadratic Equations. Euclid, Books I., II., and III. And one of the following special subjects, to be selected by the Candidate:—Geology.—Text Book: Page's "Introductory Text Book." Heat.—Text Book: "Orme on Heat." Chemistry.—Text Book: Gill's "Chemistry for Schools," omitting chapters xiii., xvi., xx., and xxi. Natural History.—Text Book: Nicholson's "Elementary Text Book of Zoology," or Oliver's "Elementary Botany," Part I.

Candidates must send their names to the Secretary, on or before the 25th of September, and specify, at the same time, the special subject in which they desire to be examined.

The examination will be held at the College, and will commence on Monday, the 4th October.

Two similar Exhibitions will be awarded at the next examination of "Persons not members of the University," which will be held at Durham, and elsewhere, in June next, to those candidates who shall most distinguish themselves in subjects allied to Physical Science.

Scholarships.

T. Y. Hall Scholarship.—This Scholarship, of the yearly value of £20, tenable for three years by students attending two or more of the classes, will be awarded on the result of the first examination for the Associateship in Science.

Charles Mather Scholarship.—This Scholarship, of the yearly value of about £40, will be awarded on the result of the Final Examination for the Associateship in Science, and is tenable for one year from the time of obtaining the Associateship in Science, provided the Scholar continues his studies in the College to the satisfaction of the Professors.

Nathaniel Clark Scholarship.—This Scholarship, of the value of £15 for one year, will be awarded in October to that student who shall pass the First Examination for the Associateship in Science, and who shall be most distinguished in Chemistry and Geology. The Scholar will be required to attend the classes of Chemistry and Geology, so as to be qualified to take those subjects for the Final Examination for the Associateship in June next.

OWENS COLLEGE, MANCHESTER.

Professor and Director of the Chemical and Metallurgical Laboratories.—H. E. Roscoe, B.A., Ph.D., F.R.S., F.C.S.

Professor of Organic Chemistry.—C. Schorlemmer, F.R.S.

Demonstrators and Assistant Lecturers.—Mr. W. C. Williams, F.C.S., P. P. Bedson, D.Sc., and Watson Smith, F.C.S.

Lecture Courses.

Systematic Chemistry.—Junior Class.—Tuesday, Thursday, and Saturday, from 9.30 to 10.30 a.m., during Michaelmas and Lent Terms. Comprising—(1) The laws of Chemical Combination; (2) a description of the physical and chemical properties and the mode of preparation of the Non-Metallic Elements and of their Compounds.

Senior Class.—Monday, Wednesday, and Friday, from 9.30 to 10.30 a.m., during the Michaelmas and Lent Terms, comprising—(1) The Chemistry of the Metals and of their most important Compounds; (2) Organic Chemistry.

The instruction in Systematic Chemistry is given by means (*a*) of Experimental Lectures and (*b*) of Tutorial Classes.

Fee—For each Class, £2 12s. 6d.; for both Classes, £4 14s. 6d.

A Tutorial Class, meeting in Sections, will also be held, which all members of the Junior and Senior Classes will be required to attend, unless specially exempted by the Principal and the Professor. Extra fee for this Class, 10s. 6d. This fee is not included in the composition fees payable by regular Students.

Organic Chemistry.—Professor C. Schorlemmer, F.R.S., Monday, Wednesday, and Friday, from 10.30 to 11.30 a.m.

General Course (from October to the end of March).—The subject of this course is the Chemistry of the Carbon Compounds, wherein the branch of Organic Chemistry is more fully and completely treated than in the general course in Systematic Chemistry.

Extended Course (from the beginning of April to the end of the Session).—This course is suited to the requirements of students preparing for the B.Sc. examination and for those who have previously attended the general course and wish to become more intimately acquainted with the subject. The course will treat of the History of the Old and New Theories, and of the most recent important discoveries in Organic Chemistry, &c.

Fee for the General Course, £2 12s. 6d.; for the Extended Course, £1 11s. 6d.; for both Courses, £3 10s.

Chemical Philosophy.—Prof. C. Schorlemmer, F.R.S., Saturday, from 9.30 to 10.30 a.m.

Sketch of the History of Chemistry; Development of Modern Chemistry; Chemical Law and Theories; Relation of Chemistry to Physics.

Fee, £1 11s. 6d.

Technological Chemistry.—Watson Smith, F.C.S., Monday, from 2.30 to 3.30 p.m.

I. The Alkali and Sulphuric Acid Manufacture; Bleaching Powder and Liquor; Potassium Chlorate; Carbon Bisulphide.

II. Destructive Distillation of Coal; Gas Manufacture; Distillation of Coal-tar: Ammonia and Ammonium Salts from Gas-liquor.

Fee, £1 11s. 6d., in addition to which the Sessional Fee of 7s. will be required from all who have not paid the usual Admission Fee on entering the College.

Analytical Chemistry.—Mr. W. C. Williams, F.C.S. Thursday, from 10.30 to 11.30 a.m.

This Course will treat of the methods of Qualitative and Quantitative Analysis, and is intended to supplement the instruction in Practical Chemistry.

Fee, £1 11s. 6d.

Analytical and Practical Chemistry.

LABORATORY COURSES.

The Chemical Laboratories will be open for Students daily from 9.30 a.m. until 4.30 p.m., except on Saturdays, when they will be closed at 12.30 p.m.

Fees for the Session—For six days per week, £21; for four days per week, £17 17s.; for three days per week, £13 13s. Students entering the Laboratory Class at or after Christmas will be charged two-thirds of the fees for the whole Session.

Fees for shorter periods—For six months, £17 17s.; for five months, £15 15s.; for four months, £13 13s.; for three months, £10 10s.; for two months, £7 7s.; for one month, £4 4s. Students entering under this scale are entitled to work on every day during the week.

The Metallurgical Laboratory will be open daily during the same hours as those of the Chemical Laboratories, for instruction in Practical Assaying and the examination of Ores and Metallurgical products.

Fees the same as those for the Chemical Laboratory Course.

A Course of Lectures on Metallurgy will probably be given in the course of the session if a sufficient number of students offer themselves.

Entrance Exhibitions.

I. Victoria Exhibition (Classics), £15.

II. Wellington Exhibition (Greek Testament), £15.

III. Dalton Mathematical Exhibition, £15. Renewable for a second year.

IV. Grammar School Scholarship, £17 per annum, tenable for three years; open to scholars of the Manchester Grammar School only.

V. Two Oxford and Two Cambridge Local Exhibitions, giving free admission to lecture classes in the College for one year, and renewable for two years further, are awarded annually on the results of the Oxford and Cambridge Examinations held in Manchester in December, 1880, and June, 1881.

VI. Gilchrist Scholarship, £50 per annum, tenable for three years; awarded on the results of the Matriculation Examination of the University of London, in June, 1881.

VII. Rumney Scholarship, £45 per annum, tenable for three years.

VIII. Ramsbottom Scholarship, £40 per annum, tenable for two years. The next competition will take place in 1882.

IX. Crace-Calvert Scholarship, £25 per annum, tenable for two years. The next competition will take place in June, 1882. This scholarship is open only to duly qualified members of the Evening Chemistry Classes.

Fellowship.

The Langton Fellowship, £150 per annum, tenable for three years. Candidates must have been students in the College for not less than three sessions, and must during their studentship or within one year after the close of the same have obtained a degree of some University of the United Kingdom, or been elected to the Associateship of the College. The next competition for the Fellowship will take place in 1881.

Scholarships.

The following (except the Shakspere Scholarship) are open to the competition of students of the College only.

I. Victoria Scholarship (Classics), £40 per annum, tenable for two years.

II. Wellington Scholarship (Greek Testament), £20 per annum, tenable for two years.

III. Shuttleworth Scholarship (Political Economy), £50, tenable for one year.

IV. Shakspere Scholarship (English Language and Literature), £40 per annum, tenable for two years.

V. Bradford History Scholarship, £45 per annum, tenable for one year, and renewable for a second year.

VI. Dalton Chemical Scholarships, two, each of £50 per annum, tenable for two years.

VII. Dalton Mathematical Scholarships, one Senior and one Junior Scholarship, of the value of £25 each, tenable for one year.

VIII. Platt Scholarships (Physiology), two, one offered annually, £50 per annum, tenable for two years. (Two Platt Physiological Exhibitions of £20 each are offered for competition at the end of the Session 1880-1, open to first and second years' students in Physiology.)

IX. Heginbottom Physical Scholarship, £30 per annum, tenable for two years.

X. Ashbury Scholarships (Engineering), two, each of £25 per annum, tenable for two years.

Prizes.

I. Lee Greek Testament Prizes, one of £25 and one of £12 10s. value.

II. Classical Prizes.—Essay, value £5. Junior Class Prizes, value £5 and £2 10s.

III. Shuttleworth History Prize, value £5.

IV. English Essay and Poem Prizes, each of the value of £5.

V. Early English Text Society's Prizes.—A selection of the Society's publications offered to the competition of students in the Day and in the Evening Classes respectively.

VI. New Shakspere Society's Book Prizes.

VII. Cobden Club Book Prizes (Political Economy).

VIII. Dalton Natural History Prize, value £15.

IX. Engineering Essay Prize, value £5.

FIRTH COLLEGE, SHEFFIELD.

Professor of Chemistry and Experimental Physics.—T. Carnelley, D.Sc.

First Year's Course.—Inorganic Chemistry, including Laws of Chemical Combination, Non-Metallic Elementary Bodies, &c. Tuesday, Thursday, Saturday, from 9 to 10 a.m. Fee, £3 13s. 6d.

Second Year's Course.—Inorganic and Organic Chemistry, including Properties of Metals and their Compounds, and the Chemistry of the Carbon Compounds. Monday, Wednesday, Friday, from 9 to 10 a.m. Fee, £3 13s. 6d.

Laboratory.—Working hours to be arranged between Professor and Students. Students proposing to work si days a week can be entered for periods of one, two, o three months on payment of a certain proportion of the Sessional Fees, which are—For six days a week, £18 18s.; four, £14 14s.; three, £10 10s.; two, £7 10s. 6d.; one, £4 4s. Fees for short periods:—For one month, £3 13s. 6d.; two months, £6 6s.; three, £9 9s.; four, £12 1s. 6d.; five, £14 3s. 6d.

UNIVERSITY COLLEGE OF WALES, ABERYSTWYTH.

Professor of Chemistry.—T. S. Humpidge, B.Sc.(Lond.), Ph.D., F.C.S.

Lecture Courses.

1. Introductory and Chemistry of the Non-Metals. Monday, Tuesday, Wednesday, and Friday, at 12 noon. From September 15 to January. Fee for the Course, £1.

2. Chemistry of the Metals, on the same days from January to June. Fee for the Course, £2.

3. Organic Chemistry. A Course of 40 Lectures adapted for the Second B.Sc. Examination (London), and other similar examinations. Fee, £1.

Laboratory Courses.

The Chemical Laboratory will be open daily (except Saturdays) for practical laboratory work from 2 to 6 p.m., and at other times on making special application to the Professor. Fee to Students taking Lecture Courses, 15s.

Practical Chemistry for Medical Students.—On Wednesday and Friday, from 3 to 6 p.m., from January to June. Fee to Students taking Lecture Courses, 10s.

ROYAL COLLEGE OF SCIENCE FOR IRELAND,
Stephen's Green, Dublin.

Professor of Practical and Theoretical Chemistry.—W. Noel Hartley, F.C.S.

The Chemical and Metallurgical Laboratories, under the direction of Mr. Hartley, are open every week-day during the Session, except Saturday. Instruction is given in the different branches of Analytical Chemistry, including Assaying, and in the methods for performing Chemical Research. Fee, for the Session of nine months, £12; or for three months, £5; or for one month, £2.

There are four Royal Scholarships of the value of £50 each yearly, with Free Education, including Laboratory Instruction, tenable for two years; two become vacant each year; they are given to Students who have been a year in the College. There are also nine Exhibitions attached to the College, of the yearly value of £50 each, with Free Education, including Laboratory Instruction. tenable for three years; three become vacant each year.

A Diploma of Associate of the College is granted at the end of the three years' course.

ANDERSON'S COLLEGE, GLASGOW.

Professor of Chemistry.—William Dittmar, F.R.S.E.
Chief Assistant.—M. T. Buchanan.
Lecture Assistant.—John Lennox.
Junior Assistants.—R. N. Lennox, James M. Bowie, and John McArthur.

A Course of 100 Experimental Lectures on Chemistry: Daily, Saturdays excepted, from 10 to 11, commencing about the end of October. The Lectures up to the end of the year are devoted to the elements of Chemical Philosophy and to the Chemistry of the Non-metallic Elements. After the new year the Course divides into two branches, viz., the Chemistry of the Metals (on the Mondays and Tuesdays) and Organic Chemistry, select chapters (on the Wednesdays, Thursdays, and Fridays). Six written examinations are held during the Session, which all the members of the class are required to attend.

Fee, £2 2s.

The Laboratory for Practical Instruction in all branches of analysis, including technical assaying, and for original research is open daily (Saturdays excepted) during the Winter Session from 10 to 5, during Summer from 9.30 to 5. Advanced students may obtain permission to work privately on Saturday also until 11 p.m. The teaching is conducted on the tutorial system, each student working by himself and on his own subject. The Laboratory is furnished with all the necessaries for chemical investigation.

Fee for the Winter Session, £10 10s.; Summer Session, £6 6s.; two sessions, if paid in advance, £15 15s., or £2 2s. per month.

Evening Classes.

Lectures on Industrial Chemistry, by Professor W. Dittmar, F.R.S.E., on Friday Evenings, commencing in October. Fee, 2s. 6d.

The following two courses have have been instituted specially for students preparing for the examinations held by the Government Department of Science and Art:—(1) A preparatory course of Scientific and Practical Chemistry. Mondays, 7.30 to 10. Fee, £1 1s. (2) A similar course for more advanced students. Wednesdays, 7.30 to 10. Fee, £2 2s.

THE "YOUNG" CHAIR OF TECHNICAL CHEMISTRY, ANDERSON'S COLLEGE.

Professor.—Edmund J. Mills, D.Sc. (Lond.), F.R.S.
Senior Assistant.—Mr. J. Snodgrass.
Junior Assistant.—Mr. J. Bicket.

This Chair has for its object the instruction of Students in Chemistry as applied to the various branches of industry in Chemical and other works, Metallurgy, Agriculture, &c.

Lectures.—*Principal Course.*—A Course of Fifty Lectures will be delivered on Mondays, Tuesdays, and Wednesdays, at 10 a.m., commencing on November 1st. The Lectures will be illustrated with Experiments, Diagrams, and Models, as well as by the actual Inspection of Manufacturing Processes; and the progress of the Students will be tested by periodical Examinations. The earlier Lectures will have reference to units of weight and measure, to the calculations necessitated by Chemical operations, and to the nature and laws both of the Chemical process and its results. A particular subject will then be considered in comparatively minute detail, embracing for this session Oils, Paints, and Varnishes. These lectures are intended to qualify for the examination of the City and Guilds of London Institute for the Advancement of Technical Education.

Fee for the Course, Two Guineas. To Laboratory Students, One Guinea.

Subsidiary Course.—A subsequent Course of Thirty Lectures will be delivered on Mondays, Tuesdays, and Wednesdays, at 10 a.m. These Lectures are more particularly intended for Dyers, Colour Manufacturers, Brewers and Distillers, Tar Rectifiers, Drysalters, and others interested in a knowledge of Technical *Organic* Chemistry.

Fee for the Course, Two Guineas.

Technical Physical Chemistry.—Mr. J. Snodgrass, Senior Assistant, has arranged to deliver a series of Thirty Lectures and Demonstrations on Physico-Chemical Measurements.

Fee for the Course, Half-a-Guinea. Laboratory Students, Half-a-crown.

Laboratories.—The Laboratories are open daily from 10 to 4, and on Saturday from 10 to 1 o'clock for practical working by the Students, under the superintendence of the Professor and his Assistants.

The Fee for attending the Laboratories is £20 per Session of Nine Months, £14 10s. for Six Months, £7 10s. for Three Months, or £2 10s. per month.

Students must have a fair acquaintance with elementary Chemistry.

The New Laboratory Buildings, immediately contiguous to the former site, are now erected and occupied. They comprise four stories, with a lecture room in the rear, and are exclusively devoted to the purposes of this Chair.

The Trustees, having had under consideration the requirements of Inventors, Patentees, and others whose investigations require isolation and privacy, as well as professional advice, have included in the arrangements Five Private Laboratories, which will, it is expected, meet a demand hitherto unsupplied in this country.

Library.—A Students' Library Society was founded in 1875. Its objects are to provide a collection of standard chemical works, and to maintain a regular supply of chemical journals. A large number of works have already been purchased or bestowed, and nine journals are received. Annual subscription, Half-a-crown.

Memorandum as to Bursaries.

The Trustees of the "Young" Chair have the superintendence of the Bursaries—regulating the appointment and terms on which they shall continue to be held.

The Nominees of Donors to be appointed if they pass the necessary examinations.

The Bursaries are of the amount of £50 each per annum, tenable for three years, during which the Bursars shall be required to give their whole time and attention to the Lectures and Laboratory duties of the "Young" Chair, paying the ordinary fees. Candidates to have attained sixteen years of age on application, to be of good moral character, and to pass such examinations as may be prescribed by the Trustees in the ordinary branches of an English education and the elementary principles of Chemistry. The Bursaries to be liable to forfeiture on the Bursars failing to exhibit approved progress under the Professor of the Chair, or being guilty of conduct, in the opinion of the Trustees, unworthy of their position. The Bursaries are only given to those whose means are limited, and who intend following some branch of Manufacturing Chemistry.

CHEMICAL LECTURES, CLASSES, AND LABORATORY INSTRUCTION.

City of London College, 52, Leadenhall Street, E.C. —Chemical Lecturer—Thomas Eltoft, F.C.S., F.I.C. The courses of lectures are as follows:—

Elementary Electricity	Mondays,	6.30—7.30.
Elementary Chemistry	,,	7.30—8.30.
Elementary Organic Lectures..	Tuesdays,	6.30—7.30.
Advanced and Elementary Organic Practical Class	,,	7.30—9.0.
Advanced Organic Chemistry, Lectures	,,	9.0—10.0.
Elementary Inorganic Chemistry, Practical Class	Wednesdays,	8.15—9.45.
Advanced Inorganic Chemistry, Lecture	Thursdays,	7.15—8.15.
Do. Practical Class	,,	8.15—10.0.

The above classes will cover all the work required for Matriculation, College of Surgeons, First B.Sc., Preliminary Scientific, and Second B.Sc. (University of London). The above lectures will also prepare for the South Kensington Examinations. Fees: Lectures for the Session 15s.; Practical Chemistry, 15s. All Students are expected to sit at the May Examination. Lectures commence on Sept. 27th at 7.30, and are continued on every Monday throughout the Session.

Crystal Palace Company's School of Art, Science, and Literature. School of Practical Engineering. *Principal*—Mr. J. W. Wilson, Assoc. Inst. C.E.—This school was established with the purpose of affording to Students of Civil or of Mechanical Engineering the advantage of thorough practical instruction in the rudiments of either profession, and in the manipulation of materials. The leading object is to prepare Students, by systematic practical instruction, for professional articles, so that on entering an Engineer's office or works the pupil may at once be useful to his Principal, and enabled to take advantage of the opportunities for learning open to him, because he has mastered the elementary details of the profession. The school is also available for Students already articled, who desire instruction either in the offices or shops. The Colonial Section is designed particularly for gentlemen who are going to the Colonies or abroad, as explorers or settlers. The object proposed is to afford them so much practical knowledge of scientific and mechanical work and expedients as shall enable them best to utilise the means at their disposal, especially when entirely dependent on their own resources.

Special Courses of Lectures are delivered by independent Lecturers not on the Staff of the School, and are an addendum to the curriculum. These Special Courses include a Course of Six Lectures on the Physiography of the Principal Districts of the World; a Course of Six Lectures on the Chemistry of Manufactures and Mines; and a Course of Six Lectures on the Mechanics of Practical Mining.

Ladies' Division.—The School was established to utilise the valuable Courts and Collections of the Crystal Palace for the purposes of instruction in Art, Science, &c., so that education of the highest class might be afforded on reasonable terms under most advantageous conditions. The system of tuition is, for some subjects, in the manner of private tutorial instruction by the best masters, but other subjects are taught on the University method, in accordance with the regulations laid down by the Syndicate of the University of Cambridge, by whom some of the lectures and classes are conducted. A student may take lessons in one or several studies at option. The School is a centre for both the University of Oxford and the University of Cambridge Local Examinations, the Oxford Examination for Women, and for the Cambridge Higher Local Examination. The following examinations will be holden in the Ladies' Division during 1880-81:—Cambridge Local, December, 1880; Oxford Local and Oxford Examination, for Women, May, 1881; Cambridge Higher Examination, June 1881. The session opens on October 1.

The Courses of Science Lectures during the ensuing term will include one on Physical and one on National Geography, on Mondays, by Prof. H. G. Seeley, F.R.S., and another on Botany, on Weduesdays, by Mr. Henry N. Read, M.A., of St. John's College, Cambridge. Fees: Twelve Lectures and Classes, £1 1s. One Lecture, 1s.

Berners College of Chemistry and the Experimental Sciences, 44, Berners Street, W.—Prof. E. V. Gardner, F.A.S., M.S.A. The Laboratory is open morning and evening throughout the year.

Birkbeck Literary and Scientific Institution, Southampton Buildings, Chancery Lane.—Inorganic Chemistry. Lectures:—Elementary, Tuesdays, 8.30 to 9.30; Advanced, Saturdays, 7 to 8. Practice:—Elementary, Saturdays, 4 to 6; Advanced, Saturdays, 8 to 10. Teacher, Geo. Chaloner, F.C.S. Organic Chemistry:—Course of Thirty Lectures will be given on Tuesday evenings, at 7 o'clock, by Mr. H. Chapman Jones, F.C.S., commencing on October 5th. Practical Organic Chemistry:—This Class will meet in the Laboratory of the Institution, under Mr. Chapman Jones's direction, on Saturdays from 4 to 6 and from 8 to 10 p.m.

New Central School of Chemistry and Pharmacy, 173, Marylebone Road, London.—Mr. A. P. Luff, F.I.C., F.C.S., and Mr. J. Woodland, F.C.S., M.P.S.

Onslow College of Science, 183, Kings Road, Chelsea, S.W.—Special Evening Classes in Inorganic and Organic Chemistry, &c. The Chemical Laboratory is open on Friday Evenings from 8 to 10 p.m. and on Saturday from 2.30 to 10 p.m.

Royal Veterinary College, Camden Town.—Professor of Chemistry, Mr. R. V. Tuson.

School of Pharmacy of the Pharmaceutical Society of Great Britain, 17, Bloomsbury Square.—The school opens on Friday, the 1st of October. Lectures on Chemistry and Pharmacy, by Professor Redwood, on Monday, Tuesday, and Wednesday mornings, at 9 a.m. The Laboratories for Practical Instruction in Chemistry as applied to Pharmacy, &c., under the direction of Prof. Attfield, will be open daily at 10 a.m. throughout the Session. They are fitted up with every convenience for the study of the principles of Chemistry by personal experiment, synthetical as well as analytical. They are specially designed for the student of Pharmacy, but are also well adapted for the acquirements of a knowledge of Chemistry in its application to Medicine, Manufactures, Analysis, or Original Research. There is no general class for simultaneous instruction, each student following an independent course of study always determined by his previous knowledge; pupils can therefore enter for any period at any date. Fees, One Course, £3 3s.; an entire Session—Two Courses, £4 4s.; Perpetual Admission, £5 5s. *Council Prizes.*—At the end of each of the five months' Courses of Lectures on Chemistry and Pharmacy, and Botany and Materia Medica, a Bronze Medal and Certificates of Merit, and at the close of the Session (ten months) a Silver Medal and Certificates of Honour and Merit, are offered for competition by the Council. In the Class of Practical Chemistry, the Silver Medal, two Bronze Medals, and Certificates of Honour and Merit, offered by the Council, are competed for at the end of the Session only.

South London School of Chemistry, 325, Kennington Road.—Dr. John Muter, F.C.S. Daily, at 10 a.m. Sixty Lectures on Theoretical Chemistry, and Junior and Senior Course of Practical Chemistry.

St. Thomas Charterhouse School of Science, Goswell Road, E.C.—Chemical Lecturer—Thomas Eltoft, F.C.S., F.I.C. A course of 40 Lectures on each of the following subjects will be given:—Elementary Inorganic Chemistry, Advanced Inorganic Chemistry, Elementary Organic Chemistry, Advanced Organic Chemistry. After each lecture the student will do practical work. The Chemical Laboratory is capable of accommodating 60 students. It is thoroughly ventilated, and has been fitted up at a large cost. All apparatus and chemicals are found. The work is so arranged as to prepare students for any examination in chemistry. The Lectures will commence on September 28th, at 6.30 and 9.0 p.m. Lectures are held on Tuesdays, Wednesdays, and Thursdays. Fees: Lectures and Practical Work, 25s. For further particulars apply to the Secretary, Mr. Chas. Smith.

The Westminster College of Chemistry and Pharmacy, Lambeth Road, S.E.—Messrs. Wills and Wootton. Daily, at 9.30 a.m. Theoretical and Practical Chemistry.

CHEMICAL LECTURES AT LONDON HOSPITALS.

Chemical Schools and Colleges.	Winter Session.						Summer Session.					
	Lectures on Chemistry.	Days and Hours.	Fees.				Lectures on Chemistry.	Days and Hours.	Fees.			
			One Course.		Perpetual.				One Course.		Perpetual.	
			£	s.	£	s.			£	s.	£	s.
St. Bartholomew's Hosp. and College	Dr. Russell, F.R.S.	M. W. F., 9	6	16/6	9	9	Dr. Russell	M. Tu. F., 11 [to 1	3	3	3	3
Charing Cross Hospital and College	Mr. Heaton	M. W. Th., 11	5	5			Mr. Heaton	M. W.	4	4		
St. George's Hospital ..	Mr. Donkin	Tu. Th. S., 11½	7	7	8	18	Mr. Donkin	Daily at 10	4	4		
Guy's Hospital	Dr. Debus, F.R.S., and Dr. Stevenson	Tu. Th. S., 11	7	7			Dr. Debus	M. W. F., 10 to 1	7	7		
King's College and Hosp.	Mr. Bloxam, F.C.S., Mr. Thomson, and Mr. Johnson	M. W. Th., 10¼	8	8	11	11	Mr. Bloxam and Mr. Thomson & Mr. Johnson	M. W. Th., 10¼	6	6	8	8
London Hospital	Dr. Tidy	M. W. F., 10	7	7			Dr. Tidy	M. Th. S., 9	5	5		
St. Mary's Hospital ..	Dr. Wright	M. Th. 10, W. S., 9	6	16/6			Dr. Wright	W. F. S.	4	4		
Middlesex Hospital ..	Mr. W. Foster	M.W.Th. Fr., 3	6	6	8	8	Mr. W. Foster	M. W. F., 3	3	3		
St. Thomas's Hosp. & Schl.	Dr. Bernays	Tu. Th, F., 10	7	7			Dr. Bernays	M. Th. F., 10	6	6		
University Col. & Hosp.	Dr. Williamson, F.R.S.,	Daily (ex. S.) 11	7	7	9	9	Dr. Williamson	Daily (ex. S.) 11	5	5		
Westminster Hospital ..	Dr. Dupré, F.R.S.	W. Th. F., 3	6	6			Dr. A. Dupré	M. W. F., 10	4	4		

Birmingham.—Midland Institute.—Mr. C. J. Woodward, B.Sc. Tuesday and Thursday, at 8 p.m.; Friday, at 7; and Saturday, at 3.

Birmingham.—Queen's College.—Mr. A. Bostock Hill, M.D.

Bristol Medical School.—Mr. T. Coomber, F.C.S.

Liverpool Royal Infirmary School of Medicine.—J. Campbell Brown, D.Sc. Lond., F.C.S.

School of Technical Chemistry, 7 and 9, Hackin's Hey, Liverpool.—Mr. A. Norman Tate.

College of Chemistry, Liverpool—Mr. S. H. Johnson, F.C.S.

Leeds School of Medicine.—Prof. T. E. Thorpe, Ph.D., F.R.S.

Leeds Mechanics' Institution.—Mr. G. Ward, F.C.S.

Manchester Mechanics' Institution.—Mr. W. A. Watts, M.A., and Mr. Yates. Evening Classes.

Queenwood College, near Stockbridge, Hants.—Mr. E. W. Prevost, Ph.D., F.C.S., F.R.S.E.

Salford Working Men's College, Evening Classes.—Teacher of Chemistry, Mr. G. H. Hurst. Lectures on Organic and Inorganic Chemistry and practical laboratory instruction.

Sheffield Borough Analysts' Laboratory, 1 and 3, Surrey Street.—Mr. A. H. Allen, F.C.S. Day and Evening Classes.

Sheffield School of Medicine.—Mr. A. H. Allen, F.C.S. A course of Lectures on Inorganic and Organic Chemistry is delivered by Mr. Alfred H. Allen during the Winter session. The Summer Course of Practical Chemistry is under the direction of Mr. A. H. Allen.

Aberdeen School of Science and Art Mechanics' Institution.—Mr. Thomas Jamieson, F.C.S.

Dundee Literary Institution Chemical and Physical Laboratory.—Lecturer on Chemistry, Mr. Frank W. Young, F.C.S., and Mr. John Thomson.

University of Edinburgh.—Prof. A. Crum Brown, F.R.S.E.

School of Medicine, Edinburgh.—Dr. Stevenson Macadam, F.R.S.E., Mr. Falconer King, Mr. Ivison Macadam, Mr. Drinkwater, and Mr. Buchanan.

School of Pharmacy and Chemistry, Edinburgh.—The instruction qualifies for graduation in Medicine and Science in the University of Edinburgh and other Examining Boards. Chemistry, Mr. Drinkwater, F.C.S., L.R.C.P.; Materia Medica and Pharmacy, Dr. Urquhart; Botany, Mr. D. M'Alpine, B.Sc. Lond. Day and Evening Classes.

Minto House Medical School, Chambers Street, Edinburgh.—Lectures and Classes.

Glasgow University.—Prof. J. Ferguson.

Glasgow Mechanics' Institution.—Mr. R. R. Tatlock, F.R.S.E., F.C.S.

Glasgow Veterinary College.—Mr. Stephen Cooke, F.C.S.

School of Chemistry, 138, Bath Street, Glasgow.—Dr. Wallace, Mr. Tatlock, and Dr. Clark. Day and Evening Classes.

Chemical Laboratory, 144, West Regent Street, Glasgow.—Dr. Milne. Day and Evening Classes.

Analytical Laboratory, 88, Hope Street, Glasgow.—Dr. A. T. Machattie, F.C.S. Day and Evening Classes.

Queen's College, Belfast.—Dr. Letts.

Queen's College, Cork.—Dr. Maxwell Simpson.

Queen's College, Galway.—Dr. T. H. Rowney.

Royal College of Surgeons in Ireland.—Dr. C. A. Cameron.

Dublin, Carmichael School.—Dr. C. R. C. Tichborne.

Dublin, Catholic University.—Mr. Campbell.

Dublin, Dr. Steevens's Hospital and Medical College.—Mr. McHugh.

Pyridic Bases.—M. Oechsner de Coninck.—On the distillation of cinchonine with 3 parts of caustic potassa the author obtains a lutidine, a collidine, and a parvoline, all of them novel.—*Comptes Rendus.*

ACTION OF LIGHT UPON THE SOLUBLE IODIDES,

WITH THE OUTLINES OF

A NEW METHOD IN ACTINOMETRY.*

By Dr. A. R. LEEDS.

IN the June number of the *Philosophical Magazine* for 1879 I published an article with the above title, and gave an abstract of three preceding papers dealing with various parts of the same subject, viz. :—"Upon the Titration of Hydrochloric Acid for Chlorine, and of Sulphuric and Nitric Acids for Hyponitric Acid" (1st and 2nd papers, *Proc. Am. Chem. Soc.*, vol. ii., 1878, and *Jour. Am. Chem. Soc.*, vol. i., 1879); also, "Upon the Influence of Light upon the Decomposition of Iodides" (*Jour. Am. Chem. Soc.*, vol. i., 1879). In the first of these papers I gave an account of the anomalous reaction which first attracted my attention to the subject, and led me to undertake a series of laborious quantitative determinations extended over an interval of three months. This was, that on titrating very dilute solutions of the acids with potassium iodide (1 c.c. acid to 1000 c.c. water) no change occurred immediately, but at the end of twelve hours an amount of iodine had been set free corresponding to 0·42 c.c. of a standard sodic hyposulphite solution in the case of the hydrochloric acid, to 0·28 c.c. with the sulphuric acid, and to 0·24 c.c. with the nitric acid. After decolorisation with the hyposulphite slow change again set in, and a series of titrations—beginning with October 27th, 1878, and extended to December 5th, 1878, eleven in all—was given, showing that a constant source of change was operating upon the mixtures of very dilute acids and iodide, the summations of the changes effected during the interval being, for the hydrochloric acid 1·23 c.c., for the sulphuric acid 5·17 c.c., and for the nitric acid 2·41 c.c. This source of constant change was thought to be the diffused light of the laboratory, and in the second paper the results of testing this hypothesis under a great variety of conditions and with many precautions as to purity of acids, iodides, &c., were detailed at length. In the third paper the various phenomena noted are brought into reconciliation by recognising that *oxygen was essential to the reaction*, no change whatsoever occurring either in the dark or in the strongest sunlight during several days' exposure, in case the presence of oxygen were rigorously excluded. This observation likewise explained the real difficulty at the root of the acrimonius controversy between Schönbein and Fischer, Houzeau, and Sauvage,† as to whether or no perfectly pure potassium iodide in dilute solution is decomposed by free sulphuric acid.‡ The answer to this question, as determined by the author from the experiments quoted, was that either in light or darkness free acid will decompose potassium iodide, if oxygen is present, even when the dilution is carried to the one-four-thousandth part; but in case oxygen is absent, solutions of potassium iodide with excess of free acid, and both solutions of considerable strength, may be kept for days in the dark or in strong sunlight without a liberation of iodine.

Furthermore, all the cases of soluble iodides experimented upon, and the acids—organic acids as well—were shown to undergo a change represented by the equation—

$$4MI+4HA+O_2=4MA+2H_2O+2I_2,$$

where M indicates the monovalent basic, and A the monobasic acid radical.

In the earlier experiments (2nd paper), before the indispensable co-operation of oxygen in the decomposition of the iodides in presence of free acid had been recognised, the law of change of the iodides in the dark appeared to be quite different from that operating when they were exposed to the light. In the former case the amount of decomposition of the iodide was approximately proportional to the degree of concentration; in the latter, the reverse appeared to be true. This curious anomaly was done away with by the next step in the investigation; for in the preliminary trials bottles of different forms and material had been used in holding the solutions. But it was evident that not only would the varieties in shape greatly alter the relative surfaces of exposure to the solar ray, but differences in tint of the glass—yellowish, white, pink, &c.—strongly affect the percentage of the transmitted actinic component of the beam. For these reasons, comparison-tubes of uniform bore and dimensions (25 c.m. in length, 3 c.m. in diameter), such as the author has proposed in his colour comparator,* all made of the same thin perfectly colourless glass, were substituted. The anomalies now disappeared.

Before proceeding further, however, it was essential to determine to what extent the pure acids themselves underwent decomposition in the sunlight, and to this end 50 c.c. of nitric acid of sp. gr. 1·4, 50 c.c. of sulphuric acid of sp. gr. 1·84, and 50 c.c. of hydrochloric acid of sp. gr. 1·202, were hermetically sealed in bottles, and exposed at a south window from Dec. 10th, 1878, to Jan. 21st, 1879. "At the end of this period 1 c.c. of each was taken, diluted with 1 litre water, potassium iodide and starch-water added, and allowed to stand for twenty-four hours in darkness." They gave on titration—

1 c.c. HNO_3 = 14·80 c.c. $Na_2S_2O_3$
,, H_2SO_4 = none
,, HCl = none

This experiment was the first of a series which are given at length in the articles referred to, by which it was shown that the decomposition of soluble iodides in the presence of light and of free nitric acid, does not confirm to the same general law which operates in the case of acids not of themselves decomposable by sunlight: the equations which explain the different deportment of nitric acid were given in full, and the necessity of rejecting its employment in making actinometric measurements was emphatically pointed out.

"This investigation suggested a convenient method for studying the variations in the actinic force of the solar ray, which was to determine the amount of iodine set free by its action under identical conditions. In the following experiments, the first which were instituted in this direction, no attempt was made to determine absolute values. A series of comparison-tubes, each containing 1 c.c. H_2SO_4, 1 c.c. KI, and 5 c.c. starch-water, with the amounts of water specified, were exposed at times and during intervals given, upon a comparator, the frame of which was kept normal to the solar ray."

RELATIVE ACTION OF THE SUN'S RAYS (JAN. 22, 1879).

	20 c.c. H_2O. M.grm. I.	60 c.c. H_2O. M.grm.	100 c.c, H_2O. M.grm.
9 — 9·30 a.m.	0·24	0·27	0·38
9.30—10	0·22	0·29	0·37
10 —10.30	0·24	0·40	0·40
10.30—11	0·39	0·76	0·71
11 —11.30	0·67	1·25	1·29
11·30—12	0·67	1·25	1·46
12 —12.30 p.m.	0·55	1·25	1·54
12.30—1	0·60	1·50	1·73
1 —1·30	0·50	1·13	1·31
1·30—2	0·40	0·75	0·92

(clouded over)

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* Dr. Leeds's letter announcing the dispatch of the MS. of this article, was dated June 10, 1880, and was duly delivered in about twelve days from that time. The MS. itself was, however, delayed in the post, and after an extra double journey across the Atlantic, only arrived the first week in July. Further time has been consumed, owing to the necessity of sending a proof to the author.—*Ed.* C.N.

† *Jour. fur Prakt. Chem.*, 1845, xxxiv., p. 492, and *Comptes Rendus*, 1868, lxvii., pp. 663, 714, 1138.

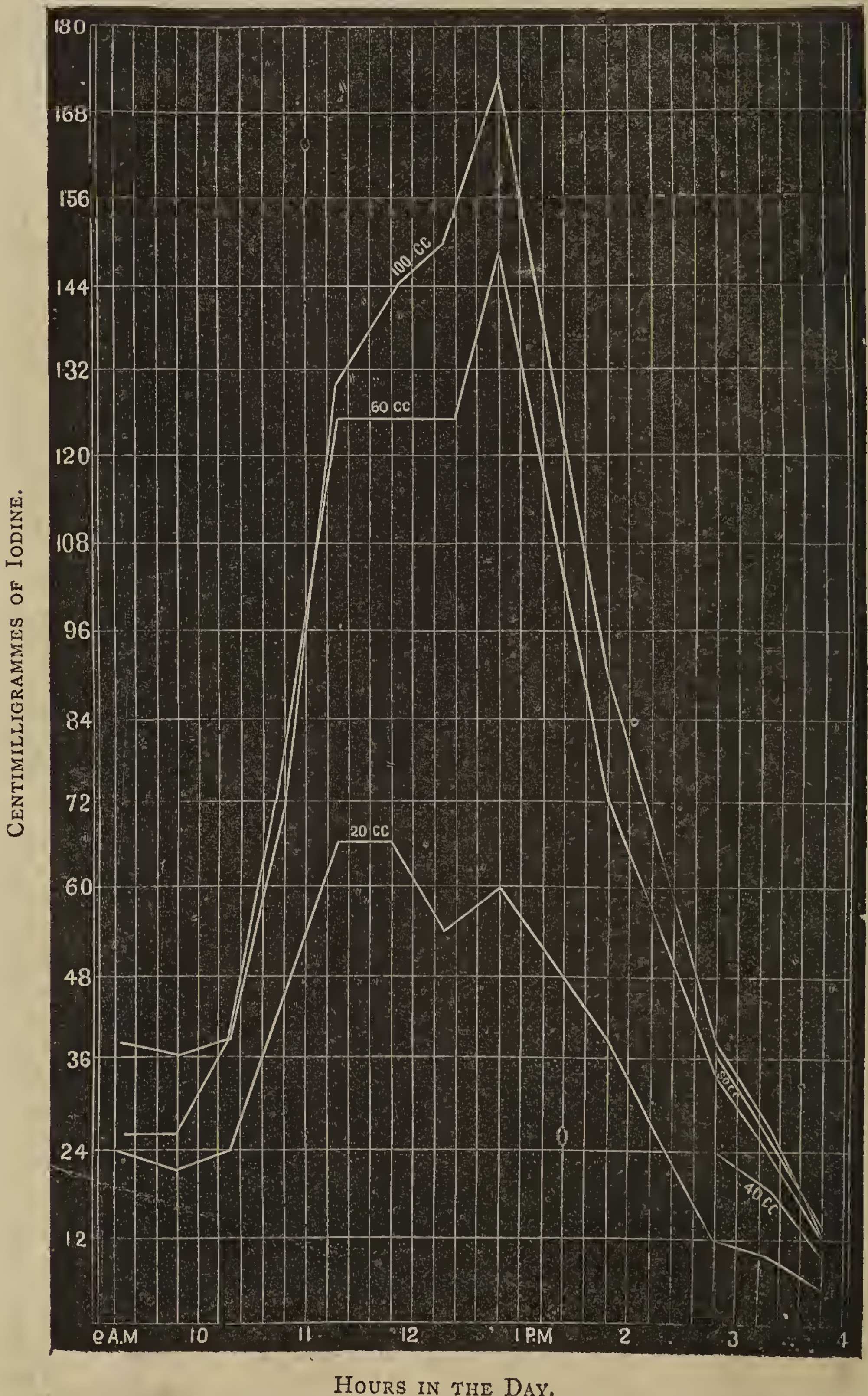
‡ See, also, Fischer, *Pogg. Ann.*, lxvi., p. 168; *Jour. Prakt. Chem.*, xxxiv., p. 186. Also, Dulk, *Jour. Prakt. Chem.*, xxxiv., p. 344, criticism on Schœnbein's paper, "Some Notes upon Potassium Iodide."

* CHEMICAL NEWS, vol. xxxvii., p. 229.

Continued (*Jan.* 24).

	20 c.c. H_2O.	40 c.c. H_2O.	60 c.c. H_2O.	80 c.c. H_2O.	100 c.c. H_2O.
2.30—3	0·125	0·24	0·33	0·38	0·40
3 —3·30	0·11	0·18	0·24	0·27	0·28
3·30—4	0·07	0·10	0·12	0·13	0·12

Fig. I.



"It will be seen on inspection of the accompanying diagram that the curve representing the amount of iodine set free in the 40 c.c. solution and the 80 c.c. solution was intermediate between the curves of 20 and 60, and 60 and 100 respectively. Moreover, the determination of the liberated iodine is so easily made that we possess in this method a convenient form of actinometry, lending itself readily to various practical applications. The chlorine-hydrogen method of Draper, which received at the hands of Bunsen and Roscoe the various instrumental refinements essential to accurate quantitative results, requires a corresponding complexity of apparatus and nicety of manipulation, and is therefore carried into execution with difficulty by the working chemist. The method just described needs little or no apparatus beyond that essential to any well-equipped laboratory."

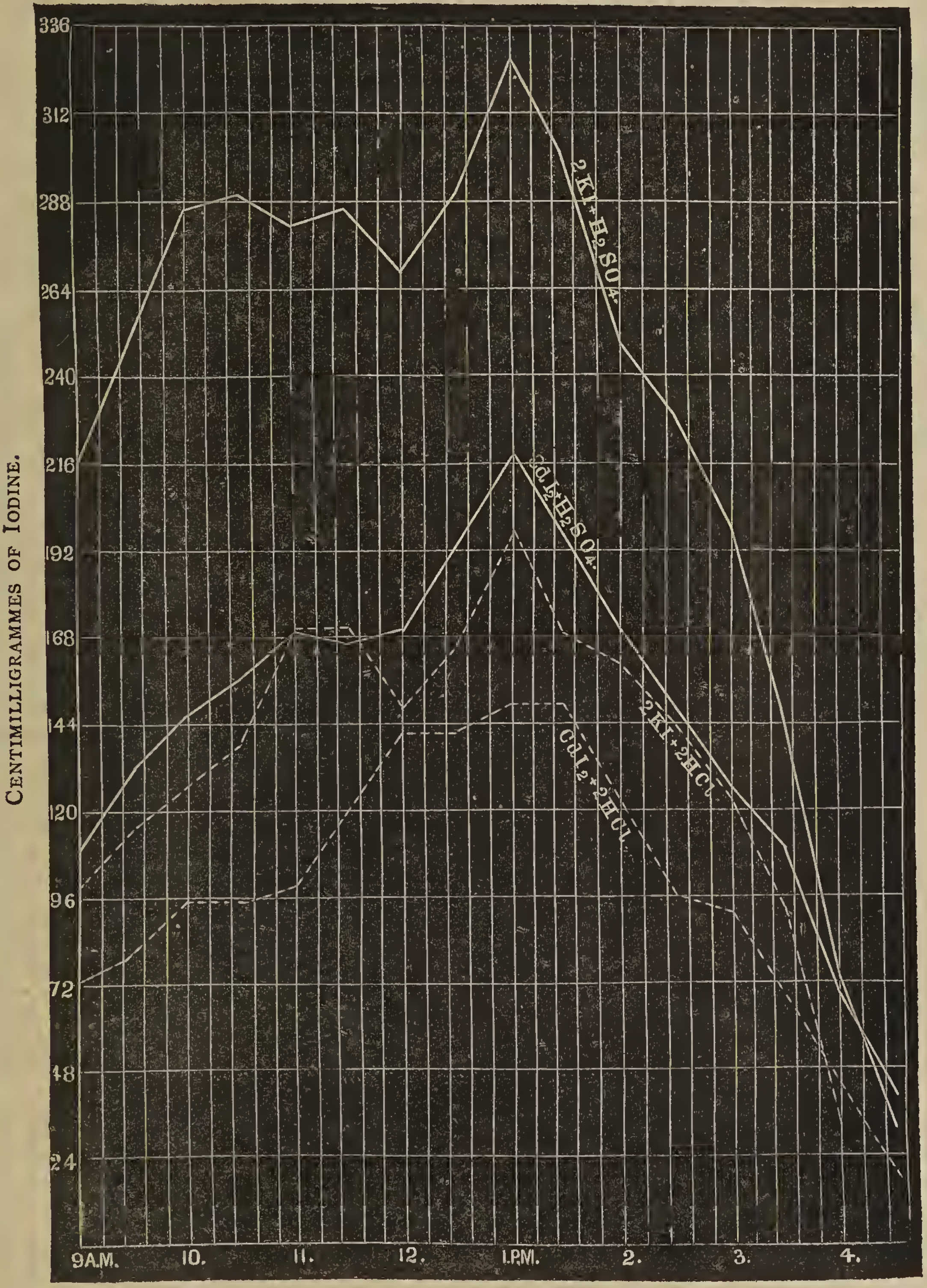
The next question investigated (3rd paper) was—"*To determine whether a change of base influenced the rate of decomposition in the sunlight, when the same acid was employed; or with the same base when different acids were used.*"

In solving this question, the intention was to employ

ACTINOMETRIC DETERMINATION, FEBRUARY 27, 1879.

Reagents.	A.M. 9 to 9.30.	9.30 to 10.	10 to 10.30.	10.30 to 11.	11 to 11.30.	11.30 to 12.	12 to 12.30.	P.M. 12.30 to 1.	1 to 1.30.	1.30 to 2.	2 to 2.30.	2.30 to 3.	3 to 3.30.	3.30 to 4.	4 to 4.30.	4.30 to 5.	Mns
1 c.c. H_2SO_4+1 c.c. KI ..	215	250	285	290	280	285	270	290	325	300	250	225	200	155	75	35	233
,, HCl ,, ,, ..	100	115	125	140	170	170	155	165	200	170	160	145	125	90	35	23	131
1 c.c. H_2SO_4+1 c.c. CdI_2	115	130	145	155	170	165	170	195	220	195	170	150	130	110	70	43	146
,, HCl ,, ,,	75	85	100	100	105	120	140	140	150	150	120	95	90	65	35	20	99
1 c.c. H_2SO_4+1 c.c. LiI ..	120	130	135	145	160	150	160	170	155	145	150	140	125	105	65	43	131
,, HCl ,, ,, ..	75	80	95	100	100	100	110	120	120	110	100	75	70	50	30	18	84
1 c.c. H_2SO_4+1 c.c. NH_4I	130	140	155	165	180	170	180	190	195	180	170	150	120	105	70	45	146
,, HCl ,, ,,	85	95	100	100	115	115	115	120	115	115	110	85	90	60	30	18	92

FIG. II.



FEBRUARY 27, 1879. HALF-HOURS.

chemically equivalent solutions of the acids, but by an error in the calculations the solutions actually employed in this and the subsequent parts of the work were of such strength that 1 c.c. of the sulphuric acid corresponded to 0·43 c.c. of the hydrochloric and to 0·504 c.c. of the nitric acid. The experiments were made and the woodcuts executed before the error was discovered, which accounts for the publication of the results as given in the articles alluded to, and from which only I shall quote in the present communication. But whilst I was unable at that time to undergo the expense of having new plates made, and published these somewhat imperfect results just as they stood, I repeated, in March, 1879, the experiments with truly equivalent solutions, and shall in a subsequent paper make them known, with appropriate graphic representations.

In all the earlier trials starch-water was used in making up the solutions in the comparison-tubes to the desired volume, since "*the supposition had been that its affinity for the liberated iodine, though feeble, had nevertheless been one of the agencies at work in determining the fact of a decomposition.*" But a series of trials is given (3rd paper) with the iodides of potassium, cadmium, and lithium, and with the three mineral acids, which showed that in every instance the amount of decomposition was greater when the addition of starch was omitted. The dense precipitate of starch iodide suspended in the tubes cut off to a large degree the action of the light upon the solutions. Moreover, the disturbing effect of such an opaque precipitate was so great that the striking quantitative relationships existing between the amounts of iodine liberated by the unequal chemism of different acid radicals for the same metallic base were not made manifest, until by doing away with the starch the reactions were rendered normal. The most important instance of this constant difference of chemism which as yet has been worked out is that between $\frac{1}{2}H_2SO_4$ and Cl in the presence of KI, CdI_2, NH_4I, and LiI solutions exposed to sunlight.

The progress which had been made in solving the question above italicised will be gathered from an examination of § VII. (3rd paper), "Upon the Actinometric Determination of Sunlight," which begins by stating that—"In order to compare the decomposition effected by the sunlight, after passing through various thicknesses of the earth's atmosphere, and the relative amounts of iodine set free under these circumstances, when different salts were employed, the determinations summarised in the accompanying table (p.149) were made. The day was unusually brilliant and unclouded, from beginning to close. The amounts of iodine are given in centimilligrms. The solutions were all made up to 100 c.c.

Unfortunately, as before stated, the solutions were not chemically equivalent, 1 c.c. of the H_2SO_4 corresponding to 0·43 c.c. HCl, and the potassium iodide solution being of 20 per cent, the other iodides of 10 per cent. But if we multiply the means of the number of centi-milligrms. of iodine, in each case set free in the presence of the sulphuric acid, by 0·43 (the number expressing the relative strength of the hydrochloric acid), and divide the products into the amounts of iodine liberated by the hydrochloric acid, we obtain the following ratios :—

H_2SO_4+KI $= 233 \times 0{\cdot}43 = 100$
HCl ,, $= 131 \times 1 = 131$, or 1 : 1·31
$H_2SO_4+CdI_2$ $= 146 \times 0{\cdot}43 = 64$
HCl ,, $= 99 \times 1 = 99$, or 1 : 1·55
H_2SO_4+LiI $= 131 \times 0{\cdot}43 = 56$
HCl ,, $= 84 \times 1 = 84$, or 1 : 1·50
$H_2SO_4+NH_4I$ $= 146 \times 0{\cdot}43 = 63$
HCl ,, $= 92 \times 1 = 92$, or 1 : 1·46
Mean ratio for 128 experiments, = 1 : 1·45

We have here a set of ratios obtained with 128 solutions exposed during the course of one day, under identical circumstances in each corresponding experiment of time of exposure, temperature, surface, intensity and quantity of light, size and nature of vessels, &c. The ratio 1 : 1·31, obtained with the potassium iodide solution, is retained, though it might without violence be thrown out, inasmuch as this solution contained 20 per cent of the iodide, the other three but 10 per cent of their respective iodides. We have reason, therefore, for bringing forward the hypothesis that the chemism of the Cl radical as compared with that of the $\frac{1}{2}SO_4$ radical, and as made manifest by the amount of metathetical decomposition taking place in sunlight in mixtures of their hydrogen salts with the iodides of certain metallic radicals, may be expressed by a certain ratio ; and without claiming anything further than the suggestion of some quantitative relationship not as yet fully apprehended, this ratio might be expressed as a law of specific actinic forces under the formula :—

The chemism of the chlorine radical is to that of the $\frac{1}{2}SO_4$ radical (measured by the relative amounts of iodine liberated by each respectively in solutions of the metallic iodides exposed to sunlight), as 1·45 : 1. This formula holds good, as was shown in the unpublished actinometric determinations made March 26, 1879, for rigorously chemically equivalent solutions of the acids. But the ratio requires change when the actinic power emanates from a different source,—the magnesium lamp, for example. This will be forcibly shown farther on. A similar law appears to be true of the amounts of iodine set free from iodides of different metals, when exposed to the action of sunlight in presence of the same acid, these amounts standing in definite ratios one to another.

Before leaving this diagram, I wish to point out the remarkable depressson in the $2KI+H_2SO_4$ curve and the $CdI_2+H_2SO_4$ curve between the hours of 11 a.m. and 1 p.m. As soon as this tendency was noted I naturally anticipated the passage of some visible mist or haze across the sun. Such, however, was not the case, the sky appearing just as brilliant and blue during this interval as before and after. I deemed it not improbable that this absorption of the actinic component of the solar ray was due to the passage of some mass of aqueous vapour, or some other alteration in the physical or chemical constitution of an interjected atmospheric layer, which, though invisible to the sense of sight directly, had been written down in the sensitive chemical fluid by the blurred tracing of the actinic rays. This, indeed, was an animating thought—that we possess in the undulations of the solar beam, which are too rapid to affect vision, a delicate probe to reach down through various parts and thicknesses of the earth's atmosphere, and to tell what alterations of constituents may be taking place there. To test this idea, a set of experiments was undertaken upon the absorbing action of vapours, gases, &c., upon the chemical component of the solar, electric, and magnesium light under known conditions in the laboratory, prior to their extension to an actino-chemical analysis of the atmosphere. The circumstances narrated below will cause the publication of the results, though in a somewhat incomplete state, at once.

§ IX. (3rd paper) was devoted to the "Influence of Heat upon the Decomposition of the Iodides." Three series of trials were instituted in this direction. The first was made before the *haupt role* played by oxygen in these decompositions was recognised. It consisted in noting the intervals through which the same amounts of potassium iodide, sulphuric acid, and starch liquor, diluted with 20 c.c. 60 c.c., and 100 c.c. water, ran through a scale of colours very faint rose, faint rose, rose, violet, and blue, as the temperature rose from 30° to 95°. On continuing the heating, the colours disappeared in inverse order, due to the gradual conversion of the indicator into dextrin under the influence of the dilute acid.

The second series of trials was upon the "Effect of Heat in Closed Flasks." Of course the use of starch was abandoned in these and subsequent trials. The final result obtained, to quote from the original paper, was that —"In other words, when potassium iodide was diluted 600 times, and heated nearly to the boiling-point (93°) in a closed flask for six hours, the amount of iodine set free was so small that it could not be estimated, and when diluted 1200 times no change whatever took place."

The third series was upon the "Effect of Heat in Flasks entirely Deprived of Air." The previous displacement of the air held in solution in the liquids was effected by a long-continued current of carbonic anhydride. When this was done the solutions of potassium iodide, both with sulphuric and hydrochloric acid, were heated for two hours to a temperature five degrees above the boiling-point of water without any liberation of iodine whatsoever. Similar acidified solutions, heated to the same temperature in contact with air, turned *yellow immediately*.

IV. (3rd paper) was upon the "Decompositions Effected by the Electric Light. The experiments were performed by arranging the solutions in comparison tubes, so as to form the sides of a cylinder with the focus of the electric light at the geometric centre. The electric arc was taken between carbon-points, and was produced by electricity generated with a dynamo-electric machine run by steam-power, its illuminating capacity varying during the course of the experiment, according to photometric measurements made by Prof. Morton and Mr. Beckmayer, between 7000 and 7500 candles." Reducing the observations in the same manner as was done with the actinometric determination of sunlight, we have, the results being given in m.grms. of iodine :—

ACTINOMETRIC MEASUREMENT OF ELECTRIC LIGHT.

	15 mins.	20 mins.	7½ mins.	Mean.	Ratios.
1 c.c. H_2,SO_4+1 c.c. KI—	1·65	3·90	3·00	2·85×0·43=1·22	
1 c.c. HCl+1 c.c. KI—	1·05	3·40	2·00	2·15×1 =2·15	1 : 1·76
1 c.c. H_2SO_4+1 c.c. CdI_2—	1·80	3·15	2·25	2·40×0·43=1·03	
1 c.c. HCl+1 c.c. CdI_2—	1·25	2·50	1·35	2·02×1 =2·02	1 : 1·96

The same remarks are true of the chemical decompositions of the iodides effected by the electric, as when the decomposition resulted from the sunlight. But the amounts of iodine liberated in each trial were relatively several times greater, and the coefficient of chemical energy of the chlorine radical 1·86, instead of 1·46. This exaltation of the relative chemical energy of the chlorine under the influence of the electric light will be still more strikingly exhibited when the results obtained with rigorously equivalent solutions are published.

V. (3rd paper) was devoted to the "Comparison of Effects of Sunlight and Electric Light after passing through Absorbing Media." The experiments were performed as usual, except that the comparison-tubes were surrounded by a layer, 3 c.m. in thickness, of the absorbing medium—ammoniacal solution of copper for the blue, neutral potassium chromate for the yellow, and fuchsine for the red. They were made of such strength that the outlines of an object could be seen through them with equal distinctness, though in each case feebly. Reducing the observations as before, we have :—

EFFECT OF ABSORBING MEDIA:

	Sunlight (1 hour).	Ratios.	Electric Light (1 hr.)	Ratios.
Blue : 1 c.c. H_2SO_4—	2·38×0·43=1·02	1	6·00×0·43=2·58	1
1 c.cc. HCl—	2·22×1 =2·22	2·2	4·50×1 =4·50	1·7
Yellow : 1 c.c. H_2SO_4—	0·67×0·43=0·33	1	0·50×0·43=0·22	1
1 c.c. HCl—	0·45×1 =0·45	1·4	0·45×1 =0·45	2
Red : 1 c.c. H_2SO_4—	1·53×0·43=0·66	1	.4·95×0·43=2·15	1
1 c.c. HCl—	0·83×1 =0·83	1·3	3·00×1 =3·00	1·5

The fact most striking in this tabular view is, that while the greater strength of the actinic component of the electric beam is very manifest, the yellow absorbing medium absorbed 85 per cent of the actinic energy, which was transmitted through the blue solution, and 83 per cent of that which passed through the red. Of the actinic energy of the sunlight 32 per cent of that which passed through the blue was absorbed by the yellow, and 50 per cent of that going through the red. But the actinic energy of that part of the *sun's* actinism, which passed through the fuchsine, was but one-third of that of the *electric light*. I have said actinic energy rather than actinic rays, because the absorption-spectra for the invisible part of the spectra of the sun and electric light possessed by these substances, has, so far as I know, not been mapped. To quote from the original—"Better to understand what occurs in this instance, a study of the absorption-spectra (chemical) of the media and of the various iodides in the several portions of the sun and electric light, has been entered upon (February, 1879).

VI. (3rd paper) was upon the "Decomposition Effected by the Magnesium Light." Reducing the observations (one set obtained by an hour's exposure) to the same standard as before, we have :—

	M.grm. I.	Ratio.
1 c.c. H_2SO_4+1 c.c. KI	= 0·050 × 0·43=0·0215	
" HCl " "	= 0·075 × 1 =0·075	1 : 3·5
" H_2SO_4+1 c.c. CdI_2	= 0·035 × 0·43=0·015	
" HCl " "	= 0·050 × 1 =0·050	1 : 3·3
" H_2SO_4+1 c.c. LiI	= 0·040 × 0·43=0·017	
" HCl " "	= 0·050 × 1 =0·051	1 : 3
" H_2SO_4 + NH_4I	= 0·060 × 0·43=0·026	
" HCl "	= 0·070 × 1 =0·070	1 : 3

The results were very surprising for their smallness, because the experiments were arranged in the same way as the electric light, the focus of the lamp being at the geometric centre of a cylinder, formed by the comparison-tubes, the cylinder being of 6 ins. radius. For while to the eye the white light of the burning magnesium was painful, the chemical decomposition effected by it was but the $\frac{1}{275}$th part of that effected by the electric light during the same interval, and but $\frac{1}{72}$nd part of that effected by the sun. The coefficient of comparative chemical energy exhibited by the chlorine atom under these circumstances is 3·2, as against 1·86 in case of the electric light, and 1·45 in that of the sun.

VIII. (3rd paper) was devoted to the "Comparison of Actinic Intensities for Equal Times." Reducing these observations as before, the amounts of liberated iodine being given in m.grms., we have :—(See Table on next page).

These phenomena are graphically represented in the accompanying diagram, the observed results being taken for abscissæ and ordinates, and the magnesium ordinates multiplied by 10 to bring its curve up into the field of view.

SUMMARY.

I. The part played by oxygen is the essential fact in the decomposition of soluble iodides in acidified solutions, no decomposition taking place either in darkness or in the strongest sunlight, in case oxygen is absent. The principal exception to the rule is when nitric acid is employed.

II. In case oxygen is present, the reaction takes place according to the equation—

$$4MI+4HA+O_2=4MA+2H_2O+2I_2,$$

where M stands for any monovalent basic radical, and A a monobasic acid radical. This is true of the solutions both in darkness and in light.

III. In case sufficient oxygen is present to permit of a normal reaction, the amount of iodine liberated in the darkness is strictly proportional to the degree of concentration up to the limit to which the above equation is true. In the light the same law holds good; the surface of exposure and the quantity and intensity of the light remaining the same, the degree of concentration the variable.

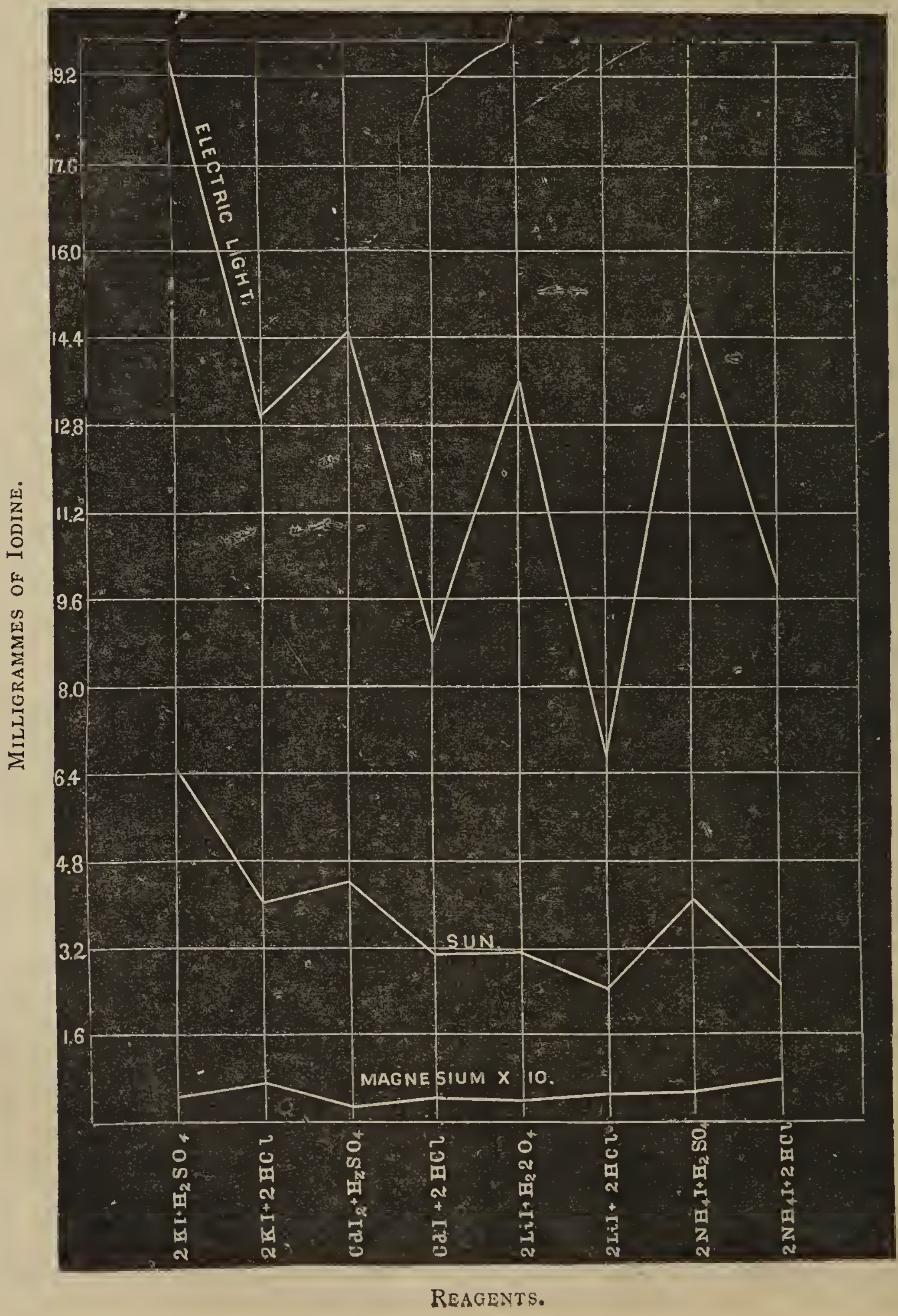
IV. In case sufficient oxygen is present and the degree of concentration is fixed, the amount of iodine liberated increases in the same ratio as the time of exposure to the light. There is likewise much reason for supposing that it stands in a similar numerical relation to the number and intensity of the chemical rays falling upon the solution.

V. With a constant actinic energy and with equivalent solutions of acids, the amounts of iodine liberated differ

Decomposition during Equal Times (1 hour).

Reagents.	Electric Light.		Sun.		Magnesium.		Ratios.
	Observed.	Reduced.	Observed.	Reduced.	Observed.	Reduced.	
H_2SO_4+KI	19·5	12·38	6·5	2·3	0·05	0·022	560 : 100 : 1
HCl ,, ...	13·0	13·00	4·0	4·0	0·075	0·075	173 : 53 : 1
$H_2SO_4+CdI_2$	14·6	6·28	4·4	1·9	0·035	0·015	420 : 126 : 1
HCl ,, ...	8·8	8·8	3·0	3·0	0·05	0·05	176 : 60 : 1
H_2SO_4+LiI	13·7	5·99	3·1	1·3	0·04	0·017	350 : 83 : 1
HCl ,, ...	6·5	6·5	2·4	2·4	0·05	0·05	130 : 48 : 1
$H_2SO_4+NH_4I$	15·0	6·45	3·9	1·7	0·06	0·026	248 : 65 : 1
HCl ,, ...	9·8	9·8	2·3	2·3	0·07	0·07	140 : 33 : 1
					Means of H_2SO_4		395 : 94 : 1
					,, ,, HCl		155 : 50 : 1
					,, ,, all		275 : 72 : 1

Fig. III.



by constant quantities, which appear to be coefficients of comparative chemical energy of the acid radicals under these conditions. A similar law appears to apply to the various basic radicals of the iodides in the presence of the same acid.

VI. In the absence of oxygen no decomposition takes place, either in light or darkness, nor in dilute nor quite strong solutions, when the solutions are *heated* for hours to a temperature 5° above the boiling-point of water. In the presence of oxygen instantaneous and abundant decomposition sets in at this temperature and correspondingly at lower temperatures.

Applications of the Iodo-Acid Method.—1. To determine the amount of chemical work done by the sun's rays while passing through various layers of the earth's atmosphere, either during each hour in the same day at any given place, or at different places for each day and season of the year. Curves representing such actinometric measurements at Hoboken for January 22 and 24, 1879, and February 27, 1879, are given.

2. To determine the absorptive coefficients for the chemical rays of the sun (or other powerful actinic light) of various gases, vapours, &c., under known conditions in the laboratory, and then apply the knowledge thus obtained to an actino-chemical analysis of the atmosphere.

3. To map the actinic spectra of the sun, electric, magnesium, and other powerfully actinic illuminants, and the absorption-bands for the invisible rays, when the light from these sources passes through solids, mineral and organic compounds in dissolved or liquid form, and vaporous or gaseous bodies.

4. To determine the comparative coefficients of combining and decomposing power of elements in compounds subject to metathesis by actinic forces, and to find these actino-equivalents for invisible rays of various refrangibility.

After the extended account published in the June number of the *Philosophical Magazine* for 1879, and sent about the same time to different gentlemen in Europe, I was very much surprised when I saw an article published in the Chemical News, vol. xli., p. 211, being a paper read before the Royal Society, April 29, 1880, by Dr. R. Angus Smith, in which, beneath the title "Measurement of the Actinism of the Sun's Rays and of Daylight," he gives an account of a process, which in every point runs parallel with and coincident with my own, except in several important omissions. This, I think, will appear on consideration of the following resumé of Dr. Smith's work. He states:—"The fundamental fact is that when iodide of potassium in solution is treated with nitric acid, so small in quantity as to cause no change of colour in dull diffused light, a change takes place when the same mixture is brought into clear light; iodine is set free and the solution becomes yellow. The amount of iodine set free can be titrated with great exactness by the use of hyposulphite, as is well known. In these two facts lies the whole process, the first is the new part, the second makes the first quantitative, and it is of course part of the novelty."

I admit the novelty exhibited by Dr. Smith, in selecting, of all others, nitric as the acid forming part of the fundamental fact of the process as he states it. For it is true that in the 128 determinations of the iodine liberated from four iodides in the presence of sulphuric and hydrochloric acids and titrated by the use of standard (1 c.c. = 5 m.grms. I) sodic hyposulphite and 1-10th standard, these 128 determinations being all made in one day, February 27, 1879, I did not use nitric acid. And the reasons for its omissions are stated in many places (2nd and 3rd papers). For, while the reaction in dilute solution (Eq. 2, § X., 3rd paper) for sulphuric acid is—

$$4KI + 2H_2SO_4 + O_2 = 2K_2SO_4 + 2H_2O + 2I_2,$$

and for hydrochloric acid (Eq. 6)—

$$4KI + 4HCl + O_2 = 4KCl + 2H_2O + 2I_2,$$

that for nitric acid is not only the normal one of—

$$4KI + 4HNO_3 + O_2 = 4KNO_3 + 2H_2O + 2I_2 \text{ (Eq. 3)},$$

but the secondary ones, $2HNO_3 = N_2O_4 + H_2O + O$, of $2HNO_3$ (in sunlight) $= N_2O_3 + H_2O + O_2$ (Eq. 4)[*] and $2KI + 2HNO_3 + N_2O_3 = 2KNO_3 + H_2O_2 + N_2O_2 + I_2$ (Eq. 5). It had been abundantly demonstrated by many experiments (what was well known before) that nitric acid of itself, no iodides being present, spontaneously breaks up into one or more lower oxides of nitrogen and free oxygen in the sunlight, and generates in the body of the solution itself an abnormal supply of that oxygen, which, to my way of thinking, is the fundamental factor in these cases of actino-chemical change. Later on, Dr. Smith suspects that the nitric acid itself in dilute solution was subject to decomposition, and substitutes for his new and fundamental fact the use of sulphuric acid. I think Dr. Smith will grant that this is not new, inasmuch as he will find it stated at great length, *Phil. Mag.* for 1879, p. 396. Neither, however, is it fundamental, for an equivalent amount of hydrochloric acid possesses a greater coefficient of iodine-liberating power when placed in an actinic field.

Dr. Smith furthermore states:—"3. It is known that strong acid liberates iodine. Weak acid does so after a long time, but the process is hastened by light." This generalised statement may lead to confusion and error, if left unqualified by a precise idea of the conditions under which it is false, and those under which it is true. For when oxygen is excluded,[†] "decomposition of an acid solution of potassium iodide occurs only after the concentration has attained to some point between one-third and one-tenth the weight of the water employed."

The acidifying body referred to in the experiments quoted, is sulphuric acid, and a more precise limit of concentration is not given, because of the undue multiplication of trials thereby entailed. But decomposition ensues in this instance from the production of an entirely different set of conditions on the attainment of the limit, and the sulphuric acid splits up, thereby furnishing the essential oxygen by an endo-chemical change. Instead of—

$$4KI + 2H_2SO_4 + O_2 = 2K_2SO_4 + 2H_2O + 2I_2$$

(Eq. 2, §X., 3rd paper), the equation given for this case is $2KI + 2H_2SO_4 = K_2SO_4 + 2H_2O + SO_2 + I_2$ (Eq. 1).

With regard to the statement that weak acid liberates iodine after a long time, but the process is hastened by light, it appears to the author that herein is contained the fundamental proposition of Dr. Smith's labours, and that as such it demanded elaborate experimental verification. For, although in a qualified sense the proposition is true, yet the working out of the exact conditions under which it holds good constituted the preliminary and most laborious portion of the work performed by the author in the three papers previously referred to and in others following them. Moreover, if these facts and conditions be so well known and generally accepted that a method of actinometry might be founded upon them without independent proof, yet the evidence of their being commonly ignored is to be seen in the very frequent publication of erroneous observations and conclusions concerning processes in which soluble iodides are concerned. Let me instance again the bitter controversy between Schönbein and Fischer, Houzeau and Sauvage, as to the *fact* of a dilute potassium iodide solution being decomposed in the presence of very dilute sulphuric acid.[‡] More

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[*] Gay-Lussac states that the decomposition is into nitrous acid. He adds that nitric acid, whose density is as low as 1·3235, is not decomposed by light, except in the presence of a certain quantity of concentrated sulphuric acid, when even the most dilute acid undergoes change. *Ann. Chim. Phys.*, vol. i., p. 397, 1816.

[†] "Upon the Detection and Estimation of Nitrous Acid in Potable Waters, Acids, &c., with Potassium Iodide," by the Author, Chemical News, vol. xl., p. 61; *J. Amer. Chem. Soc.*, i., p. 136.

[‡] I would ask the gentlemen concerned, in the controversy alluded to in this reference, to refer as far back as 1840, to the article by Schönbein entitled "Researches on the Nature of the Odour given off by certain Chemical Reactions" (*Comptes Rendus*, x., p. 706; *Froriep. Notizen*, xix., p. 292; *Ann. Elec.*, vi., p. 108); also to his article upon the "Production of Ozone by Chemical Means," 1844 (*Arch. de l'Elec.*, iv., pp. 333 and 454). Let me ask them to read likewise the critique by Fischer (*Berl. Jahrb. f.*

striking still, the universal employment of an acidified solution of potassium iodide for titrating a currrent of air or oxygen, carrying with it ozone, or ozone and peroxide of hydrogen. It is but a few weeks ago since in certain experiments instituted to discover whether or no ozone is given off by the slow oxidation of phosphorus in moist air, the most abundant reaction for ozone was obtained when the current of ozonised air was heated to 200°, before its passage into an acidified solution of potassium iodide. But at a temperature of 200°, 97 per cent of the ozone present in ozonised air or oxygen, is *destroyed at once*, and the residue very speedily.[*]

If more evidence were needed to show that the spontaneous decomposition of a soluble iodide in presence of dilute acid and oxygen is a fact generally ignored, it is to be found in the tests recommended in "Fresenius's Qualitative Analysis" for the detection of free chlorine in hydrochloric acid, and of hyponitric in sulphuric acid: the latter "when diluted with twenty parts of water must not impart a blue tint to a solution of iodide of potassium mixed with starch paste."[*] "But if the trial be made in an atmosphere of carbonic acid, care being used to expel previously all traces of air in reagents and in the dilute acid solution, the test and estimation of percentage may be satisfactorily performed."[†] A like proof may be found in the customary mode of applying Trommsdorf's method for the detection and estimation of "Nitrous Acid in Potable Waters."[‡] When the zinc-iodide starch reagent and water under examination are left without previous expulsion of dissolved oxygen, the amount of iodine set free by spontaneous decomposition, even in the diffused light of a laboratory, may much exceed the total quantity of nitrous acid present. In truth, an amount admitting of quantitative determination may apparently be found when, as examined by the Griess test (meta-diamidobenzol), not a trace of nitrous acid is actually present.

Finally, examine in this connection the admirable memoir of Schöne "Upon the Quantitative Estimation of Hydrogen Peroxide."[||] For in one series of trials this investigator employs concentrated solutions of potassium iodide and hydrochloric acid to determine minute amounts of hydrogen peroxide dissolved in different quantities of water. In another series the solutions were boiled in contact with the air, and the amounts of hydrogen peroxide found, which in most trials exceeded the quantities taken, corrected by a figure obtained by boiling similar solutions, *but containing no hydrogen peroxide*, during an

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Wissensch. Kritik., 1844) upon Schönbein's pamphlet "Ueber die Erzengung des Ozons auf Chemischen Wege," followed as it was by a second attack from the *savan* of Breslau (*Schles. Gesell. Uebersicht.*, pp. 98 and 107; *Pog. Ann.*, lxvi., p. 163; *J. Pr. Chem.*, xxxv., p. 351), in which he advanced the hypothesis that the ozone discovered by Schönbein was probably a peroxide of hydrogen. We have seen how, thirty-five years later, this hypothesis was again brought forward by Mr. Kingzett, without reference to the battle of these illustrious teachers of a past generation, and without comment upon Schönbein's reply to Fischer's attack (*J. Pr. Chem.*, xxxiv., p. 492; *Pog. Ann.*, lxv., p. 190), Mr. Kingzett asserting that "*there is no known process of slow oxidation which has been established to produce ozone*," and that the substance formed in the aërial oxidation of phosphorus is peroxide of hydrogen. Whilst the controversy was raging in Germany between Schönbein and Fischer, it was opened in England by Prof. Williamson's "Researches on Ozone" (*Chem. Soc. Mem.*, ii., p. 395; *Liebig's Ann.*, liv., p. 127; *Phil. Mag.*, xxvii., p. 372; *Comptes Rendus*, March, 1845), in which Dr. Williamson states, as the result of elaborate experiments performed with exceeding care, that there were reasons for believing in the existence of two kinds of ozone,—one, the ozone given off in electrolysis, and which he regarded as a higher oxide of hydrogen, differing from the previously well-known peroxide, and the other formed by the action of phosphorus on moist air. I cannot regard the rejoinder of Schönbein (*Pog. Ann.*, lxvi., p. 292; *Phil. Mag.*, xxvii., p. 450)—in which he showed, 1st, that the chemical and physical properties of ozone are not the properties of peroxide of hydrogen; and 2nd, that whatever might be the true nature of ozone, the gaseous matter obtained in the electrolysis of water was in all respects identical with that formed by the action upon air of moist phosphorus—otherwise than as a masterpiece of cogent reasoning, and as an argument for *the identity of ozone from whatever source produced* even at the present day unsurpassed. His eminent English antagonist never replied, and the doctrines of Schönbein on this point passed into the text-books, and remained there without a dissenting voice, until—unsupported by any show of evidence that he had performed more precise experiments on his own part, or that he had in any particular demonstrated errors in the work of those whom he so attacked—Mr. Kingzett pronounced them false. Perhaps even more surprising, as certainly more novel, are the polemical methods adopted by Prof. McLeod. For, stepping down unasked of the two principals in the contest, into this particular arena of controversy, and without even the fine courtesy traditional to a free lance of previous salutation, by name or token of reference, Prof. McLeod proceeds forthwith to demolish his two antagonists, and makes them both to be in error. This too without invoking, by *as much as a single footnote*, the shades of the mighty contestants who took part in the much greater battle fought on these points, more than a quarter of a century before, to be spectators of the fray. The historical aspect of this important question is of great interest. In 1853, Baumert thought he had succeeded in proving the existence of two kinds of ozone; the one prepared by the electric discharge, which he regarded as allotropic oxygen; the other afforded by the electrolysis of acidulated water, to which he assigned the formula H_2O_3—teroxide of hydrogen. (Breslau, *Schles. Gesell. Verhandl. Uebersicht.*, 24; *Ann. de Chim. et Phys.*, xxxix., p. 477; *J. Pr. Chem.*, lix., p. 350; *J. de Pharm.*, xxiv., p. 381; *Phil. Mag.*, vi., p. 51; *Ann. der Chem. u. Pharm.*, lxxxviii., p. 221; *Pog. Ann.*, lxxxix., p. 38.) He passed the electrolytic oxygen, evolved in such a manner as to exclude the presence of hydrogen, through a very long sulphuric acid tube, and thence into an absorption apparatus containing *potassium iodide*, and provided with a proper sulphuric acid guard. In case the matter of ozone and oxygen were identical, the weight of oxygen equivalent to the weight of iodine set free by the ozone should have been equivalent to the total gain in weight by the absorption apparatus. But, according to the experiments, this weight was less, and the numbers apparently assigned to electrolytic ozone the formula H_2O_3. Three years later the conclusions of Baumert were overthrown by Andrews, in his classic research upon the "Constitution and Properties of Ozone." (*Phil. Trans.*, 1856, p. 1; *Royal Society Proc.*, vii., p. 475; *Ann. de Chim.*, xlvii, p. 181; *Journ. de Pharm.*, xxx., p. 3; *Ann. der Chem. und Pharm.*, xcvii., p. 371; *Pog. Ann*, xcviii., p. 435.) He demonstrated the fatal error in Baumert's experiments, in that the electrolytic ozone had contained a minute but determinable quantity of carbonic acid, which, unless exceeding precautions be taken, is always present in the evolved gas. Finding that the results obtained with a *neutral solution of potassium iodide* varied irrationally and uncontrollably, Andrews in despair abandoned the use of the *neutral*, and substituted a slightly *acidified* solution. The free hydrochloric acid prevented the fixation of any traces of carbonic anhydride by the liberated base of the iodide, the anomalies disappeared, and by very numerous experiments he succeeded in establishing that the weight of the substance which produced the decomposition (the active oxygen or ozone) was exactly equal to the weight of the iodine set free, and therefore no hydrogen as well could have been present. The fact that the foregoing, which is the main truth, was thus triumphantly established by Andrews has probably been the reason why the method he followed (of titrating ozone in an acidified solution of iodide) has been generally adopted and practised, even up to the present day, with, as we have previously seen, the commission of very serious error. For while Baumert failed in his reply (*Pog. Ann.*, xcix., p. 88) to invalidate the principal conclusion of Andrews, he nevertheless did succeed in experimentally proving that all of them were vitiated by a constant error. Baumert showed that when a stream of electrolytic ozone was passed through three absorption apparatus, the first and second containing a neutral, the third an acidified solution of potassium iodide, all the ozone underwent decomposition in the first, no iodine whatsoever was liberated in the second, but in the third—*the acidified solution*—an amount of iodine was set free equivalent to many times (from four to ten) that corresponding to the ozone reaction in the neutral solution. This last Baumert correctly ascribed to the decomposing action of oxygen upon an *acidified* solution of potassium iodide, and the curious fact remains that both the total increments in weight due to the fixation of absorbed ozone (and oxygen), and the amounts of oxygen as determined by titration equivalent to the iodine liberated, were both false in Andrews's experiments, and in excess of the absolute quantities of ozone operated upon. Yet inasmuch as these two weights—in the absence of an extraneous third body, like carbonic acid, which could be fixed by the titrating fluid—were necessarily equal, the constant error in Andrews' results did not excite general attention. Fortunately, as above remarked, the conclusions arrived at by Andrews were true, and the accompanying fallacy—concealed beneath the halo surrounding a great name—was perpetuated in the works of later experimenters, and has led many astray. Ten years later than the memoir referred to, the facts made known by Baumert, concerning the decomposition of an acidified iodide solution in the presence of oxygen, were re-discovered by Payen (*loc. cit.*), who extended his trials to the action of acetic, nitric, oxalic, and other acids in dilute conditions, upon solutions of potassium iodide in and out of contact with air, but without accompanying them with the beautifully quantitative methods of Baumert. While both experimenters noted the essential participation of oxygen in the change, they appeared to have overlooked the role of actinism.

Since writing the above I have found (CHEMICAL NEWS, xxxvi., p. 110, from *Gazz. Chim. Italiana*, vii., fasc. vi.) a note entitled "Action of Light and Carbonic Acid upon Aqueous Solutions of Iodide of Potassium, and on Ozonoscopic Paper," by. G. Peelagri. I am unable to judge, from this short note, to what extent the above results have been anticipated by the Italian experimenter, and I have no means of present access to the original paper.

[*] "Peroxide of Hydrogen and Ozone," CHEMICAL NEW

[†] "Fresenius's Qualitative Analysis," 6th ed. ,pp. and 3

[‡] CHEMICAL NEWS, vol. xli., p. 63; *Ibid.*, p. 62.

[||] *Zeitsch. für Anal. Chem.* vol. xviii. (1879), p. 1

equal interval. True values, it appears to me, might have been found in both series of trials had the experiments been performed with the exclusion of every trace of oxygen from the solutions. It is the more surprising, inasmuch as in the same article this most careful experimenter calls attention to the method of Gernez[*] for freeing solutions of oxygen by a current of an indifferent gas, and the experiments of Baümert,[†] Payen,[‡] and Lehmann[‖] upon the decomposition of hydriodic acid by oxygen.

Dr. Smith goes on to state—4. "Heat even to the boiling-point does not act so well as light (experiments being made in sealed tubes to prevent loss of iodine)." Unfortunately, Dr. Smith could not have arranged the conditions of his experiment in a manner better calculated to lead to false conclusions. For, in sealing up the tubes, he prevented the access of oxygen, the very element whose presence is essential to the occurrence of the reaction with dilute solutions, whether the experiment be performed in light or darkness, in the heat or cold. And the sum total of the iodine which Dr. Smith could have liberated under the conditions given, even had the tubes been kept at the boiling-heat or several degrees above (see results obtained at 105°, *supra*) for hours, would have been the small amount equivalent to the oxygen sealed up at the same time with the solutions in the tubes.

It is possible that, in the experiment performed by Dr. Smith, a small amount of the dilute nitric acid may have undergone decomposition by heating into hyponitric acid and oxygen. But this is extremely improbable, and there is reason for believing that the total amount of iodine set free in the experiment cited was the exact chemical equivalent of the oxygen of the air which was sealed up at the same time with the acidified solution in the tube. And, as a corollary to this proposition, had Dr. Smith taken the precaution to exclude every trace of free oxygen from the tube, even with the dilute nitric acid no change of the potassium iodide solution would have occurred on heating to the boiling-point, and on continuing the heating for hours.

The seventh point is: "The solution of iodide of potassium as hitherto obtained is subject to change. An old solution—that is, one nearly a month old—was found more sensitive than a new one in all cases tried." I find it difficult to understand the precise meaning of the epithet "sensitive" as applied in this connection, unless the sensitiveness be due to a decomposition of the nitric acid employed, and a corresponding liberation of iodine. Instead of being styled sensitive, the solution might with more propriety perhaps be termed spoiled, and as such unfit for employment in actinometry.

Dr. Smith states, in the ninth place, "The amount of allowance for temperature is not made out. It is not certain that any is required in the cases when weak acid is used. The weather has not allowed any combined action of great light and heat; but with heat and light in the rays from an electric light with a parabolic reflector the action was very rapid." I do not find in the experiments detailed by Dr. Smith upon the effects of temperature (see above) a basis for the above generalisation. Neither do I perceive the reason for this dependence upon the weather, when it might be deemed the essential pre-requisite of accurate experiment that the changes due to the two separate causes should be carefully discriminated, and the effects properly assignable to each determined by previous trials under conditions admitting of exactitude and control. Nor do I recognise a test of the latter character in the experiment performed in the concentrated beam from an electric light.

Finally, the manner in which the iodo-acid method is applied to solar actinometry appears irrational. For Dr. Smith proposes to expose this actinometer during successive hours of "sunshine and cloud alternately"—during times when the weather is "changeable," during intervals of "sun and showers," during weather which is "foggy, with a gleam of sunshine." In what way is the value of a meteorological observation enhanced by knowing that, during periods in which the sun may or may not have been shining, a few milligrammes more or less of iodine were set free in an acidified solution?

When, however, we call to mind the frequent experience in photography, that there are states of the atmosphere when, with a clear sky, the quality of the light does not appear to be favourable to the making of a successful picture; or the fact previously noted, that, no visible change being manifest in a brilliant blue sky, the curve of chemical intensity underwent a great depression; or note the immense amount of chemical work done as the horizontal ray passes through the myriad organisms and pestilential germs, miasms, vapours, and countless emanations from the earth's surface that admit of oxidation under solar influence,—then, I think, we become impressed with the large practical value of an easy and rapid method of actinometry. The actinic component of the solar ray has its units of work, as well as the thermal, and the day has come when, in the utilisation of available energy, it will repay us to economise the forces in the sunbeams at present wasted.

REPLY TO DR. LEEDS'S CRITICISM.

By R. ANGUS SMITH, F.R.S.

A preliminary notice of an enquiry madeby me was lately published in the *Proceedings of the Royal Society*: it was on a mode of measuring the actinism of the sun's rays and of daylight. It appears that nearly the whole purely chemical part of the investigation, and more besides, was made previously by Dr. Leeds, of Philadelphia, and I suppose I ought to have known of it. However, amongst the many things to be read, I overlooked it, and the best proof is that I sent my results to the Royal Society, where no man would knowingly send another man's work; and it was so little known here, that, like myself, no one recognised Dr. Leeds's work in it, so far as I know. The real reason may lie in Dr. Leeds not seeing fully the bearing of his work, and so not calling attention to the points that interested me: a quotation to follow explains this. When two men with good eyes go on the same road they see the same things: in scientific matters this is common enough, and no one is to blame if only the second and following give due credit to the first as soon as they know his precedence. When I look at Dr. Leeds's work, it shows a fulness and completeness which mine cannot claim, and I greatly admire his investigations. His aim was different from mine, but I think mine would easily have come out of his. He must not forget that I was seeking a practical result for a manufacturing country. At the same time I think that Dr. Leeds has taken unnecessary trouble to criticise my preliminary note, although of course it was necessary to assert his priority. As to the rest of the literature, I should never have thought of introducing it; I passed it over with an allusion. In a preliminary note one does not enter on history, and how much I would have said in the full memoir intended I cannot say. I sent only bare notes; these notes are bare to save time; longer memoirs are sometimes useful, but are less read. However, Dr. Leeds objects to my results, and therefore, I suppose, does not claim them wholly, although I think he fairly might. The enquiry is to my mind a very neat one; I may speak of it freely now, as I will readily call it by Dr. Leeds's name, at least so far as the decomposition goes, or altogether if he prefers, although he rejects the conclusion.

I had for some years been seeking a mode of measuring the light in a smoky town compared with that in the country around, and have talked on the subject, but never

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[*] *Comptes Rendus*, lxiii., p. 883; *Zeitsch. f. Chem.* [2], ii., 717.
[†] *Pog. Ann.*, xcix., p. 92.
[‡] *Comptes Rendus*, lxii., p. 254.
[‖] *Arch. Pharm.* [3], ii., p. 25; *Chem. Centr.*, 1873, p. 152.

found a method suitable. This one arose out of a discussion chiefly among photographers several years ago as to the decomposition of nitrates in sunlight. I thought of measuring it, but found nitrates unnecessary in association with the iodide. Still the real work is only from last winter. Dr. Leeds came by another way I suppose, but still he was before me, and I am sorry I did not know it, and yet he kindly sends me his papers. Well, if he sent this I must have overlooked it. Had I seen it I would have leapt at it as the very thing I so long *desired*. He does not seem to me to value his process sufficiently. But while I thus freely give all the observations to Dr. Leeds, it is still to be explained why neither I nor anyone I know of had found from his papers the value of the process. Indeed, he even now objects to this as a meteorological instrument, because he says—"In what way is the value of a meteorological observation enhanced by knowing that during periods in which the sun may or may not have been shining a few milligrammes more or less of iodine were set free in an acidified solution?"

Now this is the very thing which I pretend to see, and I suppose solves the difficulty. I do not intend to expose the solutions to sunshine and cloud alternately, as Dr. Leeds supposes; the alternations alluded to are those of the weather. We do not always notice every decomposition explained by writers when the result is not shown to have an important bearing. Still, I am surprised at what Dr. Leeds says on this, because when I now read the extracts from his papers it seems to me clear that he *was measuring the actinism of the daylight, and making important observations in meteorology*, and he is not doing himself justice. When the solutions are exposed all day the decomposition goes on, so far as I can make out, exactly according to the amount of sunlight. If a cloud comes the decomposition diminishes; if the cloud is very dark, or if there is a fog, requiring us to light gas, no decomposition appears. There is a haze line which I should be glad to measure, at which line no result is given.

Dr. Leeds brings in the ozone theories. I have wasted much time on ozone. In my paper alluded to I avoid carefully all theories, and Dr. Leeds will see, if he reads more carefully, that I am quite aware of the action of oxygen, and I may add that the amount of iodine freed in the heat used had no relation to the amount of oxygen.

The remark on this by Dr. Leeds seems to lead to wrong conclusions. Many thoughts passed through my mind before I decided to overleap in my notes a great desert of chemical enquiry. I not only learnt that oxygen was necessary, but my trials showed that all the oxygen could not be used for the purpose, and I consider that I found that ozone was not the agent. In fine, it led me to a region of speculation regarding which I prefer to be silent until I learn something; whether it be the silence of ignorance or not, it is not well to remark it, but ignorance it happens to be. I shall go on with my practical work, and I hope to persuade Dr. Leeds of the value of this process in meteorology, although obstructions unforeseen may take place. On this point, perhaps, I do not understand him well, although I have quoted correctly. I am applying it to the testing of the purity of water, as it surpasses in suitable cases the colorimetric methods. He may depend on full justice from me, and I certainly desire no more. And while I thus express myself, I cannot help adding that I am much pleased at once to recognise Dr. Leeds's prior claims, even although there are some hints in the article which might have been left out, and I hope he will be satisfied with me in carrying out the process for the practical purposes which I have before me. As my desire was to find the meteorological results to which I allude, whilst I searched for a method by others, so my wish now is to see the value of that fixed on. Dr. Leeds well knows that the examination of the air of town and country has been interesting to me all my life; but it requires one to leave the style of science and to enter on literature more than is usual, in order to explain the strange conjunction of lines of thought in man's mind, leading chemists as well as others to be deaf for a while to words spoken or written and to follow their own ways, to which they seem pleasantly guided by nature. A great man, Priestley, used to forget altogether his own enquiries, whilst he still retained his usual vigour, and in haste it happens that one equally great may be deprived of his due attention, but as soon as it is discovered, good feeling and common sense soon put the matter in its proper light.

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ADDRESS TO THE MATHEMATICAL AND PHYSICAL SECTION OF THE BRITISH ASSOCIATION.

By Prof. W. GRYLLS ADAMS, M.A., F.R.S.
President of the Section.

It has been said by a former President of this Section of the British Association that the President of a section ought to occupy your time, not by speaking of himself or his own feelings, but by a review "more or less extensive of those branches of science which form the proper business of this section." He may give a rapid sketch of the progress of mathematical science during the year, or he may select some one special subject, or he may take a middle course, neither so extensive as the first nor so limited as the second.

There are many branches of science which have always been regarded as properly belonging to our Section, and the range is already wide; but it is becoming more and more true every day that the sciences which are dealt with in other sections of the Association are becoming branches of physics, *i.e.*, are yielding results of vast importance when the methods and established principles of physics are applied to them. I wish to direct your attention to investigations which are being made in that fertile region for discovery, the "border land" between chemistry and physics, where we have to deal with the constitution of bodies, and where we are tempted to speculate on the existence of matter and on the nature of the forces by which the different parts of it are bound together, or become so transformed that all resemblance to their former state is lost. It is not long since the theory of exchanges became thoroughly recognised in the domain of radiant heat, and yet it is already recognised and accepted in the theory of chemical combination. Just as the molecules of a body which remains at a constant temperature are continuously giving up their heat motion to surrounding molecules, and getting back from them as much motion of the same kind in return, so in a chemical compound which does not appear to be undergoing change, the combining molecules are continuously giving up their chemical or combining motions to surrounding molecules, and receiving again from them as much combining motion in return. We may say that each molecule is, as far as we can see, constantly dancing in perfect time with a partner, and yet is continuously changing partners. When such an idea of chemical motion is accepted, we can the more easily understand that chemical combination means the alteration of chemical motion which arises from the introduction of a new element into the space already occupied, and the consequent change in the motion of the new compound as revealed to us in the spectroscope. We can also the more readily understand that in changing from the old to the new form or rate of motion, there may be a development of energy in the shape of heat motion which may escape or become dissipated wherever a means of escape presents itself. We know from the experiments of Joule and of M. Favre that as much heat is absorbed during the decomposition of an electrolyte as is given out again by the combination of the substances composing it.

We are making rapid strides towards the exact determination of those relations between the various modes of motion or forms of energy which were so ably shadowed forth, and their existence established long ago, by Sir William Grove in his Correlation of the Physical Forces, where, in stating the conclusion of his comparison of the mutual interchange of physical forces, he distinctly lays down the principles of energy in this statement. "Each force is definitely and equivalently convertible into any other; and where experiment does not give the full equivalent, it is because the initial force has been dissipated, not lost, by conversion into other unrecognised forces. The equivalent is the limit never practically reached."

The laws of Faraday, that (1) when a compound is electrolysed the mass of the substance decomposed is proportional to the quantity of electricity which has produced the change, and that (2) the same current decomposes equivalent quantities of different substances, *i.e.*, quantities of their elements in the ratio of their combining numbers, have given rise to several determinations of the relation between chemical affinity and electromotive force. In a paper lately communicated to the Physical Society, Dr. Wright has discussed these several determinations, and has given an account of a new determination by himself. The data at present extant shew that when 1 grm. of hydrogen unites with 7·99 grms. of oxygen there are about 34,100 units of heat given out, making the latent heat of dissociation of 1 grm. of water equal to 3797 units. The results obtained are compared with the heat given out by the combustion of hydrogen and oxygen, and the value of the mechanical equivalent of heat is deduced from these determinations.

The value of this mechanical equivalent obtained by Dr. Wright, which depends on the value of Clark's standard cell, and therefore depends on the value of the ohm, agrees fairly well with Joule's determination from the heat produced by an electric current in a wire, but is greater than Joule's value as obtained from his water-friction experiments. This may be accounted for by supposing an error in the value of the ohm or B.A. unit, making it too large by 1·5 or 2 per cent. Kohlrausch has also made comparisons of copies of the B.A. unit with standard coils, and comes to the conclusion that the B.A. unit is 1·96 per cent too large. On the other hand, Prof. Rowland, in America, has made a new determination, and finds that according to his calculations the B.A. unit is nearly 1 per cent too small. These differences in the values obtained by different methods clearly point to the necessity for one or more new determinations of the unit, and I would venture to suggest that a determination should be made under the authority of this Association, by a Committee appointed to carry out the work. And it is not sufficient that this determination should be made once for all, for there is reason to think that the resistance of standard coils alters with time, even when the material has been carefully selected. It has been found that coils o platinum silver which were correct copies of the standardf ohm have become so altered, and have their temperature coefficients so changed, that there are doubts as to the constancy of the standards themselves. Pieces of platinum silver alloy cut from the same rod have been found to have different temperature coefficients. The value 0·031 for 1° C. is given by Matthiesssen for this alloy, yet two pieces of wire drawn from the same rod have given, one 0·021 per cent, and the other 0·04 per cent for 1° C. Possibly this irregularity in the platinum-silver alloys may be due to something analogous to the segregation which Mr. Roberts has found to take place in copper-silver alloys in their molten state, and which Matthiessen in 1860 regarded as mechanical mixtures of allotropic modifications of the alloy.

A recommendation has been made that apparatus for determining the ohm should be set up in London, and that periodically determinations be made to test the electrical constancy of the metals and alloys used in making coils. A committee should be authorised to test coils and issue certificates of their accuracy, just as is done by the Kew Committee with regard to meteorological instruments. The direct relation between heat and chemical work has been established, and the principles of conservation of energy been shown to be true in chemistry by the experiments of Berthelot and of Thomsen, so that we may say that when a system of bodies passes through any succession of chemical changes, the heat evolved or absorbed when no external mechanical effect is produced depends solely upon the initial and final states of the system of bodies, whatever be the nature or the order of the transformations. The extension of this principle to the interaction of the molecules and atoms of bodies on one another is of vast importance in relation to our knowledge of the constitution of matter, for it enables us to state that each chemical compound has a distinct level or potential which may be called its own, and that when a compound gives up one of its elements to another body, the heat evolved in the reaction is the difference between the heat of formation of the first compound, and that of the resulting product.

We have become accustomed to regard matter as made up of molecules, and those molecules to be made up of atoms separated from one another by distances which are great in comparison with the size of the atom, which we may regard as the smallest piece of matter that we can have any conception of. Each atom is surrounded by an envelope of ether which accompanies it in all its movements. The density of the ether increases rapidly as an atom is approached, and it would seem that there must be some force of attraction between the atom and its ether envelope. All the atoms have motions of translations in all possible directions, and according to the theories of Maxwell and Boltzmann, and the experiments of Kundt, Warburg, and others on the specific heat of vapour, in one-atom molecules in the gaseous state there is no motion of rotation. According to the theory of Pictet, the liquid state being the first condensation from the gaseous state must consist of at least two gaseous atoms combined. These two atoms are bound to one another through their ether envelopes. Then the solid state results from the condensation of a liquid, and so a solid molecule must consist of at least two liquid molecules, *i.e.*, at least four gaseous molecules, each surrounded by an atmosphere of ether. M. Pictet imagines these atoms to be centres of attraction; hence in the solid with four such centres the least displacement brings into action couples tending to prevent the molecule from hoisting as soon as external forces act upon it. All the molecules constituting a solid will be rigidly set with regard to one another, for the least displacement sets in action a couple or an opposing force in the molecules on one another.

Let us now follow the sketch which M. Pictet has given of changes which we may consider it to undergo when we expend energy upon it. Suppose a solid body is at absolute zero of temperature, which may be regarded as the state in which the molecules of a body are in stable equilibrium and at rest, the application of heat gives a vibratory motion to the molecules of the solid, which increases with the temperature, the mean amplitude of vibration being a measure of the temperature. We may regard the sum of all the molecular forces as the specific heat of the body, and the product of the sum of all the molecular forces by the mean amplitude of the oscillations; *i.e.*, the product of the specific heat and the temperature will be the quantity of heat or the energy of motion of the body. As more and more heat is applied, the amplitude of vibration of the molecules increases until it is too great for the molecular forces, or forces of cohesion, and the melting-point of the solid is reached. Besides their vibratory motion, the molecules are now capable of motions of translation from place to place among one another. To reduce the solid to the liquid state, *i.e.*, to make the amplitude of vibration of the molecules sufficient to prevent them from coming within the sphere of the forces of cohesion, requires a quantity of heat which does not appear as temperature or molecular motion, and hence it is termed the latent heat of fusion. The temperature remains constant until the melting is complete, the heat being spent in bursting the bonds of the solid. Then a further application of heat increases the amplitude of vibration, or raises the temperature of the liquid at a rate depending on its specific heat until the succession of blows of the molecules overcomes the external pressure and the boiling-point is reached. An additional quantity of heat is applied, which is spent in changing the body to a gas, *i.e.*, to a state of higher potential, in which the motion of translation of the molecules is enormously increased. When this state is attained, the temperature of the gas again begins to increase, as heat is applied, until we arrive at a certain point, when dissociation begins, and the molecules of the separate substances of which the body is composed have so large an amplitude of vibration that the bond which unites them can no longer bring them again into their former positions. The potential of the substances is again raised by a quantity which is proportional to its chemical affinity. Again, we may increase the amplitude of vibration, *i.e.*, the temperature of the molecules, and imagine the possibility of getting higher and higher degrees of dissociation.

If temperature means the amplitude of vibration of the molecules, then only those bodies which have their temperatures increased by the same amount when equal amounts of heat are applied to them can possibly combine with one another; and so the fact that the increase of temperature bears a fixed ratio to the increase of heat may be the cause in virtue of which bodies can combine with one another. Were other bodies to begin to combine together at any definite temperature, they would immediately be torn to pieces again when the temperature is even slightly raised, because the amplitudes of vibration of their molecules no longer remain the same. This idea of temperature is supported by the fact that a combining molecule of each substance requires the same amount of heat to raise its temperature by the same number of degrees, the atomic weights being proportional to the masses of the combining molecules. The celebrated discovery of Faraday, that in a voltameter the work done by an electric current always decomposes equivalent quantities of different substances, combined with the fact that in the whole range of the physical forces work done is equivalent to the application of heat, is quite in accordance with the view that no molecule can combine with another which has not its amplitude of vibration altered by the same amount when equal quantities of heat are applied to both. As soon as we get any divergence from this state of equal motions for equal increments of heat, then we should expect that a further dissociation of molecules would take place, and that only those which are capable of moving together can remain still associated. Just as in the change of state of a body from the solid to the liquid, or from the liquid to the gas, a great amount of heat is spent in increasing the motion of translation of the molecules without altering the temperature, so a great amount of heat is spent in producing dissociation without increasing the temperature of the dissociated substances, since the principle of conservation of energy has been shown by M. Berthelot to hold for the dissociation of bodies. We may conveniently make use of the term latent heat of dissociation for the heat required to dissociate a unit of mass of a substance.

We may thus sum up the laws of physical and chemical changes:—

1. All the physical phenomena of change of state consist in the subdivision of the body into molecules or particles identical with one another.

2. The re-constitution of a body into a liquid or a solid being independent of the relative position of the molecules, only depends on the pressure and temperature.

3. Dissociation separates bodies into their elements, which are of different kinds, and the temperature remains constant during dissociation.

4. The reunion of dissociated bodies depends on the relative position of the elements, and so depends on the grouping of the molecules. The atomic weight being the mass of a molecule as compared with hydrogen, the specific volume, *i.e.*, the atomic weight divided by

the density, is the volume or *mean free path* of a molecule.

Building up his theory of heat on these principles, M. Pictet arrives at a definite relation between the atomic weight of a body, its density, its melting-point, and its coefficient of expansion, which may be stated thus—

The volume of a solid body will be increased as the temperature rises by an amount which is proportional to the number of the molecules in it, and inversely as its specific heat. At a certain temperature peculiar to each body, the amplitude of heat oscillation is sufficient to melt the solid, and we are led to admit that for all bodies the intermolecular distance corresponding to fusion ought to be the same. The higher the point of fusion of a body the shorter, on this theory, must be its heat vibrations. The product of the length of *swing* (the heat oscillations) by the temperature of fusion ought to be a constant number for all solid bodies.

A comparison of the values of the various quantities involved in these statements shows a very satisfactory agreement between theory and experiment, from which it appears that for many different substances the product of the length of swing by the temperature of fusion lies between 3·3 and 3·7 for most substances. Not many values of the latent heat of dissociation have been made. In order to determine it, say, for the separation of oxygen and hydrogen, we should have to determine the amount of work required to produce a spark in a mixture of oxygen and hydrogen, and to measure the exact amount of water or vapour of water combined by the spark as well as the range of temperature through which it had passed after its formation.

Our usual mode of producing heat is by the combination of the molecules of different substances, and we are limited in the production of high temperatures, and in the quantity of available heat necessary to dissociate any considerable quantity of matter. If we heat vapours or gases, we may raise their temperature up to a point corresponding to the dissociation of their molecules, and we are limited in our chemical actions to the temperatures which can be obtained by combining together the most refractory substances, as we are dependent on this combination for our supply of heat.

The combination of carbon and hydrogen with oxygen will give us high temperatures, so that by the oxyhydrogen blowpipe most of the salts and oxides are dissociated. The metalloids bromine, iodine, sulphur, potassium, &c., are the results of the combination of two or more bodies bound together by internal forces much stronger than the affinity of hydrogen or carbon for oxygen, for approximately they obey the law of Dulong and Petit.

For higher temperatures, in order to dissociate the most refractory substances, we require the electric current, either a continuous current, as in the electric arc from a battery or a dynamo-machine, or, more intense still, the electrical discharges from an electrical machine or from an induction coil.

This electric current may be regarded as the most intense furnace for dissociating large quantities of the most refractory substances, and the electric spark may be regarded as something very much hotter than the oxyhydrogen blowpipe, and therefore of service in reducing very small quantities of substances which will yield to no other treatment. The temperature of the electric arc is limited, and cannot reach above the temperature of dissociation of the conductor, and in the case of the constant current, which will not leap across the smallest space of air unless the carbons have first been brought in contact, the current very soon ceases when the point of fusion has been reached. Yet in the centre of the arc we have the gases of those substances which form the conductor; and, as Professor Dewar has shown, we have the formation of acetyline and cyanogen and other compounds, and therefore must have attained the temperature necessary for their formation, *i.e.*, the temperature of their dissociation. The temperature of the induction spark, or, at least, its dissociating power, is higher than that of the arc. We know that the spark will pass across a space of air or a gaseous conductor, and we are limited by the dissociation of the gaseous conductor, and get only very small quantities of the dissociated substances, which immediately re-combine, unless they are separated. If the gases formed are of different densities they will diffuse at different rates through a porous diaphragm, and so may be obtained separated from one another. As the molecules of bodies vibrate they produce vibrations of the ether particles, the period of the oscillations depends on the molecules of the body, and these periodic vibrations are taken up by their ether envelopes and by the luminiferous ether, and their wave-length determined by means of the spectroscope. The bright line spectrum may be regarded as arising from the vibratory motions of the atoms. As the temperature is increased, the amplitudes of oscillation of the molecules and of the ether increase, and from the calculations of Lecoq de Boisbaudran, Stoney, Soret, and others, it would appear that many of the lines in the spectra of bodies may be regarded as harmonics of a fundamental vibration. Thus Lecoq de Boisbaudran finds that in the nitrogen spectrum the blue lines seen at a high temperature correspond to the double octave of certain vibrations, and that, at a lower temperature, red and yellow lines are seen which correspond to a fifth of the same fundamental vibrations.

The bright line spectrum may be regarded as arising from the vibratory motions of the atoms. A widening of the lines may be produced at a higher temperature by the backward and forward motions of the molecules in the direction of the observer. A widening of the lines may also be produced by increase of pressure, because it diminishes the free path of the molecules, and the disturbances of the ether arising from collisions becomes more important than vibrations arising from the regular vibrations of the atoms. Band spectra, or channelled space spectra, more readily occur in the case of bodies which are not very readily subject to chemical actions, or, according to Professors Liveing and Dewar, in the case of cooler vapours near the point of liquefaction.

The effects of change of temperature on the character of spectra is very well illustrated by an experiment of M. Wiedemann with mixtures of mercury with hydrogen or nitrogen in a Geissler's tube. At the ordinary temperature of the air the spectrum of hydrogen or nitrogen was obtained alone; but on heating the tube in an air-bath the lines of mercury appeared and became brighter as the temperature rose, and at the same time the hydrogen lines disappeared in the wider portion of the tube and at the electrodes. The hydrogen or nitrogen lines disappeared first from the positive electrodes and in the luminous tuft, and as the temperature rose disappeared altogether. With nitrogen in a particular experiment, up to 100° C., the nitrogen lines were seen throughout the tube, but from 100° to 230° the nitrogen lines appear towards the negative pole, and the mercury lines are less bright at the negative than at the positive pole, while about 230° C. no nitrogen lines appear. The experiments of Roscoe and Schuster, of Lockyer, and other observers, with potassium, sodium, and other metalloids in vacuum tubes, from which hydrogen is pumped by a Sprengel pump, also show great changes in the molecular condition of the mixture contained in the tubes when they are heated to different temperatures. The changes of colour in the tube are accompanied by changes in the spectrum. Thus Lockyer finds that when potassium is placed in the bottom of the tube, and the spark passes in the upper part of it, as the exhaustion proceeds and the tube is slightly heated, the hydrogen lines disappear, and the red potassium line makes its appearance; then, as the temperature is increased, the red line disappears, and three lines in the yellowish green make their appearance, accompanied by a change in the colour of the tube, and at a higher temperature, and with a Leyden jar joined to a secondary circuit of the induction coil, the gas in the tube becomes of a dull red colour, and with this change a strong line comes out in the spectrum, more refrangible than the usual red potassium line. In this case, on varying the conditions, we get a variation in the character of the spectrum, and the colours and spectra are different in different parts of the tube. In Lockyer's experiments, at the temperature of the arc obtained from a Siemens dynamo-machine, great differences appear in different parts of the arc: for instance, with carbon poles in the presence of calcium, the band spectrum of carbon, or the carbon flutings and the lines of calcium, some of them reversed, are seen separated in the same way as mercury and hydrogen, the carbon spectrum appearing near one pole and the calcium near the other, the lines which are strongest near that pole being reversed or absorbed by the quantity of calcium vapour surrounding it. On introducing a metal into the arc, lines appear which are of different intensities at different distances from the poles, others are strong at one pole and entirely absent at or near the other, while some lines appear as broad as half-spindles in the middle of the arc, but are not present near the poles. Thus, the blue line of calcium is visible alone at one pole, the H and K lines without the blue line at the other.

We may probably regard these effects as the result, not of temperature alone, but must take into account that we have powerful electric currents which will act unequally on the molecules of different bodies according as they are more or less electro-positive. It would seem that we have here something analogous to the segregation which is observed in the melting of certain alloys to which I have already referred.

The abundance of material in some parts of the arc surrounding the central portion of it gives rise to reversal of the principal lines in varying thicknesses over the arc and poles, so that bright lines appear without reversal in some regions, and reversals or absorption lines without bright lines in others. The introduction of a substance into the arc gives rise to a flame of great complexity with regard to colour and concentric envelopes, and the spectra of these flames differ in different parts of the arc. Thus, in a photograph of the flame given by manganese, the line at wave-length 4234·5 occurs without the triplet near 4030, while in another the triplet is present without the line 4234·5.

The lines which are reversed most readily in the arc are generally those the absorption of which is most developed in the flame; thus the manganese triplet in the violet is reversed in the flame, and the blue calcium line is often seen widened when the H and K lines of calcium are not seen at all. In consequence of the numerous changes in spectra at different temperatures, Mr. Lockyer has advanced the idea that the molecules of elementary matter are continually being more and more broken up as their temperature is increased, and has put forward the hypothesis that the chemical elements with which we are acquainted are not simple bodies, but are themselves compounds of some other more simple substances. This theory is founded on Mr. Lockyer's comparisons of spectra and the maps of Angstrom, Thalèn, Young, and others, in which there are coincidences of many of the short lines of the spectra of different substances. These short lines are termed basic lines, since they appear to be common to two or more substances. They appear at the highest temperatures when the longest lines of those substances and those which are considered the test of their presence are entirely absent.

Mr. Lockyer draws a distinction between weak lines, which are basic, *i.e.*, which would permanently exist at a higher temperature in a more elementary stage, and other weak or short lines which would be more strongly present at a lower temperature, in a more complex stage of the molecules. Thus, in lithium, the red line is a low temperature line, and the yellow is feeble; at a higher temperature, the red line is weak, the yellow comes out more strongly, and the blue line appears; at a higher temperature still, the red line disappears, and the yellow dies away; whilst at the temperature of the sun the violet lithium line is the only one which comes out strongly. These effects are studied by first producing the spectrum of the substance in the Bunsen flames, and observing the changes which are produced on passing a spark through the flame; thus, in magnesium a wide triplet, or set of three lines (5209·8, b^1 and b^2), is changed with a narrow triplet (b^1, b^2, and b^4) of the same character. We have here what some observers regard as a recurrence of the same harmonic relation of the vibrations of the same body at a higher temperature.

If the so-called elements are compounds, they must have been formed at a very high temperature, and as higher and higher temperatures are reached the dissociation of these compound bodies will be effected, and the new line spectra, the real basic lines of those substances which show coincidences, will make their appearance as short lines in the spectra. In accordance with this view, Mr. Lockyer holds that the different layers of the solar atmosphere may be regarded as a series of furnaces, on the hottest of which, A, we have the most elementary forms of matter capable of existing only in its uncombined state; at a higher and cooler level, B, this form of matter may form a compound body, and may no longer exist in a free state at the lower temperature; as the cooler and cooler levels, C, D, and E, are reached, the substances become more and more complex and form different combinations, and their spectra become altered at every stage. Since the successive layers are not at rest, but in a state of disturbance, we may get them somewhat mixed, and the lines at the cooler levels, D and E, may be associated with the lines of the hotter levels; these would be basic or coincident lines in the spectra of two different compounds which exist at the cooler levels D and E. We might even get lines which are not present in the hottest furnace, A, coming into existence as the lines of compounds in B and C, and then extending among the lines belonging to more complex compounds which can only exist at a lower temperature, when they might be present as coincident weak lines in the spectra of several compound bodies. Thus Mr. Lockyer regards the calcium lines H and K of the solar spectrum as evidence of different molecular groupings of more elementary bodies. In the electric arc with a weak current the single line 4226 of calcium, which is easily reversed, is much thicker than the two lines H and K; but the three lines are equally thick with a stronger current, and are all reversed. With a spark from a large coil, and using a condenser, the line 4226 disappears, and H and K are strong lines. In the sun the absorption bands H and K are very broad, but the band 4226 is weak. Prof. Young, in his observation of the lines of the chromosphere, finds that H and K are strongly reversed in every important spot and in solar storms; but the line 4226, so prominent in the arc, was only observed three times in the chromosphere.

One of the most interesting features among the most recent researches in Spectrum Analysis is the existence of rhythm in the spectra of bodies, as has been shown by M. Mascart, Cornu, and others, such as the occurrence and repetition of sets of lines, doublets, and triplets in the spectra of different substances and in different parts of the spectrum of the same body. Professors Liveing and Dewar, using the reversed lines in some cases for the more accurate determination of wave-lengths, have traced out the rhythmical character in the spectra of sodium, potassium, and lithium. They show that the lines of sodium and potassium form groups of four lines each, which recur in a regular sequence, while lithium gives single lines, which, including the green line, which they show really to belong to lithium, though it was ascribed to cæsium by Thalèn, also recur in a similar way. In these three metals the law of recurrence seems to be the same, but the wave-lengths show that the whole series are not simple harmonics of one fundamental, although between some of the terms very simple harmonic relations can be found. Between the lines G and H are two triplets of iron lines, which, according to Mr. Lockyer, do not belong to the same molecular grouping as most of the other lines. In many photographs of the iron spectrum these triplets have appeared almost alone. Also the two triplets are not always in the same relation as to brightness, the more refrangible being barely visible with the spark. Combining this with Young's observations, in which some short weak lines near G appear in the chromosphere 30 times, while one of the lines of the less refrangible triplet only appears once, and with the fact that in the solar spectrum the more refrangible triplet is much the more prominent of the two, Mr. Lockyer is led to the conclusion that these two triplets are again due to two distinct molecular groupings. There is one difficulty which must be taken account of in connecwith Mr. Lockyer's theory with regard to the production of successive stages of dissociation by means at our command:—(1) By combustion of different substances; (2) by an electric arc, which will probably give slightly different temperatures according as it is produced by different dynamo-electric machines; (3) by the induction spark without; and (4) with a condenser.

At each stage of the process there must be a considerable absorption of heat to produce the change of state, and our supply of heat is limited in the electric arc because of the dissociation of the conductors, and more limited still in quantity in the electric spark or in the discharge through a vacuum tube; also, we should expect a recombination of the dissociated substances immediately after they have been first dissociated. Hence it seems easier to suppose that at temperatures which we can command on the earth, the dissociation of molecules by the arc or spark is accompanied by the formation of new compounds, in the formation of which heat and light, and especially chemical vibrations, would be again given out, giving rise to new spectra, rather than to suppose that we can reach the temperatures necessary for successive stages of dissociation.

To the lines C, F, the line near G, and *h* belonging to hydrogen, which have a certain rhythmical character, Mr. Lockyer adds D_3 and Kirchoff's line "1474," regarding "1474" as belonging to the coolest or most complex form, and rising to F, which is again subdivided into C and G, using the spark without a condenser, which again gives *h* with the spark and condenser, which is again split up and gives D_3, a more simple line than *h*, in the chromosphere. Professors Liveing and Dewar, on the other hand, trace a rhythmical character or ratio between three of the brightest lines of the chromosphere, two of which are lines "1474" and "*f*" of Lorenzoni, similar to the character of C, F, and *h* of hydrogen, and also trace a similar relation between the chromospheric line D_3 and "1474" to the ratio of the wave-lengths of F and the line near G. They infer the probability that these four lines are due to the same at present unknown substance as had been suggested by Young with regard to two of them.

The harmony of this arrangement is somewhat disturbed by the fact that D_3 lies on the wrong side of "1474" to correspond with the line near G of the hydrogen spectrum.

If we enquire what our sun and the stars have to say to these changes of spectra of the same substance at different temperatures, Dr. Huggins gives us the answer.

In the stars which give a very *white* light, such as Sirius or α Lyræ, we have the lines G and *h* of hydrogen and also H, which has been shown by Dr. Vogel to be coincident with a line of hydrogen; but the K line of calcium is weak in α Lyræ, and does not appear in Sirius. In passing from the white or hottest stars to the yellow stars like our sun, the typical lines diminish in breadth and are better defined, and K becomes stronger relatively to H, and other lines appear. In Arcturus we have a star which is probably cooler than our sun, and in it the line K is stronger in relation to H than it is in the solar spectrum, both being very strong compared with their state in the solar spectrum.

Professors Liveing and Dewar find that K is more easily reversed than H in the electric arc, which agrees with the idea that this line is produced at a lower temperature than H.

Besides the absence or weakness of K, the white stars have twelve strong lines winged at the edges, in which there are three of hydrogen, viz., G, *h*, and H, and the remaining nine form a group which are so related to one another that Dr. Huggins concludes they probably belong to one substance. Three of these lines are said by Dr. Vogel to be lines of hydrogen.

Liveing and Dewar have made considerable progress in determining the conditions and the order of reversal of the spectral lines of metallic vapours. They have adopted methods which allow them to observe through greater thicknesses of vapour than previous observers have generally employed. For lower temperatures tubes of iron or other material placed vertically in a furnace were used, and the hot bottom of the tube was the source of light, the absorption being produced by vapours of metals dropped into the hot tube and filling it to a greater or less height. By this means many of the more volatile metals—such as sodium, thallium, iridium, cæsium, and rubidium, magnesium, lithium, barium, strontium, and calcium—each gave a reversal of its most characteristic line or pair of lines, *i.e.*, the red line of lithium, the violet lines of rubidium and calcium, the blue line of strontium, the sharp green line of barium (5535), and no other lines which can certainly be ascribed to those metals in the elementary state.

For higher temperatures tubes bored out in blocks of lime or of gas carbon, and heated by the electric arc, were used. By keeping up a supply of metal, and in some cases assisting its volatilisation by the admixture of a more volatile metal, such as magnesium, and its reduction by some easily oxidisable metal, such as aluminium, or by a current of coal-gas or hydrogen, they succeeded in maintaining a stream of vapour through the tube so as to reverse a great many lines. In this way the greater part of the bright lines of the metals of the alkalies and alkaline earths were reversed, as well as some of the strongest lines of manganese, aluminium, zinc, cadmium, silver, copper, bismuth, and the two characteristic lines of iridium and of gallium. By passing an iron wire into the arc through a perforated carbon electrode they succeeded in obtaining the reversal of many of the lines of iron. In observing bright line spectra they have found that the arc produced by a De Meriten's machine arranged for high tension gives, in an atmosphere of hydrogen, the lines C and F, although the arc of a powerful Siemens machine does not bring them out, and they have observed many metallic lines in the arc which had not been previously noticed. The temperature obtained by the De Meritens machine is thus higher than that obtained in the Siemens machine.

From observations on weighed quantities of sodium, alone and as an amalgam, introduced into a hot bottle of platinum filled with nitrogen, of which the pressure was varied by an air-pump, they conclude that the width of the sodium lines depends rather on the thickness and temperature of the vapour than upon the whole quantity of sodium present. Very minute quantities diffused into the cool part of the tube give a broad diffuse absorption, while a thin layer of compressed vapour in the hot part of the tube gives only narrow absorption lines. Professors Liveing and Dewar have observed the reversal of some of the well-known bands of the oxides and chlorides of the alkaline earth metals. The lines produced by magnesium in hydrogen form a rhythmical series extending all across the well-known B group, having a close resemblance in general character to the series of lines produced by an electric discharge in a vacuum tube of olefiant gas. The series appears at all temperatures except when a large condenser is employed along with the induction coil, provided hydrogen is present as well as magnesium, while they disappear when hydrogen is excluded, and never appear in dry nitrogen or carbonic oxide.

From their experiments on carbon spectra they conclude with Angstrom and Thalén that certain of the so-called "carbon bands" are due to some compound of carbon with hydrogen, probably acetylene, and that certain others are due to a compound of carbon with nitrogen, probably cyanogen.

They describe some ultra-violet bands: one of them coincides with the shaded band P of the solar spectrum which accompanies the other violet bands in the flame of cyanogen, as well as in the arc and spark between carbon electrodes in the nitrogen. All the bands which they ascribe to a compound of carbon and nitrogen disappear when the discharge is fallen in a non-nitrogenous gas, and they reappear on the introduction of a minute quantity of nitrogen. They appear in the flame of hydrocyanic acid, or of cyanogen, even when cooled down as much as possible, as shown by Watts, or when raised to the highest temperature by burning the cyanogen in nitric oxide; but no flames appear to give these bands unless the burning substance contains nitrogen already united with carbon.

As the views of Mr. Lockyer with regard to the multiple spectra of carbon have very recently appeared in the pages of *Nature*, I need only say that these spectra are looked upon as supporting his theory that the different flutings are truly due to carbon, and that they represent the vibrations of different molecular groupings. The matter is one of very great interest as regards the spectra of comets, for the bands ascribed to acetylene occur in the spectra of comets without the bands of nitrogen, showing that either hydrocarbons must exist ready formed in the comets, in which case the temperature need not exceed that of an ordinary flame, or else nitrogen must be absent, as the temperature which would produce acetylene from its elements would also produce cyanogen, if nitrogen were present.

Quite recently Professors Liveing and Dewar have, simultaneously with Dr. Huggins, described an ultra-violet emission spectrum of water, and have given maps of this spectrum. It is not a little remarkable that by independent methods these observers should have deduced the same numbers for the wave-lengths of the two strong lines at the most refrangible end of this spectrum.

Great attention has been paid by M. Mascart and by M. Cornu to the ultra-violet end of the solar spectrum. M. Mascart was able to fix lines in the solar spectrum as far as line R (3179), but was stopped by the faintness of the photographic impression. Professor Cornu has extended the spectrum still farther to the limit (2948), beyond which no further effect is produced, owing to complete absorption by the earth's atmosphere. A quartz-reflecting prism was used instead of a heliostat. The curvature of the quartz lens was calculated so as to give minimum aberration for a large field of view. The Iceland spar prism was very carefully cut. A lens of quartz was employed to focus the sun on the slit. Having photographed as far as possible by direct solar light, Professor Cornu compared the solar spectrum directly by means of a fluorescent eyepiece with the spectrum of iron, and then obtained, by photographing, the exact positions of the iron lines which were coincident with observed lines in the solar spectrum. M. Cornu states that the dark absorption lines in the sun and the bright iron lines of the same refrangibility are of the same relative importance or intensity in their spectra, indicating the quality between the emissive and the absorbing powers of metallic vapours; and he thinks that we may get by the comparison of bright spectra with the Sun some rough approximation to the quantity of metallic vapours present in the absorption layers of the sun's atmosphere. He draws attention to the abundance of the magnetic metals—iron, nickel, and magnesium—and to the fact that these substances form the composition of most meteorites. M. Cornu has studied the extent of the ultra-violet end of the spectrum, and finds that it is more extended in winter than in summer, and that, at different elevations, the gain in length of the spectrum for increase of elevation is very slow on account of atmospheric absorption, so that we cannot hope greatly to extend the spectrum by taking elevated observing stations. The limit of the solar spectrum is reached very rapidly, and the spectrum is sharply and completely cut off at about the line U (wave-length 2948). From photographs taken at Viesch in the valley of the Rhone and at the Riffelberg, 1910 metres above it, M. Cornu finds the limits to be at wave-lengths 2950 and 2930 respectively.

In the actual absorption of bright line spectra by the earth's atmosphere, M. Cornu observed among others three bright lines of aluminium, which M. Soret calls 30, 31, and 32 (wave-lengths about 1988, 1930, and 1860), and he found that 32 could not be seen at the distance of 6 metres; but on using a collimator, and reducing the distance to 1½ metres, the line 32 became visible, notwithstanding the absorption of the extra lens; at 1 metre, line 32 was brighter than 31, and at a quarter of a metre 32 was brighter than either 30 or 31. With a tube 4 metres in length between the collimator and prism ray 32 is not seen; but when the tube is exhausted, ray 31 gains in intensity and 32 comes into view, and gradually gets brighter than 31, whilst 30 changes very little during the exhaustion. With the same tube he found no appreciable difference between the absorption by air very carefully dried and by moist air, and concludes that this absorption is not due to the vapour of water, and it follows the law of pressure of the atmosphere, which shows it to be due to the whole mass or thickness of the air. Also, M. Soret has shown that water acts very differently on the two ends of the spectrum, distilled water being perfectly transparent for the most refrangible rays, since a column of water of 116 c.m. allowed the ray 2060 in the spectrum of zinc to pass through: on the other hand, water is so opaque to the ultra-red rays that a length of 1 c.m. of it reduces the heat-spectra of metals to half their length and one-quarter of their intensity.

In concluding my address I wish to draw attention to some of those magnetic changes which are due to the action of the sun, and which are brought about by means of the ether which brings us his radiant heat and light. In his discussion of the magnetic effects observed on the earth's surface, General Sabine has shown the existence of diurnal variations due to the magnetic action of the sun; also the magnetic disturbances, aurora and earth currents, which are now again beginning to be large and frequent, have been set down to disturbances in the sun.

Although iron, when raised to incandescence, has its power of attracting a magnet very greatly diminished, we have no proof that it has absolutely no magnetic power left, and with a slight magnetic action the quantity of iron in the sun would be sufficient to account for the diurnal variations of the magnetic needle. During the last few weeks I have been engaged in examining the declination curves for the month of March, 1879, which have been kindly lent to the Kew Committee by the Directors of the Observatories of St. Petersburg, Vienna, Lisbon, Coimbra, and Stonyhurst. On comparing them with the Kew curves for the same period, I find the most remarkable coincidences between the curves from these widely distant stations. It was previously known that there was a similarity between disturbances at different stations, and in one or two cases a comparison between Lisbon and Kew had been made many years ago by Senor Capello and Professor Balfour Stewart, but the actual photographic magnetic records from several stations have never been previously collected, and so the opportunity for such comparisons had not arisen. Allow me in concluding my address to draw attention to a few of the more prominent features of these comparisons which I have made. On placing the declination curves over one another, I find that in many cases there is absolute agreement between them, so that the rate of change of magnetic disturbances at widely distant stations like Kew, Vienna, and St. Petersburg is precisely the same; also, similar disturbances take place at different stations at the same absolute time. It may be stated generally, for large as well as small disturbances, that the east and west deflections of the declination needle take place at the same time and are of the same character at these widely different stations.

There are exceptions to this law. Some disturbances occur at one or two stations and are not perceived at another station. Many instances occur where, up to a certain point of time, the disturbances at all the stations are precisely alike, but suddenly at one or two stations the disturbance changes its character. For instance: on comparing Kew and St. Petersburg, we get perfect similarity followed by deflections of the needle opposite ways at the same instant, and in some such cases the maxima in opposite directions are reached at the same instant, showing that the opposite deflections are produced by the same cause, and that the immediate cause or medium of disturbance in such a case is not far off: probably it is some change of direction or intensity of the earth's magnetism arising from solar action upon it.

Generally, after an hour or two, these differences in the effects of the disturbance vanish, and the disturbances again become alike and simultaneous. In such cases of difference, if the curve tracing of the horizontal or the vertical force be examined, it is generally found that, at the very instant of absolute time, with the beginning of these opposite movements there was an increase or a diminution in the horizontal force, and that the horizontal force continues to change as long as there is any difference in the character of the declination curves. It is clear, then, from these effects that the cause or causes of magnetic disturbances are in general far distant from the earth's surface, even when those disturbances are large; but that not unfrequently these causes act on magnetic matter nearer to the surface of the earth, and therefore at times between two places of observation, and nearer to one than another, thus producing opposite effects on the declination needle at those places: in such cases the differences are probably due to changes in the earth's magnetic force. Now, if we imagine the masses of iron, nickel, and magnesium in the sun to retain even a slight degree of magnetic power in their gaseous state—and we know from the researches of Faraday that gases are some of them magnetic—we have a sufficient cause for all our terrestrial magnetic changes, for we know that these masses of metal are ever boiling up from the lower and hotter levels of the sun's atmosphere to the cooler upper regions, where they must again form clouds to throw out their light and heat, and to absorb the light and heat coming from the hotter lower regions; then they become condensed, and are drawn again back towards the body of the sun, so forming those remarkable dark spaces or sun spots by their downrush towards the lower levels.

In these vast changes, which we know from the science of energy must be taking place, but of the vastness of which we can have no conception, we have abundant cause for these magnetic changes which we observe at the same instant at distant points on the surface of the earth, and the same cause acting by induction on the magnetic matter within and on the earth may well produce changes in the magnitude or in the direction of its total magnetic force. These magnetic changes on the earth will influence the declination needles at different places, and will cause them to be deflected: the direction of the deflection must depend on the situation of the earth's magnetic axis or the direction of its motion with regard to the stations where the observations are made. Thus both directly and indirectly we may find in the sun not only the cause of diurnal magnetic variations, but also the cause of these remarkable magnetic changes and disturbance over the surface of the earth.

NOTE ON SILVER SULPHATE.*

PHILIP BRAHAM, F.C.S.

The silver sulphate was shown as brilliant transparent crystals of high refractive power. They were produced by pouring on a plate of pure silver strong sulphuric acid and adding a few drops of strong nitric acid. At first there was a slight action, bubbles of gas being liberated. In a day or two the whole of the sulphuric acid acquired a deep purple tint, probably due to the formation of some oxide of nitrogen. After a lapse of two to three weeks the purple tint sinks towards the silver and a slight brown tint can be seen on the surface. The layer above the silver being colourless, about this period long crystals form, which re-dissolve, and the liquid becomes colourless. In the course of a few days brilliant specks are seen, which develop into perfect crystals of a regular octahedral shape. The crystals shown had taken over six months in growing.

THE ANALYSIS OF A CRYSTALLINE SALT, PREPARED BY THE ACTION OF H_2SO_4 WITH A TRACE OF HNO_3 ON METALLIC MERCURY ACTING IN THE COLD.*

By PHILIP BRAHAM, F.C.S.

The action whereby these crystals were produced was a very slow one, extending over two years. The crystals were in the form of rhombic prisms, and remained transparent only as long as they were covered with the most concentrated sulphuric acid, an instant of exposure to the air or exposure for a few hours under the acid changed them into an amorphous white powder. The crystals were so remarkably deliquescent that after being freed from acid and dried at 100° C., they absorbed over 5 per cent of their own weight of moisture in five minutes, so that it became almost impossible to be certain of the real weight of a sample. As the crystals were found to be attacked by carbon disulphide, ether, and amyl-alcohol, they were dried and freed from acid by absorption with asbestos. Carbonate of soda and potassic hydrate both produced black precipitates (containing traces of red or yellow) when boiled with the salt showing the presence of a *mercurous* salt containing small quantities of mercuric.

A quantitative determination of the acid made by boiling a weighed amount with solution of sodic carbonate and precipitating as barium sulphate gave the following results:—2·02 grains yielded in one experiment 1·8 $BaSO_4$ = 0·62 of SO_3 or 30·7 per cent of SO_3. In another experiment—12·63 grains yielded 11·06 of $BaSO_4$ = 3·8 of SO_3 = 30 per cent SO_3. The mercury was estimated by reduction with HCl and $SnCl_2$, washing the precipitated metal and drying at 100° C.

(1.) Salt taken 6·07 grains: Hg found 3·81 = 63 per cent = Hg_2O 65·52 per cent.

(2.) Salt taken 11·48 grains: Hg found 8·05 = 70 per cent = 72·8 Hg_2O.

In this case the salt had been left exposed under the acid for some hours, and had become partially changed, so that in drying on asbestos it readily broke up, the actual analysis being performed on the less changed parts of the substance.

The salt is thus certainly *a mercurous sulphate*, but, as normal mercurous sulphate, Hg_2SO_4, contains 80·64 per cent of mercury, and as the highest percentage found, even in the partially changed salt, was 70 per cent, whilst the lowest was 63, and this from a sample which had certainly absorbed a little water in weighing, it appears certain that the salt, whilst still unchanged under the acid in which it was formed, consists of mercurous bisulphate, $HgHSO_4$, which contains 67·3 per cent of Hg, and 27 per cent of SO_3. The excess of SO_3 found (3 per cent) arises from two causes.

(1.) The difficulty of weighing without absorption of water and consequent decomposition.

(2.) The difficulty of perfectly freeing from H_2SO_4 without decomposing it entirely.

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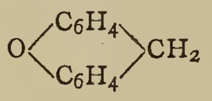
* A paper read before the Chemical Section of the British Association, Swansea Meeting, 1880.

* A paper read before the Chemical Section of the British Association, Swansea Meeting, 1880.

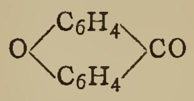
ETHERIFICATION OF PHENOLS.

At the meeting of the Zurich Chemical Society, held on July 19th, Professor Weith communicated the results of a research on the etherification of phenols, which he and Professor Merz had undertaken along with their demonstrator, Dr. Iuvalta.

It is already known that phenol, on heating with zinc chloride to 300 to 350° C., is converted into diphenyl oxide, but the conversion is not complete. Diphenyl oxide is formed more abundantly when aluminium chloride is mixed with an excess of phenol and the mixture is distilled. Along with it there is formed a compound, $C_{13}H_{10}O$, which is, by oxidising agents, converted into a keton, $C_{13}H_8O_2$. The latter on fusing with potassium hydrate splits up smoothly into phenol and salicylic acid, from which the following structural formulæ must be inferred for the above-named substances:—

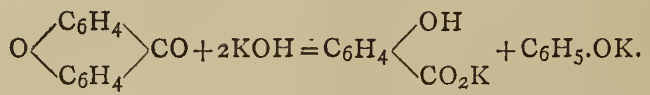


$$O<\begin{matrix}C_6H_4\\C_6H_4\end{matrix}>CH_2$$



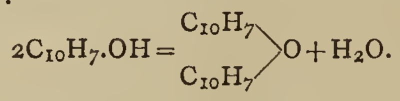
$$O<\begin{matrix}C_6H_4\\C_6H_4\end{matrix}>CO$$

The formation of salicylic acid is represented by the equation:—



$$O<\begin{matrix}C_6H_4\\C_6H_4\end{matrix}>CO+2KOH=C_6H_4<\begin{matrix}OH\\CO_2K\end{matrix}+C_6H_5.OK.$$

Much easier than the etherification of ordinary phenol is that of the two isomeric naphthols, especially that of the β compound. As proved by the giving off of water, it takes place already on passing hydrogen chloride through the boiling naphthols. The dinaphthyl ethers are freed from any unchanged portions of the original substance by a solution of an alkali, and purified by distillation and recrystallisation. They are evidently formed according to the equation:—

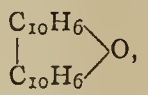


$$2C_{10}H_7.OH=\begin{matrix}C_{10}H_7\\C_{10}H_7\end{matrix}>O+H_2O.$$

The β-dinaphthyl oxide crystallises most easily from petroleum spirit in white laminæ with a mother-of-pearl or silvery lustre, abundantly soluble in hot alcohol, petroleum spirit, and glacial acetic acid, but only sparingly so in the cold liquids. Their melting-point is 105° C.

The α-dinaphthyl ether crystallises from hot spirit of wine in small, colourless, rhombic plates; from petroleum spirit similar but thicker crystals are obtained, sometimes looking like rhombohedra. The solubilities of the α-compound are similar to those of the β-dinaphthyl ether. It melts at 110° C.

In the preparation of the α-dinaphthyl ether there is always obtained a substance which appears in needles, united in tufts, and is undoubtedly identical with a dinaphthylene oxide—



$$\begin{matrix}C_{10}H_6\\|\\C_{10}H_6\end{matrix}>O,$$

prepared by Mr. Unzeitig in the laboratory of Professor Graebe, at Geneva, by distilling the lead compound of α-naphthol. The melting-point was found at 182 to 182·5° C., whilst Unzeitig states 180°; the compound was quite colourless from the first. The same dinaphthylene oxide is formed along with an acid compound of high boiling-point, where α-naphthol is boiled very persistently in a capacious retort fitted with a reflux cooler.

Under similar circumstances β-naphthol yields considerable quantities of β-dinaphthylene oxide, $\beta(C_{10}H_6)_2O$, which crystallises, when quite pure, from benzene or from a mixture of benzene and spirits of wine in thoroughly white laminæ of a strong silvery lustre, melting at 161° C. The two dinaphthylene oxides are only sparingly soluble even in boiling spirit of wine, but abundantly in boiling benzene, and they are most easily crystallised by adding hot spirit of wine to their hot solution in benzene.

Similar to the naphthylene oxides also their picric acid compounds melt at a higher temperature than those of the dinaphthyl ethers, viz., the picrate of α-dinaphthylene oxide at 173°, that of the β-compound at 170° to 171°, that of α-dinaphthyl oxide at 113·5° to 114°, of β-oxide at 122° to 122·5°.

CORRESPONDENCE.

A REPLY TO DR. LEEDS.

To the Editor of the Chemical News.

Sir,—In his paper printed in the Chemical News, vol. xlii., p. 147, Dr. Leeds has again referred to the production of ozone by the atmospheric oxidation of phosphorus, and has particularly called attention to the part I have taken in the controversy.

I like the enthusiastic and earnest spirit in which Dr. Leeds writes of the older chemists and their researches, but, strictly speaking, he is not quite accurate in his statement that I pronounced the doctrines of Schönbein to be false. I simply raised some doubts as to the entire correctness of these views, which were expressed at a time when the means of deciding the questions involved were by no means so ample or as certain as they have since become. Moreover, my objections were not of the theoretical character Dr. Leeds would seem to imply, since my own investigations led me to assert that (apart from the oxidation of phosphorus, which was the subject under discussion) "there is no known process of slow oxidation which has been established to produce ozone."

The severity of Dr. Leeds's assertion, in reply, made me resolve to investigate the oxidation of phosphorus for myself, and having done so I find that although the doubts raised by me are not justified by the results, yet Schönbein did not arrive at a complete knowledge of the process, neither did Dr. Leeds. In fact, my results go a little further, and I have discovered a few new points which on continued investigation are calculated to throw still more light upon a field of chemistry so successfully cultivated by Schönbein.

A discussion of this sort, conducted in a kindly spirit and with the utmost regard to ascertained facts, is not calculated to obstruct the progress of chemical science. To take the present case in illustration of this, my paper published in the Chemical News, vol. xl., p. 96, induced Professor McLeod to enter this "particular arena of controversy," and led both Dr. Leeds and myself to study the subject concerned unknown to each other, and the result is that Schönbein's fame comes out, if anything, all the brighter for the cloud of controversy, and the sum of chemical knowledge is increased. Progress is made in this way. With these few remarks I may let the matter rest until I make my report to the Chemical Society next November.
—I am, &c.,

C. T. Kingzett.

September 18, 1880.

Cause of the Acid Reaction of Animal Tissues after Death.— Marie Ekunina.—The acid reaction is due to a decomposition of the fluids in the tissues effected immediately after death by the action of Schizomycetes. At first volatile fatty acids appear derived from the incipient decomposition of the albumen, speedily followed by the two lactic acids produced from glycogen. The richer a tissue in carbohydrates, the longer this acid reaction prevails after death, as in the liver, the muscles, and the lungs. It is briefest and faintest in the pancreas. In the later hours of putrefaction, the lactic acids disappear and are succeeded by succinic acid. Sooner or later an alkaline reaction sets in throughout the tissues, much ammonia being evolved from the decomposition of the albumen.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Note.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 5, August 2, 1880.

The Optical Properties of Mixtures of Isomorphous Salts.—H. Dufet.—The author has verified the law which he communicated to the Academy, April 8, 1878, *i.e.*, that a crystal formed of a mixture of two isomorphous salts has indices of refraction, which vary continuously with its composition, so that the variation in the value of the index is proportional to the number of equivalents of one of the salts introduced into the mixture.

Influence of Temperature on the Distribution of Salts in their Solutions.—C. Soret.—In all the salts which the author has studied the concentration of the heated portion decreases and that of the cold part increases. The difference thus established increases with the orginal concentration. In the series of the alkaline chlorides the difference is so much the greater for the same absolute concentration as the molecular weight is higher. The phenomenon seems to have no relation with the curve of solubility.

Rise of the Zero-point in Mercurial Thermometers.—J. M. Crafts.—The zero-point rises further and more quickly in thermometers of "crystal" glass than in those free from lead. The rise of the zero-point is much more rapid at the outset, and tends probably towards a limit for a very prolonged heating at a fixed temperature. The effect of an elevated temperature renders the thermometer more stable under the influence of heat at any lower temperature.

Development by Pressure of Polar Electricity in Hemihedral Crystals with Inclined Surfaces.—MM. Jacques and Pierre Curie.—Whatever may be the determining cause, whenever a hemihedral non-conducting crystal with inclined surfaces, there is a formation of electric poles in a certain direction; whenever the crystal expands the disengagement of electricity takes place in an opposite direction.

Moniteur Scientifique, Quesneville.

April, 1880.

Report on a Memoir of MM. Boutmy and Faucher on the Industrial Preparation of Dynamite.—The authors recommend randannite, a siliceous mineral found abundantly in Auvergne, as a substitute for kieselguhr.

Action of Permanganate upon Potassium Cyanide.—E. Baudrimont.—This reaction produces much nitrite and a little urea in an alkaline medium, whilst, if the liquid is acidified with sulphuric acid, urea is formed in abundance. The simultaneous formation of two incompatible compounds, urea and nitrous acid, under the influence of permanganate, shows that the nitrogen of the cyanogen is exposed at once to an oxidising action and to hydrogenisation.

Comparative Solubility of Lead Phosphate and Arseniate in Dilute Acetic Acid.—Armand Bertrand.—One part of lead arseniate dissolves in 2703·05 parts of dilute acetic acid at 38·94 per cent. One part of lead phosphate requires only 782·90 parts of the same acid.

Solubility of Recently Precipitated Carbonate of Lime in Ammoniacal Salts in Presence of an Equivalent Proportion of Sodium Chloride.—Armand Bertrand.—At the temperature of 10° to dissolve 1 grm. calcium carbonate there are required:—

Ammonium hydrochlorate	13·980 grms.
„ sulphate	8·380 „
„ nitrate..	14·438 „

June, 1880.

Virulent Diseases, and in particular on the Malady commonly known as Poultry-Cholera.—M. Pasteur.—From the *Comptes Rendus.*

Review of Chemical Researches Published in Germany.—A. Kopp.—A series of extracts from the *Berichte der Deutsch. Chem. Gesellschaft.*

Composition of Cinchonine.—Z. H. Skraup.—Already noticed.

Products of the Oxidation of Cinchonine.—Z. H. Skraup.—From *Liebig's Annalen*, for June, 1879.

Contribution to the Knowledge of Cinchonidine.—Z. H. Skraup and G. Vortmann.—From *Liebig's Annalen.*

Falsification of Alimentary Substances.—An abstract of the German laws on the adulteration of food, the local arrangements for putting them in force, and the results obtained.

Industrial Society of Mulhouse.—*Session April* 14.—A letter from Dr. Reimann on the subject of dyeing silica, together with accompanying specimens, was laid before the meeting. Precipitated silica is dyed as fast and more easily than infusorial tubes, and here capillarity does not intervene.

A sealed paper from the firm of Scheurer-Rott, opened at the last session, refers to an improvement in alizarin steam reds, by printing upon pieces previously prepared with emulsive oil, mixed in certain cases with hypochlorite of soda. For producing cadmium sulphide directly upon the fibre, M. Schmid prints a mixture of arsenious acid, sulphur, sodium acetate, and cadmium nitrate. A fine yellow is obtained by steaming for 1 to 2 hours and an orange is produced by increasing the sodium acetate.

Session May 12.—A sealed paper by the firm of Hofer-Grosjean was opened, referring to improvements in dissolving caseine. Equal parts of caseine and lime are recommended.

A sealed paper by the firm of Poirrier was opened, relating to the production of fast blues by the reaction of aldehyd upon the violets of methyl and methylaniline.

A paper by M. Ziegler was opened, recommending an addition of arsenic or boracic acid to the colour beck in dyeing madder reds and roses.

Toxicology.—An Italian commision, including among its members Prof. Selmi, is examining the methods for the detection of poisonous alkaloids in the viscera, with especial reference to the so called "ptomaines,"—alkaloids which under certain circumstances may be generated during the putrescence of animal matter.

Celluloid.—An account of the manufacture of this product.

July, 1880.

German Law on the Falsification of Alimentary Substances.—Continued from the June number, and not capable of useful abstraction.

Manufacture of Factitious Butter in the United States.—A compilation from American and English sources. Factitious butter contains only 1·823 per cent of butyrine, caprine, caproine, and capryline, as against 7·432 per cent in the natural product.

Manufacture of Vinegar by means of Bacteria.—E. Wurm.—A translation from *Dingler's Polyt. Journal* (1880, p. 325). The process followed is that described by M. Pasteur in 1868, in his *Etudes sur le Vinaigre.*

Non-existence of Mercury in the Mineral Water of the Source "Du Rocher" at Saint Nectaire.—Jules Lefort.—The nature of this paper will appear sufficiently from the title.

Contributions to the Study of Quinamine.—A. C. Oudemans, Junr.—From *Liebig's Annalen.*

Products of the Decomposition of Quinine.—Dr. M. W. Ramsay and J. J. Dobbie.

Note on Safranine.—R. S. Dale and C. Schorlemmer, F.R.S.

Mauveine and its Kindred Colouring Matters.—W. H. Perkin, F.R.S.—The three above papers are from the *Journal of the Chemical Society.*

On Aspidospermine.—Georg Fraude.—From the *Beriehte der Deutsch. Chem. Gesellschaft.*

Oil of Sheep's Feet.—Th. Chateau.—The author maintains that this oil is much superior to horse-foot oil and neat's-foot oil with which it is generally confounded in commerce. When pure it is of a very pale yellow colour, and weighs 0·915 grm. per litre.

Purification and Refining of Fatty Matters.—Octave Allaire.—To recognise if an oil is pure, the author takes a piece of carbonate of soda (crystal), the size of a nut, dissolves it in its own bulk of water, and shakes it up with the oil under examination in a bottle. If the oil becomes turbid, and gives on settling a solid bulky deposit, it has been badly purified. Oils which act upon the metal of lamps and form deposits of verdigris are also to be rejected as impure. Commercial samples often contain 10 to 15 per cent of free oleic acid.

Bulletin de la Société Chimique de Paris,
No. 9, May 5, 1880.

Remarks on the Platinum Sulphocyanide of V. Marcagno.—G. Wyrouboff.—This body is not a platinum sulphocyanide, but a potassium platino-sulphocyanide, having in its anhydrous state been long ago analysed by M. Buckton. Nor does it yield a proof of the octo-atomicity of platinum, which in this case, as in many others, is tetratomic.

A Compound of Titanium Tetrachloride and Acetyle Chloride.—Armand Bertrand.—On mixing these two bodies the compound in question is immediately precipitated in small yellow brilliant spangles resembling lead iodide. On exposure to moisture these crystals undergo a change, liberating hydrochloric acid. They may be preserved in dry air, or preferably in dry hydrochloric acid. They melt at 25° to 30° and crystallise on cooling. In contact with alkaline solutions they are decomposed, forming an alkaline acetate and chloride, titanic acid which is precipitated, and water. The analysis of the crystals yielded results agreeing with the formula $C_2H_3OCl+TiCl_4$.

Certain Properties of Mixtures of Methyl Cyanide with Common Alcohol and Methylic Alcohol.—C. Vincent and B. Delachanal.—In order to separate methyl cyanide from alcohol it is necessary to submit the mixture to fractionated distillation to order to classify the products; then to dissolve the largest possible quantity of calcium chloride in the mixture, boiling at the lowest temperature in order to absorb the alcohol; then to distil again in the water-bath, and to submit again the product thus obtained to fractionated distillation. A very rich cyanide is thus obtained, from which the last traces of alcohol are eliminated by distillation over a small quantity of phosphoric anhydride, and by rectification to remove the small quantity of ethyl oxide and acetate which take rise from the reaction of the phosphoric acid.

Determination of Urea by Sodium Hypobromite.—C. Mehu.—The author criticises the memoirs of M. Fauconnier and M. Jay (*Bulletin de la Soc. Chimique*, xxxiii., pp. 102 and 105). In opposition to the former of these chemists he finds that the presence of cane-sugar distinctly augments the quantity of nitrogen evolved from urea by means of sodium hypobromite. In opposition to M. Jay he considers that it is easy to meet with starch syrups, which give off merely an insignificant quantity of ammonia if heated with caustic soda.

Oxalic Acid.—A. Villiers.—Already noticed.

Electrolysis of Malonic Acid.—Edme Bourgoin.—Malonic acid presents the following characters:—In the free state it is very stable as against the current, and becomes concentrated in the positive compartment,—a two-fold character by which it is removed from its lower homologue, oxalic acid. In a concentrated alkaline solution it does not in any case give rise to a hydrogen carbide,—which distinguishes it from succinic acid.

Aceto-benzoic Anhydride.—W. H. Greene.—The differences which M. Loir conceives he has observed between acetyle benzoate and benzoyl acetate do not exist. These are one and the same body, whose constitution is the same whatever is its mode of formation, and whose reactions under the same conditions are identical.

Constitution of the Salts of Rosaniline and of Analogous Colouring-Matters (Second Memoir).—A. Rosenstiehl.—If rosaniline is a triamidic aromatic carbinol its salts will be of two classes: the first series is to be regarded as the ether of a tertiary aromatic alcohol; the second series comprises the salts of this ether, which is itself a triacid amine.

Resin of Palisander Wood.—A. Terreil and A. Wolff.—The authors ascribe to this resin the composition—$C_{21}H_{21}O_8$. It is very soluble in alcohol in all proportions, less soluble in ether, chloroform, and carbon disulphide, and insoluble in water. Nitric acid transforms it into an acid, which crystallises in very fine orange-yellow needles, united in tufts.

MISCELLANEOUS.

Schools of Chemistry, &c.—The following information was received too late for insertion in our Students' Number:—

Science and Art Department, South Kensington.—*Examinations, May*, 1881.—Inorganic Chemistry: Friday, 20th; 7 to 10 p.m. Practical Inorganic Chemistry: Saturday, 21st; Elementary Stage, 7 to 10 p.m.; Advanced Stage, 6 to 10 p.m.; Honours, 2 to 10 p.m. Organic Chemistry: Friday, 27th; 7 to 10 p.m. Practical Organic Chemistry: Saturday, 28th; Elementary Stage, 7 to 10 p.m.; Advanced Stage, 6 to 10 p.m.; Honours, 2 to 10 p.m. (Analysis Tables and works on Chemistry may be used in the Advanced Stage and Honours Examinations). Examinations in Practical Chemistry can only be held in Laboratories complying with the requirements of the Science Directory. Each candidate must have a separate set of apparatus and reagents. No examination in Practical Chemistry will be held at South Kensington.

Bedford College, 8 and 9, York Place, Portman Square, for Ladies only.—Chemical Lecturer—Thomas Eltoft, F.C.S., F.I.C. *Matriculation Examination.*—This course will consist of about 35 lectures, so arranged as to cover the whole of the work for the above examination. After each lecture, the student will do practical work. Lectures commence October 16. *First B.Sc. Examination.*—This course will consist of about 40 lectures. Practical Chemistry following each lecture. Lectures and Laboratory work, Wednesdays and Saturdays, throughout the session. For terms, see special prospectus, which can be obtained on application to the Secretary, Henrietta Le Breton.

Royal Agricultural College, Cirencester.—Lecturer, Dr. E. W. Prevost, F.C.S., F.R.S.E.

Queenwood College, Hants.—Lectures on Inorganic Chemistry and Physics, and Laboratory Classes. Resident Lecturer, Dr. H. Wilson Hake, F.C.S., F.I.C.

Peculiar Reaction of Stannous Chloride with Potassium Chlorate.—If 2 parts of stannous chloride and 1 part of potassium chlorate, both finely powdered, are ground together in a porcelain mortar, the mass becomes very hot, gives off watery vapour and chlorous acid, whilst the yellowish white residue, if dissolved in boiling water, deposits, on cooling, potassium perchlorate in splendid micaceous crystals. The mother-liquor contains potassium chlorate and tin oxychloride.—*Polytech. Notizblatt.*

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ADDRESS TO THE GEOLOGICAL SECTION OF THE BRITISH ASSOCIATION.

By H. CLIFTON SORBY, LL.D., F.R.S., F.G.S.,
President of the Section.

In selecting a subject for an address to be given in accordance with the custom of my predecessors, I was anxious that it should be, in some way or other, connected with the locality in which we have met. If I had been adequately acquainted with the district I should have thought it incumbent on me to give such an outline of the general geology of the surrounding country as would have been useful to those attending this meeting. I am, however, practically a stranger to South Wales, and must therefore leave that task to others. On reflecting on the various subjects to which I might have called your attention, it appeared to me that I could select one which would be eminently appropriate in a town and district where iron and copper are smelted on so large a scale, and, as I think, also equally appropriate from a geological point of view. This subject is the comparative structure of artificial slags and erupted rocks. In making this choice I was also influenced by the fact that in my two anniversary addresses as President of the Geological Society I have recently treated on the structure and origin of modern and ancient stratified rocks, and I felt that, if in the present address I were to treat on certain peculiarities in the structure of igneous rocks, I should have described the leading conclusions to which I have been led by studying the microscopical structure of nearly all classes of rocks. It would, however, be impossible in the time now at disposal to treat on all the various branches of the subject. Much might be said on both the purely chemical and purely mineralogical aspects of the question; but though these must not be ignored, I propose to draw your attention mainly to another special and remarkable class of facts, which, so far as I am aware, have attracted little or no attention, and yet, as I think, would be very instructive if we could fully understand their meaning. Here, however, as in so many cases, the observed facts are clear enough, but their full significance somewhat obscure, owing to the want of adequate experimental data or sufficient knowledge of general physical laws.

A considerable amount of attention has already been paid to the mineral constitution of slags, and to such peculiarities of structure as can be learned independently of thin microscopical sections. A very complete and instructive work, specially devoted to the subject, was published by Von Leonhard about twenty-two years ago, just at the time when the microscope was first efficiently applied to the study of rocks. Since then Vogelsang and others have described the microscopical structure of some slags in connexion with their studies of obsidian and other allied volcanic rocks. At the date of the publication of Von Leonhard's work the questions in discussion differed materially from those which should now claim attention. There was still more or less dispute respecting the nature and origin of certain rocks which have now been proved to be truly volcanic by most unequivocal evidence; and I am not at all surprised at this, since, as I shall show, there is such a very great difference in their characteristic structure and that of the artificial products of igneous fusion, that, but for the small portions of glass inclosed in the constituent crystals, described by me many years ago under the name of "glass cavities," there would often be no positive proof of their igneous origin. There was also considerable doubt as to the manner in which certain minerals in volcanic rocks had been generated. The observed facts were sufficient to prove conclusively that some had been formed by sublimation, others by igneous fusion, and others deposited from more or less highly heated water; but it was difficult or impossible to decide whether in particular cases certain minerals had been formed exclusively by one or other process, or sometimes by one and sometimes by the other, or by the combined action of water and a very high temperature. I must confess that, even now that so much may be learned by studying with high magnifying powers the internal structure of crystals, I should hesitate very much in deciding what were the exact conditions under which certain minerals have been formed. This hesitation is probably as much due to inadequate examination and to the want of a complete study of typical specimens, both in the field and by means of the microscope, as to the unavoidable difficulties of the subject. Such doubt, however, applies more to the origin of minerals occurring in cavities than to those constituting a part of true rock masses, to which latter I shall almost exclusively refer on the present occasion. In the formation of these it appears to me that sublimation has occurred to a very limited extent. In many cases true igneous fusion has played such a leading part that the rocks may be fairly called *igneous*, but in other cases, water, in some form or other, has, I think, had so much influence that we should hesitate to call them *igneous*, and the term *erupted* would be open to far less objection, since it would adequately express the manner of their occurrence, and not commit us to anything open to serious doubt.

In studying erupted rocks of different characters, we see that at one extreme they are as truly igneous as any furnace product, and, at the other extreme, hardly, if at all, distinguishable from certain deposits met with in mineral veins, which furnish abundant evidence of the preponderating, if not exclusive, influence of water, and have very little or nothing in common with products certainly known to have been formed by the action of heat, and of heat alone. Between these extremes there is every connecting link, and in certain cases it is almost, if not quite, impossible to say whether the characteristic structure is due more to the action of heat than of water. The great question is whether the presence of a small quantity of water in the liquid or gaseous state is the true cause of very well-marked differences in structure; or whether greater pressure, and the necessarily slower rate of cooling, were not the more active causes, and the presence of water in one state or another was merely the result of the same cause. This is a question which ought to be solved by experiment; but I fear it would be almost impossible to perform the necessary operations in a satisfactory manner.

What I now propose to do is to describe a particular class of facts which have lately attracted my attention, and to show that the crystalline minerals in products known to have been formed by the action of heat alone have a certain very well-marked and characteristic structure, which is gradually modified as we pass through modern and more ancient volcanic to plutonic rocks, in such a manner as to show at once that they are intimately related, and yet differ in such characteristic particulars that I think other agencies than mere heat must have had great influence in producing the final result.

In dealing with this subject I propose, in the first place, to describe the characteristic structure of products formed artificially under perfectly well-known conditions, and then to pass gradually to that of rocks whose origin must be inferred, and cannot be said to have been completely proved.

Crystalline Blowpipe Beads.

Some years ago I devoted a considerable amount of time to the preparation and study of crystalline blowpipe beads, my aim being to discover simple and satisfactory means for identifying small quantities of different earths and metallic oxides, when mixed with others; and I never supposed that small objects would throw any light on the structure and origin of vast masses of natural rock. The manner in which I prepared them was as follows:—A small bead of borax was so saturated with the substance under examination at a high temperature that it became opaque either on cooling or when slowly re-heated. It was again fused so as to be quite transparent, and then very slowly cooled over the flame. If properly managed, the excess of material held in solution at a high temperature slowly crystallised out, the form and character of the crystals depending on the nature of the substance and on the presence of other substances added to the bead as test reagents. By this means I proved that in a few exceptional cases small simple solid crystals are formed. More frequently they are compound, or occur as minute needles; but the most characteristic peculiarity is the development of complex skeleton crystals of extreme beauty, built up of minute attached prisms, so as to give rise to what would be a well-developed crystal with definite external planes, if the interspaces were all filled up. In many cases the fibres of these skeletons are parallel to three different axes perpendicular to one another, and it might be supposed that the entire skeleton was due to the growth of small needle-shaped crystals all uniformly elongated in the line of one crystalline axis, so that the resulting mass would be optically and crystographically complex; but in some cases the different systems of fibres or needles are inclined obliquely, and then the optical characters enable us to prove that the separate prisms are not similar to one another, but developed along different crystalline planes, so as to build up one definite crystal, mechanically complex, but optically and crystographically simple, or merely twinned. In a few special cases there is a well-pronouced departure from this rule, and truly compound groups of prisms are formed. In the centre, that is a definite simple prism; but instead of this growing continuously in the same manner, so as to produce a larger prism, its ends, as it were, break up into several smaller prisms, slightly inclined to the axis of the first; and these secondary prisms, in like manner, break up into still smaller, so as ultimately to give rise to a curious complex brush-like growth, showing in all positions a sort of fan-shaped structure, mechanically, optically, and crystographically complex.

I have done my best to describe these various kinds of crystals seen in blowpipe beads as clearly as can be done without occupying too much time, but feel that it is impossible to make the subject as simple as it really is without numerous illustrations. However, for the purpose now in view, it will, I trust, suffice to have established the fact that we may divide the crystals in blowpipe beads into the following groups, which on the whole are sufficiently distinct, though they necessarily pass one into the other.

1. Simple crystals.
2. Minute detached needles.
3. Fan-shaped compound groups.
4. Feathery skeleton crystals.

It must not be supposed that crystals of one or other of these groups occur promiscuously and without some definite relation to the special conditions of the case. Very much depends upon their chemical composition. Some substances yield almost exclusively those of one group, and other substances those of another, whilst in some cases a difference in the rate of cooling and other circumstances give rise to variations within certain limits; and, if it were possible to still further vary some of the conditions, these limits would probably be increased. Thus, for example, the earliest deposition of crystalline matter from the glassy solvent is sometimes in the form of simple solid prisms or needles, but later on in the process it is in the form of compound feathery tufts; and if it were possible to cool the beads much more slowly whilst they are very hot, I am inclined to believe that some substances might be found that in the early stage of the process would yield larger and more solid crystals than those commonly met with. This supposition, at all events, agrees with what takes place when such salts as potassium chloride are crystallised from solution in water. Some of my blowpipe beads prove most conclusively that several perfectly distinct crystalline substances may be contemporaneously deposited from a highly heated vitreous solvent, which is an important fact in connection with the structure of igneous rocks, since some authors have asserted that more than one mineral species cannot be formed by the slow cooling of a truly melted rock. The great advantage of studying artificial blowpipe beads is that we can so easily obtain a variety of results under conditions which are perfectly well known, and more or less completely under control.

Artificial Slags.

I now proceed to consider the structure of slags, and feel tempted to enter into the consideration of the various minerals found in them which are more or less perfectly identical with those characteristic of erupted rocks; but some of the most interesting, like the felspars, occur in a well-marked form only in special cases where iron ores are smelted with fluxes, seldom, if ever, employed in our own country, so that my acquaintance with them is extremely small. My attention has been mainly directed to the more common products of our blast-furnaces. On examining these, after having become perfectly familiar with the structure of blowpipe beads, I could see at once that they are very analogous, if not identical, in their structure. In both we have a glassy solvent, from which crystals have been deposited; only in one case this solvent was red hot, melted borax, and in the other glassy, melted stone. Thus, for example, some compounds, like what I believe is Humboldtilite, crystallise out in well-marked solid crystals, like those seen occasionally in blowpipe beads, whereas others crystallise out in complex feathery skeletons, just like those so common in and characteristic of the beads. In both we also often see small detached needles, scattered about in the glassy base. These skeleton crystals and minute needles have been described by various writers under the names *crystallites*, *belonites*, and *trichites*. Though we have not the great variety of different forms met with in the beads, and cannot so readily vary the conditions under which they are produced, yet we can, at all events, see clearly that their structural character depends both on their chemical constitution and on the physical conditions under which they have crystallised. None of my microscopical preparations of English slags appear to contain any species of felspar, but several contain what I believe is some variety of angite, both in the form of more or less solid prisms, and of feathery skeletons of great beauty and of much interest in connection with the next class of products to which I shall call your attention, viz., rocks artificially melted and slowly cooled.

Rocks Artificially Melted.

I have had the opportunity of preparing excellent thin microscopical sections of some of the results of the classic experiments of Sir James Hall. I have also carefully studied the product obtained by fusing and slowly cooling much larger masses of the basalt of Rowley, and have compared its structure with that of the original rock. Both are entirely crystalline, and, as far as I can ascertain, both are mainly composed of the same minerals. Those to which I would especially call attention are a triclinic felspar and the angite. The general character of the crystals is, however, strikingly different. In the artificial product a considerable part of the angite occurs as flat feathery plates, like those in furnace slags, which are quite absent from the natural rock, and only part occurs as simple solid crystals, analogous to those in the rock, but much smaller and less developed. The felspar is chiefly in the form of elongated, flat, twinned prisms, which, like the prisms in some blowpipe beads, commence in a more simple form and end in complex fan-shaped brushes, whereas in the natural rock they are larger than in the artificial, and exclusively of simple character. On the whole, then, though the artificially melted and slowly cooled basalt is entirely crystalline, and has a mineral composition closely like that of the natural rock, its mechanical structure is very different, being identical with that of blowpipe beads and slags.

Volcanic Rocks.

Passing now to true natural igneous rocks, we find some, like obsidian, which closely correspond with blowpipe beads, slags, and artificially melted rocks, in having a glassy base, through which small crystalline needles are scattered; but the more completely crystalline volcanic rocks have, on the whole, a structure very characteristically unlike that of the artificial products. I have most carefully examined all my sections of modern and ancient volcanic rocks, but cannot find any in which the angite or magnetite is crystallised in feathery skeletons. In the case of only one single natural rock, from a dyke near Beaumaris, have I found the triclinic felspar arranged in just the same fan-shaped, brush-like groups as those in similar rocks artificially melted and slowly cooled. The large solid crystals in specimens from other localities sometimes show that towards the end of their growth small flat prisms were developed on their surface, analogous to those first deposited in the case of the artificial product. In slags composed almost exclusively of what I believe is Humboldtilite, the crystals are indeed uniformly as simple and solid as those in natural rocks, but the examination of different blowpipe beads shows that no fair comparison can be made between altogether different substances. We must compare together the minerals common to the natural and the artificial products, and we then see that, on the whole, the two classes are only just distinctly connected by certain exceptional crystyls and by structural characters which, as it were, overlap enough to show that there is a passage from one type to the other. In the artificial products are a few small solid crystals of both angite and a triclinic felspar, which closely correspond to the exceptionally small crystals in the natural rocks; but the development of the great mass of the crystals is in a different direction in the two cases. In the artificial products it is in the direction of complex skeletons, which are not seen in the natural rock; but in the natural rock it is in the direction of large simple solid crystals, which are not met with in the artificial products. There is a far closer analogy in the case of partially vitreous rocks, which, independent of the true glassy base common to them and the artificial products, often contain analogous crystalline needles. Even then, however, we see that in the artificial product the crystals tend to develop into complex skeletons, but in the natural rocks into simple solid crystals.

It must not be supposed that these facts in any way lead me to think that thoroughly crystalline modern and ancient volcanic rocks were never truly fused. The simple, large, and characteristic crystals of such minerals as angite, felspar, leucite, and olivine, often contain so many thoroughly well-marked glass enclosures, as to prove most conclusively that when the crystals were formed they were surrounded by, and deposited from, a melted glassy base, which was caught up by them whilst it was still melted. This included glass has often remained unchanged, even when the main mass became completely crystalline, or has been greatly altered by the subsequent action of water. I contend that these glass enclosures prove that many of our British erupted rocks were of as truly igneous origin as any lava flowing from a modern volcano. The difference between the structure of such natural rocks and that of artificial slags must not, in my opinion, be attributed to the absence of true igneous fusion, but to some difference in the surrounding conditions, which was sufficient to greatly modify the final result, when the fused mass became crystalline on cooling. The observed facts are clear enough, and several plausible explanations might easily be suggested, but I do not feel at all convinced that any single one would be correct. That which first suggests itself is a much slower cooling of the natural rocks than is possible in the case of the artificial rocks; and I must confess that this explanation seems so plausible that I should not hesitate to adopt it, if certain facts could be accounted for in a satisfactory manner. Nothing could be more simple than to suppose that skeleton crystals are formed when deposition takes place in a hurried manner, and they so overgrow the supply that they develop themselves along certain lines of growth before there has been time to solidly build up what has been roughly sketched in outline. I cannot but think that this must be a true and, to some extent, active cause, even if it be inadequate to explain all the facts. What makes me hesitate to adopt it by itself is the structure of some doleritic rocks when in close contact with the strata amongst which they have been erupted. In all my specimens the effects of much more rapid cooling are perfectly well marked. The base of the rock when in close contact is sometimes so extremely fine-grained that it is scarcely crystallised, and is certainly far less crystalline and finer grained than the artificial products to which I have called attention, and yet there is no passage towards those structures which are most characteristic of slags, or at least, no such passage as I should have expected if these structures depended exclusively on more rapid cooling.

We might well ascribe something to the effect of mass, but one of my specimens of basalt melted and slowly cooled in a small crucible is quite as crystalline as another specimen taken from a far larger mass, though I must confess that what difference there is in this latter is in the direction of the structure characteristic of natural rocks. The presence or absence of water appears to me a very probable explanation of some differences. When there is evidence of its presence in a liquid state during the consolidation of a rock we can scarcely hesitate to conclude that it must have had some active influence; but in the case of true volcanic rocks the presence of liquid water is scarcely probable. That much water is present in some form or other is clearly proved by the great amount of steam given off from erupted lavas. I can scarcely believe that it exists in a liquid state, except at great depths, but it may possibly be present in a combined form or as a dissolved vapour under much less pressure, and the question is whether this water may not have considerable influence on the growth of crystals formed prior to the eruption, before it was given off as steam. I do not know one single fact which can be looked upon as fairly opposed to this supposition, and it is even to some extent supported by experiment. M. Daubrée informs me that the crystals of angite formed by him at a high temperature by the action of water have the solid character of those in volcanic rocks, and not the skeleton structure of those met with in slags. The conditions under which they were formed were, however, not sufficiently like those probably present during the formation of erupted lavas to justify our looking upon the explanation I have suggested as anything more than sufficiently plausible, in the absence of more complete experimental proofs.

Granitic Rocks.

I now proceed to consider rocks of another extreme type, which for distinction we may call the granitic. On the whole, they have little or nothing in common with slags, or with artificial products similar to slags, being composed exclusively of solid crystals, analogous in character only to slag-crystals of very different mineral nature. As an illustration, I would refer to the structure of the products formed by fusing and slowly cooling upwards of a ton of the syenite of Grooby, near Leicester. Different parts of the resulting mass differ very materially, but still there is an intimate relation between them, and a gradual passage from one to the other. The most characteristic feature of those parts which are completely crystalline is the presence of beautiful feathery skeleton crystals of magnetite, and of long flat prisms of a triclinic felspar, ending in complex, fan-shaped brushes. There are no solid crystals of felspar, hornblende, and quartz, of which the natural rock is mainly composed, to the entire exclusion of any resembling those in the melted rock. As looked upon from the point of view taken in this address, the natural and artificial products have no structural character in common, so that I think we must look for other conditions than pure igneous fusion to explain the greatly modified results. We have not to look far for evidence of a well-marked difference in surrounding circumstances. The quartz in the natural rock contains vast numbers of fluid cavities, thus proving that water was present, either in the liquid state or as a vapour so highly compressed that it afterwards condensed into an almost equal bulk of liquid. In some specimens of granite there is indeed clear proof that the water was present as a liquid, supersaturated with alkaline chlorides, like that inclosed in the cavities of some minerals met with in blocks ejected from Vesuvius, which also have to some extent what may be called a granitic structure. In the case of one very exceptional and interesting granite, there is apparently good proof that the felspar crystallised out at a temperature above the critical point of water—that is to say, at a temperature higher than that at which water can exist as a liquid under any pressure—and it caught up highly compressed steam, comparatively, if not entirely, free from soluble salts; whereas the quartz crystallised when the temperature was so far lowered as to be below the critical point, and the water had passed into a liquid, supersaturated with alkaline chlorides, which have crystallised out as small cubes in the fluid cavities, just as in the case of minerals in some of the blocks ejected from Vesuvius.

Confining our attention, then, to extreme cases, we thus see that rocks of the granitic type differ in a most characteristic manner from the products of artificial igneous fusion, both in the structure of the crystals and in containing liquid water, inclosed at the time of their formation. The question then arises, whether these differences were due to the presence of the liquid water, or whether its presence and the characteristic structure were not both the effects of the great pressure of superincumbent rocks. I do not see how this can be decided in a perfectly satisfactory manner, but must confess that I am inclined to believe that, whilst great pressure was necessarily the reason why the water did not escape as vapour, the presence of liquid water during final consolidation must have had a very considerable influence in modifying the structure of the rock, and had a great share in developing what we may call the granitic type.

It would be very instructive to follow out the gradual passage from one extreme type to another far more completely than is possible on the present occasion. The most interesting examples of rocks intermediate between the granitic and volcanic types that I have been able to examine in adequate detail, are the various Cornish elvans and other quartz felsites, which furnish all but a complete passage from pitchstone to granite. Some specimens prove that quartz may crystallise out from and inclose a perfectly glassy base, without a trace of liquid water; and at the same time other specimens prove equally well that, as we approach the granitic type, the quartz was not deposited from a glassy solvent, but inclosed more or less water. In the few intermediate cases there appears to be evidence of the conjoint presence of uncombined water and melted stony matter. On the whole, if we take into consideration only the external form of the larger crystals, rocks of the granitic type are very much as though the crystals met with in truly volcanic rocks had been strained out from the glassy or fine-grained base, and the intermediate spaces filled with quartz. The internal structure of the crystal is, however, very different, the cavities in one class containing glass, and in the other water. This most essential and characteristic difference proves that rocks of the true granitic type cannot have been formed simply by the more complete crystallisation of the general base of the rock. If the crystals in granite were analogous to those developed in volcanic rocks, and the only essential difference were that the residue crystallised out more slowly and completely, so as to give rise to a more coarsely crystallised base, the crystals first formed ought not, as I think, to differ so essentially as that in one case they should inclose only glass, and in the other only water. Taking all into consideration, we can therefore scarcely suppose that the crystals in granitic rocks were deposited from a truly melted, dry, glassy solvent, like those in volcanic rocks or in slags.

General Results.

I have, I trust, now said enough to show that the objects here described may be conveniently separated into three well-marked groups, viz., artificial slags, volcanic rocks, and granitic rocks. My own specimens all show perfectly well-marked and characteristic structures, though they are connected in some cases by intermediate varieties. Possibly such connecting links might be more pronounced in other specimens that have not come under my notice. I must, however, base my conclusions on what I have been able to study in an adequate manner, by examining my own preparations, and leave it for others to correct any errors into which I may have been led from lack of more numerous specimens. In any case the facts seem abundantly sufficient to prove that there must be some active cause for such a common, if not general, difference in the structural character of these three different types. The supposition is so simple and attractive, that I feel very much tempted to suggest that this difference is due to the presence or absence of water as a gas or as a liquid. In the case of slags it is *not* present in *any* form. Considering how large an amount of steam is given off from eruptive lavas, and that, as a rule, no fluid-cavities occur in the constituent minerals, it appears to me very plausible to suppose that those structures which are specially characteristic of volcanic rocks are in great measure, if not entirely, due to the presence of *associated* or *dissolved vapour*. The fluid-cavities prove that water was sometimes, if not always, present as a *liquid* during the consolidation of granitic rocks; and we can scarcely hesitate to conclude that it must have had very considerable influence on the rock during consolidation. Still, though these three extreme types appear to be thus characterised by the absence of water, or by its presence in a state of vapour or liquid, I think we are scarcely in a position to say that this difference in the conditions is more than a plausible explanation of the differences in their structure. At the same time, I do not know any facts that are opposed to this conclusion, and we should, perhaps, not greatly err in thus correlating the structures, even though the water was not the essential and active cause of the differences.

Confining our attention to the more important crystalline constituents which are common to the different types, we may say that the chief structural characters of the crystals are as follows:—

a. Skeleton crystals.
b. Fan-shaped groups.
c. Glass-cavities.
d. Simple crystals.
e. Fluid-cavities.

These different structural characters are found combined in different ways in the different natural and artificial products; and for simplicity I will refer to them by means of the affixed letters.

The type of the artificial products of fusion may generally be expressed by $a+b$ or $b+c$, that is to say, it is characterised by skeleton crystals and fan-shaped groups, or by fan-shaped groups and glass cavities. In like manner the volcanic type may be expressed occasionally by $b+c$, but generally by $c+d$; and the granitic by $d+e$. These relations will be more apparent if given in the form of a table, as follows:—

Slag type	$a+b$ $b+c$
Volcanic type	$b+c$ $c+d$
Granitic type	$d+e$.

Hence it will be seen that there is a gradual passage from one type to the other by the disappearance of one character and the appearance of another, certain characters the meanwhile remaining common, so that there is no sudden break, but an overlapping of structural characteristics. It is, I think, satisfactory to find that, when erupted rocks are examined from such a new and independent point of view, the general conclusions to which I had been led are so completely in accord with those arrived at by other methods of study.

Conclusion.

And now I feel that it is time to conclude. I have necessarily been compelled to give only a general account of the subject, and perhaps, for want of adequate description, many facts may appear more complex than they really are. Some are, indeed, of anything but simple character, and their full explanation is, perhaps, beyond our present power. The greater part are, however, much more simple and easy to observe than to describe; and, even if I have failed to make everything as plain as I could wish, I hope I have succeeded in making the principal point sufficiently clear to show that the structure of slags and of analogous artificial products throws much light on the structure and origin of the various groups of erupted rocks. I feel that very much still remains to be learned, and, as I think, could be learned, by the further extension of this method of inquiry. What strikes me most is the great necessity for the more complete application of experimental methods of research; but to carry out the experiments necessary to clear up the essential difficulties of the subject would, I fear, be a most difficult undertaking. In the meantime all that we can do is to compare the structure of known artificial products with that of natural rocks, and to draw the best conclusions we can from the facts, as viewed in the light of our present knowledge of chemistry and physics. My own impression is that there is still much to be learned respecting the exact conditions under which some of our commonest rocks were formed.

ON A RELATION BETWEEN THE ATOMIC VOLUMES OF CERTAIN ELEMENTS AND THE HEATS OF FORMATION OF SOME OF THEIR COMPOUNDS.*

By WALTER WELDON, F.R.S.E.

When chlorine, bromine, and iodine unite with the same element in the same atomic proportions, the heat of formation of the chloride is always greater than that of the

TABLE I.

	Atomic Weights.	Atomic Volumes.	Thermal Values of Compounds.									
Mg	24·0	13·79	$MgCl_2$	151·0	$MgBr_2$	140·0	MgI_2	108·0	MgOaq	149·8		
Zn	64·9	9·07	$ZnCl_2$	97·2	$ZnBr_2$	86·2	ZnI_2	60·0	ZnO	86·8		
Al	27·3	10·7	Al_2Cl_6	321·8					Al_2O_3	390·0		
Fe	55·9	7·2	Fe_2Cl_6	192·0					Fe_2O_3	191·2		
Ag	107·66	10·28	AgCl	29·2	AgBr	22·7 27·7	AgI	+13·8 +15·9				
Au	196·2	10·18	AuCl	5·8	AuBr	5·0	AuI	− 0·2				
Sr	87·2	34·88	$SrCl_2$	184·6	$SrBr_2$	168·0	SrI_2	136·0	SrO	131·0	SrS	99·2
Pb	206·4	18·15	$PbCl_2$	85·2	$PbBr_2$	77·0	PbI_2	53·2	PbO	51·2	PbS	17·8
Fe	55·9	7·2	$FeCl_2$	82·0					FeOaq	69·0	FeS	23·8
Co	58·6	6·9	$CoCl_2$	76·4					CoOaq	63·4	CoS	21·8
Ni	58·6	0·7	$NiCl_2$	74·6					NiOaq	60·8	NiS	19·4
Mg	24·0	13·79	MgCl2	151·0	$MgBr_2$	140·0	MgI_2	108·0	MgOaq	149·8		
Cd	111·6	12·9	$CdCl_2$	93·2	$CdBr_2$	84·2	CdI_2	55·8	CdOaq	66·4		
Na	23·0	23·65	NaCl	97·7	NaBr	90·7	NaI	74·2	Na_2Oaq	135·6	Na_2S (dis.)	103·2
Ag	107·66	10·24	AgCl	29·2	AgBr	27·7	AgI	15·9	Ag_2O	3·0	Ag_2S	3·0

TABLE II.

	Atomic Weights.	Atomic Volumes.	Thermal Values of Compounds.									
Ag	107·66	10·22827	AgCl	29·2 29·4	AgBr	22·7 27·7	AgI	13·8 15·9	Ag_2O	6·0 7 0	Ag_2S	3·0
Hg	199·8	14·7	$\frac{Hg_2Cl_2}{2}$	40·9 41·3	$\frac{Hg_2Br_2}{2}$	32·1 39·2	$\frac{Hg_2I_2}{2}$	24·2 29·2	HgO	30·6	HgS	9·9
Tl	203·4	17·1	TlCl	48·6	TlBr	46·4	TlI	35·6	Tl_2O	42·2		
Li	7·02	11·9	LiCl	93·8								
Na	23·00	23·65	NaCl	97·7	NaBr	90·7	NaI	74·2	Na_2O (dis.)	135·6	Na_2S (dis.)	103·2
K	39·14	45·2	KCl	105·0	KBr	100·4	KI	85·2	K_2O (dis.)	139·6	K_2S (dis.)	112·6
Ca	39 9	25·4	$CaCl_2$	170·2	$CaBr_2$	151·6	CaI_2	118·6	CaO (dis.)	149·5	CaS	92·0
Sr	87·2	34·88	$SrCl_2$	184·6	$SrBr_2$	168·0	SrI_2	136·0	SrO (dis.)	158·2	SrS	99·2
Cu	63·3	7·0805	$CuCl_2$	51·6	$CuBr_2$	42·8			CuO	37·2	CuS	10·2
Cu_2	126·6	14·1610	Cu_2Cl_2	66·2	Cu_2Br_2	60·0	Cu_2I_2	43·8	Cu_2O	40·8		
Pb	206·4	18·153	$PbCl_2$	85·2	$PbBr_2$	77·0	PbI_2	53·8	PbO	51·4	PbS	17·8
As	74·9	13·0	$AsCl_3$	69·8 74·6					As_2O_3	146·0		
Sb	122·0	18·2	$SbCl_3$	86·3					Sb_2O_3	168·0		
B	11·0	4·104	BCl_3	108·5	BBr_3	73·1			$\frac{B_2O_3}{2}$	156·0		
Si	28·0	11·244	$SiCl_3$	157·6	$SiBr_3$	120·4			SiO_2	211·0		

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* Read before Section B of the British Association, Swansea Meeting, August 26th, 1880.

TABLE III.

Pb	206·4	11·37	18·153	$PbCl_2$	85·2	4·693	=1·0	$PbBr_2$	77·0	4·235=1·0
Cu	63·3	8·94	7·0805	Cu_2Cl_2	66·2	4·674	=1·0	Cu_2Br_2	60·0	4·237=1·0
Mg	24·0	1·74	13·793	$MgCl_2$	151·0	10·939		$MgBr_2$	140·0	10·15
,,	,,	1·7	14·117	,,	151·0	10·696	=1·5	,,	140·0	9·917=1·5
Zn	64·9	7·15	9·0796	$ZnCl_2$	97·2	10·708		$ZnBr_2$	86·2	9·496=1·5
Cd	111·6	8·65	12·907	$CdCl_2$	93·2	7·22	=1·0	$CdBr_2$	84·2	6·523=1·0
Hg	199·8	13·59	14·7019	$HgCl_2$	62·8	4·268	=1·5	Hg_2Br	60 8	4·135=1·5
,,	,,	,,	,,	Hg_2Cl_2	82·6	2·807	=1·0	Hg_2Br_2	78·4	2·666=1·0
Ag	107·66	10·47	10·2827	AgCl	29·2	2·839	=1·0	AgBr	27·7	2·693=1·0
Hg	199·8	13·59	14·7019	$\frac{Hg_2Cl_2}{2}$	41·3	2·807	=1·0	$\frac{Hg_2Br_2}{2}$	39·2	2·666=1·0
Tl	203·6	11·86	17·1669	TlCl	48·6	2·831	=1·0	TlBr	46·4	2·679=1·0
Au	196·2	19·26	10·1869	AuCl	5·8	0·565	=1·0			
Ag	107·66	10·47	10·2827	AgCl	29·2	2·839	=5·0			
Sr	87·2	2·5	34·88	$SrCl_2$	184·6	5·29	=4·0	$SrBr_2$	168·0	4·813=4·0
Ca	39·9	1·57	25·414	$CaCl_2$	170·2	6·70	=5·0	$CaBr_2$	151·6	5·972=5·0
Sr	87·2	2·5	34·88	$SrCl_2$	184·6	5·29	=9·0	$SrBr_2$	168·0	4·813=9·0
Pb	206·4	11·37	18·153	$PbCl_2$	85·2	4·693	=8·0	$PbBr_2$	77·0	4·235=8·0
Li	7·02	0·59	11·9							
Na	23·0	0·97225	23·656							
K	39·14	0·865	45·259							
Tl	203·6	11·86	17·1669							
Mn	54·8	8·019	6·8337	$MnCl_2$	112·0	16·404	=1·5	$MnBr_2$	100·0	14·633=1·5
Fe	55·9	7·8	7·2	$FeCl_2$	82·0	11·38	=1·0	$FeBr_2$	70·0	9·72 =1·0
Co	58·6	8·5	6·9	$CoCl_2$	76·4	11·00	=1·0			
Ni	58·6	8·8	6·7	$NiCl_2$	74·6	11·12	=1·0			
P	31·0	2·34	13·2	PCl_3	75·8	5·74	=1·0			
As	74·9	5·76	13·0	$AsCl_3$	74·6	5·74	=1·0			
As	74·9	5·67	13·0	$AsCl_3$	74·6	5·65	=6·0			
Sb	122·0	6·7	18·2	$SbCl_5$	86·3	4·73	=5·0			
Si	28·0	2·49	11·2449	$SiCl_4$	159·2	14·01	=7·0			
Sn	117·8	7·29	16·1	$SnCl_4$	127·2	8·00	=4·0			
K	39·14	0·865	45·259	KCl	105·0	2·31	=5·0	KBr	100·4	2·218=4·0
Na	23·0	0·97225	23·656	NaCl	97·7	4 13	=9·0	NaBr	90·7	3·834=7·0
Na	23·0	0·97225	23·656							
Li	7·02	0·59	11·9							
Na	23·0	0·97225	23·656	NaCl	97·7	4·13	=7·0	NaBr	90·7	3·834=7·0
Ag	107·66	10·47	10·2827	AgCl	29·2	2·83	=5·0	AgBr	27·7	2·693=5·0

bromide, and the heat of formation of the bromide always greater than that of the iodide; the order of the heats of formation of the haloid compounds being thus the inverse of that of the atomic weights of the respective halogens. There is a similar relation between the heats of formation of oxides and sulphides and the atomic weights of oxygen and sulphur; the heat of formation of an oxide being always greater than that of the corresponding sulphide of the same element.

In a recent communication to the Academy of Sciences (*Comptes Rendus*, 5 Juillet, 1880), Berthelot points to certain instances in which a similar relation exists between the atomic weights of analogous positive elements and the heats of formation of their compounds, and expresses the opinion that this relation is probably general, although masked, in the cases of some families of positive elements, "*par diverses circonstances mal connues.*"

Nearly all the data bearing on this point which we at present possess are collected into the following two Tables. In both Tables, the elements bracketed together are more or less analogous elements, and are arranged in the order of their atomic weights. The order of the heats of formation of corresponding compounds, with the same negative element, of the positive elements bracketed together in Table I., is in each case the inverse of that of the atomic weights of the positive elements; but the order of the heats of formation of corresponding compounds, with the same negative element, of the positive elements bracketed together in Table II., is in all cases the same as that of the atomic weights of the positive elements. The relation between atomic weights and heats of formation of compounds which obtains in the case of chlorine, bromine, and iodine, and also in that of oxygen and sulphur, is thus exhibited by some families of positive elements, but not by others. It is therefore not of the nature of a general law.

If, however, we compare the heats of formation of corresponding compounds, with the same negative element, of positive elements of the same family, not with the atomic weights of the positive elements, but with their atomic weights divided by their specific gravities,—that is to say, with their atomic volumes,—we find what does appear to be the nature of a general law. We find that, *for corresponding compounds, with the same negative ele-*

TABLE III. (*continued*).

PbI_2	53·8	2·765=1·0	PbO	51·4	2·824=1·0						
Cu_2I_2	43·8	2·96 =1·0	Cu_2O	40·8	2·881=1·0						
ZnI_2	60·0	6·610=1·5									
CdI_2	55·8	4·223=1·0									
HgI_2	44·8	3·046=1·5	HgO	31·0	2·108=1·5				$HgCy_2$	60·16	4·18=1·5
Hg_2I_2	58·4	1·983=1·0	Hg_2O_2	42·2	1·435=1·0						
AgI_2	15·9	1·546=1·5				Ag_2S	6·0	0·29 =1·0	$AgCy_2$	27·9	=2·71=1·0
$\frac{Hg_2I_2}{2}$	29·2	1·983=2·0				HgS	18·8	1·16=4·0			
TlI	35·6	2·073=2·0									
SrI_2	136·0	3·610=4·0	SrO (dis.)	158·2	6·1 =1·5	SrS	99·2	2·842=4·0			
CaI_2	118·6	4·669=5·0	CaO (dis.)	149·5	4·52 =1·0	CaS	92·0	3·622=5·0			
SrI_2	136·0	3·610=4·0				SrS	99 2	2·842=3·0			
PbI_2	53·8	2·765=3·0				PbS	17·8	0·98 =1·0			
			LiO (dis.)	166·6	7·0 =6·0	Li_2S (dis.)	115·2	4·9 =4·0			
			Na_2O (dis.)	224·2	4·755=4·0						
			K_2O (dis.)	208·6	2·305=2·0	K_2S (dis.)	112·6	1·247=1·0			
			Tl_2O (dis.)	39·2	1·146=1·0						
MnI_2	68·0	10·14 =2·0	MnO aq	94·8	13·872=1·5	MnS	45·2	6·6 =2·0			
FeI_2	38·0	5·26 =1·0	FeO aq	69·0	9·583=1·0	FeS	23·8	3·3 =1·0			
			CoO aq	63·4	9·2 =1·0						
			NiO aq	60·8	9·1 =1·0						
			As_2O_3	146·0	5·51 =6·0						
			Sb_2O_3	168·0	4·61 =5·0						
			SiO_2	219·0	19·47 =9·0						
			SnO_2	141·2	8·434=4·0						
KI	85·2	1·882=3·0	K_2O (dis.)	208·6	2·305=1·0	K_2S (dis.)	112·6	1·247=3·0	KCy (dis.)	83·4	1·97=3·0
NaI	74·2	3·137=5·0	Na_2O (dis.)	224·2	4·755=2·0	Na_2S (dis.)	103·2	2·18 =5·0	$NaCy$ (dis.)	78·8	3·33=5·0
			Na_2O (dis.)	224·2	4·755=1·0	Na_2S (dis.)	103·2	2·18 =3·0			
			Li_2O (dis.)	166·6	7·00 =1·5	Li_2S (dis.)	115·2	4·9 =7·0			
NaI	74·2	3·137=2·0									
AgI	15·9	1·546=1·0									

ment, of positive elements of the same family, the heats of formation are greater the greater the atomic volumes of the positive elements. This law, moreover, sometimes holds good in the case of corresponding compounds of elements belonging not to the same family, but to allied families.

In Table I. the atomic volumes of the elements bracketed together follow an order the inverse of that of the atomic weights of those elements. In Table II., for the elements bracketed together the order of the atomic volumes is the same as that of the atomic weights. While the order of the heats of formation of corresponding compounds, with the same negative element, of the positive elements bracketed together, is in the one case the inverse of, and in the other the same as, the order of the atomic weights of the positive elements, in both cases the order of the heats of formation is the same as that of the atomic volumes of the positive elements.

To this rule two exceptions may be suggested, though only two, so far as the data of thermal chemistry at present go. Both are exceptions which I believe will disappear as our knowledge of the apparently exceptional bodies becomes more complete. One of these bodies is cadmium. This metal is usually classed with magnesium and zinc, although it is known to have close resemblances also with indium. Compared with magnesium, it obeys the law. Its atomic volume is lower than that of magnesium, and the heats of formation of its compounds are lower than those of the corresponding magnesium compounds. With zinc, however, cadmium does not compare as the law would require it to do if zinc and cadmium really belonged to the same family. While the atomic volume of cadmium is greater than that of zinc, the heats of formation of cadmium compounds are lower than those of the corresponding zinc compounds. To my mind this is simply another confirmation of the view of those who class cadmium with indium rather than with zinc. If this view be sound, the heats of formation of indium compounds—none of which have yet been measured—will be found to be greater than those of cadmium compounds, the atomic volume of cadmium being 12·9, and that of indium 15·3.

The other apparently exceptional body is manganese. While the heats of formation of the compounds of manganese are greater than those of the corresponding compounds of iron, the specific gravity now usually assigned to manganese would make its atomic volume less than that of iron. The values found for the specific gravity of manganese by different observers vary from a little over 7·0 to a little over 8·0. If the real value of the specific gravity of manganese were about the mean of these extremes, manganese—regarded as belonging to the iron family—would obey the law. Either, then, the true value of the specific gravity of manganese is somewhat lower than the value now ordinarily accepted for it; or the more intimate family relations of manganese are, not with iron, cobalt, and nickel, but with some of the other metals with which we know that it is closely allied; or else this metal really constitutes—if it be admitted that cadmium does not belong to the zinc family—the only exception to what would otherwise be, so far as the data at present go, an absolutely general law.

In all cases yet known, then, unless those of cadmium and manganese be really exceptions, the heats of formation of corresponding compounds, with the same negative element, of positive elements belonging to the same family, are greater the greater the atomic volumes of the positive elements. Having arrived at this fact, it was natural to proceed to enquire whether or not there is any strictly proportional relation between the atomic volumes of analogous positive elements and the heats of formation of their compounds. The result has been to show that in many cases the heats of formation of corresponding compounds of analogous positive elements with the same negative element are so related to the atomic volumes of the positive elements that the heats of formation, per *equal volumes* of the positive elements, are sensibly equal, and that in most other cases the heats of formation of the compounds, per unit volume (say per cubic centimetre) of their respective positive elements, exhibit a close approach to proportional relations of a very simple kind.

Take, for example, the case of lead and cuprosum. If we divide by the value of the atomic volume of Pb the values of the respective heats of formation of $PbCl_2$, $PbBr_2$, PbI_2, and PbO, and if we similarly divide by the atomic volume of Cu_2 the heats of formation of the corresponding cuprous compounds, we obtain quotients which compare together as follows:—

$PbCl_2$ .. .. .. 4·693 = 1·0
Cu_2Cl_2 .. .. .. 4·674 = 1·0

$PbBr_2$ .. .. .. 4·235 = 1·0
Cu_2Br_2 .. .. .. 4·237 = 1·0

PbI_2 .. .. .. 2·765 = 1·0
Cu_2I_2 .. .. .. 2·96 = 1·0

PbO .. .. .. 2·824 = 1·0
Cu_2O .. .. .. 2·881 = 1·0

We thus find that the quantity of heat liberated during the combination with Cl of a given volume of Pb, to form $PbCl_2$, and the quantity of heat liberated during combination with Cl of the same volume of copper, to form Cu_2Cl_2, are sensibly equal; and that the heats of formation, per equal volumes of the two metals, of $PbBr_2$ and Cu_2Br_2, of PbI_2 and Cu_2I_2, and of PbO and Cu_2O, respectively, are also sensibly the same.

An example of other simple proportional relations is furnished by calcium and strontium. The heats of formation of the haloid compounds of these metals, per cubic centimetre of each metal respectively, compare as follows:—

$SrCl_2$ .. .. .. 5·29 = 4·0
$CaCl_2$ .. .. .. 6·70 = 5·0

$SrBr_2$ .. .. .. 4·813 = 4·0
$CaBr_2$ .. .. .. 5·972 = 5·0

SrI_2 .. .. .. 3·610 = 4·0
CaI_2 .. .. .. 4·669 = 5·0

Table III. gives a large number of further examples. The second, third, and fourth columns of that Table give respectively the values employed for the atomic weights, the specific gravities, and the atomic volumes of the positive elements. In the succeeding columns the values first given after the symbols of compounds are those of their molecular heats of formation. The values next given are those of the quotients obtained by dividing the molecular heats of formation of the compounds by the atomic volumes of their positive elements. These quotients represent in each case kilogramme-calories per cubic centimetre of positive element. The numbers which follow, and which are comparable only where bracketed together, show the approximate proportions of these quotients to each other. In many cases the proportions of the quotients to each other are only approximately those of the numbers following them; but the approximations are in nearly all cases so close that, if we bear in mind that the values taken for the heats of formation cannot be regarded as being absolutely accurate, while the values taken for atomic weights and specific gravities are certainly also, in many cases, only more or less close approximations to the truth, it will be difficult to avoid the conclusion that between the quantities of heat liberated during the combination with the same negative element of equal volumes of analogous positive elements, very simple proportional relations certainly exist.

In many cases simple proportional relations are found also between the quotients obtained by dividing the molecular heats of formation of correspoding compounds of analogous positive elements with the same negative element, not by the atomic volumes of the positive elements only, but by the sums of the atomic volumes of both elements, the atomic volumes taken for the negative elements being the lowest they are known or believed to be capable of assuming in the free state. In the following examples the atomic volume of oxygen is taken at 16, in accordance with Dumas' theoretical conclusion respecting it, supposed by some to have been confirmed by the experimental results of Cailletet:—

$$\frac{\text{MgO aq}}{\text{Vol. Mg} + \text{Vol. O}} = 4·98 = 1·5$$

$$\frac{\text{ZnO aq}}{\text{Vol. Zn} + \text{Vol. O}} = 3·32 = 1·0$$

$$\frac{\text{ZnO aq}}{\text{Vol. Zn} + \text{Vol. O}} = 3·32 = 1·5$$

$$\frac{\text{CdO aq}}{\text{Vol. Cd} + \text{Vol. O}} = 2·2 = 1·0$$

Table IV., which is to be read in the same manner as Table III., compares the heats of formation, per cubic centimetre of each positive element, of B_2O_3, SiO_2, CO_2, and P_2O_5:—

TABLE IV.

B	11·0	2·68	(1)	4·104	B_2O_3	312·6	38·05	= 2·0	= 8·0
Si	28·0	2·49	(2)	11·2449	SiO_2	219·2	19·49	= 1·0	= 4·0
C	11·9	3·519	(3)	3·42	CO_2	100·0	29·237	= 2·0	= 6·0
P	31·0	2·34	(4)	13·205	P_2O_5	363·8	14·5	= 1·0	= 3·0

(1) Amorphous B. (3) Diamond.
(2) Amorphous Si. (4) Metallic P.

This Table shows that the quantities of heat liberated during the combination with oxygen of a cubic centimetre of boron, to form boric acid, B_2O_3, is to the quantity of heat liberated during the combination with oxygen of a cubic centimetre of silicon, to form silicic acid, SiO_2, very nearly as 2 to 1; that the quantity of heat liberated during the combination with oxygen of a cubic centimetre of carbon, as diamond, to form CO_2, is to the quantity of heat liberated during the combination with oxygen of a cubic centimetre of crystallised phosphorus, to form P_2O_5, also very nearly as 2 to 1; and that the heats of formation of B_2O_3, CO_2, SiO_2, and P_2O_5, per equal volumes of the respective positive elements, are to each other very nearly as 8, 6, 4, and 3. We have here a suggestion that the heats of formation of all compounds with the same negative element of non-metallic positive elements, whether belonging to the same family or not, are simple multiples of one unit quantity.

ON THE REFRACTION EQUIVALENTS OF THE DIAMOND, AND THE CARBON COMPOUNDS.*

By Dr. GLADSTONE, F.R.S.

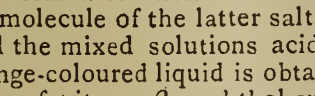
The specific refraction of a substance is its refractive index minus one divided by its density, and this, which has for some time been recognised as an important physical property, promises now to be of great service to the chemist in deciding the rational constitution of many compounds. The refraction equivalent of a substance is this specific refraction multiplied by the atomic weight. Now the refraction equivalent of crystallised carbon, or diamond, is 5·0, and that is the value of this element in the large majority of its compounds; but in certain cases—as, for instance, the whole of the aromatic series—the carbon has a higher refractive value. Brühl, in a recent elaborate paper, has put forward the theory that wherever there is a double carbon atom with bands latent, the refraction equivalent is raised by about 2·0. The author of the present paper supported this view by some of his own observations hitherto unpublished, but contended that it failed to explain several other cases of high specific refraction, as, for instance, the essential oils, whether of the $C_{10}H_{16}$ or $C_{15}H_{24}$ class, which give refraction equivalents about three above the normal; the compounds containing cinnamyl; and especially the hydrocarbons which are richest in carbon, such as naphthalene, anthracene, and pyrene, which have a specific refraction far in excess of that calculated on Brühl's theory. It is remarkable that, whereas the refraction of the carbon increases rapidly in these hydrocarbons as the proportion of hydrogen diminishes, it has only the normal value in diamond where there is no hydrogen at all.

NITROSO-β-NAPHTHOL-SULPHONIC ACID.

(Preliminary Note.)

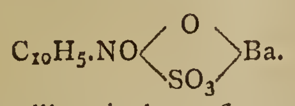
By RAPHAEL MELDOLA.

When a solution of a salt of β-naphthol-sulphonic acid (mono) is mixed with a solution of sodium nitrite in the proportion of 1 molecule of the latter salt to 1 mol. of the sulphonate, and the mixed solutions acidulated with dil. HCl, a deep orange-coloured liquid is obtained, from which the barium salt of nitroso-β-naphthol-sulphonic acid is precipitated on the addition of ammonia and $BaCl_2$. I have prepared and examined this nitroso-acid, and several of its salts and derivatives. It forms two classes of salts, typified by the barium compounds—



$$\left.\begin{array}{l}C_{10}H_5.NO.HO.SO_3\\C_{10}H_5.NO.HO.SO_3\end{array}\right\rangle Ba$$

And—



$$C_{10}H_5.NO\left\langle\begin{array}{c}O\\SO_3\end{array}\right\rangle Ba.$$

The former crystallises in large flat orange needles containing 1 molecule of water of crystallisation, and the latter in microscopic green needles containing 2 mols. of water of crystallisation.

The most characteristic reaction of the nitroso-sulphonic acid is that with the phenols and amines, which results in the formation of a series of red and violet colouring-matters.

I hope to give full details of the experiments upon this new acid and some of its derivatives in a paper shortly to be presented to the Chemical Society.

Atlas Works, Hackney Wick
September 27, 1880.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Note.—All degrees of temperature are Centigrade, unless otherwise expressed.

Correspondenz-Blatt des Vereines Analytischer Chemiker.
No. 9, May, 1880.

The Sophistication of Oils.—M. Maumené.—In this memoir, which will be concluded in a future number, the author criticises the methods of Lefebvre, Rousseau, Cailletet, Calvert, Poutet, Boudet, and Heidenreich, and finds them all unsatisfactory.

Quantitative Determination of Glycerin. — Prof. Liebermann has examined the methods of Griessmayer and Lallieu for the determination of glycerin in beer, and gives the preference to the former.

Artificial Diamonds.—A report on the crystals obtained by Mr. Mactear and Mr. Ballantyne Hannay.

Beer Analysis by Prof. Vierthaler, of Trieste.—H. Lintner.—The author criticises severely Prof. Vierthaler's report and analysis of a certain Austrian beer.

Chemiker Zeitung.
No. 15, April 8, 1880.

Rubidium-thallium Chloride. — On evaporating a mixed solution of thallium terchloride and rubidium chloride Godeffroy obtained large well-defined crystals of the double salt. An analogous cæsium thallium double chloride was obtained in a similar manner.—*Zeitschrift Oest. Apoth. Vereins.*

Occurrence of Ozone on the Evaporation of Different Liquids.—A few drops of ether or alcohol are let fall upon a paper equably moistened with cadmium iodide-starch solution, and the volatile liquids are set on fire. After their evaporation the paper is found to be turned blue from the formation of ozone.—*Polyt. Notizblatt.*

No. 16, April 15, 1880.

Value of Reverted Phosphoric Acid in Superphosphates.—H. and E. Albert.—The writers contend for the utility of these reverted phosphates, and call for more experimentation.

Behaviour of Infusorial Earth with Dyes.—Dr. Reimann.—With reference to G. Engel's memoir on this subject the author maintains that he had observed as early as 1870 that silica in every state is readily dyed in virtue of surface attraction.

Behaviour of Bone-glue on Dry Distillation.—G. L. Ciamician and H. Weidel.—The authors did not obtain nitriles of the fatty acids and pyridine bases, but pyrrol, homo-pyrrol, and further members of this series, especially a crystalline body which they name pyrocoll, a derivative of Schwanert's carbo-pyrrolic acid. It was not obtained from the albuminoids.—*Sitzber. Ak. Wissen. Wien.*

Determination of Fecula in Potatoes.—P. Behrend, M. Märcker, and A. Morgen.—The starch is liquefied by boiling for four hours with water at 135° to 140° (3 grms. air-dry, finely-powdered starch to 50 c.c. water. The hot filtrate is completely converted into sugar by mixing with 20 c.c. hydrochloric acid and digestion for three hours in

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* Communicated by the Author. Read before the Chemical Section of the British Association, Swansea Meeting, 1880.

the water-bath. After approximate neutralisation with potassa lye, and precipitation with 7 to 8 c.c. basic lead acetate, the liquid is made up to 500 c.c. In 200 c.c. of this solution, after the addition of 50 c.c. of sulphuric acid at 1 per cent, the sugar is determined by one of the methods proposed by the authors in the *Chem. Central-Blatt* (1878, p. 584) after the removal of the lead precipitate.

Bulletin de la Société d'Encouragement pour l'Industrie Nationale.

No. 77, May, 1880.

This number contains no original chemical matter.

MISCELLANEOUS.

Royal School of Mines.—Prof. W. Chandler Roberts, F.R.S., will deliver the Introductory Lecture to his course on Metallurgy, at 3 p.m., on Monday next, October 4th, at the Science Schools, South Kensington Museum.

Ozone.—MM. P. Hautefeuille and J. Chappuis have condensed ozonised oxygen at a pressure of 75 atmospheres and a cold of −23°. On removing the pressure signs of liquefaction appeared. In a condensed state, ozone is an indigo-blue gas. Full particulars will be given in next weeks CHEMICAL NEWS.

Trefriw Mineral Water.—An exceptionally well compiled Guide Book to North Wales, gives the following as the composition of the Trefriw mineral water :—" A strong solution of sulphate of iron and sulphate of alumina, with some sulphate of magnesia and soda and a little chloride of manganese, nickel, and *colchicum*." What a wonderful remedy for gouty and rheumatic patients!

City and Guilds of London Institute for the Advancement of Technical Education.—City Technical Science Classes.—

CHEMISTRY.—*Evening Classes.*—Dr. Armstrong will deliver a course of about thirty lectures on Organic Chemistty, with special reference to its industrial applications, on Mondays, 8.30 to 9.30 p.m., commencing October 4th. The first part of the course will chiefly consist of an account of the principles of Organic Chemistry. After Christmas, the Chemistry of the Coal Tar Products, their uses and the production of dyeing materials from them will be very fully considered. The Chemistry of Brewing, Spirit Distilling and Vinegar making processes will form the subject of the lectures after Easter. It is especially desirable that all who propose to attend either the second or third part of the course should also attend the first part, in order to gain the necessary preliminary knowledge. To give students attending the lecture an opportunity of making experiments in connection with the subjects treated of in the lectures, and also of discussing the lectures with the Professor, the laboratory will be open on Monday evenings at 6.30 to 8.30 o'clock. Students who shall have regularly attended the first part of this course, including the laboratory practice, and who shall on examination have given satisfactory evidence of proficiency, will receive a certificate which will entitle them to the full technological certificate on subsequently passing in any subject of the Technological Examinations of the Institute in which Organic Chemistry is one of the science subjects required. *Day Lectures.*—Dr. Armstrong will deliver a course of about twenty-four lectures on Tuesday and Friday afternoons at 4 to 5 o'clock, commencing October 5th. Although the chief object of these lectures is to afford such preliminary training as is necessary for those who may desire later on to study particular branches of Applied Chemistry, more than usual attention will be given to matters of technical importance. *Laboratory Classes.*—On and after October 4th, the laboratory will be open daily, Saturdays excepted, from 10 to 1 and 2 to 5 o'clock, for students desiring instruction in special applications of chemistry. A Laboratory Class for students attending the day lectures on chemistry, will be held on Tuesday and Friday afternoons at 1.30 to 4 o'clock. An Evening Laboratory Class will be held on Wednesdays at 6.30 to 9 o'clock, commencing Wednesday, October 6th. The individual requirements of students attending this class will as far as possible be considered. The Photographic Chemistry class will be continued on Wednesday evenings at 6.30 to 9 o'clock, commencing October 6th. It is proposed that students in this class should chiefly devote themselves to the study of Spectrum Photography and of Emulsion processes.

PHYSICS.—*Evening Courses.*—Professor Ayrton will deliver a course of evening lectures on Electrical Instrument Making, on Tuesdays at 8.30 to 9·30 o'clock, commencing October 5th, the first twelve of the lectures being given before Christmas. These lectures will be fully illustrated with experiments and practical illustrations, and especially adapted to the requirements of persons engaged, or intending to engage, in this industry. The practical details of the manufacture, adjustment and testing of electro-magnets, galvanometers, shunts, resistance coils, and condensers will be fully entered into, and the students will have the opportunity of performing themselves the experiments suggested at the lectures, as well as of obtaining information from the Professor regarding technical difficulties, by attending a special laboratory course, to be held on Tuesday evenings from 6.30 to 8.30 o'clock On Friday evenings, at 8.30 to 9.30 o'clock, commencing October 8th, Professor Ayrton will deliver a course of lectures, the first twelve being given before Christmas, on Weighing Appliances and Motor Machinery, adapted to the wants of makers and users of machinery. Lectures 1—3 will be especially suitable for Balance, Scale, Weighing Machine, and Dynamometer makers, and will form an introduction to the succeeding lectures, which will be on the relative efficiency of Steam, Hot Air, and Gas Engines, of Water Motors, and of Dynamo-electric Engines, and on the transmission of Power in factories and over long distances: the remaining lectures will be on Hydraulic Power Machinery. A special laboratory class for students attending this course will be held on Friday evenings from 6.30 to 8.30 o'clock. The physical laboratory will also be available on Tuesday and Friday evenings, from 6.30 to 9.30 o'clock, for students desirous of obtaining a practical knowledge of other branches of Technical Physics. *Day Lectures.*—Professor Ayrton will deliver a course of about 24 lectures on Monday and Wednesday afternoons, at 4 to 5 o'clock, commencing October 4th, on the Electric Light. *Day Laboratory Classes.*—A laboratory class especially suited for students attending the afternoon course of lectures will be held on Mondays and Wednesdays, at 1.30 to 4 p.m. Also on and after October 4th, the physical laboratory will be open daily (Saturdays excepted) from 10 to 1, and 2 to 5 o'clock, for Students desiring individual practical instruction in Technical Physics. Students attending regularly any of the above courses and passing a satisfactory examination will receive a qualifying certificate, entitling them to the full Technological Certificate of the Institute on passing the Technological examinations in allied subjects. Chemical and Physical Laboratories open daily (Saturday excepted) from 10 to 1, and 2 to 5 o'clock.

ERRATA.—Page 51 (July 30), col. 1, line 30, *for*—
$Sb^{-III}H^{+III}$, $As^{-III}H^{+III}$, *read* $Sb^{-III}H_3^{+III}$, $As^{-III}H_3^{+III}$

Col. 1, line 42, *for*—

$$Pb_3^{VI}(PO_4)^{-VI=0} \text{ read } Pb_3^{VI}(PO_4)_2^{-VI=0}$$

Col. 2, line 15, *for* $10H_2O$ *read* $5H_2O$.
,, ,, 20 ,, HNO_3 ,, $8HNO_3$.
,, ,, 40 ,, NO ,, 2NO.

THE CHEMICAL NEWS.

Vol. XLII. No. 1089.

ON THE SPECIFIC ROTATORY POWER OF CANE- AND INVERT-SUGAR.[*]

By ALFRED H. ALLEN.

According to the observations generally accepted, the angular rotation produced on a ray of polarised light by a plate of quartz 1 millimetre in thickness, is 24 angular degrees for the mean yellow ray or transition tint. In the construction of the polarising saccharimeter, which is known by his name, Soliel used a scale according to which these 24 angular degrees were divided into 100 parts, and he directed the user of the instrument to make a solution of the sugar of such strength that, if pure cane-sugar, a column of 20 centimetres in length would exert a rotatory power of exactly 100 solid divisions, or 24 angular degrees.

The formula commonly used for calculating the apparent specific rotatory power of optically active bodies in solution is as follows:—

$$S = \frac{a}{l \times \frac{c}{100}}$$

In this formula I have substituted S for the clumsy symbol $[a]$, which is usually used to denote specific rotatory power; a is the angular rotation observed; l, the length of the solution traversed by the ray of light (expressed in decimetres); and c is the concentration of the solution, or, in other words, the number of grammes of the solid present in each 100 c.c. of the solution.

According to the results of numerous observers, the value of S for cane-sugar is +73·8° for the mean yellow or transition tint. Substituting this value for S in the above equation, 24° for a, and 2° for l, we obtain:—

$$73{\cdot}8^\circ = \frac{24^\circ}{2 \times \frac{c}{100}}\ ; \quad \text{whence } c = 16{\cdot}26.$$

Hence the true quality of cane-sugar to be taken for use with Soliel's saccharimeter is 16·26 grms., and not 16·19, 16·35, or any other weight. If it be contended that either of these alternative quantities is the right one to employ, then it follows that +73·8° is not the true value of $[a]_j$, or S_j; for cane-sugar, though this is the figure used in all our books.

According to Tuschmidt, a solution of cane-sugar, which before inversion produces a deviation corresponding to 100 divisions of the Soliel scale (=24°), after inversion, if the observation be made at 0° C., causes a deviation of 44·16 divisions in the opposite direction. This result is closely concordant with that of Casamajor, who gives −44 as the deviation of a solution of inverted sugar, which before inversion read +100 Soliel divisions. Casamajor also states that the diminution in rotatory power for increased temperature is 0·5 division for each degree centigrade, the solution becoming optically inactive at 88° C. (not at 90° C. as usually stated). Hence at 14° C., the inverted solution will read −37 divisions, and at 15° C. −36·5 divisions. These conclusions closely correspond with those of Tuschmidt, who gives a formula according to which the deviation of the inverted solution at 14° C. is −37·09 divisions.

In calculating the specific rotatory power of invert-sugar, Watts and many other authorities have apparently fallen into an error.[*] They appear to have assumed that a solution of inverted sugar, after correction for change of bulk, contained the same percentage of solid as before inversion. This, of course, is contrary to fact, 95 parts of cane-sugar yielding 100 of invert-sugar. Hence this increase in weight must be taken into consideration in calculating the specific rotatory power of the inverted sugar. Thus, according to the data already given, at a temperature of 15° C.,—

$$S_j = \frac{-36{\cdot}5 \times 0{\cdot}24}{2 \times \frac{16{\cdot}26}{95}} = -25{\cdot}6^\circ.$$

This number corresponds to a value of −25·94° for S_j, at 14° C., instead of 25·0°, as is generally assumed.

If the number 16·19 be adhered to as the normal weight of sugar per 100 c.c. this value becomes,—

$$S_j = \frac{-37 \times 0{\cdot}24}{2 \times \frac{16{\cdot}19 \times 100}{100 \times 95}} = -26{\cdot}05 \text{ at } 14^\circ \text{ C.},$$

against −25°, as generally taken.

If the specific rotatory power of invert-sugar be taken as −26° at 14° C. (which is the mean of the above values), and O'Sullivan's figure +57·6 be adopted as the value of S_j for dextrose, then the specific rotatory power of lævulose at 14° C. is −109·6°, instead of −106°, as usually taken.

$$26 \times 2 + 57{\cdot}6 = 109{\cdot}6.$$

To sum up, the corrected values of S_j for cane-sugar, invert-sugar, dextrose, or lævulose are:—

	S_j.
Cane-sugar..	+73·8
Invert-sugar	−25·6 at 15° C.
Dextrose	+57·6
Lævulose	−108·8 at 15° C.

Broch has found the deviation produced by a thickness of 1 m.m. of quartz equal to 21·67° for the sodium ray, while Girard and De Luynes take the same value as 21·8. Brown and Heron adopt the number 21·54. Tollens gives the number 66·5 as the value of S_D for cane-sugar solutions containing 10 per cent and upwards, a value which corresponds to 21·63. The mean of these four determinations is 21·66. Hence the foregoing values of S_j for the different varieties of sugar may be calculated into the corresponding values for S_D by the factor—

$$\frac{21{\cdot}66}{24} = 0{\cdot}9025.$$

AN ECONOMICAL PROCESS FOR TREATING MINERAL AND OTHER SUBSTANCES CONTAINING SMALL QUANTITIES OF PHOSPHATE OF LIME.

By C. H. ALLDRED, F.C.S.

Having during the summer been making some investigations upon the decomposition of mineral and other phosphates of lime, the following results may be of interest. In trying nearly all the reagents capable of dissolving $3CaOP_2O_5$ I used a solution of SO_2 in water. This, as is well known, although it readily dissolves an artificially precipitated $3CaOP_2O_5$, does not so easily dissolve a mineral phosphate which is intimately mixed with SiO_2, CaO, Fe_2O_3, $CaSO_4$, &c., especially if it is an artificial product which has been heated or fused; and if the solution is warmed to assist decomposition, of course the SO_2 is driven off, and any phosphate of lime which had been

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[*] A paper read before the Chemical Section of the British Association, Swansea Meeting, 1880.

[*] Even in the new edition of "Miller's Organic Chemistry," edited by Armstrong and Groves, the specific rotatory power of invert-sugar is wrongly stated.

taken up is precipitated as a bisulphited phosphate of lime, which remains insoluble, mixed with the material under treatment. Thus SO_2 cannot be practically employed in this manner. To prevent the occurrence of this difficulty I tried the following plan:—The material employed was a refuse product containing about 22 per cent of $3CaOP_2O_5$, 12 per cent of Fe_2O_3 and Fe_3O_4, and the remainder consisting of CaO, SiO_2, MgO, &c. This was finely ground, and placed in a small boiler lined with lead, fitted with a hot-water jacket. It was then nearly filled with a saturated solution of SO_2 in water. The boiler was then firmly closed up, and the whole heated until the water in the outer jacket (which was not under pressure) boiled, and kept at this temperature for five or six hours; it was then allowed to cool, and the contents filtered. The filtrate contained a large amount, nearly the whole (but a quantitative experiment was not made), of the $3CaO+P_2O_5$, and only a small amount of iron. The former can be precipitated either by adding CaO, or, better, by boiling, when it is precipitated in a fine white powder as a nearly pure bisulphited phosphate of lime, which has a high value as a fertiliser. In putting this process into practical application it would only be necessary to have two boilers lined with lead or other suitable metal. Into the first (A) is charged the phosphatic material, and the solution of SO_2 then heated, but by means of a water-jacket, up to 100° C., when there will be a pressure of about 20 to 30 lbs. per square inch. After being kept at this temperature for, say, six hours, a tap is opened near the bottom, connected with a pipe which leads to the boiler (B), which should be placed on a lower level than A. When this is filled with the liquor it is boiled, the expelled SO_2 being led off from an escape pipe, which will conduct the gas into cold water, where it may be re-absorbed, and the solution be used for the treatment of a fresh batch of raw material. The liquor and precipitated bisulphited phosphate of lime in the boiler B can be run off into settling tanks, when the phosphate can be filtered, pressed, and dried. In this process the SO_2 would to a great extent be recovered, the only loss being that combined with the phosphate of lime and the loss from leakage. To make up these losses, for the supply of SO_2 I propose preparing it from pyrites, sulphur, or H_2SO_4 and coke.

The adoption of this process offers an economical means of treating many substances containing $3CaO,P_2O_5$ in quantities too small to pay for treatment by acids, and especially for localities where large deposits of phosphates occur, but too poor to pay for transit in their natural state.

106, Leadenhall Street, E.C.,
September 30, 1880.

A SIMPLE PROCESS OF SLOW ACTINOMETRY.

By ARTHUR DOWNES, M.D.

Dr. Leeds's paper, published in the Chemical News, page 147, induces me to submit the following remarks:—Of the various methods of actinometry hitherto suggested there is not one, so far as I am aware—though on this point I would speak with all diffidence—which is adapted for the measurement of solar chemical work for weekly or monthly periods, and which is at the same time so simple that it may be used by anyone with little trouble and without necessity for elaborate apparatus. In 1877-78 some experimental observations by Mr. Blunt and myself* were published demonstrating the destructive action of sunlight upon bacteria and other low organisms, and we adduced evidence to show that this was due to excessive oxidation, under insolation, of their protoplasmic substance. In the course of this enquiry we discovered that a decinormal solution of oxalic acid was after a certain interval entirely decomposed by sunlight by oxidation, thus—

$$2C_2O_4.H_2+2O_2=4CO_2+2H_2O+O_2;$$

the view taken in the above formula being that in sunlight the affinity of oxygen for the basylous hydrogen is so stimulated as to exceed that of the chlorous radicle C_2O_4, just as, under other circumstances, oxygen itself may be superseded by chlorine. In darkness the balance is not disturbed, and ordinary oxygen has no power to displace the oxalyl radicle. Solutions of oxalic acid may be preserved for long periods if the light be carefully excluded, and care be taken to prevent decomposition by fungi.

Wishing, in case I had an opportunity of resuming certain points in connexion with our original enquiry, to have the means of expressing arithmetically the oxidising power of sunlight during periods of days, weeks, or months, it occurred to me, early in the present year, to turn this oxalic acid reaction to account.

Hitherto want of time and other circumstances have prevented me from working out the problem fully. My procedure has been to expose in small-sized test-tubes a measured quantity of decinormal oxalic acid, and, after the desired period of insolation, to determine the amount of oxidation by direct titration with permanganate. In this way I have made weekly observations since last March, and, rough as these are, the result leads me to believe that this process may be made one of great value to the meteorologist, agriculturist, and sanitarian.

The chief difficulties in the way of its application appear to be, first, that the tubes employed must be as nearly as possible of identical section-area, the extent of surface exposure being, as may be expected *à priori*, a most important factor in the result. Until within the last few days I have, for various reasons, not been able to obtain a set of tubes fulfilling this essential requirement. Mr. Casella, of Holborn, has, however, with great care selected me some which, save for the thickness of the glass, appear likely to answer my purpose excellently. They have a sectional area of 4 square centimetres. Another difficulty which I hope to overcome or reduce to a minimum is the apparently varying rate of oxidation according to the concentration of the solution.

To both these sources of difficulty and error it appears to me that both Dr. Leeds's potassic iodide process and the ferric oxalate process, suggested by Draper years ago, add another from which the oxalic process is quite free, viz., variation of colour.

I may note in conclusion that in a paper* "On the Behaviour of the Alkaline Oxalates in Sunlight," read June 19, 1879, Mr. Blunt and I remarked that we had satisfied ourselves, first, that in a Sprengel vacuum a 10 per cent solution of pure potassic iodide remains perfectly colourless in sunlight; and, secondly, that decomposition invariably occurs in an insolated solution when no other gases than nitrogen and oxygen are present. We concluded, therefore, that the well-known decomposition of potassic iodide in sunlight was due to the oxidation of the alkali metal.

THE STABILITY OF CALOMEL.

By PHIL. HOGLAN, Ph.G.

In the *Druggists' Circular* for August, 1880, is a statement from M. Verne, a French pharmaceutist, on the Stability of Calomel, which statement is copied from the *Bulletin de Thérapeutique*. M. Verne finds that calomel mixed with chloride of sodium, sugar, or citric acid undergoes no change, and that therefore the asserted danger of prescribing them at the same time is fictitious. The conversion of calomel into corrosive sublimate by the chlorides of the alkaline metals, and also by the organic acids, has for a long time been a much vexed question; and the purpose of the following experiments was to discover the cause of the great discrepancies on this subject, and also to ascertain the correctness of M. Verne's assertion that "the

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* *Proceedings of the Royal Society*, No. 184, 1877; and No. 191, 1878

* *Proc. Roy. Soc.*, No. 197, 1879.

danger of acid drinks when using calomel is pure prejudice."

Experiments.

A. Calomel was mixed with a solution of chloride of sodium, and after standing ten days, at the temperature of 78° F., the filtered liquid gave no evidence of the presence of corrosive sublimate by the stannous chloride test.

B. Calomel and citric acid were treated in the same manner, and after fifteen days the filtered liquid gave a slight greyish tinge with stannous chloride, indicative of the presence of corrosive sublimate.

C. Calomel and sugar were treated in the same manner, and after fifteen days no evidence of corrosive sublimate could be found in the filtered liquid.

D. Calomel and solution of chloride of sodium were maintained at a temperature of 98° F. (temperature of the body) for one hour, when the filtered liquid gave evidence of the presence of corrosive sublimate.

E. Calomel and sugar and calomel and citric acid, treated in the same manner at the same temperature, also gave a reaction indicating the presence of corrosive sublimate.

F. Calomel and water, treated in like manner for three hours at 98° F., gave a very slight reaction with stannous chloride. Not so marked as previous ones.

These experiments show, first, that calomel is slowly converted into corrosive sublimate by water at the temperature of the body. Secondly, that chloride of sodium, citric acid, and sugar greatly promote the conversion of calomel into corrosive sublimate, and hence are more or less dangerous when present in the system with calomel. Thirdly, that the discrepancies in regard to the stability of calomel are, in part, accounted for by taking into consideration the temperature at which the experiments have been conducted. Fourthly, that at the temperature of the human body calomel is an unstable compound.

Newcomerstown, Ohio,
September 10, 1880.

ON THE LIQUEFACTION OF OZONE, AND ON ITS COLOUR IN THE GASEOUS STATE.

By MM. P. HAUTEFEUILLE and J. CHAPPUIS.

Ozone, as commonly prepared, possesses in oxygen so small a tension, 53 m.m. at most, that its physical properties are scarcely known and distinguished from those of oxygen. We know the difficulties which M. Soret has so skilfully overcome in determining the density of ozone by operating upon oxygen slightly ozonised.

Among the physical constants of this body its formation-heat has been accurately determined by M. Berthelot, notwithstanding the degree of dilution in which it occurs when issuing from the effluve apparatus. The preparation of a mixture very rich in ozone is the first condition to be fulfilled in order to acquire new notions concerning this curious substance. We have formerly established that the isomeric transformation of oxygen submitted to the electric effluve follows simple laws, and that the proportion of ozone increases very little with the pressure for each temperature, whilst it is quintupled in passing from 20° to −55°. If withdrawn from the action of the electric discharges the mixture of ozone and oxygen ceases to be a homogeneous system in equilibrium. Nevertheless the mixture remains without appreciable alteration during the whole time that a constant temperature is maintained, operating below 0°. This relative stability of ozone has enabled us to compress the mixture, and to obtain tensions of ozone of several atmospheres.

As it is necessary to prepare the ozone designed for these experiments at the highest tension possible, oxygen must be ozonised at a very low temperature. Consequently it is kept for a quarter of an hour in an apparatus for alternative discharges, the slender concentric glass tubes of which are plunged in methyl-chloride; then it is caused to pass into the cylinder terminated by a capillary tube of Cailletet's apparatus. This reservoir, of about 60 c.c. capacity, originally a vacuum, and kept at −23°, cannot be filled at once under a pressure bordering upon 760 m.m., and is rapidly connected 5 times in succession with the effluve apparatus, the capacity of which cannot exceed 20 c.c. In an hour and a quarter we succeed by this means in filling the cylinder with a mixture of oxygen and ozone very rich in the latter gas.

The cylinder is then taken out of the methyl-chloride, and separated from the effluve apparatus by a stroke of the file; the gas which it contains is slowly driven back by means of mercury cooled down to 0° into the capillary tube, which is kept at −23°.

The mercury which transmits the action of the hydraulic press does not impoverish the gaseous mixture as rapidly as might be apprehended; there is formed on the surface of the metal a solid varnish, which rapidly limits the action; the heating of the gas during compression is more to be feared. Notwithstanding these difficulties we succeed in increasing the tension of the ozone in a very high proportion. From the first strokes of the piston the capillary tube becomes a sky-blue. This colouration becomes more marked in proportion as the volume of the gas is reduced, and if the tension of the gas is brought by compression to several atmospheres the gas is an indigo blue, and the meniscus of mercury, seen through the gas, a steel-blue. The blue colour of the gas becomes less intense, and the mercury resumes its ordinary metallic aspect when the tension of the ozone is diminished.

The above mixture contains enough ozone to permit the observation of a dense white mist at the moment of release after a compression of 75 atmospheres. It is therefore not necessary to compress ozonised oxygen as much as pure oxygen (300 atmos.), in order that a sudden release may occasion the momentary formation of a mist, the certain sign of liquefaction or even solidification. A comparative study of mixtures of oxygen and ozone, and of oxygen and carbonic acid, shows that under conditions which are fairly comparable the release must be stronger for ozone than for carbonic acid in order to make a mist appear. Ozone is therefore a little less easily liquefied than carbonic acid.

The mixture of oxygen and ozone, being an explosive gas, should always be compressed slowly and refrigerated. If these conditions are not observed the ozone is decomposed with the liberation of heat and light, and there is a strong detonation attended with a yellowish flash. M. Berthelot has shown that the transformation of oxygen into ozone absorbs 14·8 cals. per equivalent ($O_3 = 24$ grms.). Ozone therefore ranks among the explosive gases, and our experiments show that like them it is capable of a sudden decomposition. A part of these new facts may also be observed in oxygen which has been passed slowly at common temperatures through an effluve apparatus. For if the gas is rapidly compressed in a capillary tube placed in water at 25° the ozone is often destroyed with explosion. But if the same gas is cooled down to −23° the ozone which it contains may be brought to a tension of 10 atmospheres, and may be preserved for hours under these conditions of pressure and temperature if the gas is separated from the mercury by a column of sulphuric acid. We observe then almost as distinctly as in the former experiment, which is more difficult to perform, that ozone is a gas of a beautiful sky-blue. Its colour at a tenfold density is so intense that we have been able to see it in a tube of 0·001 metre in diameter when operating in a very badly lighted room of the laboratory of the Ecole Normale.

It is therefore ascertained that under a strong pressure ozone is a coloured gas, but is it the same with ozone at the tension of a few millimetres? The blue colour is as characteristic of ozone as its odour, for at all tensions it is recognised on examining a stratum of the gas of sufficient depth. In order to render it apparent it is merely needful to interpose between the eye and a white surface a tube of 1 metre long traversed by the current of oxygen which has passed through Berthelot's effluve apparatus. The colour of the gas then resembles that of the sky, and is deeper or lighter according as the oxygen has remained a longer or shorter time in the apparatus, and is consequently more or less rich in ozone. As soon as the effluve is interrupted the blue colour disappears, the ozonised oxygen being replaced by pure oxygen.—*Comptes Rendus.*

[The authors add in a note that in a future communication they will make known the part which this coloured gas may play in the atmosphere, and what may be its effects upon the different radiations.]

NOTES OF WORK BY STUDENTS OF PRACTICAL CHEMISTRY IN THE LABORATORY OF THE UNIVERSITY OF VIRGINIA.

No. IX.

Communicated by J. W. MALLET,
Professor of General and Applied Chemistry in the University.

(53.) *On the Existence and Properties of Phosphorus Pent-Iodide.* By FRANK HAMPTON, of Columbia, South Carolina.

IT is usually said that phosphorus unites with iodine in but two proportions, forming a di-iodide and tri-iodide, but no pent-iodide corresponding to the higher bromide or chloride. In Gay Lussac's chief memoir[*] upon iodine, however, containing the results of the experiments made by him with the newly-discovered element of Courtois, he states that on bringing together 1 part of phosphorus and 16 of iodine a grey-black crystallised substance was formed, fusible at 29° C., and producing on contact with water a colourless solution which contained hydriodic and phosphorous acids; while 1 part of phosphorus with 24 of iodine gave a black substance, fusible "*en partie*" at 46°, and dissolving in water with elevation of temperature, the solution containing hydriodic and *phosphoric* acids and exhibiting a dark brown colour from the presence of free iodine. Mr. Hampton undertook to examine the latter of these reactions, and to ascertain if possible with certainty whether phosphorus pent-iodide can really be obtained.

He made several experiments with concordant results. In some the two elements were brought together in a neutral atmosphere of pure nitrogen, without any other additional substance being present, but it was found difficult to regulate the violence of the action and to prevent objectionable rise of temperature. In others, therefore, carbon disulphide was used to dissolve and so dilute the phosphorus before the addition of the iodine; and with proper precautions this was found to produce no change in the final result, while rendering the process much more easily manageable. The following notes were made of one of the experiments by this latter method, which was carried out with all possible care:—

A supply of nitrogen was prepared by burning phosphorus in bell-glasses filled with air, and the gas stored in a large gas-holder. It was purified by passing it first through a long tube coated on the inside with phosphorus maintained at a temperature a little above its melting-point, then through a solution of potassium hydrate, then over pumice-stone soaked with a stronger solution of the same, then through strong sulphuric acid, and finally over pumice-stone moistened with pure sulphuric acid of full strength. A slow stream of this gas was passed continuously through a flask, of 300 c.c. capacity, furnished with inlet and outlet tubes, in which flask 0·995 grm. of ordinary crystalline phosphorus was dissolved in a few cubic centimetres of carbon disulphide. 41·589 grms. of pure iodine were added in portions, with due precaution against any admission of air; this being a little in excess of the 40·767 grms. required by the formula PI_5. When the solution of the iodine was complete, the carbon disulphide with most of the excess of iodine was gotten rid of by distillation at 45° C. under slightly reduced pressure, the flask being immersed in a large vessel of water kept at the temperature named, and the outlet tube connected with a vessel in which exhaustion to the extent of a few inches of water was produced by a filter-pump. To remove the last traces of iodine without allowing it to condense upon the neck of the flask, a U-shaped tube of about 5 m.m. in diameter had been previously passed through the cork closing the flask, with the bend inside reaching nearly to the bottom, and through this tube a current of cold water was circulated. When all of the iodine removable at 45°, with the exception of mere traces, had been driven off, the temperature was raised to 50°, and being kept for some time at this, the material at the bottom of the flask was slowly and in small quantity volatilised under the reduced pressure, the last portions removed on the exterior of the U-shaped condensing tube proving to contain phosphorus in sensible amount, and giving the same results on decomposition with water or an alkaline solution as the main portion left behind in the flask. On raising the temperature still further to 55° signs of decomposition became apparent, purple vapours of iodine being again given off, and the appearance of the residue changing at the points most heated. The flask was now quickly cooled, keeping up the stream of nitrogen, and immediately afterwards a solution of sodium hydrate, from which all oxygen had been expelled by previous boiling, was carefully run in through a tube provided for the purpose and hitherto kept closed, avoiding any entrance of air, until the alkali was in very slight excess. The flask was cooled from the outside with ice-water to prevent rise of temperature, to which there was a strong tendency. These precautions were necessary in order to negative the assumption afterwards that phosphorous acid or its sodium salt had been first formed and then converted by heat or the presence of oxygen into the corresponding phosphoric compound. Ammonio-chloride of magnesium, as commonly used for the precipitation of phosphoric acid, was now run in as long as it produced a precipitate, and this precipitate was filtered off without access of air. The bulk of the precipitate being too large for accurate ignition and weighing, it was re-dissolved in hydrochloric acid, and phosphoric acid determined in a definite fraction of the solution. The filtrate from the original precipitate was also brought to a definite volume, and in one fraction of it iodine was determined as iodide of silver, while in another the phosphites were oxidised to phosphates by means of nitric acid, and the phosphoric acid determined as magnesium and ammonium phosphate.

The following results were obtained:—

Phosphorus found present in original alkaline solution as sodium phosphate ..		1·371 grms.
Ditto, ditto, as sodium phosphite		0·562 ,,
		1·933 ,,
Hence, phosphorus volatilised in heating materials together		0·062 ,,
Total phosphorus employed		1·995 ,,
Total iodine found present in alkaline soln.		35·040 ,,
1·371 grms. P requires for PI_5 (calc.)	28·016 grms. of I	
0·562 grm. P requires for PI_3 (calc.)	6·890 grms. of I	
	34·906	34·906 grms.

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[*] *Annales de Chemie*, 91, 10 (August 1, 1814).

The apparent slight excess of iodine (the alkaline solution having been colourless) undoubtedly represents a real but much smaller loss of phosphorus in the two determinations of the latter.

It appears, therefore, that phosphorus pent-iodide is obtainable by the direct union of its elements, and that in this particular experiment the mass remaining in the flask, after partial decomposition from over-heating had taken place, consisted approximately of—

PI_5	80·0
PI_3	20·0
	100·0

Taking the material in the flask as it appeared when decomposition had not yet set in, or had occurred to but a trifling extent, the pent-iodide formed a very dark crimson, almost black, mass of crystalline structure, made up seemingly of thick needles or slender prisms, unlike the stouter crystals or flat scales of iodine itself. Its limit of stability was found pretty close to 50° C.; above this temperature it decomposed, giving off iodine, and becoming reduced to the tri-iodide. This dissociation is of course paralleled by that experienced at successively higher temperatures by the penta-bromide and penta-chloride. At 50°, as has been stated, the pent-iodide was under reduced pressure volatilised in minute amount, the small quantity collected on the condensing tube giving on treatment with water phosphoric as well as phosphorous and hydriodic acids. On simple exposure to air of the main product from the flask it absorbed moisture, and gave off heavy yellowish fumes of suffocating odour. In one experiment, in which carbon disulphide was used, and the attempt made to distil it off under full atmospheric pressure, a red substance was noticed in small quantity, which may have been identical with that observed by H. Jahn* and Baeyer.†

(54.) *On the Composition of Dufrenite from Rockbridge Co., Va.* By F. A. MASSIE, of the University of Virginia.

During the last three or four years several American dealers in minerals have sold specimens labelled "Dufrenite, Rockbridge Co., Va.," but I have been unable hitherto to find out upon whose authority the determination of species rests, by whom, at what precise locality, or under what geological conditions the mineral in question was found. No description or analysis having been published, so far as I know, a good specimen was placed in Mr. Massie's hands for examination.

The mineral occurs in nodular masses of various sizes, up to several inches in diameter, showing well-marked radiated structure on being broken, the mass being made up of slender prisms closely compacted. Hardness = a little over 4; sp. gr. = 3·454; faint silky lustre; colour very dark blackish green, almost black, streak greyish green. On the outside of the nodules a layer, 5 to 10 m.m. thick, of altered mineral has a red-brown colour. Nearly opaque; readily fusible before the blowpipe flame; gives off water when heated to redness in a glass tube, slightly decrepitating, and gradually changing to a red-brown colour; soluble in strong hydrochloric acid. Choice portions of the mineral, appearing under a lens quite homogeneous, were selected for analysis.

Qualitative chemical examination proved that the iron exists partly in the ferrous and partly in the ferric state, and that neither calcium nor the alkaline metals, arsenic nor fluorine, is present. The ferrous iron was determined in a portion of the mineral decomposed by strong sulphuric acid in an atmosphere of carbon dioxide. Phosphoric acid was determined by means of ammonium molybdate, and manganese by bromine. Full analysis yielded the following results, two or three of which were obtained by averaging several closely accordant determinations. Dried over sulphuric acid 0·03 per cent, and heated in a steam-bath 0·8 per cent, of water, manifestly non-essential, was lost and the residue contained—

Ferric oxide	50·89
Alumina	0·29
Ferrous oxide	6·06
Manganous oxide	0·24
Magnesia	2·16
Phosphorus pentoxide	31·66
Silica	0·20
Water	8·35
	99·85

The minute amount of silica probably exists as quartz being left as a gritty powder undissolved by hydrochloric acid. Neglecting this, and counting the little aluminium along with ferric iron, and manganese and magnesium with ferrous iron, the above figures correspond pretty nearly to a mixture of normal dufrenite,—

$$Fe'''_2(HO)_3PO_4,$$

with anhydrous ferrous pyrophosphate,—

$$Fe''_2P_2O_7,$$

in about the ratio of 3 molecules of the latter to 4 of the former. It would require but little alteration of the figures, however, to make the residue ferrous *ortho-* instead of pyro-phosphate, which is more probable. It is to be noted that in analyses already on record of dufrenite from Siegen* and from Allentown, New Jersey,† ferrous iron has been found, but no notice is taken of this in the formula usually written for the mineral.

(To be continued.)

CHARACTERISTIC TESTS OF BEES'-WAX, VEGETABLE AND ARTIFICIAL.‡

By H. HAGER.

THE analysis makes the following processes necessary:—

1. *Determining the Specific Gravity.*—In case the mass does not contain moisture, cut off five or more small pieces or melt a few grammes in an evaporating dish supplied with a lip, drop the wax on a pane of glass, which has previously been wiped off with a moist cloth, and place the glass into cold water. The drops will either readily drop off at the slightest touch, or are removed at the expiration of half an hour with a knife. The specific gravity is then determined as usual by the floating test, which consists in mixing alcohol and water in such a proportion that the wax-drops will float in the mixture without showing a tendency to rise or sink. The drops containing air-bubbles can be readily recognised and are rejected. The specific gravity of pure yellow or white wax lies between 0·956 and 0·964, usually between 0·958 to 0·960. If, therefore, the specific gravity of the suspected wax is not between 0·956 to 0·964 it is very probably adulterated. Wax containing an admixture of stearic acid, resin, or Japan vegetable wax has usually a higher, and wax adulterated with tallow, paraffin, or ceresin, a lower specific gravity.

2. *Dissolving in Hot Chloroform or in a Fatty Oil.*—The solution of dry bees'-wax is clear, but slightly cloudy in case the wax was moist, but ought not to contain any sediment, which, if present, ought to be collected and determined (mineral substances, starch, &c.).

3. *Borax Test.*—6 to 8 c.c. cold saturated borax solution are heated in a test-tube with a piece of the suspected wax as large as a bean, until it melts, when the mixture is shaken gently. The aqueous liquid becomes slightly cloudy if the wax is pure bees'-wax, but never becomes milky. When set aside for cooling the layer of wax collects

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* *Ber. der Deutsch. Chem. Gesell.*, 13, 127, 135.

† *Ann. de Chem.*, 155, 266.

* Schnabel—Rammelsberg; *Hand. d. Min. Chem.*, 329.

† Kurlbanna, *Amer. Jour. Sci.*, ii., 23, 423.

‡ Translated and condensed from *Pharm. Centralh.*, April 8, 1880 pp. 119-121, by Louis von Coltzhausen, Ph.G.

on the surface of the liquid, leaving the latter almost clear, or only slightly turbid or half translucent. If it becomes milky immediately, and remains translucent and milky even after cooling, either Japan vegetable wax or stearin are present. Resin and Brazil vegetable wax act like pure bees'-wax in this test.

4. *Soda Test.*—Mix in a test-tube a piece of wax, the size of a bean, and 6 to 7 c.c. sodium carbonate solution (1 salt, 6 water). Heat in such a manner that the upper layer of the liquid is heated without agitation until the wax is melted. If much froth is noticed at the wax layer stearic acid is probably present. The whole is then heated to the boiling-point. If the liquid becomes milky, milky-yellowish, or milky-white, Japan wax or stearic acid is present. In this case the aqueous liquid remains milky, even after cooling; in case Japan wax is present, more or less pasty, or even stiff, while if stearic acid is present the milky layer is usually liquid, and often appears crossed by clear layers. The wax layer on the surface is usually brittle and not hard.

Pure bees'-wax appears hard after cooling slowly, and the aqueous layer is cloudy, but not milky, frequently semitranslucent and entirely liquid.

If the bees'-wax contains pine resin this test will cause an entire separation of the resin. Instead of merely heating to the boiling-point, allow to boil for one minute, and then to cool gradually. The mass then consists of three layers, an upper hard wax layer, a liquid, slightly turbid, middle aqueous layer, and a loose or flocculent resin layer on the bottom. The resin can then, of course, be readily determined quantitatively.—*American Journal of Pharmacy.*

NOTICES OF BOOKS.

Elements of Chemistry, Theoretical and Practical. By William Allen Miller, M.D., D.C.L., LL.D. Revised and in great part re-written by H. E. Armstrong, Ph.D., F.R.S., Sec. Chem. Soc., and Charles E. Groves, F.C.S., Sec. Inst. Chem. Part III.—Chemistry of Carbon Compounds, or Organic Chemistry. Section 1.—Hydrocarbons, Alcohol, Ethers, Aldehydes, and Paraffinoid Acids. Fifth Edition. London: Longmans and Co.

The reviewer has far more difficult tasks than to deal with re-issues of a work which has once for all established itself in public opinion, since censure, praise, and mere exposition have each and all been anticipated. The present case is, however, somewhat exceptional, since the organic portion of Prof. Miller's work has been almost entirely re-written by the editors, who have merely retained such portions of the original as have remained unaffected by recent discoveries.

The editors remark that since the appearance of Kekulé's "Lehrbuch" no comprehensive work on the organic compounds has been attempted. Hence their task has been far from easy. They hope, however, that the summary which they have given of the carbon compounds, as they are regarded by modern chemistry, will be found trustworthy. They consider that "Mendelejeff's so-called periodic law may be regarded simply as an extension of the laws which govern the carbon compounds to the elements generally." They give the opinion that what is most required in chemistry is "the accurate and complete examination of individual carbon compounds, for chemists have hitherto too often been satisfied if they simply prepared a given substance, analysed it, and described a few of its reactions; a compound is seldom obtained in quantity sufficient to admit of its complete purification and the subsequent determination of its physical constants. These constants can be most advantageously determined by those who have the necessary apparatus at their command, and, above all, special experience in such matters; and if each chemical school would make the preparation by its students of certain substances in a state of purity a part of the ordinary course of study, and would give notice that these particular compounds were at the service of experts for the determination of physical constants, much would be done to facilitate the study of a comparatively neglected branch of science."

This is, we consider, a most excellent suggestion; the preparation of pure bodies would be in itself a wholesome discipline for the students, and many researches would be undertaken which are now shunned on account of the loss of time involved in obtaining pure materials.

The first chapter of the work is devoted to organic analysis, ultimate and proximate, the determination of vapour densities, and their use in ascertaining molecular formulæ.

In the second chapter the editors treat of quantivalence, the classification of the carbon compounds, and the theory of compound radicles and of chemical types. They give in a note the caution as "scarcely necessary" that the "graphic illustrations employed to aid the conception are not intended in any way to represent the relative position of the atoms, but merely to show whether the compound is a saturated or unsaturated group or molecule." We fear that this caution is very often more or less consciously lost sight of by chemists. The theory of organic radicles they also very prudently declare "cannot be supposed to afford a correct representation of the constitution of organic bodies, but it is a convenient fiction which materially facilitates the retention of their composition in the memory, and the comprehensions of the changes which such compounds undergo." From this point of view it becomes utterly unimportant whether an organic radicle can be isolated or not, or even whether it would be capable of independent existence, or what would be its behaviour in contact with the substances which it accompanies.

The third chapter treats of the action of reagents on carbon compounds. Here the authors lay due emphasis on the value of limited oxidation in the study of organic compounds.

Into the remaining chapters of the work, which are devoted to a description of the principal classes of organic bodies, we cannot here enter.

In Chapter X., § 11, the editors take into consideration the insufficiency of the present theory of isomerism, which at any rate does explain the case of the terpenes and citrenes,—sets of bodies which are chemically undistinguishable, but whose optical behaviour is quite different. They then proceed to an examination of the asymmetric carbon atom hypothesis proposed by Van 't Hoff and Le Bel, which, however, they consider scarcely sufficient.

A companion volume is to contain the acid of the aromatic or benzenoid group, the basic nitrogenous compounds, and the organo-metallic compounds. The whole will undoubtedly form an admirable compendium of organic chemistry.

Bulletin of the Washington Philosophical Society. Vol. I., March, 1871, to June, 1874; Vol. II., October 10, 1874, to November 2, 1878; and Vol. III., November 9, 1878, to June 19, 1880.

The word "philosophical" in the title of the Washington Society is used, we are told, "not to denote, as it generally does in the present day, the unbounded field of speculative thought which embraces the possible as well as the actual of existence, but to be used in its restricted sense, to indicate those branches of knowledge that relate to the positive facts and laws of the physical and moral universe." We should have thought that "Scientific Society" would have been the better name.

Among the memoirs which have been laid before this learned body there are not a few which were of great interest at the date of their publication, but which have become so well known that any abstract or criticism would now be quite out of date. Of many other papers we find here merely the titles, with the intimation that they have been published elsewhere. The memoirs to which the greatest proportion of space is accorded seem those treating on mathematics and on certain portions of political economy.

Prof. Joseph Henry's discourse on the Method of Scientific Observation (vol. ii., p. 163) contains exceedingly sound views. The death of the speaker followed very shortly after, and the remainder of the volume is taken up with an account of his life and researches.

In vol. iii. mention is made of a paper by Mr. L. F. Ward, on the "Origin of the Chemical Elements," but it is unfortunately not inserted even in abstract. About a fourth part of the volume is devoted to a paper and accompanying discussions on the "Silver Question." Chemistry, as far as can be judged from the printed papers, does not seem a favourite subject with the Philosophical Society of Washington.

If we might suggest, these volumes would be greatly improved by the addition of an index or a table of contents.

Traité d'Anæsthésie Chirurgicale, contenant la Description et les Applications de la Méthode Anæsthésique de M. Paul Bert. Par le Docteur J. B. Rottenstein. Paris: Germar Baillière et Cie. (Treatise on Surgical Anæsthesia, containing the Description and the Applications of the Anæsthetic Method of M. Paul Bert.)

This work belongs to the sphere of practical surgery rather than to that of chemistry. After a survey of the rise and progress of anæsthesia, the author describes the properties and preparation of nitrous oxide, ether, chloroform, amylene, and chloral, and passes thence to a consideration of their respective advantages and of the appliances for their administration. He gives the preference to M. Paul Bert's, which consists in the inhalation of a mixture of equal volumes of air and nitrous oxide under the pressure of two atmospheres. For very brief operations nitrous oxide may be used alone. Where Bert's apparatus is not to be had, the author recommends the method of Clover,—*i.e.*, the initial use of nitrous oxide followed up by ether. Chloroform should only be employed under exceptional circumstances.

CORRESPONDENCE.

THE THALLEIOQUIN TEST.—A RECLAMATION.

To the Editor of the Chemical News.

Sir,—Turning over some recent numbers of your journal, I met with an old friend disguised by a new name.

Mr. Charles Frederick Zeller, in an inaugural essay, thus delivers himself on the above-named subject:—

"This test, which is one of the most beautiful of the alkaloidal tests, was accidentally discovered by M. J. J. Andre, in 1835," who is stated to have presented a memoir to the College of Pharmacy of Paris, "On the Action of Acid on Quinia," which was transferred to the *American Journ. of Pharm.*, in 1836.

Now, I think the evidence subjoined will prove that the discovery was really made by a then student at Guy's Hospital, and afterwards either independently discovered by M. Andre, or merely adopted by him as a waif and a stray. M. Andre, if yet in the flesh, may determine this question, or, failing him, his scientific executor, Mr. Zeller, will doubtless fulfil this duty and restore this little leaf of his chaplet to the rightful owner.

Anyone who cares to do so, and who will consult the *Philosophical Magazine and Journal of Science* of the year 1835, page 158, will find a letter addressed to the Editors, containing the following statements, which will suffice for my purpose:—

"Morphia.—If a solution of this substance or any of its salts be mixed with a strong solution of chlorine, and ammonia be added, a brown colour will pervade the solution . . . . but if quinia, or any of its salts, be treated in the same way, a beautiful green colour will be observed, which will become red on the addition of an acid. The test is a delicate one, and will detect a grain of either of these alkaloids in a pint of solution, &c., &c.

"(Signed) H. A. Meeson.

"Guy's Hospital, January 7, 1835."

Mr. Meeson submitted his observations as being possibly of value "in a medico-legal point of view," and I submit them to your readers as justifying my ascription of the merit of the discovery of this test to this student of Guy's, who, I think, died young, instead of M. J. J. Andre.—I am, &c.,

J. Denham Smith.

Fair Lawn, Combe Wood, Sept. 29, 1880.

FRACTIONAL DISTILLATION OF BENZOL.

To the Editor of the Chemical News.

Sir,—Reading through Schutzenberger's "Traité des Matières Colorantes" the other day, I met with the following, to me, remarkable statemen. Htaving described the apparatus for, and the process of, fractional distillation, he says:—"La benzine continue à distiller jusqu'à ce qu'elle soit épuisée et la température du vase réfrigérant monte jusqu'à ebullition. A première vue on croirait que l'eau bouillante, échauffant l'appareil, permettrait aux vapeurs de passer; mais, au contraire, l'ébullition de l'eau refroidit considérablement les vapeurs; *et plus l'eau bout, plus elle condense de vapeurs, ou inversement*"—the italics are mine. Can any reader kindly explain by what law of caloric this takes place, if it does take place, which I doubt. It certainly does not occur on a large scale as in rectifying large quantities of coal-tar benzols.—I am, &c.,

W. W. Staveley.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Note.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 6, August 9, 1880.

Experiments on the Discharge in a Rarified Medium.—A. Righi.—If whilst a Crookes's tube (*e.g.*, one of those where the negative electrode is cylindrical or spherical) is illuminated with a Ruhmkorff's coil a conductor communicating with the negative electrode is brought near the glass, and at the same time the discharge is made by means of a magnet to bend towards the same side of the tube, we perceive that a dark spot is produced in the midst of the green fluorescence, where the glass is charged negatively. It seems then that the glass becomes luminous at the point where it acts as a positive electrode. The same appears from the following experiments:—One of these tubes is isolated by means of long threads of silk, and a ball communicating with the positive conductor of a Holtz machine is brought near to its side. The electrodes of the tube diffuse positive electricity in the air, whilst negative electricity is diffused by a point applied to the conductor. The green fluorescence appears on the electrised side of the tube, which acts as the positive electrode in the internal discharge. The author approached the same ball or a metallic point to the side of a small cylindric Geissler's tube containing phosphorescent calcium or strontium sulphide. The powder becomes very strongly luminous opposite to the ball if this is negative, but very slightly if it is positive. If the ball is negative and is placed near the extremity of the tube, so that one of the electrodes is between the ball and the powder, the shadow of the electrode is distinctly projected upon the powder. We thus obtain in a common Geissler tube one of the striking phenomena described by Mr. Crookes. The pale blue or violet light which fills Crookes's tubes is strongly influenced by the hand or by conductors brought near to the tube. The author connected the electrodes of the tube with the conductors of the Holtz machine (without condensers), arranging an interruption where sparks might pass over, and alternately approached to it isolated balls, communicating with the two electrodes. He always found that the light due to the discharge is attracted by the positive and repelled by the negative ball, that is the discharge acts like a body negatively electrified. Mr. Crookes has shown that every isolated conductor introduced into one of his tubes becomes charged positively. The following experiment may explain this phenomenon:—If whilst the current of a Holtz machine is passed into a long Crookes tube, there is presented to it at a right angle by one of its electrodes a small Geissler tube, the other electrode of which is held in the hand, a discharge is seen in the latter tube directed from the long tube towards the hand. If the potential of the positive conductor is reduced to zero the small tube becomes dark, but is traversed by the discharge if we touch the negative conductor. It seems, then, that the long tube almost entirely has a potential differing little from that of the positive electrode, and that near the negative electrode a very great fall of the potential takes place. It is therefore probable that during the discharge the negative electrode becomes heated much more than the positive. This the author thinks is proved by the following experiment:—The induced discharge of a coil is passed into the electric radiometer, holding it in such a manner that the vanes may not turn. The discharge is then interrupted and the apparatus placed in its normal position, taking care not to shake it, so as to make it turn in the ordinary direction, or to give it a negative rotation. Soon the mill begins to turn in the same direction, and almost with the same speed as if still traversed by the discharge. The cause which makes the radiometer turn is probably the heat evolved when the discs act as a negative electrode. Hence the cause of the mechanical actions peculiar to the negative electrode will be the same as in the radiometer. The electric force of the negative electrode upon the molecules which depart from it negatively charged, tends to give them a direction normal to the surface of the electrode. When they impinge upon the glass they are discharged, and the glass becomes luminous. We often see, in fact, discharges passing from the positive electrode to the fluorescent portions of the glass.

Certain Properties of Flames.—M. Meyreneuf.—A flame produces upon the jet that feeds it two contrary effects, which in general do not compensate each other. It occasions an afflux of gas by the current of expanded products which rise above it, and at the same time a repulsion from the expansion due to the combustion. Either of these effects may be made to predominate by modifying the size of the orifice. Repulsion predominates in flames of distinctly conical outline and afflux in cylindrical flames.

Refraction-Indices of Aqueous Solutions of Acetic Acid and Sodium Hyposulphite.—M. Damien.—The point of solidification of the acetic acid employed was 16·7°. From 20°—8° the indices varied per degree by about 0·0004. A graphic representation of the results shows that the variations of the coefficient A and those of the density d are quite analogous. The author represents the refringent power of a body by $\frac{A-1}{d}$. His experiments confirm the empirical law of Landolt and Wüllner. For one and the same body the expression $\frac{A-1}{d}$ is constant, independent of the temperature and the refrangibility of the light.

An Improvement Introduced in Bunsen's Battery by M. Azapis.—M. Ducretet.—This improvement consists essentially in substituting for the acidulated water in contact with the zinc, a solution, at 15 per cent of potassium, of caustic potassa, common salt, or sal-ammoniac. The intensity of the current is not inferior to that of Bunsen's battery, the zinc does not require to be amalgamated, it wears less, and the constancy of the current is remarkable.

Researches on the Combustion-heats of Certain Bodies of the Fatty Series.—W. Louguinine.—The compound of which the author treats is aceto-acetic ether.

Spectra of Ytterbium and Erbium.—R. Thalén.—M. Marignac discovered, in 1878, that the earth previously called erbia is in reality a mixture of two distinct earths: the one white, to which he gave the name ytterbia, and the other of a pure rose colour, presenting well-marked absorption-bands, *erbia*. A new spectral examination of these bodies became, therefore, requisite, and was undertaken in 1879 by M. Lecoq de Boisbaudran, as far as ytterbia is concerned. On submitting the aqueous ytterbium chloride obtained by M. Marignac to the action of the induction-spark, he found that the spectrum of ytterbium consists chiefly of bands, grouped between the solar rays D and F, and almost all shaded from the red towards the violet. But in conformity with views generally admitted the spectra of simple bodies do not consist of degraded bands, but of rays perfectly distinct; we are therefore led to think that the induction-spark used by M. Lecoq de Boisbaudran was not sufficiently powerful, and that in consequence the spectrum observed by him should be ascribed to the chloride or oxide but not to the metal itself. The author proceeds to describe the results of the spectroscopic examination of ytterbium chloride obtained by M. Nilson. By this examination he has found that the spectrum of ytterbium is a spectrum of rays, and that these rays are with slight exceptions identical with those formerly ascribed to erbium by M. Höglund. Thus it may truly be said that the spectrum of ytterbium has been observed and compared with the solar spectrum five years before chemists had succeeded in separating this body. For the positions of the rays in question the reader is referred to the plate in the *Journal de Physique*, iv., p. 33 (1875). The author, however, subjoins the wave-lengths of the principal rays due in his opinion to ytterbium. The emission spectrum of true erbium remains to be determined. By means of erbium chloride prepared by M. Clève the author ascertained the principal rays of this metal, but they are in general rather faint. Among the rays due to erbium there are only three which can be found in the erbium-spectrum of Höglund above-mentioned, all the others being completely new. It must be added that the chemical product is not absolutely pure, but mixed with a little ytterbium, as is proved by the presence of the ray 6221 in the spectrum of the body under examination. We see then that one of two bodies mixed in Höglund's so-called erbium, *i.e.*, ytterbium, has appropriated almost all the brilliant rays observed by the author in 1873, whilst the other, true erbium, retains but a very small number. In return the latter body possesses beside the new brilliant rays which the author points out, not merely the well-known absorption-bands, but also the spectrum of luminous bands observed on igniting its oxide in the flame of a gas blowpipe. On repeating Bahr's experiment upon the emission-spectrum, he finds that this latter spectrum belongs exclusively to erbia and not to ytterbia, which might have been foreseen on recalling the fact observed by M. Bunsen in 1866, that the black absorption-bands coincide exactly with the luminous bands mentioned. The wave-lengths of the chief rays of the emission-spectrum due to erbium are given in the subjoined table, the numbers being only approximate. In fact, by reason of the feeble intensity of the rays of erbium, the author could employ only three flint-glass prisms, each of 60°.

Ytterbium.

Wave-length.	Intensity.	Wave-length.	Intensity.
6463·0	5	5476·0	1
6274·0	5	5447·5	4
6221·0	1	5431·5	4
6159·5	4	5352·0	1
6151·5	4	5346·5	2
6004·0	3	5345·0	2
5990·0	4	5334·0	1
5983·5	3	5300·0	4
5994·0	4	5279·0	4
5836·0	3	5257·0	4
5818·0	3	4993·5	4
5770·0	4	4935·0	3
5718·5	4	4785·5	2
5651·0	4	4725·0	2
5587·5	4	4575·5	4
5555·5	1	4518·0	4

Erbium.

Wave-length.	Intensity.	Wave-length.	Intensity.
6076·0	4	4951·0	2
5881·0	4	4899·0	2
5871·0	4	4871·5	3
5826·0	2	4830·0	4
5762·0	3	4819·0	3
5756·0	4	4794·5	4
5485·0	4	4674·0	2
5343·5	3	4605·5	2
5256·0	2	4500·5	3
5217·0	3	4419·0	4
5188·0	3	4252·5	4
5164·0	4		

On Thulium.—P. T. Clève.—The author has announced a year ago that the old erbia contains three earths having absorption-spectra, the metals of which he named thulium, erbium, and holmium, the latter being identical with M. Soret's X, but not with M. Delafontaine's philippium. Thulia, which is found in the fractions between ytterbia and true erbia, is very difficult to isolate, especially as it is present in the crude earths in a very small proportion. The author has nevertheless succeeded in extracting it in a state of purity sufficient to place its existence beyond a doubt. This earth is white and its salts are colourless, like those of ytterbia, but their solutions present to the spectroscope two absorption rays which do not appear in the spectrum of pure erbia, *i.e.*, a ray in the red part ($\lambda = 6840$) very strong, and one in the blue part ($\lambda = 4645$) which is very broad in concentrated solutions rich in thulia. The molecular weight of the fraction richest in thulia which the author has been able to obtain is 129·6 (RO), but this fraction contained a small proportion of erbia, which might reduce the molecular weight by 0·2. As the earth contained also a quantity of ytterbia impossible to estimate the number 129·8 is to be regarded merely as a maximum. The maximum atomic weight of thulium is 170·7, if we accept the formula Tm_2O_3 for the oxide. A mixture of ytterbia and erbia, the molecular weight of which (RO) should be 129·6, ought to be strongly red, and its solution should display the absorption rays of erbia very distinctly, which is not the case with the fraction described. The existence of thulium is still better proved by its spectroscopic characters, which M. Thalén has examined and which he will describe in a future communication.

Secondary Reaction between Sulphuretted Hydrogen and Sodium Hyposulphite.—F. Bellamy.—On boiling a solution of bisodic sulphite with soluble sulphur in order to obtain sodium hyposulphite, nothing particular is perceived during ebullition, and the filtrate, on standing, deposits crystals of hyposulphite and remains clear. With insoluble sulphur an odour of sulphuretted hydrogen is given off during boiling, and the liquid sometimes becomes opalescent; when set to crystallise after filtration it continues to give off sulphuretted hydrogen, becomes turbid and milky, and deposits along with the crystals a considerable quantity of white sulphur. Boiling sodium sulphite dissolves insoluble sulphur in a larger proportion than soluble.

Acid Obtained by M. Boutroux in the Fermentation of Glucose.—M. Maumené.—The author points out that he described hexenic acid in his "Traité Théorique et Pratique de la Fabrication du Sucre."

A New Process for Producing Malleable Nickel of Different Degrees of Hardness.—J. Garnier.—Melted nickel is rendered brittle by holding a larger or smaller quantity of oxygen in solution. The author succeeds in removing this oxygen by the addition of phosphorus. Nickel containing not more than 3-1000ths of phosphorus is soft and malleable, but above this dose its hardness increases at the expense of its malleability. One method used by the author for incorporating the phosphorus consists in adding to the melted metal in suitable proportion a nickel phosphide, obtained by melting together a mixture of calcium phosphate, silica, coke, and nickel.

On Propyl-neurine.—H. G. Morley.—The author prepares this compound by causing trimethyl-amine to react upon propylenic chlorhydrine.

Chemisches Central-blatt.
No. 15, 1880.

Constitution of so-called Nitrous Vapours and their Preparation.—O. N. Witt.—The author concludes, in opposition to Lunge, that nitrogen trioxide, N_2O_3, does not exist as such in a gaseous state, and that in the very moment of its formation it is resolved into a mixture of nitrogen di- and tetroxide, having the same percentage composition, but not the same constitution.

Vanadium in Commercial Caustic Soda.—E. Donath.—If the aqueous solution of certain commercial sodas is supersaturated with sulphuretted hydrogen the liquid takes a cherry-red colour from the formation of a sulpho-vanadiate.—*Polyt. Journal.*

Phosphates and Arseniates of Zinc and Cadmium.—W. Demel.—*Vienna Sitzungsberichte.*

Certain Cobalti-cyanide Compounds.—J. Schüler.—The author describes lead cobalti-cyanide and its combinations with lead hydroxide, and lead nitrate and potassium and ammonium lead cobalti-cyanides.—*Vienna Sitzungsber.*, 79, 2 abt., 302-319.

Specific-heat of Uranous-uranic Oxide and Atomic Weight of Uranium.—J. Donath.—The specific-heat in question is 0·07979, and that of uranium, 0·0497. The atomic weight is 120.—*Vienna Sitzungsber.*, 79, 2 abth., 699-704.

The Albuminoids of Various Oil-seeds.—H. Ritthausen.—Not capable of useful abridgment.—*Pflüger's Archiv.*, 21, 81-104.

Formation of Fats in the Lower Fungi.—C. v. Nägeli and O. Loew.—The chemical nature of the food of the fungi seemed to be almost without influence upon the formation of fatty matter.—*Bayr. Akad. d. Wissenschft.*

Value of Nitrogenous Manures in the Development of Plants.—V. T. Magerstein.—The best experimental results were obtained with urea; then followed potassium nitrate, and then ammonium sulphate. Plants confined to atmospheric nitrogen only remained backward in their growth.—*Listy Chem.*, 4, 74-76.

Metha-emoglobine.—F. Marchand.—If blood is mixed with potassium chlorate an absorption-band gradually appears, near that of hæmatine, but nearer to the green of the spectrum. It belongs to methæmoglobine, a substance obtained from hæmoglobine, by gentle oxidation and capable of being re-converted into hæmoglobine by reducing agents, such as ammonium sulphide.—*Virchow's Archiv.*, 77, 489.

A Method of Determining the Topography of Chemical Processes in the Animal System.—W. Kochs.—Small quantities of hippuric acid were formed on digesting blood along with sodium benzoate and glycocoll for some hours at 35° to 40° with finely cut kidney. The question was then whether this synthesis depended on the vitality of the cells. Portions of kidney were therefore exposed for two hours to a temperature of $-20°$. When they were again thawed and comminuted the synthesis could no longer be effected. No hippuric acid was formed if the blood was digested with sodium benzoate and glycocoll without kidney. Nor could the synthesis be effected with portions of liver.—*Pflüger's Archiv.*, 20, 64.

Influence of Light upon the Development of Animals and their Metamorphosis of Matter.—S. L. Schenck.—Light increases the evolution of carbonic acid and the absorption of oxygen. It acts not merely through the eyes but through the whole surface.—*Untersuch. zur Naturlehre des Menschen und der Thiere*, 12, 266.

Fluctuations of Urea in the Urine in Consequence of Irritation of the Liver by the Electric Current.—M. Stolnikow.—Electric irritation of the liver strikingly increases the secretion of urea. Recent liver comminuted and mixed with fresh defibrinised blood yielded much urea after exposure for an hour to an induction-current. Without the current a mere trace was obtained.—*Petersburg Med. Wochenschr.*, 1879.

Destiny of Ammonia in Healthy Persons, and Source of Sugar and Behaviour of Ammonia in Diabetic Patients.—A. Adamkiewicz.—In experiments with sal-ammoniac the ammonia disappeared from the system, probably as urea. Sal-ammoniac increased the decomposition of the albuminoids. Diabetic patients form sugar from the albuminoids. The injestion of sal-ammoniac diminishes the excretion of sugar.—*Virchow's Archiv.*, 76.

No. 16, 1880.

Gluconic Acid.—M. Hönig.—To obtain this acid in quantity the author treats grape-sugar with bromine and water in Champagne bottles, mixes the liquid obtained with elutriated cuprous oxide, with constant stirring till the precipitate of cuprous bromide takes a distinct red colour, filters quickly, and passes sulphuretted hydrogen into the filtrate. After filtering off the copper sulphide the liquid is gently heated to expel sulphuretted hydrogen, treated with silver oxide to remove hydrobromic acid, filtered, and again treated with sulphuretted hydrogen.—*Vienna Sitzungsberichte*, 78, 2 Oct.

A New Isomer of Gluconic Acid.—M. Hönig.—An account of the preparation and properties of paragluconic acid.—*Vienna Sitzungsber.*, 80, 2 abth.

Detection of Mercury.—M. Jüptner.—The scarlet modification of mercuric iodide in the light of a spirit lamp with sodium chloride placed upon the wick appears perfectly white. No other pigment possesses the same property.—*Oest. Zeitung*, 28, 92.

Determination of Urea.—E. Pflüger.—Liebig's method of titration with mercuric nitrate may give errors of 14 per cent. The author shows how such are to be avoided.—*Pflüger's Archiv.*, 21, 248.

Examination of Potassium Iodide and Bromide and Hydrobromic Acid. Detection of Chlorine, Iodine, and Bromine.—H. Hager.—At common temperatures silver iodide is almost insoluble in 10 per cent ammonia, whilst silver bromide and chloride are soluble; silver bromide and iodide are insoluble in cold solution of ammonium, 15 to 17 per cent; silver chloride dissolves in this solution.

No. 17, 1880.

Sulphur Compounds of Chromium.—Max Groeger.—A mixture of chromium hydroxide and sulphur, ignited in a current of hydrogen, yields a chromium sulphide, Cr_3S_4. The chromites, if treated in the same manner, yield corresponding sulphur compounds. All these sulphides are insoluble in water and in hydrochloric acid, but dissolve with oxidation in nitric acid or *aqua regia*.—*Wien. Anzeiger*, 1880, 62.

Photo-Chemistry of Silver.—J. M. v. Eder.—Oxidising acids are decidedly injurious to the sensibility to light; other acids less so, and alkaline chlorides and bromides in a much smaller degree. Alkalies increase the sensibility to light.—*Wien. Anzeiger*, 1880, 45.

Animal Tar.—H. Weidel and G. L. Ciamician.—On distilling gelatin free from fatty matter the authors obtained no nitriles of the fatty acids, but large quantities of ammoniacal salts along with pyrrol, homo-pyrrol, and other members of this series, besides a spendidly crystallised body, pyrocall, $C_{10}H_6N_2O_2$—*Wien. Anzeig.*, 1880, 62.

Aldehyd Resin.—G. L. Ciamician.—On treatment with zinc-powder there is obtained from 30 to 40 per cent of a hydrocarbon mixture consisting of ethyl-benzol, meta- and para-ethyl-toluol, and methyl-naphthalin.—*Wien. Anzeiger*, 1880, 46.

Cinchomeronic Acid.—H. Skraup.—Pyridin-tricarbonic acid, an oxidation-product of cinchonic acid, if carefully heated to 120° to 125°, is resolved into carbonic acid and pyridin-dicarbonic acid, which is identical with cinchonic acid.—*Wien. Anzeiger*, 1810, 46.

Decomposition of Nitro-sulph-hydantoin with Bases, and on Nitroso-thio-glycolic Acid.—R. Andreasch.—The most dilute solution of nitroso-thio-glycolic acid in water gives a splendid dark blue colour with ferric chloride.—*Wein. Anzeiger*, 1880, 42.

On α-Ethyl-phenol.—W. Suida and S. Plohn.—This ethyl-phenol is identical with the ethyl-phenol obtained by Ciamician on distilling gum ammoniac with zinc-powder.—*Wein. Anzeiger*, 1880, 40.

Oxycuminic Acid.—E. Lippmann and R. Lange.—The authors have analysed several salts of this acid.—*Wein. Anzeiger*, 1880, 48.

The Tannic Acid of Oak-bark.—C. Etti.—Already noticed.

Chemiker Zeitung.

No. 17, April 22, 1880.

Manufacture of Bone-black.—O. Cordel.—This paper deals rather with the economic political phase of the manufacture than the chemical processes involved.

Barff's Process for Protecting Iron from Rust.—This paper does not admit of useful reproduction without the accompanying illustration.

The Removal of the last Traces of Sulphuric Acid after Refining Petroleum.—H. Perutz.—Caustic alkali is used in this process, as sodium carbonate does not remove the free sulphur. The author has succeeded in effecting this operation by means of soda-lime. He obtains his oils free from pyren and chrysen, as well as from pyrogenous resins. Both chrysen and pyren can be easily nitrised, and will certainly play a part in the colour industry. Petrocen, a green colouring-matter, prepared in America from petroleum residues, could doubtless be obtained from them.

Presence of Alcohol in Animal Tissues.—J. Béchamp.—Alcohol and acetic acid are constant and necessary products of putrefaction. The author has also discovered alcohol in small quantities in normal and quite recent animal tissues, a fact which should be remembered in chemico-judicial investigations.—*Ann. Chim. Phys.*, xix., pp. 406—421.

The Ultra-violet Rays.—J. L. Schönn.—The author has examined the ultra-violet spectra of cadmium, zinc, thallium, indium, manganese, iron, and aluminium.—*Wiedemann's Ann. Phys. Chem.*

No. 18, April, 1880.

Physical Isomerism and its Importance in Chemical Research.—The author points out that the phenomena of polymorphism, allotropy, &c., meet with their simplest explanation in the assumption of different molecular magnitudes in the bodies which present such modifications. The molecules of ammonium nitrate condensed at 168° are simpler than those condensed at 127°. At the latter temperature, the molecules of the first kind have united together to form larger totals, and heat is liberated. Monoclinar sulphur bears to the rhombic form a similar relation to that of acetylene to benzol.

Practical Results with Haupt's Gas Furnace.—A. Ziegler.—This memoir cannot be usefully produced without the accompanying illustration.

Certain Ultramarine Compounds (a continuation).—Karl Heumann.—From *Annalen der Chemie*.

MISCELLANEOUS.

New Gas Heating Burner.—We have recently been using a new gas heating burner made by Mr. Thomas Fletcher, of Warrington, which is specially adapted for many laboratory purposes. It has from three to four times the power of any burner similar in appearance. The flame is *solid*, intensely hot, and perfectly free from smell. It cannot be damaged by the dirtiest work. In case the perforated copper dome gets choked with dirt, it can, when the burner is warm, be lifted off and washed or brushed clean. Any liquid spilt so as to get inside the burner flows out by the side tube without the possibility of damaging the burner. The body of the burner is cast all in one piece without a joint, thus doing away with one great fault, causing liability to leakage in most of the burners at present in use.

The Hollway Process in Queensland.—We learn from the *Gympie Times* that Mr. Joseph, of the Gympie Pyrites Works, has been for some time past experimenting in the endeavour to save the gold from "mill refuse." The new appliances consist of apparatus for practically putting into operation the recent discovery of Mr. John Hollway, whereby metallic sulphides are reduced without the aid of external fuel. Coal and wood have hitherto been used as fuel for producing heat in smelting operations. Mr. Hollway now proposes to supply their places by using up the sulphur contained in the pyrites to be operated upon, by simply forcing a current of air through numerous jets (somewhat after the Bessemer principle) with a mass of molten pyrites, whereby sufficient heat is developed and the operation is made self-supporting without the aid of other fuel than a little coke, which is required to start the furnace, and which stands in the same relation to the ore as wood does to coal in lighting an ordinary fire. The process is simplicity itself, the only wonder being that its discovery has been so long delayed. It is generally known that pyrites catches fire when roasted in a current of air; and all Mr. Hollway suggests is to give it more of that inexpensive element when sufficient heat is produced to cause the pyrites to fuse. Messrs. Dudley and Rutter have already erected at Mr. Joseph's works the engine to produce the blast, and all that now remains to be done is to finish the erection of the furnace, when this novel invention will have a trial on the products of our district, about the ultimate success of which Mr. Joseph seems sanguine. At the same time he is not without some anticipation of having to spend much time and labour in bringing the matter to a perfectly successful issue. In an economical point of view, our readers will readily perceive that if ores can be expeditiously melted without the aid of any other fuel than such as is merely required to move the steam engine, a great saving of expense is probable to ensue. The process is applicable to copper and other metals as well; indeed, Mr. Hollway is now erecting works for treating the first-named ores in other parts of the world, and should the process prove itself as meritorious as its inventor claims it to be, its use can doubtless be turned in a similar direction in this country with immense advantage to mining generally.

Les Mondes, Revue Hebdomadaire des Sciences.
No. 11, August 5, 1880.

This issue contains no physical or chemical matter which has not appeared elsewhere.

NOTES AND QUERIES.

Soft Water.—I am in search of a local in the vicinity of London where I can obtain *soft* water. Can you or any of your correspondents give me an idea in what district of London, or within 50 miles thereof, I am likely to obtain the same.—"D. D."

GREENBANK ALKALI WORKS COMPANY (LIMITED), ST. HELENS LANCASHIRE.

CELEBRATED RED HEART BRAND.

DOUBLE REFINED CAUSTIC SODA—Solid in Drums. or in a fine white powder in Barrels or Kegs, Total Impurity under $\frac{2}{100}$. For Fine Soap Makers, Oil Refiners, Dyers, Bleachers, &c. We specially recommend the *powdered* article for small consumers. Keeps perfectly without deliquescing. No trouble in breaking up, handling, or weighing exactly.

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PURE SULPHATE POTASH—Containing no Iron, Chloride, or Free Acid, only a trace of Soda.

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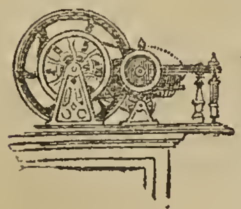

From Messrs. E. H. SMYTH & Co., 21, Duke Street, Edinburgh, and 69, Coleman Street, City, London.—The small Levigator Mill we recently got from you works most efficiently, and is a great economiser of time.

The chief feature in this machine is, that although it can be turned with ease by a boy, it has immense frictional power for levigating purposes. This is caused by the runner being driven at a much greater speed, but in the same direction, than the bowl. The surfaces being Minton ware, they are clean, smooth, and durable.

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The diameter of the bowl is 18 ins., and it and the runners are made of Minton ware. It is useful for Painters, Chemists, Grocers, and Cooks, for triturating and mixing Medicines, Vegetable Powders, Ointments, Paints, Sugar, Salt, Spices, Herbs, Meat for Potting, and will grind the COARSEST EMERY INTO A NEARLY IMPALPABLE POWDER in a few seconds. The pan can be tilted for emptying, as shown in the dotted lines; it can be worked by hand or power.

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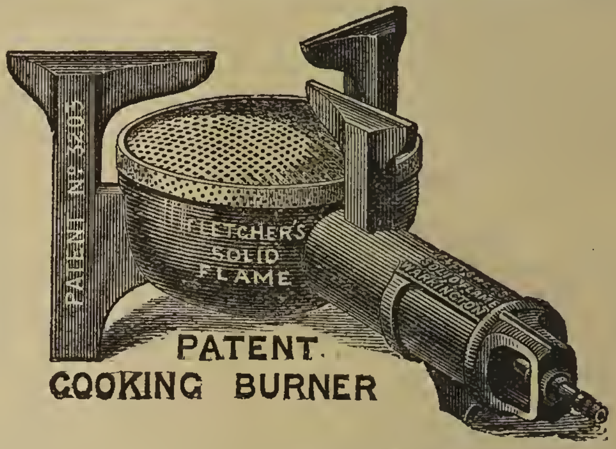

thousands are in daily use. The Gauze is done away with and replaced by a loose perforated Dome of Solid Copper, which can be taken off and cleaned when choked with dirt. The Stand is broad and firm, supporting large or small vessels safely and steadily.

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THE CHEMICAL NEWS.

Vol. XLII. No. 1090.

FURTHER NOTES ON PETROLEUM SPIRIT AND ALLIED LIQUIDS.[*]

By ALFRED H. ALLEN.

At the Sheffield meeting of the Association, I laid before the Section the results of some experiments for distinguishing commercial petroleum spirit or "benzoline" from coal-tar naphtha or benzene. I have since extended the experiments to the "shale naphtha," obtained as a secondary product of the manufacture of paraffin wax and burning oil from the bituminous shale of the south of Scotland. In physical properties shale naphtha presents the closest resemblance to petroleum spirit, both liquids being known in commerce by the same names and used for similar purposes. They are also usually stated to be identical in chemical composition, and hence I supposed that the reactions which I had found characteristic of petroleum spirit would be equally applicable to shale naphtha. Experiment has shown certain differences in the behaviour of the two liquids with reagents which point a much wider divergence in their chemical composition than is commonly suspected to exist.

The following table exhibits in a convenient form the physical characters of the volatile naphthas from petroleum, shale, and coal-tar, together with the differences observed in their solvent action on coal-tar pitch and anhydrous crystallised carbolic acid.

	Petroleum Spirit.	Shale Naphtha.	Coal-tar Naphtha and Benzol.
a. Leading constituents.	Heptane, C_7H_{16}, and its homologues.	Heptylene, C_7H_{14}, and its homologues.	Benzene, C_6H_6, and its homologues.
b. Sp. gr. of sample at 15·5° C.	0·690	0·718	0·876
c. Boiling-point of sample.	55° C.	56° C.	80° C.
d. Solvent action of the sample on coal-tar pitch.	Very slight solvent action. Liquid only coloured amber-yellow, even after very prolonged contact.	Behaves similarly to petroleum spirit.	Readily dissolves pitch, forming a deep brown solution.
e. Behaviour of the sample on agitating 3 measures of it (cold) with 1 measure of fused crystals of absolute carbolic acid (Calvert's No. 2).	No apparent solution. The liquids are not miscible.[*]	The liquids form a homogeneous mixture.	The liquids form a homogeneous mixture.

[*] For further details of this reaction see Allen's "Commercial Organic Analysis," i., 317.

From this table it appears that while shale naphtha presents the closest resemblance to petroleum spirit as regards its specific gravity, boiling-point, and solvent action on pitch, it is sharply distinguished from it by its behaviour with carbolic acid, in which respect it resembles coal-tar naphtha or benzene. The carbolic test is not applicable to mixtures of the various products, as in such cases the phenol passes readily into complete solution. In its limited solubility in rectified spirit, shale naphtha resembles petroleum spirit, and differs from benzene, which is miscible with alcohol in all proportions.

As the carbolic acid test had shown a sharp distinction between the more volatile products from petroleum and those from bituminous shale, it appeared interesting to apply it to the burning-oils obtained from different sources. The samples of burning-oil compared were a specimen of American petroleum, or "kerosene," of 0·800 sp. gr., boiling at 166° C., and one of shale oil, or "photogene," of 0·801 sp. gr., boiling at 170° C. Both samples required 16 or 17 volumes of methylated spirit for complete solution, and gave with coal-tar pitch light yellow liquids, having a greenish fluorescence. Carbolic acid, employed in the proportion already described, was found not to be miscible with either kind of oil in the cold, but the petroleum product caused it gradually to assume a deep purple and ultimately a black colour. This reaction was repeatedly obtained, but I am unable to say whether it is peculiar to the few specimens hitherto examined, or is a test which might be generally applied to distinguish petroleum kerosene from the similar product from shale.

Correspondence with a friend engaged in the paraffin industry, and to whom I am indebted for much information and various samples of shale products, gave me the clue to the differences observed in the behaviour of shale oils and petroleum oils with reagents. The explanation lies in the fact that while the naphtha and burning-oil obtained by the distillation of petroleum consist chiefly of hydrocarbons of the marsh-gas or paraffin series, having the general formula $C_n H_{2n+2}$, the similar products of the distillation of shale consist largely of hydrocarbons of the ethylene or olefine series, having the general formula $C_n H_{2n}$. This difference in composition is clearly indicated by the behaviour of the various liquids with fuming nitric acid and with bromine. Thus, while 100 measures of petroleum spirit when treated with fuming nitric acid (sp. gr. 1·45), concentrated sulphuric acid, fuming sulphuric acid, and caustic soda, employed successively, yields at least 75 measures of unchanged oil, shale naphtha on similar treatment leaves but 15 to 30 per cent of paraffins. Similar differences are observable in the case of the burning-oils, that from petroleum yielding from 55 to 80 per cent of paraffins, while from shale photogene the unchanged oil is only 35 to 40 per cent. Repetition of the experiment shows that the process is capable of yielding results agreeing within 2 or 3 per cent, and sometimes much closer.

The different susceptibility of petroleum and shale oils to the action of nitric acid is borne out by the difference in the facility with which they combine with bromine. The results obtained in this way are very remarkable, and conclusively show the predominance of olefines and other unsaturated hydrocarbons in shale products, these readily decolourising many times the volume of bromine which can be caused to combine with petroleum naphtha or burning-oil. I have strong hopes of basing a simple quantitative method on this well-known principle.

In petroleum spirit I have repeatedly found traces of benzene and its homologues, as indicated by the production of aniline. Shale products, however, appear to be wholly destitute of this series of hydrocarbons. I am informed that while chrysene is produced largely by the

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[*] A paper read before the Chemical Section of the British Association, Swansea Meeting, 1880.

distillation of shale, anthracene is met with in but very insignificant quantity, and naphthalene appears to be entirely absent. The lubricating oils produced from shale consist almost wholly of olefines, the paraffins of high boiling-point being solid at ordinary temperatures, and hence they are separated in the form of paraffin wax.

The following table shows roughly the differences in chemical composition between commercial petroleum products and the bodies of similar physical characters obtained by the distillation of shale, as deduced from the action of nitric acid on the oil.

Product.	Petroleum.	Shale.[*]
Naphtha or spirit; sp. gr. about 0·700; boiling-point about 56° C.	At least 75 per cent of hydrocarbons of the paraffin or marsh gas series, $C_n H_{2n+2}$. The remainder olefines, $C_n H_{2n}$, with distinct traces of benzene and its homologues.	At least 60 to 70 per cent of hydrocarbons of the olefine or ethylene series, $C_n H_{2n+2}$. The remainder paraffins, $C_n H_{2n+2}$. No trace of benzene or its homologues.
Photogene or burning-oil; sp. gr. about 0·800; boiling-point about 160° C.	55 to 80 per cent of higher members of the paraffin series, $C_n H_{2n+2}$. The remainder chiefly olefines.	60 to 65 per cent of higher members of the olefine series, $C_n H_{2n}$. The remainder paraffins, $C_n H_{2n+2}$.
Lubricating oil.	(?)	Almost wholly higher olefines, $C_n H_{2n}$, the paraffins of similar high boiling-point being solid. No naphthalene.
Wax.	Solid paraffins, $C_n H_{2n+2}$.	Solid paraffins, $C_n H_{2n+2}$.

[*] Since this paper was written, I have found reason to question the accuracy of the quantitative results here given. But whatever be the true proportion of unsaturated hydrocarbons, they are present in far larger quantity in the shale than in the petroleum products.

ON THE ACTION OF OILS ON METALS.[*]

By WILLIAM HENRY WATSON, F.C.S., &c.

At the Plymouth meeting of this Association I brought forward the results of some experiments, showing the actions of various oils on copper, and the conclusions arrived at were briefly these:—

1. That of the whole of the oils used, viz., linseed, olive, colza, almond, seal, sperm, castor, neatsfoot, sesame, and paraffin, the samples of paraffin and castor oils had the least action, and that sperm and seal oils were next in order of inaction.
2. That the appearances of the paraffin and the copper were not changed after 77 days' exposure.
3. That different oils produce compounds with copper varying in colour, or in depth of colour, and consequently rendering comparative determinations of their action on that metal from mere observations of their appearances impossible.

I was disposed to conclude that these experiments would indicate the relative action of the oils on other metals, simply expecting that the extent of action would vary throughout, but that the variations would be proportionate between the different oils.

Since the publication of these results, however, an interesting paper has appeared (*Pharm. Journ.*), "On the Action of Paraffin Oils on Metals," by Dr. S. Macadam. He comes to the same conclusion as myself with regard to their action on copper, but referring to iron, says "it is slightly affected by paraffin oil, and on ten days' contact the oil becomes deeper in colour and throws down a fine ferruginous sediment." Owing to this, I have lately made experiments on the action of the same oils as those previously used on copper, on iron, and the results, which are the subject of this communication, are interesting to me as showing that there is no relation between the action of an oil on copper and the action of that oil on iron; that, in fact, in several instances those oils which act largely on iron, act slightly on copper, while those which act largely on copper act little on iron. Of course the actual extent of action of the same oil (with the exception of paraffin) is greater on copper than on iron.

In order that the results might be comparable with those obtained from copper, the same quantities of metal and oil were used, viz., a piece of polished iron exposing 8 square inches of surface, this being immersed in 500 grain measures of the oil under examination. The samples thus prepared were kept in glass dishes, and agitated daily during the whole of the exposure, which extended over twenty-four days. The appearances were then noted and determinations of iron made in each by the colorimetric method, using potassium cyanide as the reagent.

In addition to the oils used in my experiments on copper, I also used a sample of refined lard oil, and a special lubricating oil prepared, by the Dee Oil Company near Chester. The following observations were made, after twenty-four days' exposure:—

1. *Neatsfoot.*—Considerable brown irregular deposit on metal. The oil slightly more brown than when first exposed.
2. *Colza.*—A slight brown substance suspended in the oil, which is now of a reddish brown colour. A few irregular markings on the metal.
3. *Sperm.*—A slight brown deposit, with irregular markings on the metal. Oil of a dark brown colour.
4. *Lard.*—Reddish brown, with slight brown deposit on metal.
5. *Olive.*—Clear and bleached by exposure to the light and air. The appearance of metal same as when first immersed.
6. *Seal.*—A few irregular markings on metal. The oil free from deposit, but of a bright clear red colour.
7. *Linseed.*—Bright deep yellow. No deposit or marks on metal.
8. *Almond.*—Metal bright. Oil bleached and free from deposit.
9. *Castor.*—Oil considerably more coloured (brown) than when first exposed. Metal bright.
10. *Paraffin.*—Oil bright yellow and contains a little brown deposit. The upper surface of the metal on being removed is found to have a resinous deposit on it.
11. *Special Lubricating.*—Metal bright. Appearance of oil not perceptibly changed.

The samples were then chemically examined, and the amounts of iron found in them were as follows:—

Neatsfoot oil (English)	0·0875 grain.
Colza ,, ...	0·0800 ,,
Sperm ,, ...	0·0460 ,,
Lard ,, ...	0·0250 ,,
Olive ,, ...	0·0062 ,,
Linseed ,, ...	0·0050 ,,
Seal ,, ...	0·0050 ,,
Castor ,, ...	0·0048 ,,
Paraffin ,, ...	0·0045 ,,
Almond ,, ...	0·0040 ,,
Special lubricating oil	0·0018 ,,

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[*] Read before the Chemical Section of the British Association, Swansea Meeting, 1880.

For comparison, the following are the results obtained of the action of these oils on copper, as previously communicated, after exposure of ten days.

	Copper found.
Neatsfoot oil	0·1100 grain.
Colza ,,	0·0170 ,,
Sperm ,,	0·0030 ,,
Olive ,,	0·2200 ,,
Linseed ,,	0·3000 ,,
Seal ,,	0·0485 ,,
Paraffin ,,	0·0015 ,,
Almond ,,	0·1030 ,,

Owing to the length of exposure being different in the two series, we cannot fix on the actual differences in the *rate* of action of any of the oils on the two metals. However, it is shown that almond oil, which acted largely on copper, acts very slightly on iron; in fact, with the exception of the paraffin and special lubricating oil (a mineral preparation), it acted less than any of the other oils on iron. The same is shown, as already mentioned, as to the action of various other oils; thus, while sperm oil acts slightly on copper, it acts considerably, compared with the others, on iron. Linseed, seal, castor, almond, and paraffin may be bracketed as having about the same and very little action on iron, while linseed, olive, neatsfoot, almond, and seal have the greatest action on copper.

The Folds, Bolton,
August, 1880.

ON THE IDENTIFICATION OF THE COAL-TAR COLOURS.*

By JOHN SPILLER, F.C.S.

Dyers and others who are in the habit of using the coal-tar colours are familiar with a number of chemical reactions by which the members of the series may generally be classified and identified. Differences are remarked in their relative affinities for various sorts of fibres, some colours being taken up freely by silk, others fixing better upon wool, and some few, like saffranin, exhibiting a special affinity for cotton. Again, as with the yellows, great differences are observed when the operator proceeds to work with a free acid or a weak alkali in the dye-bath. Primrose (naphthaline yellow) requiring the former, but not so with phosphine (crysaniline yellow), which demands a neutral or even slightly alkaline bath.

By the study of these conditions, aided by a few characteristic tests, it is often possible to identify colouring-matters of unknown or doubtful origin, and it is with the view of extending the number of such readily available tests that I recommend a more frequent appeal to the colour reactions with sulphuric acid.

For this purpose but small quantities of material are required, a few grains serving to impart a distinct colour to a comparatively large bulk of sulphuric acid, and the resulting indications are in many cases both specific and permanent. Oil of vitriol, which so readily destroys nearly all organic structures, does not carbonise any of the coal-tar colours, or does so only under severe conditions, as at high degrees of heat. Even indigo and madder, although of true vegetable origin, are known to yield up their colouring-matters to sulphuric acid, the old processes of dyeing depending upon this fact. In the manufacture of garancine from madder the woody fibre and organised tissues are destroyed by the action of sulphuric acid, whilst the alizarin glucoside survives, and with it Turkey-red goods may be dyed.† Instances might be multiplied as proof that colouring-matters, both natural and artificial, resist the attack of oil of vitriol, and the large class of sulphonates (Nicholson blues, "acid roseine," &c.) may be cited as establishing the fact that colouring-matters are not so destroyed, but form combinations with sulphuric acid.

If, then, the body under examination be dissolved in strong oil of vitriol, a colour-test is at hand whereby useful inferences may be derived as to the nature of the dye, and often its exact identity disclosed. A few direct confirmatory tests may then be applied. The most remarkable colour reactions are the following:—

Magdala (naphthaline pink)	Blue-black.
Saffranin	Grass-green, becoming indigo-blue on strongly heating.
Crysöidin	Deep orange, turning almost to scarlet on heating.
Alizarin	Ruby-red or maroon.
Eosin	Golden yellow.
Primrose (naphthaline yellow)	Difficultly soluble, first yellow, and colour discharged on heating.
Crysaniline	Yellow or brown solution, of marked fluorescent character.
Aurin	Yellowish brown, non-fluorescent.
Atlas orange	Rose colour, turning to scarlet on heating.
Atlas scarlet	Scarlet solution, very permanent on heating.
Biebrich scarlet, R.	Blue-black or deep purple.
,, ,, B.	Bluish green.
Aniline scarlet	Golden yellow, permanent on heating.
Indulin	Slatey blue to indigo, according to shade of the dye.
Rosaniline, regina, and all violets	Yellow or brownish yellow.
Phenyl and diphenylamine blues	Dark brown solutions.
Iodine green } Malachite green }	Bright yellow solutions, the former giving off iodine on heating.
Citronine	Pale cinnamon or neutral tint.

After vitriol the action of concentrated hydrochloric acid may be next tried, which distinguishes at once between saffranin and Biebrich scarlet, the former giving a violet solution and the latter being precipitated as a red flocculent powder.

Proceeding in this way, and combining the observation with the dyer's usual test, every one of the substances named can be readily identified, and much time saved in the examination of dye-stuffs.

July 28, 1880.

ON A

NEW PROCESS FOR THE PRODUCTION, FROM ALUMINOUS MINERALS CONTAINING IRON, OF ALUMINIUM SULPHATE FREE FROM IRON.*

By J. W. KYNASTON, F.C.S., F.I.C.

It is my privilege to-day to bring to the notice of the Association a new process in manufacturing chemistry, destined probably in the future to occupy an important position in that branch of technical industry to which it relates.

It will be within the knowledge of the members of this section that the consumption of salts of alumina in the arts, especially of paper making, of dyeing, and of calico printing, is already enormous in its dimensions. It will be evident also, on consideration of the purposes for which alumina is required in these arts and manufactures, that

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* Read before the Chemical Section of the British Association, Swansea Meeting, 1880.

† See W. H. Perkin's "History of Alizarin," *Journ. Society of Arts*, May, 1879.

* Read before the Chemical Section of the British Association, Swansea Meeting, 1880.

the salt used must be possessed of a considerable degree of purity, and that for some purposes almost absolute freedom from foreign matters is a necessity.

The various aluminous minerals available for the production of salts of this earth are, perhaps, with the single exception of cryolite, invariably contaminated with a greater or less proportion of iron. Even the whitest kaolin or china-clay is never wholly free from this contamination; and inasmuch as the behaviour of oxide of iron towards any of the strong acids is exactly similar to that of alumina, it is not possible to obtain a solution of the latter free from the objectional impurity. And it is owing to this difficulty that, to meet the demand for a pure compound of alumina, manufacturers have been compelled to produce the readily crystallisable double salts of alumina and potassa, or alumina and ammonia, the ordinary potassa and ammonia alums of commerce; these salts in the act of crystallisation rejecting the impurities so difficult to get rid of by any other means.

In the production of these double salts there is required, in the case of the potassa salt, for every part of alumina contained in the salt, very nearly an equal part by weight of potassa, and, in the case of the ammonia salt, a little more than one-half part by weight of ammonia (NH_4O), and in both cases one-fourth part more sulphuric acid than is required to maintain the alumina in solution. These large quantities of valuable materials are, of course, absolutely and irrecoverably wasted, their presence in the alumina salts serving no purpose whatever in the uses to which the salt is applied, and the wealth of the country is diminished by the value these materials would possess if applied to purposes for which they are essential.

This anomaly long ago attracted the attention of manufacturing chemists, and amongst investigators the name of Mr. H. D. Pochin stands pre-eminent as a successful producer of an aluminous compound so free from deleterious impurities as to be available for all the commoner purposes for which soluble alumina is required, and in the manufacture of which there is no waste of the valuable commodities essential to the production of the crystallised salts. The aluminous cake first manufactured by Mr. Pochin, and, since the expiry of his patent, by several others both in this country and on the Continent, is produced in the simplest possible manner, by the action of sulphuric acid upon well calcined china-clay. The materials are carefully intermingled by agitation, and, after being in contact for some little time, the mixture becomes hot, a violent reaction sets in, accompanied by a considerable swelling of the mass, and the evolution of vast quantities of steam. When the action ceases, the swollen mass subsides, and on cooling a hard and tough cake is obtained, which, when crushed or broken up into coarse fragments, is ready for use.

By a careful selection of the china-clay used, and by scrupulous attention to cleanliness in the manufacture, especial care being taken to prevent ferruginous contamination, Mr. Pochin's process yields a crude sulphate of alumina of fairly constant composition. It consists of from 11 to 12 per cent of soluble alumina (Al_2O_3), equal to 37 to 40 per cent of sulphate of alumina, about 20 per cent of silica and undecomposed clay, some 3 or 4 per cent of free sulphuric acid, about 38 per cent of water, and not exceeding one-half per cent of iron, but in some of the purest specimens I have found the iron present below 0·20 per cent. This is, however, unusual.

The deleterious matters present, it will be observed, are iron and free sulphuric acid; the former, when the cake is used in the manufacture of paper, giving to the paper a brownish tint or producing brown spots, and the latter gradually dissolving the wires of the gauze upon which the paper pulp is caught or deposited. But notwithstanding these defects, aluminous cake produced in this way is universally employed in the production of all but the higher qualities of paper, and it will not be easy to displace it.

We now advance a step further in the progress of this manufacture, and a new aluminous mineral is before us, one which, though only recently discovered, already occupies an important position. This mineral is bauxite, so called from the district in which it was first found, namely, at Les Baux in France. Very extensive deposits have since been found in the County Antrim in Ireland, and are worked on a large scale by the Irish Hill Mining Company. The ore is essentially a hydrate of alumina, but contains also that *bête noire* to the manufacturer, peroxide of iron in varying quantity. The French variety has not yet gained a footing in this country; the Irish mineral is already largely employed. An analysis by Mr. John Pattinson, of Newcastle-upon-Tyne, assigns to it the following composition:—

	"First Quality."	"Second Quality."
Alumina	49·80 per cent	51·53 per cent
Peroxide of Iron	1·45 ,,	4·53 ,,
Lime	0·57 ,,	0·78 ,,
Magnesia	0·12 ,,	0·20 ,,
Potash	0·01 ,,	0·02 ,,
Soda	none ,,	0·06 ,,
Silica	8·02 ,,	11·89 ,,
Titanic Acid	5·37 ,,	6·14 ,,
Sulphuric Acid	0·06 ,,	0·07 ,,
Phosphoric Acid	trace ,,	trace ,,
Combined Water	27·07 ,,	23·79 ,,
Moisture	7·50 ,,	0·90 ,,
	99·97	99·91

A noticeable feature in the character of this ore is the large quantity of titanic acid which it contains. In the French mineral it is not present.

Immediately upon the discovery of this mineral, and the determination of its composition, it was seen to be a valuable material for the production of aluminous salts, and many active and intelligent minds have since been engaged in its study. The percentage of Al_2O_3 it contains is greater than that of any other available aluminous mineral; it is more readily acted upon by sulphuric acid than china-clay, even without the necessity of submitting it to previous calcination, and it can be purchased at far less cost than the finer qualities of china-clay, whilst its proportional yield of soluble alumina is far greater.

The great impediment to its instant adoption was that it contained a much higher percentage of iron. The bulk indeed is rarely found with less than from 2½ to 3 per cent of iron in the form of Fe_2O_3, and, as from one ton there is produced about three tons of the dry sulphate, the latter contains from 0·75 to 1·00 per cent of iron, a proportion that renders the salt valueless for any but the most common purposes.

Mr. Peter Spence, the extensive alum manufacturer of Manchester, produces also this crude sulphate of alumina, and has given it the name, in recognition of its impure character, of alumino-ferric cake, and, impure as it is, it is said to be largely used in the manufacture of the lower qualities of paper.

From amongst the many attempts which have been made to produce a pure sulphate of alumina from bauxite a few may be mentioned. Premising that there is a great similarity of behaviour between peroxide of iron and alumina, the task is one of no common difficulty, whether the object be to remove iron from the mineral before attacking the latter with sulphuric acid, or to eliminate the iron from solution after being dissolved with the alumina.

Mr. H. B. Condy has endeavoured to effect the removal of iron from bauxite by reducing it from the very difficultly soluble state in which it exists in the ore to a condition in which it becomes more soluble in mineral acids; that is, by reducing it from the ferric to the ferrous state. In his process, the bauxite is mixed with a solution of sulphide of sodium, and the mixture is heated and well agitated for the space of two hours. The bauxite is then allowed to subside, the supernatant liquid is drawn off, the residue once or twice washed with water, and then digested with warm hydrochloric acid. The iron dissolved is then removed by washing and filtration. It is certain that some iron is removed from bauxite by this treatment; but even if the treatment be repeated again and again, the purification is still very incomplete, whilst the process itself is costly and tedious beyond endurance.

By another process Mr. Condy subjects bauxite to the action of sulphide of sodium by heating the materials together in a close furnace. There is then formed a compound consisting of sulphur, sodium, and alumina, soluble in water, and of insoluble sulphide of iron. On treatment with water the sulphide of iron deposits and the solution of sulpho-aluminate of soda is decomposed by acetic or carbonic acid, and aluminia precipitated in the pure state. This, dissolved in sulphuric acid, yields a pure sulphate of alumina. The process is ingenious, but the manipulation exceedingly difficult and expensive, and it will not be possible to produce in this way sulphate of alumina to replace the ordinary crystallised alums.

Dr. Loewig, of Silesia, has published a process for obtaining hydrate of alumina from a mixture of bauxite and cryolite, producing at the same time caustic soda. As this process requires the use of lime as a precipitant of the alumina, and as lime free from iron cannot be obtained, sulphate of alumina free from iron cannot be produced from this product.

Messrs. Duncan and Newlands have devoted much attention to this matter. They propose "to treat bauxite with hydrochloric acid, either strong or dilute, for several hours, preferably at a temperature approaching the boiling point, when a large proportion of the oxide of iron is dissolved along with a certain amount of alumina. The solution thus obtained is ran off, and the residual mass is washed with water." They also propose to purify bauxite by submitting a mixture of the ore "with a quantity of finely ground coal, coke, charcoal, or other carbonaceous matters, to a bright red heat, allowing as little access of air as possible, when the oxides of iron are reduced to a lower state of oxidation or even to the metallic state," or they "reduce the oxides of iron by heating the bauxite to bright redness, and then passing through the mass a current of hydrogen, carbonic oxide, coal gas, or other reducing gases."

In either case, the mass after cooling is further treated with more or less strong hydrochloric acid, which effects the solution of a portion of the iron. The bauxite treated by either of these methods is used for the production of sulphate of alumina by digesting with sulphuric acid in the ordinary way, and undoubtedly yields a purer salt than it is possible to obtain from bauxite not so treated. There is, however, still iron present in as great a proportion as is contained in the ordinary aluminous cakes prepared from the purer qualities of china-clay by a more economical method.

In the course of the investigation of this matter which I have undertaken, I have found that oxalic acid has the remarkable power of dissolving the oxides of iron contained in bauxite to a far greater extent than even strong hydrochloric or nitric acid, and this without any preliminary treatment of the ore, except the necessity of reducing it to a fine state of division. This process of purification can also be worked without material loss of the oxalic acid employed. In practice I have carried out the process as follows:—

I take finely powdered bauxite and mix it to a thick cream with a solution of oxalic acid, containing from 5 to 10 per cent of the weight of the bauxite, and about the same proportion of hydrochloric acid, the latter being used to prevent the formation of insoluble oxalation should lime be present in the bauxite. The mixture is allowed to stand at rest or with occasional stirring for a week or ten days. There is then added five or six times as much water as the bulk of the mixture. The whole is thoroughly agitated, and then allowed to stand till the bauxite is deposited. The latter is again repeatedly washed until free from oxalic acid and dried, and by treatment in the ordinary way with sulphuric acid, sulphate of alumina is obtained, containing not more than 0·10 to 0·20 per cent of iron, and about 16 per cent of alumina, a purer product than has ever before been obtained by the direct action of sulphuric acid on aluminous minerals.

The strongly acid solution of oxalate of iron (containing also some alumina) obtained in this process of purification is mixed with milk of lime, considerably more than equivalent to the oxalic acid present, but still leaving the mixture strongly acid. After standing two or three days, the oxalic acid is thus almost perfectly precipitated as oxalate of lime. This precipitate is well washed, and then boiled with its equivalent of sulphuric acid. The sulphate of lime produced is allowed to settle out, and a clear solution of oxalic acid is again obtained, available for the purification of a second portion of bauxite, as before described.

Notwithstanding the unexpectedly satisfactory result obtained by this process of manufacture, it, like those before washing is enhanced by reason of the comparative lightness mentioned, has its disadvantages. The apparatus required is of large extent for a small production. The difficulty of of the mineral; it settles from water with difficulty, and after having settled as perfectly as possible, the wet clayey mud contains an enormous proportion of water, which must be evaporated before the mineral can be used for the production of sulphate of alumina. For the same reasons it is difficult to remove the last portions of the impurities dissolved by the acid employed, and, even the purest sulphate of alumina that can be produced by such means of purification would probably have a commercial value not greatly in excess of the ordinary aluminous cakes produced more simply and economically from china-clay; and the object in view, viz., the economical production of a sulphate of alumina sufficiently pure to displace the crystallised alums, is still unattained.

These considerations led me to continue the investigation of the matter, and at the same time forced upon me the conclusion that the only satisfactory mode of purification would be one which either prevented the solution of the iron oxides when bauxite is treated with sulphuric acid, or one which effected the separation from the alumina solution of the dissolved iron by some process of precipitation.

(To be continued.)

NOTES OF WORK BY STUDENTS OF PRACTICAL CHEMISTRY IN THE LABORATORY OF THE UNIVERSITY OF VIRGINIA.

No. IX.

Communicated by J. W. MALLET,
Professor of General and Applied Chemistry in the University.

(Concluded from page 181.)

(55) *On the Incandescence Exhibited by Cupric Antimonate on being Strongly Heated.* By C. L. Allen, of Charleston, S. Carolina.

Berzelius has recorded* the observation that certain antimonates and antimonites when exposed to a slight red-heat, after water of crystallisation has been expelled, present a transient but bright incandescence, looking like combustion, without any change of weight being experienced. The glowing is said to be especially noticeable with the antimonates of copper, cobalt, and zinc. In another passage† the same author says that zinc antimonate does *not* incandesce. As this phenomenon does not

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* Berzelius, "Traité de Chimie" (Paris, 1830), ii., 499.
† *Ibid.*, iv., 269.

appear to have been observed by anyone else, it seemed worth while to try to reproduce it, determining the conditions of its occurrence.

Neutral potassium antimonate (of Fremy, better called met-antimonate) was prepared by fusing purified metallic antimony with four times its weight of nitre, washing the mass with cold water, and dissolving the residue by prolonged boiling with water. In a portion of the solution the amount of antimony actually present was determined, and after dilution an equivalent quantity of cupric sulphate was added. The light green precipitate obtained was washed and dried, finally at 100° C. The product so obtained consisted of $Cu(SbO_3)_2.4H_2O$.* Heated to about 260° C. for some time, 12·01 per cent of water was given off, and collected in a chloride of calcium tube, this corresponding to about 3 molecules of water (calc. = 11·46 p.c.). At about 360° nearly the whole of the remaining water was given off, there being a further loss of 3·36 per cent (calc. = 3·83 per cent). It required a low red-heat to drive off the last traces of water, the total loss being now equal to 15·68 per cent (calc. for $4H_2O$ = 15·29 per cent). Only by strong ignition over a blast (gas) lamp was the phenomenon of incandescence produced, but when it occurred it was quite bright and distinct. No further alteration of weight occurred, and as the temperature was much above that at which the last of the water was removed, while there is no reason to suspect any change of chemical relation between the remaining constituents, the phenomenon seems most probably one dependent upon change of physical structure only, as O. Popp† has made it likely that the analogous glowing of magnesium pyrophosphate in the ignition of ammonium-magnesium phosphate is to be considered.

Berzelius states that cupric antimonate becomes black when heated but sufficiently to drive off the water, and after ignition and glowing appears white with a slightly greenish tinge. This statement (as well as the proportion of water said to be lost) renders it probable that the salt he experimented with contained an admixture of cupric hydrate, as Mr. Allen found to be the case with a specimen he prepared by means of crude potassium antimonate insufficiently washed with cold water and retaining potassium hydrate. In such case cupric oxide is first left on moderate heating, and appears when more strongly ignited to enter into union with the antimonate, forming a basic salt. With pure cupric antimonate Mr. Allen found the original pale bluish green colour to change on heating to a more and more yellowish green, becoming finally, after incandescence and cooling, a pale greenish grey, but there was at no time any blackening.

(56.) *Analysis of Amphibole (var. Actinolite) from Amelia Co., Va.* By F. A. Massie, of the University of Virginia.

Accompanying some specimens of asbestos exhibited at the Annual Fair of the State Agricultural Society, in October, 1879, by Mr. W. R. Faulkner, of Chula, Amelia Co., Va., were several large lumps made up of light green crystals, easily crumbled apart between the fingers. The individual crystals were quite small, generally about 1 to 2 m.m. long, and ½ to 1 m.m. through laterally. They were slightly stained in some places by ferric hydrate, easily removed by hydrochloric acid. The angle $I \wedge I = 124° 30'$ was repeatedly obtained with the microscope goniometer, as also the same angle between cleavage planes. Hardness = nearly 6; sp. gr., 3·041; lustre, vitreous; colour, light grass-green; streak, almost white; As transparent as common green window-glass; fusible with difficulty before the blowpipe flame.

Analysis of picked crystalline grains, air-dried, after washing with hydrochloric acid, gave—

Silica	56·96
Alumina	4·32
Ferric oxide	2·45
Ferrous oxide	2·24
Magnesia	22·33
Lime	11·44
Water	0·31
	100·05

corresponding to the formula $(\frac{11}{14}R'' + \frac{2}{14}R''')SiO_3$, that of a meta-silicate, mainly of magnesium and calcium, with a certain amount of isomorphous replacement by dyad and triad iron and by aluminium.

(57.) *Analysis of a Highly Aluminous Pyroxene from Amhurst Co., Va.* By W. T. Page, of Norfolk, Virginia.

In immediate association with magnetic iron and the allanite found in remarkable quantity in the gneissoid rocks of Amhurst Co., in this state, masses, sometimes of a foot or more through, are found of a black pyroxene, of which specimens have several times been sent to me by the people of the neighbourhood as allanite or as iron ore. The mineral occurs in confusedly crystalline masses, with an approach to foliated structure in some parts, and in which the I cleavage can be here and there imperfectly identified. Hardness = fully 6; sp. gr. = 3·420; lustre between vitreous and sub-metallic, in places verging upon pearly; colour, black; streak, ash-grey; translucent only on the edges of their splinters; difficultly fusible before the blowpipe.

The following results were obtained on analysis by Mr. Page, the absence of ferrous iron having been ascertained:—

Silica	42·50
Alumina	15·39
Ferric oxide	11·32
Magnesia	7·50
Lime	22·67
	99·38

These figures lead very closely to the regular meta-silicate formula, counting in most of the aluminium with silicon, the extent of basic and acid replacement being represented pretty nearly by—

$$(\tfrac{6}{9}R'' + \tfrac{2}{9}R''')\ (\tfrac{12}{15}Si + \tfrac{4}{15}Al)O_3.$$

The amount of aluminium present is unusually large.

(58.) *Analysis of a Ferruginous Mineral, probably referable to Amphibole, from Amhurst Co., Va.* By A. L. Baker, of Baltimore, Maryland.

This mineral also accompanies allanite (though not in the same specimens with the preceding), and is often attached to it so closely that the line of partition of the two may be easily overlooked upon the surface. It occurs in masses of fibrous structure, like compact asbestos. Occasionally it tends to become lamellar. Hardness = 6; sp. gr. = 3·32; lustre, sub-metallic to silky; colour, black; streak, reddish grey; moderately fusible, about 3·5, before the blowpipe.

A careful examination proved that the iron was exclusively in the ferric condition. Analysis gave—

Silica	37·18
Alumina	6·29
Ferric oxide	39·01
Magnesia	3·80
Lime	14·10
	100·38

The general habit of the mineral suggests a hornblendic character, and from the above analysis may be calculated the formula—

$$(\tfrac{3}{5}R'' + \tfrac{2}{5}Fe''')\ (\tfrac{3}{4}Si + \tfrac{1}{6}Al + \tfrac{1}{6}Fe''')O_3.$$

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* Berzelius (iv., 355) says that cupric antimonate gives off on heating 19½ per cent of water, which would correspond to nearly 5½ molecules.

† *Zeitschr. f. Chem.*, 1870, 305.

But this requires the presence of basic triad iron to a very unusual extent, and the occurrence of a little of the same metal even as replacing with aluminium the silicon. Although the mineral appeared under the microscope to be homogeneous, the red-brown tinge of the streak suggests the possibility that some of the iron may have existed as ferric oxide in a state of very intimate admixture.

(59.) *On the Solubility of Carbon Disulphide in Water.*

By W. T. Page, of Norfolk, Va.

In most of the standard text-books of chemistry it is stated that carbon disulphide is "insoluble in water," "not sensihly soluble," or "nearly insoluble." It must have been often noticed that water shaken up with the sulphide acquires its odour, and hence some degree of solubility might have been inferred. The only attempt on record, so far as I know, to determine accurately to what extent solution really takes place, is that of Sestini,* who found in a single experiment that 30 c.c. of CS_2 shaken up with 8·69 litres of water at 20° to 23° C. diminished by 11 c.c. in nine days, by 1·4 c.c. in the next three days (in diffuse light), and by 0·6 c.c. in five days more (in the absence of light). He states that a part of the sulphide was decomposed, and that the coefficient of decomposition was in the light 0·35, in darkness 0·12, without giving any information as to how these determinations were made, and hence concludes that in this experiment 7·85 c.c. of CS_2 had been dissolved, or about 1 part in 1000 of water by weight: this should be, more accurately stated, 1 in 873.

Mr. Page employed pure water, deprived of air by long-continued boiling, and cooled down in a well-closed flask filled nearly up to the stopper from another like flask just before removal from the lamp. The stopper being removed for a few moments, recently-purified carbon disulphide was introduced to the extent of two or three cubic centimetres (more than the water present could dissolve), the air was expelled from the very small remaining space by carbon disulphide vapour, the well-fitted stopper was inserted, and as a further precaution against any entrance of air the stopper and upper part of the neck of the flask were surrounded by water or mercury. Each such vessel was shaken at short intervals from day to day. Some of the vessels were kept exposed to diffuse light; others were kept in the dark in a cellar. With the precautions mentioned no evidence of decomposition of the sulphide was obtained. It was found that the solution could be oxidised with tolerable rapidity at common temperature by quickly and accurately measuring off known volumes, adding to each an excess of potassium permanganate acidified with nitric acid, and keeping the whole in a well-stoppered bottle until the change was complete. The sulphuric acid formed was determined as barium sulphate. Calculating from the quantities of sulphur thus found, the following results were obtained:—

Time of Contact with Water.	Average Temperature (nearly constant).	Solubility of CS_2.
30 days	12° to 13° C.	0·203 in 100 by weight
Do.	Do.	0·198 " "
55 days	15° to 16°	0·191 " "
7 days	25° to 27°	0·168 " "
27 days	30° to 33°	0·146 " "
Do.	Do.	0·143 " "

It will be seen that the solubility diminishes with rise of temperature; but, so far as shown by these few determinations, this diminution does not occur at a rate equal to that at which the vapour-tension of CS_2 increases, as shown by the tables of Renault and Marx.

(60.) *Examination of Livingstonite from a New Mexican Locality.*

By W. T. Page, of Norfolk, Va.

About a year ago my friend, Senor Mariano Bárcena, of the City of Mexico, described, at a meeting of the Sociedad Mexicana de Historia Natural, some specimens of a mineral from Guadalcázar, in S. Luis Potosi, resembling in most respects the Livingstonite which he had previously described from Huitzuco, in Guerrero, and whose complete analysis, made in this laboratory, was published last year.*

There being some difference in the general aspect of the specimens from these different localities, Senor Bárcena thought it possible that either a special variety or a species containing its constituent sulphides of antimony and mercury in new proportion might be represented by the Guadalcázar specimens, which he had but qualitatively examined. He sent some of the latter to me, therefore, for full chemical analysis.

The main point of difference between the mineral from the original locality, Huitzuco, and that now examined is that the former occurs in small, but pretty well defined, prisms, either single or grouped together with general parallelism of direction, while the Guadalcázar mineral is in small globular masses made up of delicate capillary or needle-like crystals interlaced with each other in all directions. The colour of the latter is lead-grey, with metallic lustre; streak, cochineal-red; hardness = 2; brittle; sp. gr. = 4·41 (that of the Huitzuco mineral = 4·81, but this was less mixed with earthy gangue). Very easily fusible before the blowpipe flame, giving off copious white antimonial fumes. Yields globules of mercury when heated in a glass tube with soda. Attacked by strong, heated nitric acid, leaving a white insoluble residue. Occurs, as is the case at Huitzuco, in a gangue of gypsum, along with native sulphur, but there is also some siliceous matter present. These foreign substances were mechanically removed as far as possible beforehand, and further amounts, intimately mixed with the Livingstonite, had to be separated in the course of the analysis, the free sulphur by solution in carbon disulphide. The total quantity of sulphur obtained from the solution in *aqua regia* was checked by a fusion with nitre and sodium carbonate. The mercury was checked by distillation with lime.

Mr. Page obtained—

Sulphur (combined)	15·12
Sulphur (free)	3·67
Antimony	32·22
Mercury..	13·95
Iron	0·42
Gypsum $(CaSO_4.2H_2O)$	15·54
Insoluble siliceous residue.. ..	18·01
	98·93

Throwing out the iron and other non-essential substances, these figures give the atomic ratio—

$$S : Sb : Hg = 7{\cdot}04 : 4{\cdot}0 : 1{\cdot}04,$$

obviously corresponding to the formula already established for Livingstonite—

$$HgS.2Sb_2S_3,$$

so that there seems no reason to doubt that the mineral from Guadalcázar is correctly referred to this species—an interesting one, as was noticed in a former paper, on account of the highly acid character of the sulph-antimonite it represents.

(61.) *Examination of an Altered Livingstonite from Guadalcázar, S. Luis Potosi, Mexico.*

By W. T. Page, of Norfolk, Va.

Another specimen, from the same locality as the preceding, but of manifestly different appearance, was separately examined. It formed a nearly compact mass, with traces only of crystalline structure, of iron-grey, almost black, colour, very dark grey streak, and dull sub-metallic lustre. Hardness = 3; sp. gr. = 4·06; blowpipe characters nearly as for Livingstonite; imperfectly attacked by nitric or

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* *Gazz. Chim. Ital.*, 1871, 473. *Ber. der Deutsch. Chem. Gesell.*, 1872, 288.

* Chemical News, vol. xl., p. 186. *Revista Scientifica Mexicana*, February 1, 1880, 7.

hydrochloric acid; contained, besides the same intermixed gangue materials as those of the preceding specimens, a little of a hard clay-like substance.

The general course of analysis was the same as in the preceding case; but a large apparent loss having led to the discovery that oxide of antimony was present, the oxygen of this was determined by very careful reduction in a slow stream of hydrogen, directly weighing the water produced. The mineral had been dried at 100° C., and lost no constituent water by mere heating above this temperature. Analysis gave—

Sulphur (combined)	9·35
Sulphur (free)	10·11
Antimony	38·23
Mercury..	18·24
Oxygen	4·99
Gypsum ($CaSO_4.2H_2O$)	5·12
Iron	0·01
Alumina..	1·57
Insoluble siliceous residue.. ..	11·01
	98·63

Neglecting non-essential ingredients as before, we have the atomic ratio, as against that of Livingstonite,—

S : O : Sb : Hg = 12·84 : 13·71 : 14·0 : 4·01
S : Sb : Hg = 28·00 : 16·0 : 4
(In Livingstonite.)

No probable formula can be obtained. The substance in question seems to represent the result of chemical alteration of Livingstonite by the replacement of more than half the sulphur by oxygen, forming probably Sb_2O_3, and the removal of a further portion of the sulphur and a part of the antimony altogether. The streak being nearly black, with no reddish tinge, suggests that the mercuric sulphide has either been left free in the physical condition in which it exists in the meta-cinnabarite of California, or remains as part of a *basic* sulph-antimonite, approximating in composition to $2HgS.Sb_2S_3$. Oxidation of the same original species, accompanied by hydration, has in all probability led to the formation of the mineral Bárcenite formerly described[*] by me.

(62.) *On the Artificial Production of Livingstonite.* By A. L. Baker, of Baltimore.

The composition of Livingstonite being remarkable for the large proportion of the acid antimonious sulphide which it contains, it seemed interesting to attempt to produce it artificially, and experiments looking to this result were made by Mr. Baker. Trying various modes of fusing together the sulphides of antimony and mercury, he at first found much difficulty from the latter of these volatilising pretty rapidly at the temperature necessary for fusion. The natural Livingstonite is itself easily decomposed by heating in a glass tube closed at one end, a sublimate of mercuric sulphide being produced.

By two methods, however, a successful result was reached. The former of these (A) consisted in fusing together the two sulphides, that of mercury somewhat in excess, in a stout glass tube filled with carbon dioxide and sealed, keeping the tube in a vertical position while maintaining for some time the temperature of fusion, and after cooling removing the upper part of the fused mass, leaving at the bottom an impure portion in which the surplus mercuric sulphide had settled in consequence of its greater density. The second method (B) consisted in fusing the mixed sulphides in considerable quantity in an open Beaufaye crucible, in the upper part of which an atmosphere of carbon dioxide was maintained, lowering the temperature gradually, and dropping in now and then a compact lump of (artificially sublimed) cinnabar until the mass was on the point of solidifying. The upper portion of the cooled mass was alone taken for examination, leaving the excess of mercuric sulphide in the bottom portion.

The product from each of these processes agreed essentially with the natural mineral in physical characters. The structure was highly crystalline, much like that of the cakes of common trisulphide of antimony purified by fusion, and the colour was nearly the same, but the streak was dark cherry-red. Specific gravity = 4·928 at 32° C.

Analysis gave—

	(A.)	(B.)	Calc. for $HgS.2Sb_2S_3$.
Sulphur	24·56	24·83	24·78
Antimony ..	—	53·20	53·10
Mercury	22·40	22 71	22·12
		100·74	100·00

(63.) *On the Amount of Water of Crystallisatian of Hydrated Strontium Nitrate.* By A. L. Baker, of Baltimore.

There is still some discrepancy of statement in chemical text-books as to the extent of hydration in this salt, most recent writers giving 4 molecules of water, as found by Souchay and Lessen, and by Ordway, while some give 5 molecules, as formerly found by Laurent and earlier experimenters.

In order to ascertain whether by varying the circumstances of crystallisation more than one hydrated salt can be formed, Mr. Baker made a large number of experiments, with different rates of cooling, and of evaporation at uniform temperature, and at temperatures ranging from 2° to 32° C. In no instance was any other result obtained, when individual crystals were picked out for examination, than the anhydrous salt or that with 4 molecules of water. In a few cases a little excess over the exact 4 molecules was found, the result undoubtedly of minute amounts of mother-liquor remaining enclosed in cavities of the crystals. A number of measurements of angles were made, agreeing with those already on record.

As regards the limit of temperature at which the hydrated nitrate is obtainable, Ordway[*] states that when crystallisation takes place at temperatures above 24° C. the anhydrous salt is usually obtained. By slow evaporation at 32° C. of a saturated solution Mr. Baker obtained in one instance a mixture of anhydrous and hydrated crystals.

University of Virginia,
September 7, 1880.

CORRESPONDENCE.

ON THE SPECIFIC ROTATORY POWER OF CANE- AND INVERT-SUGAR.

To the Editor of the Chemical News.

Sir,—In a paper with the above title, which appeared in the Chemical News, vol. xlii., p. 177, Mr. Allen announces the discovery of what he considers an error in the calculation of the specific rotatory power of inverted sugar. He says:—

"In calculating the specific rotatory power of invert-sugar, Watts and many other authorities have apparently fallen into an error. They appear to have assumed that a solution of inverted sugar, after correction for change of bulk, contained the same percentage of solid as before inversion. This, of course, is contrary to fact, 95 parts of cane-sugar yielding 100 of invert-sugar. Hence this increase in weight must be taken into consideration in calculating the specific rotatory power of inverted sugar."

Now, Sir, it appears to me highly improbable that anyone who had occasion to make such a calculation should

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[*] *Amer. Jour. Sci.*, October, 1878, 306. *Boletina del Sociedad Mexicana de Historia Natural*, October 12, 1878.

[*] *Amer. Jour. Sci.*, [2], 27, 18.

omit this very obvious correction. Moreover, an error of the kind indicated above, instead of giving -25°, gives a number even *higher* than that given by Mr. Allen as the corrected one. Mr. Allen gets the value $-25{\cdot}6$ at 15° C. for $[a]j$ from the formula—

$$[a]j = \frac{-36{\cdot}5 \times 0{\cdot}24}{2 \times \frac{16{\cdot}26}{95}} = -25{\cdot}6^\circ.$$

Omitting the correction the formula gives—

$$[a]j = \frac{-36{\cdot}5 \times 0{\cdot}24}{2 \times \frac{16{\cdot}26}{100}} = -26{\cdot}93^\circ.$$

$-26{\cdot}93^\circ$ instead of -25° as Mr. Allen supposes.

It will thus be seen that no such gross error has been made in the calculation. Neither is a mistake of this kind possible in the case before us, as the value -25° given in "Watts's Dictionary" and elsewhere is the *calculated* specific rotatory power of invert-sugar, and not the *observed*. As shown by Kekulé (*Lehr. d. Organ. Chem.*, Bd. II., 349), it is the mean of the *observed* rotatory powers of dextrose $+56^\circ$ (Berthélot) and levulose -106° (Dubrunfaut), calculated on the assumption that invert-sugar consists of equal parts of dextrose and levulose; thus:—

$$\frac{-106}{2} + \frac{+56}{2} = -25^\circ.$$

This method of calculation Mr. Allen evidently does not consider erroneous, as he has used it in order to obtain the value for levulose which he gives in his paper.—I am, &c.,

Alexander Watt.

Liverpool, October 12, 1880.

DIAZO COLOURS.

To the Editor of the Chemical News.

Sir,—I should like to call your attention to an article in the Chemical News, vol. xlii., p. 60, on "Diazo Colours," and signed by Mr. Meldola, who claims that the colours I described in a paper before the American Chemical Society (wherein I mentioned a compound produced by combining three sulpho groups in the same molecule) are not new, as he had previously taken out a patent covering all such colours as above mentioned. This fact I was unaware of at the time I read my paper, otherwise it would not have been mentioned. I beg that you will insert this in the columns of the Chemical News.—I am, &c.,

James H. Stebbins, jun.

New York, September 26, 1880.

OBITUARY.

We regret to record the death of Mr. Thomas Charles Boake, Analytical Chemist, of 46, Boleyn Road, Stoke Newington, who was accidentally killed at Victoria Park Station, on the 7th instant. The deceased, who was twenty-one years of age, was the nephew of Mr. Arthur Boake, the senior partner of the firm of A. Boake and Co., of Stratford. Mr. Boake was a young chemist of great promise, he was a genial warm-heated Irishman, and is deeply regretted by all who knew him.

Inconveniences, from a Physiological Point of View, of the Substitution of Amylic Alcohol for Ether in Stas's Process for the Detection of Morphine.—MM. G. Bergeron and L. L'Hote.—As amylic alcohol, even in very small doses, produces in animals symptoms closely resembling narcotism, and as it is not readily removed from the cadaveric extract, physiological experimentation in confirmation of the chemical reactions of morphine is rendered untrustworthy.—*Comptes Rendus.*

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Note.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 7, August 16, 1880.

Observation made on a Group of Rays in the Solar Spectrum.—L. Thollon.—The author figures and describes a group of four rays, situate in the orange. Two of these rays belong to iron, their wave-lengths being respectively 5976·1 and 5974·6. The other two are telluric, and their wave-lengths are 5976·35 and 5974·36.

Cause of the Variations of the Fixed Points of Thermometers.—J. M. Crafts.—The author cites some experiments which reduce to nothing, or almost so, the part played by pressure in the permanent elevation of the zero-point. The glass blown at the lamp and exposed for a long time to the action of heat diminishes in volume by means of some internal change, and it is not demonstrated that pressure plays any part whatever in the phenomenon. The particles of glass which have been removed asunder whilst it was being blown do not return immediately to their normal position at a lower temperature; we observe disturbances for some time, and finally the glass may remain for a long time in a state of tension at the ordinary temperature. The action of heat at a given temperature (*e.g.*, 355), giving a greater mobility to the particles, favours their return to the normal position, and gives scope to a contraction. But the glass, when cooling from this latter temperature retains a part of the displacement peculiar to 355°. On heating again to a lower temperature (*e.g.*, 300) a new decrease of volume is produced, so that a very slow cooling, which produces successively all these effects upon the particles of glass, must ensure the greatest stability.

Rapid Alcoholic Fermentation.—J. Boussingault.—In order to effect rapid fermentation for the destruction of the sugar contained in wines, the sample is mixed with water and yeast, and placed in connection with an exhausting syringe, which reduces the pressure, and thus eliminates the alcohol as fast as formed. The fermentation not being checked by its presence goes on till all the sugar is decomposed.

Spectroscopic Examination of Thulium.—R. Thalén.—I have submitted to spectroscopic examination certain aqueous solutions received from M. Cleve, supposed to contain the new metal thulium, and I have studied the absorption-spectrum, that of emission, and the brilliant spectral rays produced by the induction-spark. For observations on the absorption-spectrum I have employed two concentrated solutions of the nitrate, one of which, of a rose colour, was said to be rich in erbium, whilst the other, almost colourless, was rich in thulium. On comparing the two absorption-spectra I found the majority of the bands identical in both as regards their mean positions, whilst their intensity and their breadth varied much according to the nature of the solution employed. Thus in the spectrum of thulium, where all the other bands are much fainter and narrower than the corresponding bands in the spectrum of erbium, we observe two very broad bands of striking intensity. The blackest band already indicated in the communication which M. Cléve has made on this subject (*Comptes Rendus*, lxxxix., p. 479), is situate between the Frauenhofer rays C and B, and very near B ($\lambda = 6840$) when the solution is dilute. In case of a concentrated solution it extends beyond B, and advances almost up to the ray *a* ($\lambda = 6800$—7070). This band is characterised by the circumstance that other things being equal it retains its darkness perfectly, even when the dispersion of the spectroscope is much increased. The other band, situate in the blue ($\lambda = 4650$), is very broad and dark in weak spectroscopes, whilst it is much slenderer if the dispersion becomes at all great. These two bands, especially the former, ought, it seems, to be attributed to thulium. As for the other bands, which all belong to erbium, I shall here dispense with giving the measurements which I have re-determined. As is well known the aspect of absorption-bands changes much, either with the degree of concentration and density of the absorbent liquid, or in consequence of the greater or less dispersion of the spectroscope employed. It may suffice, therefore, to refer to the values already given either by myself (*loc. cit.*) or by MM. Bahr and Bunsen (*Annalen der Chemie und Pharmacie*, bd. 137, p. 1, 1866), whose measurements agree fairly with my own. For the emission-spectrum and that produced by the induction-spark, I have made use of the purest erbium and thulium chlorides which M. Clève has prepared. On igniting in the non-luminous flame of gas a small bead of borax moistened with a solution of the chloride either of erbium or thulium, I have seen the bead shine with a very intense greenish light, the spectrum of which is composed of the luminous bands already observed by Bahr. But on carefully comparing together the spectra given by the two chlorides, I have found in some respects notable differences. In the first place the spectrum of thulium contains two new bands, one of which, very broad and intense, coincides exactly with the black absorption-band in the red part of the spectrum above-mentioned, whilst the other, situate in the blue ($\lambda = 4760$), has not a corresponding black band. On the other hand I have seen no trace of the luminous band ($\lambda = 4650$), which ought to correspond to the absorption-band already named, although this luminous band appears very intense in the irradiation-spectrum due to erbium. Hence it may be doubted whether the black band ($\lambda = 4650$) really belongs to thulium or not. Nevertheless, if we admit, at least, that the red band ($\lambda = 6840$) is peculiar to thulium, it appears that this metal, like erbium and didymium, possesses the remarkable property of giving an emission-spectrum. Lastly, in the luminous spectrum produced by the induction-spark, I have found many of the brilliant rays belonging to ytterbium, and at intervals I have seen also some of the rays of erbium, which, nevertheless, shone only for an instant. But, besides, I have observed about 15 rays which ought really to be attributed to thulium. These rays are almost all of a feeble intensity, but they will nevertheless, I think, suffice to characterise the body in question. The following are the wave-lengths of the brilliant rays of thulium which I have observed by means of three flint glass prisms of 60°:—

Wave-length.	Intensity.
5961·5	6
5896·0	2
5675·0	4
5305·7	2
5033·5	3
4733·0	6
4615·0	5
4522·0	4
4481·0	5
4386·5	4
4359·5	4
4241·5	5
4204·0	5
4187·5	5
4106·5	6
4093·0	6

In fine, the various spectroscopic facts just mentioned prove, it seems to me, the existence of the new metal thulium, though it must be admitted that chemists have not yet succeeded in separating it from two other bodies, ytterbium and erbium, with which it is at present mixed.

Absorption-spectra of the Metals forming Part of the Groups of Yttria and of Cerite.—J. L. Soret.—Important progress has lately been made in the knowledge of the metals of the groups of yttria and of cerite, and, thanks to the kindness of M. Marignac, who has permitted me to examine a great number of his products, I have been able to complete my former observations on their properties of spectral absorption. In this paper I shall speak only of the four metals which give rise to an absorption-spectrum, characterised by several rays or bands within the limits of the extent of the solar spectrum. I give an approximate diagram of these spectra for the violet and the ultra-violet portion, which is very characteristic. [The author has used chlorides dissolved in 50 equivs. of water, the thickness of the stratum being 0·01 metre. The position of the principal solar rays is indicated in the diagram.]

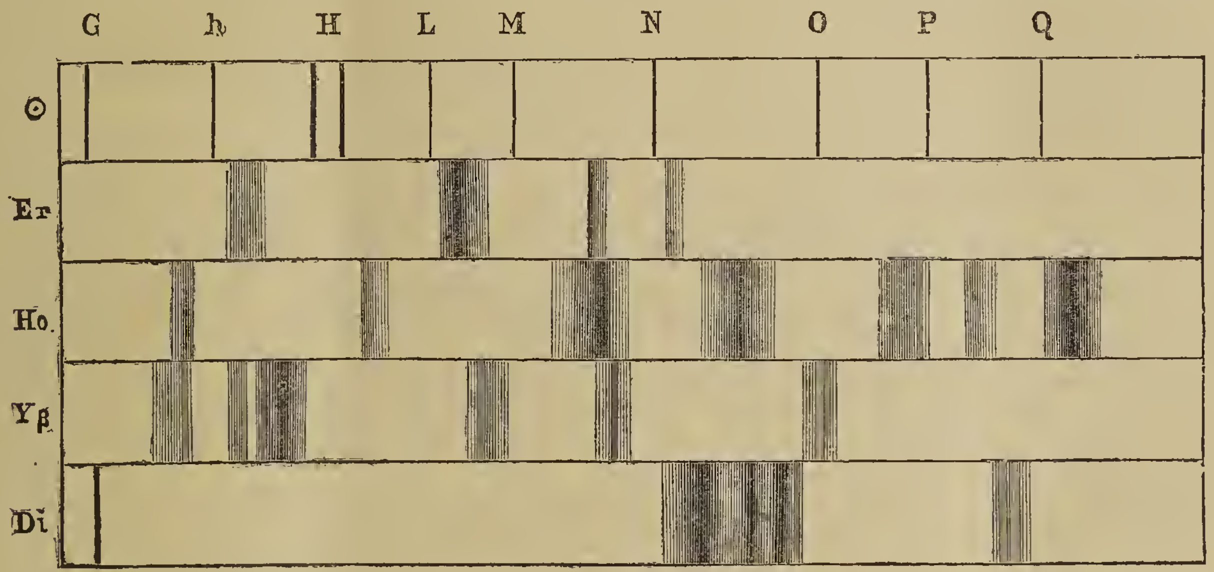
1. *Erbium.*—I have little to add to what I have already said concerning this body. An attentive study permits me to conclude that from the list of rays formerly attributed to this element (see Lecoq de Boisbaudran, "Spectres Lumineux) must be erased that of the red $\lambda = 683{\cdot}7$, which behaves in a perfectly special manner (thulium), and three rays, $\chi = 640{\cdot}5$, $536{\cdot}3$, and $485{\cdot}5$, which belong to a following metal (holmium, X). On the other hand there must be added two rays, which, to my knowledge, have not yet been pointed out; the one, very faint and difficult to distinguish, is found in the blue, $\lambda = 468$; the other, narrow and easily recognised by gas light, is in the indigo, $\chi = 442$. The diagram given of the most refrangible part of the spectrum of erbium refers to a chloride containing a large proportion of other metals, especially ytterbium. Doubtless in pure erbium the rays would be more distinct.

2. *Holmium* (earth X).—I think I have fully demonstrated the existence of a metal, which almost constantly accompanies erbium, and which I have provisionally designated X. M. Delafontaine at first considered it identical with philippium; then, in ulterior and recent researches (*Archives des Sciencez Physiques et Naturelles*, March, 1880), he has recognised that these two bodies are different, and in particular that philippium does not give a spectrum with absorption rays. The examination of the spectrum of erbium has also led M. Clève to detect a new element, for which he proposed the name holmium, but which he afterwards recognised as identical with the metal X (*Comptes Rendus*, Sept. 1, and October 27, 1879). I might urge my right to choose a new name for this element, which can no longer be confounded with philippium, and the existence of which I first established after a spectral examination, but this would be to introduce more confusion in a subject already very complex, and along with M. Marignac I formally adopt the name holmium proposed by the skilful Swedish chemist. The following is the list of rays which characterise holmium:—

Extra red	$\lambda =$	804 (?)	Very strong.
,,		753	Faint (new).
Red		640·4	Very characteristic.
Yellow-green		536·3	
Green-blue		485·5	Hitherto ascribed to erbium.
Blue (?)		474·5	Very faint, nebulous.
Indigo-blue		453—449	Coincides with a ray of erbium.
Indigo (?)		430	Doubtful.
Violet		414·5	See diagram.
Ultra-violet, solar	$\lambda =$	389—387	
	$\nu =$	368—360	
	$\omega =$	347—353	
	$\pi =$	340—336	
	$\chi =$	332·5	
	$\rho =$	327	
Extreme ultra-violet		270—240	Broad band.
,, ,,		219·5	Narrow, coincides with 25 Cd.

In the numerous products which I have examined these rays have constantly appeared to behave in the same manner; they become stronger or fainter simultaneously according to the strength of the solution. They form a well-defined group perfectly distinct from that of the rays of erbium. These facts, confirmed by the observations of M. Clève, of Lecoq de Boisbaudran, and M. Delafontaine place the existence of holmium as an element beyond doubt.

3. *Earth* Yβ (Marignac), *Samarium* (Lecoq de Boisbaudran), *Decipium* (?) (Delafontaine). Samarium is without doubt identical with the metal which M. Marignac designates provisionally as Yβ, to reserve the rights of M. Delafontaine to the discovery of this body, as it is possibly the decipium of the latter chemist (*Archives des Sciences Physiques*, May, 1880). I can only confirm the exactitude of the description which M. Lecoq de Boisbaudran gives of the less refrangible portion of the spectrum as far as the violet. The violet and ultra-violet portion is represented in the diagram. We observe:—(1.) In the violet a band η, which has been also studied by M. Lecoq de Boisdaudran; $\lambda=419$—415. It appears to me to have two maxima of intensity. (2.) A narrower band θ', a little more refrangible than h; $\lambda=408$—406; easily distinguished. (3.) A band θ, of enormous intensity; a little less refrangible than H; very characteristic. It coincides with a much less distinct band, which I have previously pointed out in this place, either in terbium or didymium, extracted from cerite. It seems to me that these latter products contain a small proportion of the new earth, manifested by a trace of the band θ. (4.) The ray μ, which I formerly ascribed to didymium, belongs equally to the new earth. Its centre falls between the first two of the four principal solar rays M; it extends about from $\lambda=376$ to $\lambda=372$; it is strong, but less distinct than θ. (5.) A ray ν, $\lambda=364$—360, a little before N; about equal to the former in intensity and in appearance. (6.) An analogous ray ω, whose centre coincides with the solar ray O; it is a little less easy to observe because the spectrum is generally faint in this region as well as in the extreme ultra-violet.



4. *Didymium.*—I have mentioned above that two of the rays which I had described as forming part of the spectrum of didymium belong in reality to the foregoing metal. I add that I have verified the absence of these two rays in solutions of didymium carefully purified for me by M. Lecoq de Boisbaudran. Thus in the violet and ultra-violet portion the rays of didymium are reduced to the fine ray, a little after G, $\lambda=427{\cdot}5$; a broad band between N and O, presenting two principal maxima of intensity, $\lambda=353$ and $\lambda=348$; lastly, a paler band, $\lambda=330{\cdot}5$—$328{\cdot}5$, a little before Q.

On Erbia.—P. T. Clève.—True erbia, characterised by its absorption-spectrum and by its fine red colour, has at last, after an exceedingly long and troublesome process, been obtained in such a degree of purity as to permit of the exact determination of its molecular weight. The method which I have made use of consists in the partial decomposition of the nitrates by heat. I have not been able to discover another process. The purest erbia which I have been able to obtain still shows traces of the absorption-rays of thulia, and of the earth X (holmia) I have made long, but vain attempts to separate them completely. It was not difficult to separate entirely one of these earths, but only by the sacrifice of a considerable quantity of material, and in the erbia thus freed from one of these earths there were still found traces of the other, and for the separation of the latter, also, the quantity of material which I possessed was not sufficient. The quantity of true erbia in the old erbia is not so considerable as that of ytterbia, which formed the bulk of the erbia obtained by Bunsen, Höglund, &c. The traces of thulia and of the earth X present in my erbia could not exert any appreciable influence on the atomic weight. I divided the earth by the partial decomposition of the nitrate into four fractions, and I determined the molecular weight of the two extreme fractions. The first gave for the metal (Er) the atomic weight 166·25; the last, 166·21 and 166. I have several times obtained the number 166 on determining the atomic weight of the purest fractions. I believe that this number is exact, or that it differs from the truth merely by a few tenths of unity. It differs from the atomic weight of ytterbium, 173, by 7 units. Erbium oxide is a powder of a most beautiful and pure rose colour, which it preserves even after strong ignition. It dissolves slowly in acids, unless they are very concentrated. Its specific gravity, as determined by M. Petterson, is 8·64. Its salts have also an intense and beautiful red colour. The nitrate, $Er_2(NO_3)_6+10H_2O$, forms large crystals, permanent in the air. The sulphate crystallises with 8 molecules of water. The double potassium sulphate, $Er_2K_2(SO_4)_4+4H_2O$ is very soluble in cold water, as is also the corresponding ammonium salt. The selenite, $Er_2O_3,4SeO_2+5H_2O$, is precipitated in a crystalline form on adding alcohol to the mixed solutions of selenious acid and erbium nitrate. The oxalate, $Er_2(C_2O_4)_3+9H_2O$, is precipitated from a hot acid solution in red microscopic crystals. The formiate is obtained on adding erbium oxide to boiling formic acid as a red powder, scarcely crystalline. It is anhydrous, and dissolves slowly in water. This solution on evaporation over sulphuric acid gives a viscid mass, which solidifies after a time. If the solution is abandoned to spontaneous evaporation beautiful red crystals of the hydrated formiate are obtained. The platino-cyanide, $Er_2Pt_3(CN)_{12}+21H_2O$, crystallises like the yttrium salt in red prisms, showing on some of their surfaces violet reflections, but a metallic green on their prismatic and pyramidal planes. The composition of the selenite, of the double potassium and ammonium sulphates, and the proportion of water in the simple sulphate, prove that erbium oxide should be written Er_2O_3.

Measure of the Intensity of certain Dark Rays of the Solar Spectrum.—M. Gouy.—The application of the author's method (*Comptes Rendus*, Dec. 15, 1879) gives the following results, the unity being ten-millionth of a millimetre :—

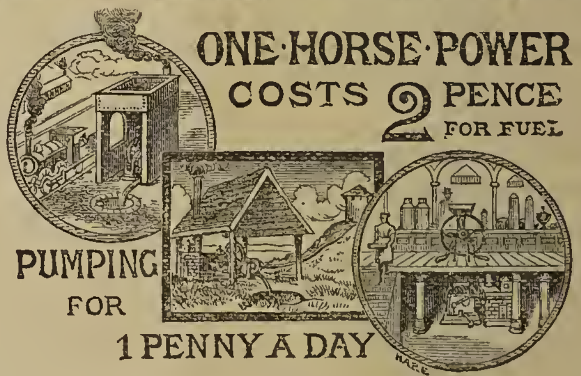
	Intensity.
Ray *c*	1·1
Ray *b*	0·9
Ray 4957 (Angström)	0·45

Group B between the wave-lengths 6866 and 6880, in the same afternoon :—

h.	*m.*	Intensity.
2	30	2·8
4	30	3·5
5	10	4·3
5	35	4·7

NOTES AND QUERIES.

Soft Water.—(Reply to "D. D.")—At St. Valery, Hastings, is a large supply of rain-water. The town water has only 8 degrees of hardness. Distance from London, 60 miles, S.E.R.—G. A. KEYWORTH.



THE CHEMICAL NEWS.

Vol. XLII. No. 1091.

A NEW PROCESS FOR THE METALLURGIC TREATMENT OF COMPLEX ORES CONTAINING ZINC.*

By EDWARD A. PARNELL, F.C.S.

The subject to which I have the gratification of inviting your attention, namely, the effective and remunerative treatment of certain complex ores containing zinc, has been an important problem in metallurgy. It is well known that the presence of zinc in considerable quantity in ores containing other metals, has a very injurious influence in the ordinary smelting processes for the extraction of the latter. In the case of copper ores, for example, the presence of much zinc necessitates additional operations in order to obtain copper of good quality; and all the zinc originally present in the ore is lost. Of still greater inconvenience is the presence of much zinc in lead ores, when the ordinary smelting processes are pursued; not only are the operations impeded thereby, but there is also a considerable loss of lead. Hence it follows that copper ores containing much zinc are rejected by the copper smelter, and lead ores containing much zinc are rejected by the lead smelter.

Further, ores of this complex character are not adapted to the use of the zinc or spelter manufacturer. From the peculiar character of his reduction process, in which the cost of earthen retorts form a considerable item in his working charges, it is essential that his raw material contain a considerable proportion of zinc: less than 25 per cent would be regarded as unsuitable. Not only so, but the presence of certain other metals, especially lead, is highly objectionable to the spelter maker from their injurious action on the earthen retorts. Zinc ore containing 8 per cent of lead would be discarded.

Hence the necessity of resorting to some means of separation before having recourse to the ordinary smelting processes. Of course the mechanical method of "dressing" is always adopted wherever it is practicable. In ordinary mixed ores containing galena and blende, this is easily accomplished by the great difference between the specific gravities of these two minerals; but in many cases separation by dressing is impracticable. Thus blende and copper pyrites resemble each other in density too closely to allow of separation by dressing. Again, blende and galena are often found so intimately mixed or combined (forming apparently a homogeneous mineral) that their separation by dressing is too difficult to be practised on the large scale; in fact, this is in some cases quite impossible. The plumbiferous blende found in Anglesea, commonly known as "bluestone" is of this character. Enormous quantities of copper ores also exist, the raising of which would be very profitable were it not for the contaminating influence of 20 per cent and upwards of zinc; but hitherto the highest prices paid for such by the smelter have been unremunerative to the miner. The cupriferous blende found in great quantities at Ain Barbar, in Algeria, is of this kind.

The extraction of zinc from such ores in a useful form has often been attempted by means of hydrochloric acid, after calcination of the ore, with the view of decomposing the chloride of zinc by lime. Serious impediments, however, to this method have prevented it from being adopted, especially the difficulty of washing the oxide of zinc from the chloride of calcium and obtaining it in a dense form, suitable for the spelter manufacturer; also, the injurious influence of the presence of chlorine in any form in the subsequent smelting of the residue containing lead and silver.

The separation of oxide of zinc from the calcined ore can of course be easily effected by sulphuric acid, but the great objection to this method has been the difficulty (hitherto generally considered insuperable on the large scale) of obtaining oxide of zinc from the sulphate, in a form suitable for the spelter manufacturer. In the process to which I would now invite your attention, this difficulty is completely overcome.

When heated alone, sulphate of zinc requires a very high temperature to effect its decomposition. Such a method is impracticable on the large scale; but when mixed with a small proportion of a deoxidising agent, sufficient to take one equivalent of oxygen from the sulphate, it is easily decomposed with the production of oxide of zinc and sulphurous acid. A mixture of 2 equivs. of zinc sulphate with 1 equiv. of carbon, heated to dull redness, affords oxide of zinc. With a larger proportion of carbon, sulphide of zinc is produced. Sulphide of zinc, whether artificial or native, is also well adapted for decomposing the sulphate. 3 equivs. of sulphate and 1 equiv. of sulphide produce 4 equivs. of oxide and 4 equivs. of sulphurous acid ($3ZnSO_4+ZnS=4ZnO+4SO_2$). Native sulphide of zinc is the reducing agent, which I prefer on the large scale for making oxide of zinc suitable for the zinc manufacturer.

I will now briefly describe the routine of operations at present pursued at the works of the Swansea Zinc Ore Company, which have been erected for carrying out the process on an extensive scale.

The principal ores operated on are the following, neither of which is "dressable."

1. Complex ore from Cavalo (Algeria), containing, as sulphides—Zinc, 17 p.c.; lead, 16 p.c.; silver, 20 ozs. per ton.
2. Cupriferous blende from Ain Barbar (Algeria), containing—Zinc, 23 p.c.; copper, 6 p.c.; silver, 6 ozs. per ton.
3. Complex ore from Italy, containing—Zinc, 20 p.c.; lead, 12 p.c.; copper, 5 p.c.; also silver.
4. "Bluestone" from Anglesea, containing—Zinc, 28 p.c.; lead, 12 p.c.; silver, 12 ozs. per ton; also gold and copper.
5. Ore from Constantine (Algeria) containing—Zinc, 12 p.c.; lead, 5 p.c.; copper, 1·5 p.c.; silver, 12 ozs. per ton; also gold.

After having been ground sufficiently to pass through a sieve of six or eight holes to the linear inch, the ore is calcined, with exposure to air, at a moderate heat. This calcination is effected in large muffle furnaces; the gas from which, when containing a sufficient proportion of sulphurous acid for making sulphuric acid, is conveyed to leaden chambers for that purpose. The sulphides of the various metals are thus converted into oxides and sulphates. A dull red heat is found by experience most favourable to the formation of sulphate of zinc; but it must be sufficient for the decomposition of ferrous and ferric sulphates. The calcined ore is next mixed with weak sulphuric acid in a revolving pan lined with lead (invented by Mr. J. W. Chenall); neutral liquors are first run off; afterwards excess of acid is introduced, and the acid liquors thus obtained, together with subsequent washings by water, are used for producing neutral liquors from a fresh charge of calcined ore.

The solution of sulphate of zinc thus obtained of course contains copper when that metal was present in the ore. Of iron it contains very little, provided the ore had been properly calcined, so as to peroxidise all that metal, the sulphuric acid combining by preference with oxide of zinc. I may mention, in passing, that a solution of ferric sulphate is decomposed when warmed with oxide of zinc, with precipitation of ferric oxide and production of sulphate of zinc.

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* A paper read before the Chemical Section of the British Association, Swansea Meeting, 1880.

From the clear neutral liquor copper is next precipitated in the usual way, either as metallic copper by means of iron or zinc, or as sulphide. The solution of sulphate of zinc is then concentrated by evaporation, and mixed when it begins to thicken with finely-ground blende, in the proportion of one equivalent to three equivalents of zinc sulphate. This mixture, after being further dried, is lastly heated to redness in a muffle furnace. The mutual reaction which I have already described takes place with production of oxide of zinc and sulphurous acid. The latter, having no admixture of gases derived from the fire, is conveyed to leaden chambers for the reproduction of sulphuric acid.

The oxide of zinc thus obtained is in a condition well suited for the manufacture of metallic zinc in the ordinary way. Its strength and purity of course depend to some extent on that of the blende used as the reducing agent. When the latter contains about 45 per cent of zinc, the oxide as withdrawn from the furnaces contains about 62 per cent of zinc. It contains no impurity which is of any inconvenience in its application for making metallic zinc. The principal impurity is oxide of iron, derived partly from the blende added in the process, and partly from the use of iron as a precipitant for the copper. The impurity which the zinc manufacturer regards as most deleterious—namely, lead—is reduced to a minimum, being only the small proportion contained in the added blende. The same may be said respecting calcium.

Occasionally the oxide of zinc contains a little magnesium, owing to the presence of magnesian minerals in the raw ore. But this metal is not present in the oxide in the objectionable form of sulphate, but in the innocuous form of oxide. It may be interesting for me to remark here that sulphate of magnesia is easily decomposed by blende and other reducing agents in a manner corresponding to sulphate of zinc. When a mixture of single equivalents of sulphate of magnesia and carbon is moderately heated, magnesia is obtained mixed with very little undecomposed sulphate. Magnesia thus prepared corresponds in density to what is known as "heavy" calcined magnesia. It will be obvious that this method of preparing that article will be found far more ready and economical than the common method of precipitation as carbonate and calcination.

I have now to revert to the portion of the ore left undissolved after treatment with dilute sulphuric acid. This, of course, contains all the lead originally present in the ore. The same may be said of the gold and silver. It might be expected that a portion of the silver would be sulphated and rendered soluble during the calcination of the ore. This is doubtless the case to a small extent, but I have always found that the minute quantity of silver thus rendered soluble is less than corresponds to the trace of chlorine present in chamber sulphuric acid, and chloride of silver is quite insoluble in solution of sulphate of zinc.

For the further treatment of this residue the ordinary well-known smelting processes are resorted to, the products being argentiferous and auriferous lead, and also copper regulus, which generally contains both gold and silver. At the works of the Swansea Zinc Ore Co. the smelting of the residue is effected in cupolas; an important appendage to which is the lead fume condenser patented by Messrs. Wilson and French, of Sheffield (brought before the notice of the British Association last year), in which the gases from the furnace are forced under water and allowed to pass up through layers of fine copper gauze. I am happy to be able to state that this condenser is now being worked very satisfactorily.

In conclusion I would remark, as will doubtless have occurred to you, that this mode of treating sulphate of zinc is capable of other applications besides the complex ores I have referred to. Such, for example, as the residue of blende after the richer portion has been separated. Immense quantities of such residue exist containing 22 per cent of zinc, the further dressing of which is not profitable. This is of no value in its present condition, but capable of profitable application by the process I have described. It may also be applied to lead fume containing zinc, and to the crude sulphide of zinc derived from burnt iron pyrites containing blende, of which a large quantity is used in Germany for making sulphuric acid.

In reply to enquiries respecting the density of the oxide of zinc thus prepared (a matter of great importance to the spelter manufacturer), Mr. Parnell stated that it is rather greater than that of ordinary calcined calamine. The oxide is in every respect admirably suited to the manufacture of zinc.

ON A

NEW PROCESS FOR THE PRODUCTION, FROM ALUMINOUS MINERALS CONTAINING IRON, OF ALUMINIUM SULPHATE FREE FROM IRON.*

By J. W. KYNASTON, F.C.S., F.I.C.

(Concluded from p. 193.)

After a long and tedious investigation, I found that arsenite of peroxide of iron is insoluble in cold solution of sulphate of alumina, if there be present in the solution a small proportion of the tetrabasic sulphate, and also a slight excess of arsenious acid. The presence of some tetrabasic sulphate in the solution seems to be essential, for the reason that carbonate of lime is the only substance which throws down the arsenite of iron. Lime itself, or even the caustic alkalies, do not render it insoluble; they produce in the liquid a deep reddish tint, but even after standing some weeks, there is little or no arsenite deposited. Carbonate of lime, on the contrary, instantly causes turbidity in the cold solution, and the arsenite of iron settles completely down. By the employment of these reactions, I found it to be possible to remove from a ferruginous solution of alumina practically all the iron existing in the ferric state, and to produce easily a sulphate of alumina from bauxite of a far higher degree of purity than has hitherto been produced from the finest quality of china-clay.

There remains, however, in sulphate of alumina so purified still a little iron in the state of protoxide, probably derived from some silicates or titanates of protoxide existing in bauxite. The proportion is about one-fourth of the whole iron contained in bauxite, so that by the arsenious acid and carbonate of lime reaction there is removed about three-fourths of the quantity present.

The removal of this ferrous iron for some time continued a great difficulty. Its separation from the alumina solution by the reaction with arsenious acid proved impracticable, inasmuch as all the ordinary oxidising agents which would peroxidise the iron converted the arsenious acid present into arsenic acid. Blowing air through the mixture was tried, but, even after prolonged blowing, scarcely a trace of iron was converted to the higher state of oxidation; and the attempt to effect its further removal by this means was abandoned. I then, almost hopelessly, resolved to try a well-known reaction for the removal of the protoxide of iron, a process first proposed to be used for this purpose, I think, by Robert Harvey, in a patent taken out by him in the year 1839. It will probably be correct to say that every worker on this subject has given more or less attention to this reaction, with a view to apply it for the separation of iron from aluminous solutions, for it is a reaction of so decided a character, and so well known to every tyro in chemistry, that it would naturally suggest itself as a means to accomplish the end in view. But just as it has

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* Read before the Chemical Section of the Britis Association, Swansea Meeting, 1880.

been found impossible to devise an analytical process for the separation of iron and alumina by the application of this reaction, so every attempt to use it in the manufacture of sulphate of alumina free from iron has also failed, and the difficulty has been considered insuperable. The reaction to which I refer is that with ferro-cyanides, the production of the well-known precipitate of Prussian blue. The reason of the failure successfully to apply this reaction hitherto is that the blue precipitate obstinately refuses to fall out of the solution; and if the proportion of iron present approaches only one-half per cent of the sulphate of alumina, scarcely any alteration in the appearance of the blue mixture is apparent, even after standing at rest for several days, and even if the solution is made excessively dilute, and the mixture be allowed to stand at rest until the visible precipitate has settled out, the solution still retains a blue colour, absolutely irremovable by any process of filtration, and, when concentrated by evaporation, so as to obtain a solid sulphate of alumina, the product possesses a deep blue colour, and has, for this reason, no commercial value.

The Prussian blue precipitate is indeed a nasty slimy mud, and so finely divided that it remains to some extent in suspension in the fluid for an indefinite period.

Notwithstanding the difficulties in the application of this reaction, I have devised a means of using it for the purification of aluminous solutions which is perfectly successful, and which involves a curious physical change in the character of the blue precipitate which has not before been observed.

When to a solution of sulphate of alumina, purified from peroxide of iron by the arsenious acid process as already described, but still containing about 0·20 per cent of iron and the excess of arsenic used, there is added a quantity of ferrocyanide of potassium exactly equivalent to the iron present, after standing a few days, the greater part of the light blue precipitate settles out. If now there be passed through the blue supernatant liquid a current of sulphuretted hydrogen to saturation, the sulphide of arsenic formed carries down with it undecomposed every particle of the suspended blue precipitate, leaving a perfectly clear solution; and when this solution is concentrated by evaporation to the solidifying point, pure sulphate of alumina, of which we have been in search, is obtained, and the degree of purity is such that the product can be used for nearly all purposes for which the crystallised alums have been required.

There is, however, in this process some loss of ferrocyanide, which under certain circumstances may be considerable, and which it is desirable to avoid. This led to the further prosecution of the investigation, and having found that a precipitate of sulphide of arsenic formed in the solution carried down with it the whole of the suspended blue precipitate, it seemed probable that another similarly flocculent precipitate produced in the liquid would effect the same purpose. The plan that naturally suggested itself was to add to the alumina solution a slight excess of ferrocyanide beyond that required to combine with the iron salt, and then sufficient solution of sulphate of copper to react upon the excess of ferrocyanide. The process gave the expected result. The minute quantity of flocculent ferrocyanide of copper apparently embraced within its grasp the previously imponderable blue precipitate, and the whole quickly settled down, leaving a clear, bright, colourless solution.

But further experience with these reagents soon led to a surprising and unexpected result. It was found that the addition of only a trace of copper, or even of zinc salt, to the aluminous solution, free from any excess of ferrocyanide, caused an aggregation of the particles of imponderable blue precipitate, and so altered its physical condition that it separated perfectly from the liquid, leaving it clear and colourless. The blue mixture, in which there was apparently no distinct precipitate, after the addition of an exceedingly small proportion of either copper or zinc salt, assumed the appearance of a colourless liquid, in which floated aggregated particles of Prussian blue. Thus there was produced a perfect separation of the whole ferrocyanide, which, by an easy process, could be re-converted into a soluble form, again to be used for the separation from aluminous solution of a further quantity of iron.

That the change in the condition of the blue ferrocyanide produced by the addition of a mere trace of copper salt is not owing to chemical decomposition of the iron salt, seems to be proved by the fact that the addition of a large excess of copper salt to freshly precipitated Prussian blue does not destroy its blue colour, whereas the opposite reaction does take effect, a precipitate of copper ferrocyanide suspended in liquid being, after some time, completely decomposed by an excess of iron solution. The change in the condition of the precipitate is, therefore, probably only a molecular alteration.

The reactions I have brought to the notice of the section in the foregoing rather lengthy disquisition have recently been applied to the commercial production of pure sulphate of alumina on a considerable scale, the plant just completed by the St. Helens Chemical Company being now equal to a weekly production of twenty-five tons, and it is in contemplation to increase it to double this quantity, or fifty tons per week.

A short description of the actual process of manufacture may not be unacceptable to the section:—Into a large lead-lined iron tank, placed at a high elevation, is ran about 48 cubic feet of sulphuric acid of 145° T., just in the condition in which it issues from the Glover Tower in the ordinary process of manufacture. Steam is blown into this until it is near the boiling point, and the strength is reduced to from 90° to 95° T. whilst boiling hot. A charge of 26 cwts. of bauxite finely ground, having been previously got ready, is thrown into the hot vitriol until 20 cwts. have been added, then 3 cwts. more, previously mixed with 140 lbs. (or about five per cent of the weight of the charge of bauxite) of common white arsenic is added, and lastly the remaining 3 cwts. of bauxite.

After the addition of from 8 to 10 cwts. of bauxite the mixture commences to swell, until it reaches to six or eight times its original bulk, and the bauxite is then more slowly added, each further addition causing a renewal of ebullition. To moderate the violence of the reaction, it is sometimes necessary to run in a little cold water; but if proper caution be observed this is unnecessary. As the reaction approaches completion, the mass subsides in the vessel, and, when a state of perfect quiescence is attained, steam is blown in for about half an hour, after which a quantity of hot water is added, so as to prevent the thickening of the mass, and then sufficient cold water to reduce the mixture to about 40° T. when cold. There is then thrown in 200 lbs. of common chalk, previously mixed with water to a thin cream, and passed through a fine sieve, and the whole is thoroughly agitated. The operation is now complete. The muddy batch of liquid, which contains in solution about 4 tons of sulphate of alumina, is ran off into one of a series of lead-lined tanks standing just on a lower level than the dissolver, and of such capacity that the batch fills it within a few inches. Here it is allowed to remain for from 12 to 14 days; but after 3 or 4 days, or when quite cold, a sample is taken, to which a little of the chalk mixture is added, and if this produces a deepening of the yellowish tint, more chalk is added to the batch, and this is continued until the addition of more chalk produces no alteration.

When the supernatant liquid is quite clear and bright it is drawn off into one of the second series of tanks, each of the same capacity as the first, placed on a still lower level, and the arsenite of iron is found at the bottom of the tank, superincumbent on the heavier mass of undecomposed bauxite, silica, &c., beneath.

The partially purified liquor now almost constantly contains in a cubic inch, which is equal to about 110 grains of sulphate alumina, 0·20 grain of iron as protoxide.

To each batch there is now added a quantity of solution of ferrocyanide of calcium, equivalent to the iron present, and, after 24 hours, 7 or 8 lbs. of sulphate of copper or sulphate of zinc; the mixture is thoroughly agitated, and allowed to stand until the Prussian blue has deposited, which requires 12 to 14 days. The colourless, or slightly blueish liquid, now free from iron, is drawn off to a stoneware tank, and the blue mud is passed to a wooden filter press, and the ferrocyanide well washed with water within the press. The pressed cake is removed from the press, broken up with water, and steam is blown into the mixture. Milk of lime is then added until the blue colour is entirely destroyed, and a solution of ferrocyanide of calcium is obtained ready for the purification of another batch of aluminous solution.

The solution of sulphate of alumina, now containing only as an impurity the excess of arsenious acid used in the process, is agitated in a stoneware vessel, and sulphide of calcium, obtained by heating together, in a gas retort, plaster-of-Paris and coke, is gradually thrown in. The arsenic is completely removed as sulphide, together with the trace of copper. The now pure solution is boiled down until the density reaches 106° to 108° T., whilst boiling hot, ran into leaden trays in which it solidifies, and the process is complete. The result is a sulphate of alumina practically free from iron, containing no free acid, and in which the percentage of alumina reaches 15 to 16, or about one-half more than that contained in the ordinary crystallised potash salt.

Before concluding my paper, I should like, with the permission of the meeting, to add, that the economical production of this pure sulphate of alumina will probably lead to the extensive prosecution of a comparatively new industrial application of salts of alumina.

In a paper, published in the year 1869, by Dr. Wallace, of Glasgow, he pointed out that in the manufacture of beet-root sugar, syrups are found containing large quantities of potash salts, and that the presence of these salts prevents the separation of a considerable proportion of the sugar by the usual process of crystallisation. According to Dr. Wallace, the proportion of these salts in beet-sugars, such as are used by refiners in this country, varies from 1 to 4 per cent, and in extreme cases contains even a larger quantity, and it is said that every 1 per cent of these salts prevents the crystallisation of 5 per cent of cane-sugar. Another great authority on beet-root-sugar, viz., Walkhoff, wrote in 1871, directing attention to this subject. He says that a process for successfully eliminating the alkalies from saccharine solutions would completely renew the face of the sugar industry and assure its future.

The investigation of the subject by the Messrs. Newlands, of London, led to the discovery by them that the elimination of potash from solutions of sugar could be readily effected by means of sulphate of alumina, the addition of this substance giving rise to the production of potash alum, which deposits from the sugar solution in minute crystals. In 1875, in conjunction with these gentlemen, Mr. James Duncan, the eminent sugar refiner, of the Victoria Docks, London, took out a patent to secure the process, and it has since been used by him most successfully, and at the present time he is producing potash alum at the rate of 1000 tons per annum, the whole of the potash required being extracted from sugar solutions by means of sulphate of alumina. Although, however, many of the principal refiners of the United Kingdom, and several other refiners in Holland, Belgium, and the United States, have taken out licenses to work the process, still it is not used to any extent elsewhere, and, so far as I have been able to ascertain, the sole reason for this abstention is the difficulty of obtaining sulphate of alumina sufficiently pure to prevent contamination of the sugar solutions. At the works of Mr. Duncan, a ferruginous sulphate of alumina is used, and afterwards the iron introduced into the sugar is removed by precipitation as sulphide by means of sulphide of calcium, which adds materially to the difficulty of the process.

Thus it appears that the economical production of pure sulphate of alumina will not only effect a saving of the immense quantity of potash now applied specially to the production of alum, but will open up a new and hitherto untapped source of wealth, in the extraction of this valuable base from saccharine solutions.

ON THE CONSTITUTION OF ULTRAMARINE.*

By H. ENDEMANN, Ph.D.

Many have been the attempts to account for the peculiar and high colour of ultramarine. Some have looked upon certain impurities as the actual bearers of the colour (iron sulphide), others speak of a blue modification of sulphur, and others again of silicium and aluminium sulphide in very fine dissemination throughout the mass, but without chemical combination with the balance of the constituents. Lately, apparently, it has become a matter of taste to abandon entirely the discussion of the derivation of colour. The statement is made that this question cannot be decided, that it is impossible, for instance, to state why certain salts of cobalt are red and certain salts of copper blue. Nobody has attempted to decide why certain copper salts are blue, but it must be acknowledged that in dealing with certain compounds of copper of blue colour, constitutional peculiarities may be readily recognised in all of them, by which they may be easily distinguished from other salts of copper which are colourless. The colour of the salt in doubtful cases is a very positive indicator with regard to the type from which the constitutional formula should be derived.

We are justified in insisting upon and foretelling for a coloured compound an unusual grouping of the atoms, if its constituents, otherwise under hitherto accepted arrangement of the atoms, would produce but colourless compounds.

If, in ultramarine, we consider the proportion of aluminium, silicium, and sodium, to the non-metallic constituents, oxygen and sulphur, we find an excess of the latter. It becomes necessary to ascertain and utilise this excess in the construction of the constitutional formula.

Aluminium is regarded as a four atomic element. All ordinary aluminium compounds are assumed to contain two aluminium atoms linked together by two of their atomicities, leaving six atomicities for combination with other elements. There is a possibility of the existence of compounds without this binding of two atoms by one atomicity,—a possibility that compounds derived from the as yet unknown compounds AlO_2 and Al_2O_4 may be prepared.

Considering the composition of ultramarine blue, we find that it consists of silica, soda, alumina, sodium monosulphide, and sulphur, the latter also in chemical combination, as it cannot be removed by solvents unless these act chemically at the same time. Most authors consider it as being united with sodium sulphide, forming polysulphurets. These substances forming ultramarine, I do not, however, consider as serving all the same purpose. To simplify the explanation, I propose for the present to regard one portion of the soda and alumina and the whole of the silica, merely as a vehicle necessary for the preparation and existence of ultramarine as they serve to facilitate the reactions in the first instance, while later in the process they may act in protecting the product against too powerful oxidation. This silicate may have a varying composition, and for this reason alone it would be difficult to correctly ascertain its quantitative relation to what may be called the colour nucleus, *i.e.*, the last group on the constitution of which the colour of the ultramarine depends. This we can suppose in ordinary ultramarine blue to be:—

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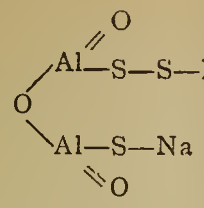
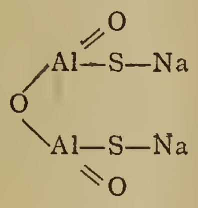
* Read before the American Chemical Society, October 7, 1880.



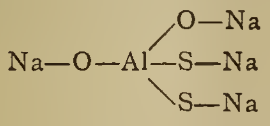
and in Heumann's ultramarine blue (*Liebig's Annalen*, 203, 190):—



The first of the two, under the influence of acid, will give two-thirds of the sulphur as such, while one-third will be obtained as H_2S. The latter (Heumann's) will give half as sulphur and half as H_2S.

While, therefore, Heumann's ultramarine-blue must be considered the purest, we see that ordinary ultramarine-blue may contain more sulphur in chemical combination. How much sulphur may thus be added greatly depends upon the process of manufacturing the ultramarine, as I will show hereafter. A substance may therefore be a good blue, and yet analysis will give no fixed relations between the *a* and *b* sulphur, as mixtures of ultramarine rich and poor in sulphur may exist, and many analyses, which have been not generally accepted as correct, should not be rejected, provided the method of analysis, which has been used, allows safe deductions to be made.

I. *Ultramarine White.*—The mother substance in the manufacture of blue ultramarine is ultramarine-white. To it, as it is white, I assign a constitution analogous to that of the neutral sodium silicate, a portion equal to half of the oxygen being substituted by sulphur.

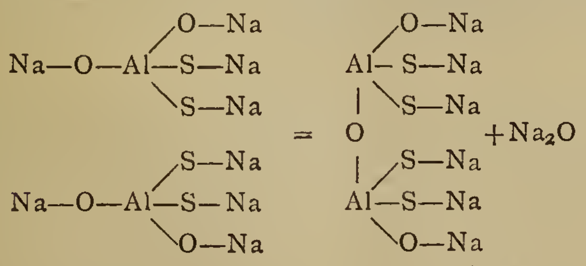


This constitutional formula includes the possibility of partial substitution of the aluminium by silicon. In the formation of ultramarine-white, the presence of the large quantity of sulphur and soda appear as the cause of the breaking of the bond between the two aluminium atoms in alumina. Two molecules of this substance acted upon by acids and water will yield 1 sulphur and $3H_2S$, which corresponds with the best ultramarine-white prepared (Ritter). Most ultramarine-whites yield by the action of aqueous acids more free sulphur, but it will be readily seen that the above formula allows a ready interpolation of furthur sulphur atoms. Dry hydrochloric gas acting upon two molecules of ultramarine-white produce directly ultramarine-blue, chloride of sodium, water, and sulphuretted hydrogen.

$$Al_2Na_8O_4S_4+6HCl=Al_2Na_2S_2O_3+6NaCl+H_2O+2H_2S\cdot$$

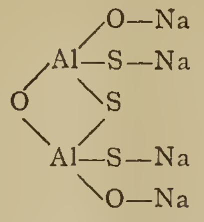
By the action of sulphurous acid gas ultramarine-white is first converted into ultramarine-green, two atoms grouping together under loss of Na_2O, forming the first coloured compound:—

II. *Ultramarine-Green*:—



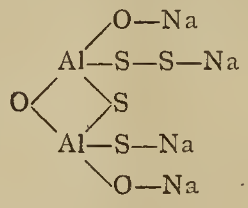
Ultramarine-white. Ultramarine-green.

which then, by the action of oxygen, which forms sodium sulphate, under further loss of Na_2S, passes into the jet-green compound—



Aqueous acids with the first greens would produce $3H_2S$ to S, and with the second green $2H_2S$ to S. It is well known that ultramarine-green contains rarely, if ever, the sulphur in such distribution; but this is easily explained by the assumption that these are derived from a white containing interpolated sulphur.

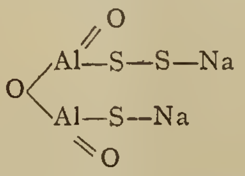
It is even possible that by the combined action of oxygen and sulphurous acid an interpolated green may be obtained, the sulphur for the sulphate being then furnished by the sulphurous acid, so that in this case but sodium is removed, leaving—



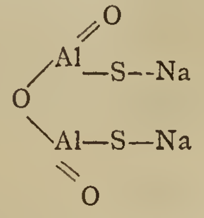
which compound, with aqueous acids, would yield $2H_2S+S_2$.

Gaseous dry HCl would produce all of these compounds, sodium chloride, and H_2S, leaving the blue on treatment with water in the insoluble residue, but this gas has perhaps rarely been used free from air and moisture, and the consequence has been that the reaction is generally accompanied by secondary ones. One of these would be that the mass would not lose so much in sulphur as is indicated by the above-stated reaction. Another is indicated by the presence of aluminium chloride in the wash-water, which is no doubt produced by water, which is produced by the action of air on the hydrochloric gas. In the formation of blue from white directly, water is one of the products of the reaction, and in this case the formation of aluminium chloride is therefore unavoidable, and this substance is therefore a regular concomitant of the reaction.

III. *Ultramarine Blue* is in the manufacture produced by the action of sulphurous acid and oxygen on the green. Thereby more sodium is removed, and the middle sulphur interpolated in one of the groups ending with Na. The sodium is converted into sulphate. The action of chlorine will produce the same product, leaving—



This product should yield with aqueous acids H_2S+S_2. Hydrochloric acid gas would form $H_2S,2NaCl$, and



which latter substance, with aqueous acids, would yield H_2S+S.

This is Heumann's ultramarine blue, which in this purity has only once been prepared, owing undoubtedly to the superior care used by Heumann in the production of this compound. Further removal of sodium, and perhaps partial substitution of sulphur by oxygen, seem then to form the violet, the so-called red, and the yellow. I could so far not find sufficient analytical data to justify formulæ for these compounds.

It is an accepted fact that if the presence of both silica and alumina for the manufacture of ultramarine is not absolutely necessary, it at least will greatly facilitate the reaction, and there is therefore a possibility not only that silica may introduce the reaction, but also that it may participate in the formation of the atomic complex, which is the cause of the colour. To what extent this is done on the ultramarines deficient in silica we cannot easily ascertain. It is certain, however, that silica will tend to increase the evolution of sulphuretted hydrogen, when such ultramarines would be acted upon by aqueous acid.

Such partial substitution may also be the cause for the varying proportions in which *a* and *b* sulphur are found to exist in the various ultramarines.

As regards the silicate next, I will quote here an analysis, made about 3 years ago for the American Ultramarine Works, which gave the following results:—

		Atomic Weight.	Atomic Relation.
SiO2	39·28 :	60	0·655
Al_2O_3	26·41 :	102·8	0·257
Na_2O	20·83 :	62	0·336
S	12·98 :	32	0·406

The sulphur being in two forms, two-thirds being obtained as S while one-third was obtained as H_2S, we divide 0·406 by 3, which gives 0·135 = 1 atom of sulphur. For S_3=0·406 we require Na_2O=0·135, and Al_2O_3=0·135 for the colour nucleus. This leaves for the silicate—

Na_2O	0·201
Al_2O_3	0·122
SiO_2	0·655

or about $3Al_2O_3,5Na_2O,16SiO_2$.

This is certainly a somewhat disagreeable rest in itself to deal with, without trying yet to add to it the colour nucleus, to obtain one formula for the whole of the substance, which we call ultramarine. Moreover, almost every sample will be found to differ from the other even from the same factory, and of course more yet when products of different factories should thus be compared.

As far as I know, in the United States, only ultramarine rich in silica is prepared, and as only in those poor in silica the whole series of compounds which are necessary for the thorough study of the genesis of ultramarine are readily recognised, I found myself unable to contribute any new facts, but had in the construction of the formula to rely only on the excellent work which has been done by chemists more favourably situated.

The literature of ultramarine contains quite a series of analyses in minute details, but not one single investigation follows quantitatively all the stages of transformation throughout the whole series of possible compounds.

Such an investigation of course would merely prove that the relation of the sulphur and oxygen to aluminium and sodium were or were not in the ratio indicated.

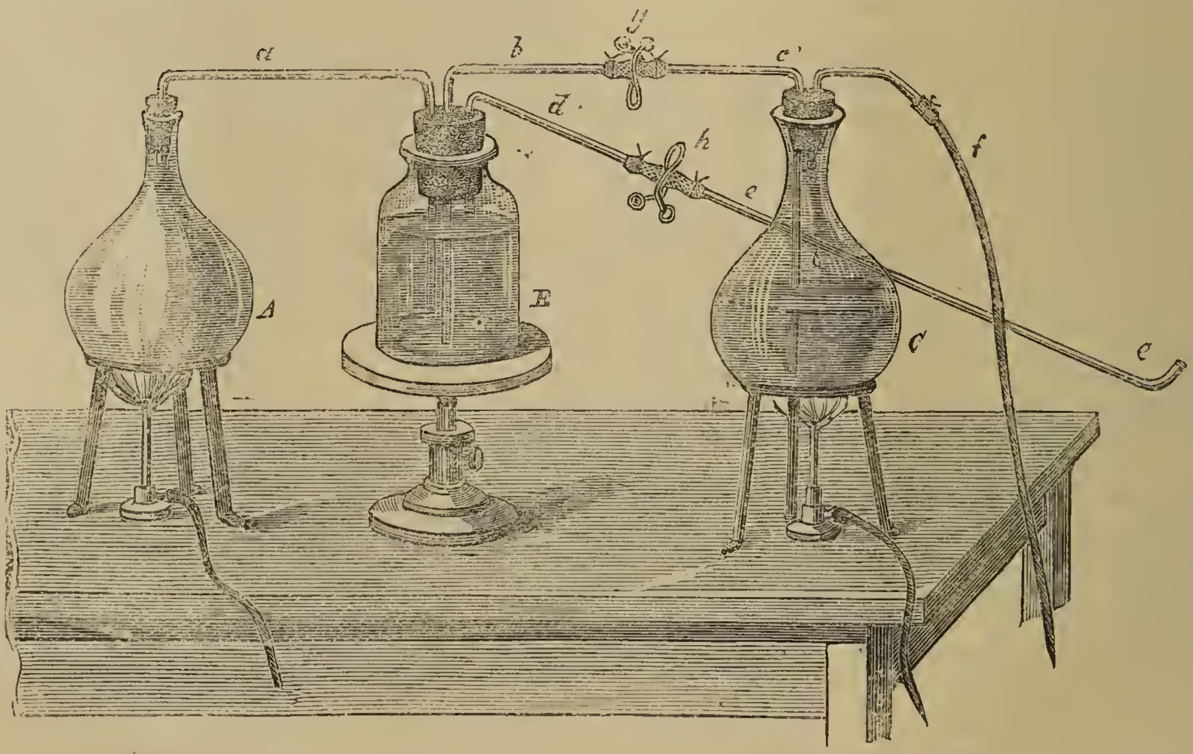
I would not demonstrate the actual existence of compounds as those assumed by me.

The possibility of the existence of such compounds will have to be proven in a more direct manner. Experiments which I have made so far to substantiate this theory have not led to any positive results.

ON BUNSEN'S METHOD FOR DETERMINING FREE OXYGEN IN WATER.

By J. KŒNIG and C. KRAUCH.

The gases are expelled from the water and collected by means of the following apparatus (*Zeitschrift Anal. Chemie*, fig. 17, p. 260, 1880). The use of this apparatus is exceedingly easy and convenient.



The vessels B and C are filled (the former entirely, the latter only half), with water which has been boiled for some time so as to be free from air. The glass tubes, *a*, *b*, *c*, are also filled by blowing into the flask, *c*, and opening the pinch-cock, *g*. The flask A is then entirely filled with water which has been thoroughly boiled, and is free from air, and quickly connected with *a*. If, whilst effecting the connection, we close the pinch-cock *g*, the stopper is generally spontaneously pressed into the flask A, the water of which is colder than that in B and C. The flask A is then boiled for some time, which brings the water in B also to a boil and after opening *g*, it is driven into the flask C. When this has taken place entirely or partially the gas flame is removed from under A. In consequence of the cooling the water flows back from C into B and A. The air which is still present collects underneath the stopper in B, and is expelled by opening the pinch-cock h, and blowing into the flask C.

This boiling and driving the water backwards and forwards from A and B into C is continued before every determination till no more air collects in B. When this is effected the pinch-cock g is closed, the flask A is quickly removed, emptied, and filled entirely with the water under examination. During this operation a little air always enters into the tube a, but by closing g, and blowing air into C, a can be easily and entirely re-filled with the de-aërated water, and when the stopper is quickly placed in the flask A, which has been entirely filled with the water to be examined, on simultaneously opening the pinch-cock g, A may be connected with B in such a manner that not a bubble of air enters. A is next heated to a boil, leaving g open, when the air present in the water, mingled with watery vapour, passes into B. The boiling in each case must, however, not be continued till all the water in B has been driven through the tube b (which reaches to its bottom) into C, as then some of the air would also be expelled.

When the water in B has been expelled down to about the middle of the vessel the flame is removed from beneath A, whereby water passes again from C to B and A, whilst the expelled air collects in B underneath the stopper; as soon as A is entirely filled with water the boiling is resumed and repeated in the same manner from 6 to 10 times. When the air has thus been completely expelled from the water in A—which is very difficult and can scarcely be effected with quantitative accuracy—the flame is removed from A, which is allowed to fill itself again with water, the pinch-cock h is opened, and the air which has collected in B is driven by blowing into C completely out of B through the tube $d\,e$ into the eudiometer. This can be easily and accurately effected. A certain quantity of water is indeed carried along into the tube of the eudiometer, but this serves to dissolve the solid potassa which is introduced in small fragments in order to absorb the carbonic acid present in the air. When no further decrease takes place the volume of the air is measured, noting the indications of the barometer and thermometer. In order, in this mixture of nitrogen and oxygen, to determine the latter, a concentrated solution of pyrogallic acid is employed, which is left in contact with the air for several hours, frequently agitating the tube.

The level of the pyrogallic solution and the column of mercury are measured, and like the water pressure reduced to mercurial pressure. In calculating the volume of gas at 0° and 760 m.m. pressure the tension of the vapour of pure water is assumed, and not that of the potassic solution of pyrogallic acid. This assumption is not quite accurate, as the potassic solution has a somewhat higher specific gravity than 1, and its tension is less than that of pure water. As in the first case too small a quantity is subtracted from the barometric reading, and in the second case too much, the trifling error is compensated. Where great accuracy is required the sp. gr. of the potassic solution should be determined. (See also "Bunsen's Gasometric Methods," 2nd edition, 1877, p. 387).—*Zeitschrift Anal. Chemie.*

PROCEEDINGS OF SOCIETIES.

THE AMERICAN CHEMICAL SOCIETY.*

The regular meeting of the Society was held, September 9th, 1880, at University Building, Washington Square, New York. The following papers were read:—

"*Displacement of the Absorption-Bands of Purpurin in Solutions of Alum.*" By Henry Morton, Ph.D. This paper drew attention to the fact that the position of the two principal absorption-bands of purpurin dissolved in alum solution was notably affected by the amount of alum in the solution. Thus, a solution of alum containing 2 per cent of the salt showed the bands at conspicuously lower points than a solution containing 1 per cent, and this again showed a lower position than a solution containing half per cent. The change seems to be gradual and to amount to a depression of the bands by addition of alum to the solution, everything else remaining the same. A similar depression was also found to be caused by heating these solutions, the band returning as they cooled. Soda, potash, and ammonia alums, and sulphate of alumina were used and found to operate similarly. In the discussion which followed the reading of this paper, Dr. Morton remarked that the displacement was towards the red end of the spectrum. In two cases the observations were $50^{\circ}\cdot26'$ and $50^{\circ}\cdot27'$ when cold, and $50^{\circ}\cdot20'$ and $50^{\circ}\cdot21'$ when hot. Dr. Morton also remarked, that the heat employed in the experiments did not destroy the purpurin.

"*Examination of the Raw Materials used for Arita Porcelain.*" By Nawokichi Matsui, Ph.D., of the School of Mines, Columbia College. The samples which form the subject of this examination came from Koransha, a large porcelain manufacturing company of Arita, in the province of Hizen, Japan. As this locality has been the great centre of this industry in Japan for about three centuries, the examination of the raw materials used there will also give a tolerably good idea of those generally used in Japan. The native minerals used in the manufacture are all white, varying considerably in hardness, and apparently showing different stages of the decomposition of the felspathic rocks. The larger part of this paper is occupied with the results of the analyses of the sixteen different materials used in the manufacture, and also with the results of determinations of specific gravity and fusibility.

NOTICES OF BOOKS.

The Laws of Health. By W. H. Corfield, M.A., M.D., &c. London: Longmans and Co.

This useful manual contains, along with much sound information and valuable advice, some matter which is at least questionable. Thus under the head "Tea, Coffee, and Chocolate" the author writes:—"In almost all countries the inhabitants have prepared decoctions from these plants, or from others possessing similar properties. They resemble all vegetable decoctions in containing an astringent substance called tannin." Now in the published analyses of the cacao-bean, freed of course from its husk or shell, and also of chocolate, we find no mention of tannin. His condemnation of the use of tea and coffee at meals is on chemical principles perfectly just. The "tea-dinner" is a fruitful cause of indigestion and debility, especially among women of the poorer classes.

We are glad to find that Dr. Corfield bestows on the intermittent system of water-supply his full condemnation.

With the author's remarks on the disposal of sewage we cannot agree. We wish that those who so perseveringly uphold irrigation—especially in a country where the soil and the atmosphere are often too moist for the most important crops—would for once endeavour to refute, if they can, the experimental evidence obtained by Mr. Smee, jun., on the ready putrescence of plants raised by sewage irrigation.

Tables for the Analysis of a Simple Salt for Use in School Laboratories. By A. Vinter, M.A. London: Longmans and Co.

This little work belongs to that class of elementary chemical treatises which are the despair of the critic. They

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* Communicated by Arthur H. Elliott, F.C.S., Recording Secretary.

are on a level with the knowledge of the day, and patient examination fails to detect in them any errors. As little does it, unfortunately, reveal any reason for their existence, or show any distinct advantage which one of these books possesses over others.

There are numbers of coal-tar colours which are beautiful, pure, easy of application. But dyers will not use them because they have no tangible superiority over other colours which are in general employment. Consequently the colour-makers decline to manufacture such compounds. Might not teachers of chemistry act on the same principle, and use old manuals, handbooks, and outlines till they can produce something decidedly better than what has gone before?

Spirit-Gravities, with Tables. By Thomas Stevenson, M.D. London: J. Van Voorst.

These Tables show in parallel columns the specific gravities of dilute alcohol at 60° F., water at 60° being = 1; the respective percentages of alcohol by weight and by volume; and the percentages of "proof-spirit." The question may be raised, what good end is served by retaining the notion of proof-spirit, a mixture of 49·24 per cent alcohol by weight with 50·76 water? Dr. Stevenson gives a history of previous tables, and of the experiments upon which they were based. He speaks in very laudatory terms of the researches of Gilpin, which, though made as early as 1790-94, were conducted with the utmost nicety, "every source of error which even the science of the present day points out being avoided, except that the weighings were not reduced to a vacuum"—a source of error which only affects the fifth decimal in the gravity. Tralles, who in 1811 was invited by the Prussian Government to reconsider the question, and whose tables are in official use over a great part of the Continent, pays the highest compliment to the accuracy of Gilpin's work. The tables of Fownes, drawn up in 1847, are pronounced less accurate than those of Gilpin. Mr. Hehner has published a set of alcohol tables based upon those of Fownes, but his numbers are said to be inaccurate to the extent of nearly ½ per cent of proof-spirit.

Dr. Stevenson's tables will prove exceedingly useful for public analysts.

Annual Report of the Board of Regents of the Smithsonian Institution for the Year 1878. Washington: Government Printing-Office.

The principal event mentioned—now, of course, no longer recent—is the death of the illustrious physicist, Prof. Joseph Henry, for many years Secretary of the Institution, and the election of Prof. S. F. Baird as his successor. The report contains a biographical memoir of Prof. Henry, by Prof. Asa Gray, and a catalogue of the memoirs and papers (149 in number) which the deceased had communicated to learned societies, scientific journals, &c.

We have, further, biographies of Condorcet and of Louis Agassiz; papers on Prof. Henry's share in the development of the telegraph; on the "Effects of Irritation on a Polarised Nerve," by Dr. B. F. Lautenbach, and a posthumous memoir by Prof. Henry, entitled "Researches in Sound, with especial reference to Fog-Signalling," drawn up for the United States Light-House Board.

Qualitative Chemical Analysis. A Guide in the Practical Study of Chemistry and in the Work of Analysis. By Silas H. Douglas, M.D., and A. H. Prescott, M.D. Third Edition, wholly revised. With a Study of Oxidation and Reduction by Otis Coe Johnson. New York: Van Nostrand.

The most novel and interesting part of this work is Mr. Johnson's "Study of Oxidation and Reduction" which is given as an appendix. The author commences with a table in which a number of bodies are arranged according to their mutual oxidising or reducing action. The acids are placed in order of the atomic weight of the element in each which suffers oxidation or reduction, whilst the bases follow the usual analytical groups. The word "bond" is used in a novel sense to signify the amount of reduction it is capable of sustaining. He defines a bond as "oxidising force" and if an element has no oxidising power it has no bonds. If its only efficacy is that of a reducing agent its bonds are represented by a negative number. To ascertain the bonds of an element a series of rules are given. Hydrogen in combination has always one bond and it is positive; oxygen in combination has two always negative. Free elements have no bonds. The sum of the bonds of any compound always = 0. Acid radicals are always negative, the bonds of the radical being equal to the number of atoms with which it is capable of combining. Metals in combination are commonly positive, their hydrogen compounds being the chief exceptions. Oxidising agents increase the bonds of some other substance whilst reducing agents diminish it. The oxidation of any substance involves the reduction of some substance. From these principles the author derives a rule for writing equations so that every equation involving oxidation may be balanced at once if the products formed are known. The number of bonds changed in one molecule of the oxidising agent shows how many molecules (or atoms, in the case of free elements) of the reducing agent must be taken. Thus $2HNO_3+3H_2SO_3=3H_2SO_4+2NO+H_2O$ The nitrogen in HNO_3 has five bonds and in NO it has but two, losing three. Therefore three mols. H_2SO_3 must be taken. The sulphur in H_2SO_3 has four bonds and in H_2SO_4 it has six, gaining 2. Therefore 2 mols. HNO_3 are needed for the reaction.

For thorough training in the principles of oxidation the author advises the student to prepare a number of compounds with pure ingredients, determining the exact proportions to be used according to the system laid down. The body of the work, the analytical portion, requires little comment. The directions given, as far as we have been able to perceive, are accurate and well arranged, and the work is enriched with useful tables.

Trade Report of Gehe and Co., Dresden. September, 1880.

This report gives as usual very full and accurate information concerning the prices of drugs and chemicals and their probable value in the immediate future. It is remarked that, with few exceptions, the aniline colours are still declining in price. The dyes most in demand are:—Fast Scarlet G G N, Phosphine N and R M, Magenta S, Methyl Violet 5B and 6B, and Methyl Green A, Eosine W, and V3 are more generally employed since the reduction in price. Considerable attention is being drawn to the milky juice of *Carica papaya*, which, however, appears difficult to collect in quantity.

CORRESPONDENCE.

THE SPECIFIC ROTATORY POWER OF CANE- AND INVERT-SUGAR.

To the Editor of the Chemical News.

Sir,—In a letter (Chemical News, vol. xlii., page 196) referring to my paper on the above subject, Mr. Alexander Watt questions my statement that "many authorities appear to have assumed that a solution of inverted sugar contained the same percentage of sugar as before inversion." In support of his scepticism Mr. Watt adduces the fact that such an error would cause the adoption of too high a number for the specific rotatory power instead of one below the truth.

In proof of my statement that some authorities have really ignored the increase of weight by inversion I may refer your correspondent to "Watts's Dictionary of Chemistry," vol. v., p. 472. He will find it distinctly involved in the calculations given, that a solution of cane-sugar which originally had a rotatory power of $+73\cdot8$ degrees will after inversion rotate 25° to the left.[*]

In the new edition of "Miller's Chemistry," Part III., Section I., edited by H. E. Armstrong and C. E. Groves, a similar error occurs on page 605. Taking the values $+57\cdot6$ and -106 as the specific rotations of dextrose and levulose respectively, the editors derive $-24\cdot2^\circ$ as the value of *aj* at 14° C. for invert sugar. So far, all is right; but they go on to say—"The number of degrees of lævo-rotation produced by a solution of the invert-sugar formed from a solution of cane-sugar of known dextro-rotatory power, is calculated by multiplying the number of degrees of dextro-rotation exhibited by the cane-sugar solution by 0·32, since the specific rotatory of cane-sugar is $[a]j=73\cdot8^\circ$ and $24\cdot2=73\cdot8\times0\cdot32$." Here the increase of weight by inversion is again distinctly ignored.[†] With the correction the factor would be 0·3455, a figure which differs considerably from the correct one, 0·37.

The above instances are sufficient to establish my statement that the increase of weight by inversion had not unfrequently been overlooked. I by no means intended to intimate that the error had invariably occurred. On reading my paper in print I noted some obscurity in the wording, and regretted that absence from home had prevented me from correcting the proof before publication.

I am indebted to Mr. Watt for pointing out the true origin of the value -25°, hitherto universally adopted as the specific rotatory power of invert-sugar. As this calculated value is dependent on an erroneous number for dextrose and a doubtful one for levulose it is quite time it became obsolete. As stated in my paper, the true value of $[a]j$ for levulose is most probably $-109\cdot6$ at 14° C., instead of -106°, as commonly stated.

While writing on the subject of specific rotation, I may call attention to the confusion caused by chemists not distinctly stating the degree of hydration of the substances operated on. Crystallised dextro-glucose contains 1 atom of water, and apparently the low results of some observers are due to the employment of the imperfectly dehydrated sample. Similarly, a recent determination of the specific rotation of milk-sugar by Mills and Hogarth is deprived of much of its value by the omission of the authors to indicate the condition of hydration of the sample.—I am, &c.,

ALFRED H. ALLEN.

Sheffield, October 16, 1880.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 7, August 16, 1880.

Polar Electricity in Hemihedral Crystals with Inclined Surfaces.—MM. Jacques and Pierre Curie.—All the facts observed hitherto agree in showing that in all the non-conductive substances with inclined surfaces which have been examined there is the same connection between the position of the hemihedral facettes and the direction of the phenomenon of polar electricity. The physical signification of the above will be better understood by saying more colloquially, but more tersely, that the more pointed extremity of the hemihedral form corresponds to the positive pole by contraction, whilst the more obtuse extremity corresponds to the negative pole.—M. P. Thenard remarked that the same phenomenon had been observed by his son fifteen years ago.

New Results of the Utilisation of Solar Heat obtained at Paris.—M. A. Pifre.—The author's improved apparatus enables him to utilise 80 per cent of the solar heat, thus obtaining, at Paris, 12·12 cal. per minute and per square metre of surface exposed to the sun.

Production of Crystals of Chromium Sesquichloride of a Persistent Green Colour.—M. A. Mengeot.—The author allows hydrochloric acid to act upon potassium bichromate dissolved in water. If the solution is allowed to evaporate for about ten months the bottom of the vessel is found lined with deep violet crystals of chromium sesquichloride, but among these large violet crystals are some small green crystals of a salt of chromium. According to all authorities the green salts are only formed at 100°; they are not crystalline, and they gradually pass into the violet condition. But the production of these green crystals takes place at common temperatures, and they have remained green for more than two years.

No. 8, August 23, 1880.

Remarkable Instance of Lightning Ascending Vertically.—A. Trécul.—The author perceived during the storm of the evening of August 19th, lightning ascending perpendicularly behind the trees of the Place Jussieu, apparently from the conductors of the wine magazine.

The Sun has a Sensible Inductive Action on the Earth, even when its Magnetic Power is simply equal to that of our Globe. Induction of the Moon by the Earth and Diurnal Lunar Variation of the Terrestrial Compass.—M. Quet.—The sun induces the earth in various manners; by its rotation, by the speed of the earth in its orbit, by the rotation of the earth, and by the variations which it experiences in its electric constitution. The electromotive forces due to the three first-mentioned causes are:—The first 14 times greater than the second, and the second 72 times greater than the third.

The Variations of the Coefficient of Expansion of Glass.—J. M. Crafts.—In his former papers the author has summed up the most important theories on the variation of the fixed points of thermometers, but the variation of the coefficient of expansion of glass, which presents a much more serious inconvenience, has hitherto escaped notice. If this coefficient varies, the interval between two fixed points varies, and the graduation becomes inexact. In thermometers, heated for a long time to 355°, the coefficient of expansion decreases, so that whilst the zero-point is raised by t degrees, the point 100° is raised to $100^\circ+t+l$.

Tungstoboric Acid.—D. Klein.—This acid differs in its constitution from various other borotungstic acids which have been prepared, and is the analogue of the unknown decatungstic acid. It is formed by the union of 9 mols. tungstic acid, 1 mol. dimetaboric hydrate, with elimination of 6 mols. water. Its composition is—

$$9WO_3,Bo_2O_3,4H_2O.$$

Products of the Distillation of Colophonium.—Ad. Renard.—The author has isolated a carbide, which he names heptene, of the sp. gr. 0·8031 at +20. It is without action upon polarised light, and boils at 103° to 106°. He examines its behaviour with reagents.

No. 9, August 30, 1880.

Dilatation and the Compressibility of Gases under Strong Pressures.—E. H. Amagat.—The author concludes from his researches that the coefficient of expansion

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[*] By a curious misprint the value +25° is in one equation printed "+27." As the sum of 27+73·8 is twice stated to be 98·8 it is clear that the 27 is really a typographical error.

[†] This is the more curious, as on page 609 of the same work the same error is not made.

of gases for temperatures above the critical temperature increases with pressure up to a maximum, on passing which it decreases indefinitely. The maximum diminishes for the more elevated temperatures, and finally disappears. For pressures lower than the critical pressure the deviation, which is at first positive at a temperature sufficiently low, becomes null, and then negative as the temperature increases; but, proceeding from a certain negative value, it diminishes indefinitely without changing its sign. For the pressures comprised between the critical pressure and a superior limit, special for each gas, the period during which the deviation is positive is preceded by a period where it is negative, so that the deviation changes its sign twice.

The Amylamines of Inactive Amylic Alcohol.—R. P. Plimpton.—A description of a diamylamine and a triamylamine, both inactive.

Intensity of Certain Phenomena of Atmospheric Electricity Observed in the North of the Sahara.—L. Amat.—In tropical countries the electric phenomena of the atmospheric stratum in contact with the soil are more distinct than in colder climates.

No. 10, September 6, 1880.

Researches on Basic Salts and on Atacamite.—M. Berthelot.—The author considers that in this compound, $CuCl,3CuO,4HO$, the water serves as the chief connecting link. A metallic salt may be completely precipitated and the resulting liquid neutralised without an equivalence between the precipitating alkali and the acid of the metallic salt, a portion of the latter being carried down in the precipitate. A great number of metallic salts behave in an analogous manner.

Contribution to the History of the Ethers.—M. Berthelot.—The transformation of the simple ethers into alcohols corresponds in a state of solution to a thermic phenomenon, which is almost *nil.*

The Law of Electro-magnetic Machines.—J. Joubert.—A mathematical paper, not susceptible of useful abridgment.

The Variations of the Fixed Points of Mercurial Thermometers, and the Means of Recognising them in the Determination of Temperatures.—J. Pernet.—The author agrees with M. Crafts that the part played by pressure in the permanent elevation of the zero-point is very trifling, if it exists at all.

Boro-deci-tungstic Acid and its Sodium Salts.—D. Klein.—If tungstic acid in excess is dissolved in a boiling solution of borax with twice its molecular weight of boric acid (crystalline), the ebullition kept up for some hours, the undissolved tungstic hydrate filtered off the resulting solution deposits crystals of boric acid and sodium polyborates. The mother-liquor, if concentrated and placed in a vacuum, deposits first borax and then the exceedingly soluble sodium salt of the new acid, containing 2 mols. of constitutional water.

Chemisches Central-blatt.

No. 18, 1880.

Appearance of Ozone on the Evaporation of Various Liquids as a Lecture Experiment.—R. Böttger.—The author recommends to moisten a piece of paper uniformly with starch containing cadmium iodide, to let fall upon it a few drops of alcohol or ether, and to set the latter liquid on fire. After its evaporation the paper is found turned decidedly blue in consequence of the formation of ozone.—*Pol. Notizblatt*, 35, 95.

Singular Behaviour of Stannous Chloride with Potassium Chlorate.—R. Böttger.—If 2 parts of stannous chloride and 1 part potassium chlorate, both previously pulverised, are rubbed together in a porcelain morter, the mixture becomes very hot, chlorous acid and watery vapour are evolved, and there remains a yellowish white mass, which, if dissolved in boiling water, deposits potassium perchlorate in micaceous crystals. The mother-liquor contains tin oxychloride.

Action of Nitric Oxide upon Organic Bodies.—E. Lippmann and R. Lange.—A preliminary notice.—*Wien. Anzeiger*, 1880, 48.

No. 19, 1880.

On Idryl.—G. Goldschmiedt.—The author describes a number of derivatives of this hydrocarbon.—*Wien. Anzeiger*, 1880, 56.

Hypochlorine and the Conditions of its Origin in Plants.—M. Pringsheim.—The author has previously demonstrated the existence of a body in the green cells of plants, which he named hypochlorine on account of its relation to chlorophyll. He describes in this paper its occurrence and its micro-chemical characters.

No. 20, 1880.

Chlorides of Camphor.—F. V. Spitzer.—The products which arise on the action of phosphorus pentachloride upon camphor are affected by the quantity of the phosphorus chloride present and by the temperature. If every increase of temperature is prevented no hydrochloric acid appears, and there is formed a homogeneous camphor dichloride in theoretical quantities. Pfaundler's dichloride and the body melting at 60° and described as monochloride, are probably merely mixtures.—*Wien. Anzeiger*, 1880, 71.

Chemical Investigations in the Bohemian Central Mountains.—J. Stoklasa.—An examination of the marls and clays of Priesen.—*Listy Chem.*, 4, 135.

Bödecker's Method of Detecting Albumen in Urine.—R. Böttger.—The urine is slightly acidified with acetic acid, and a few drops of a solution of potassium ferrocyanide are added. In presence of even very slight traces of albumen a turbidity at once appears, and in a short time there is deposited a flocculent sediment. The author considers the test exceedingly sensitive.

No. 21, 1880.

Decomposition of Simple Organic Compounds by Zinc-dust.—Hans Jahn.—The higher alcohols from ethylic alcohol upwards, on distillation over zinc-powder which was heated to 330° to 350°, were decomposed into the corresponding olefine and hydrogen. Under the same circumstances methylic alcohol is resolved into carbonic oxide and hydrogen.—*Wiener Anzeiger*, 1880, 73-74.

New Synthesis of Dimethyl-acrylic Acid.—E. Duvillier.—This compound is formed along with ethyl-isoxy-valerianic acid when brom-iso-valerianic ether is brought in contact with sodium ethylate in absolute alcohol.—*Ann. Chim. Phys.*, 19, 429.

Chloraluminium used at Cloth Works.—F. Stolba.—A sample contained 15·49 per cent Al_2Cl_6, 1·13 Al_2O_3, 2·59 $NaCl$, 0·14 Na_2SO_4, 80·65 H_2O. Apparently formed by decomposing aluminium sulphate with barium chloride.—*Listy Chem.*, 4, 193.

No. 22, 1880.

True Clay in so-called Clay Soils.—A. Funaro.—The highest proportion of clay does not exceed 33 per cent.

Physico-Chemical Analyses of Soils.—M. Pellegrini.—The author compared the methods of Schlœsing, Nœbel, and Masure, and obtained such differences as clay, 37 and 87; sand, 1·5 to 28. He considers Schlœsing's method the most satisfactory.

No. 23, 1880.

Crystalline Prussian Blue.—W. Gintl.—If recently-precipitated Prussian blue is treated with a moderate excess of hydrochloric acid at a gentle heat, it dissolves to a slightly yellowish liquid, which, on exposure to the air, gradually deposits Prussian blue as a crystalline sediment, which displays a splendid coppery lustre by reflected light. So-called Turnbull's blue dissolves in hydrochloric acid in the same manner as ordinary Prussian blue, and yields similar crystals,—a further evidence for the identity of the two compounds.

Synthesis of Chinoline.—Z. H. Skraup.—The alizarin blue of Prud'homme has the composition $C_{17}H_9NO_4$, and is probably a dihydroxylised quinon of anthrachinoline. It is formed from nitro-alizarin and glycerin, with the elimination of water. Chinoline is actually obtained on heating together nitro-benzol, glycerin, and sulphuric acid. *Wiener Anzeiger*, 1880, 69.

Function of Lime in the Life of Plants.—E. v. Raumer and Ch. Kellermann.—Lime is absolutely necessary for the life of plants, and its function is most closely connected with the utilisation of the carbohydrates.

Crystals of Hæmine.—F. Högyes.—The author has examined crystals from the blood of men, oxen, swine, sheep, dogs, cats, rabbits, guinea-pigs, mice, pole cats, poultry, pigeons, geese, ducks, *Rana esculenta* and *temporaria*. All have one only crystalline form. They belong to the monoclinar or triclinar system, probably the former.

Journal de Pharmacie et de Chimie.

May, 1880.

The Curare of the Upper Amazon.—G. Planchon.—This memoir is rather of a botanical than a chemical nature.

Commercial Sub-nitrates of Bismuth.—A. Riche.—The author points out great variability in the composition of this body.

The Alkalies of the Cinchona.—E. Jungfleisch.—Already noticed.

The Spectroscope Applied to the Chemical and Pharmaceutical Sciences.—M. Coulier.—An essay on the selection of a spectroscope and on its use in analysis.

Virulent Maladies.—M. Pasteur.—Already noticed.

Electrolysis of Malonic Acid.—E. Bourgoin.—Already noticed.

Decompositions of Copper Acetates in Presence of Water, under Influence of a High Temperature.—P. Cazeneuve.—Already noticed.

Apiculture at Paris.—In some of the suburbs of Paris vast numbers of bees are kept which subsist by plundering the sugar refineries.

June, 1880.

Curare of the Upper Amazon.—G. Planchon.—A continuation from the last number.

Composition of Officinal Sodium Arseniate.—J. Lefort.—The composition of this salt varies very considerably, especially as regards its proportion of water.

Thapsia garganica.—L. Soubeiran.—A botanical-pharmaceutical paper.

Electrolysis of the Malonates.—E. Bourgoin.—A continuation from the last number.

Method of Determining the Fatty Acids Contained in Oils.—M. Carpentin.—The author takes a small flat-bottomed flask or a medicine phial holding about 250 c.c. Into this phial are measured 50 c.c. of the sample of oil, and 100 c.c. of alcohol at 90 per cent, and 3 or 4 drops of tincture of turmeric are added. The phial is then corked and violently shaken. The phial is then placed under a Mohr's burette containing a solution of 40 grms. pure sodium hydrate per litre of water. As 40 grms. soda saturate 282 of oleic acid, 1 c.c. of the liquid, containing 0·04 grm. soda, corresponds to 0·282 grm. of oleic acid. If another fatty acid has to be determined this number is modified accordingly. The alkaline liquid is carefully dropped into the phial, which is shaken. When a red colouration appears it is corked, agitated for a considerable time till the yellow colour reappears, the alcohol having extracted a fresh quantity of acid out of the oil. These operations are continued until the red colour becomes permanent. The number of c.c. and the fraction of a c.c. consumed are then multiplied by 0·282 grm., in order to find the quantity of oleic acid present in the sample examined.

New Apparatus for Determining the Total Nitrogen in Organic Matters in General and in Urines in Particular.—M. Flavart.—The author substitutes for the glass combustion tube an apparatus of copper, which does not require so complete a desiccation of the soda-lime.

MISCELLANEOUS.

Testimonial to Prof. Attfield, F.R.S.—At the recent meeting of the British Pharmaceutical Conference the above-named gentleman resigned the position of Senior Honorary Secretary, which he has held for seventeen years. As a mark of their appreciation of his services he was presented by members of the Conference with five hundred volumes of general literature, along with a scroll, setting forth the origin of the testimonial, and bearing the signatures of the President, Prof. Redwood, F. B. Benger, and other leading officials and members.

COMPOSITION AND QUALITY OF THE METROPOLITAN WATERS.

August, 1880.

	Total Solid Matter.	Oxygen required by Organic Matter, &c.	Nitrogen as Nitrates, &c.	Ammonia.	Hardness. Clark's Scale.	
					Before Boiling.	After Boiling.
	Grs.	Grs.	Grs.	Grs	Degrees.	Degrees.
Thames Water Companies.						
Grand Junction	18·72	0·072	0·142	0·000	14·3	2·4
West Middlesex	20·76	0·080	0·125	0·000	14·3	2·0
Southwark and Vauxhall.. ..	18·39	0·060	0·125	0·000	14·3	2·4
Chelsea	18·82	0·120	0·125	0·000	14·3	2·6
Lambeth	19·72	0·080	0·125	0·000	14·0	2·8
Other Companies.						
Kent	30·17	0·000	0·410	0·000	21·2	5·2
New River	20·14	0·028	0·142	0·000	14·8	2·4
East London	20·25	0·036	0·137	0·000	15·4	2·8

(The results are stated in grains per imperial gallon of 70,000 grains.)

Note.—The amount of oxygen required to oxidise the organic matter, nitrites, &c., is determined by a standard solution of permanganate of potash acting for three hours.

The water was found to be clear and nearly colourless in all cases.

C. Meymott Tidy, M.B.

TO CORRESPONDENTS.

V. T. Magerstein's Researches.—The journal in which V. T. Magerstein's researches appear, the *Listy Chem.*, is written in the Czech language, and published in Prague. We quote it through the medium of the *Chemisches Centralblatt.*

THE JOURNAL OF SCIENCE

for OCTOBER (Price 1s. 6d.), includes—

The Vehicle of Force. By Charles Morris.
Exploration in the Far East.
The Phenomena of Fluorescence. By E. R. Hodges.
Gilbert White Reconsidered.
The Four Forces in Nature. By G. Whewell, F.I.C.

Analyses of Books. Correspondence. Notes.

London: 3, Horse-Shoe Court, Ludgate Hill.

In the High Court of Justice, Chancery Division.—In the Matter of the Companies Acts, 1862 and 1867, and in the Matter of the Patent Liquid Metallic Capsuling Paint, Gilding, and Silvering Company (Limited).

MESSRS. FRANK LEWIS and CO., will SELL by AUCTION, by order of the Official Liquidator, on the Premises of Mr. S. J. Jerrard, Ennersdale Street, Lewisham, on Wednesday next, October 27, at 12 o'clock, the STOCK-IN-TRADE and UTENSILS of the above Company, comprising about 600 gallons collodion, ether, and other spirit, 16 carboys nitric and sulphuric acid, about 200 lbs. gold, silver, and bronze powders, aniline colours, drugs, two galvanising tanks, large quantity of stoppered bottles, glass retorts, evaporating pans, also an iron safe, copying press, partitioning, and other effects.—May be viewed the day before sale and catalogues had on the premises; of F. Romer, Esq., Solicitor, No. 13, Warwick Court, Gray's Inn; of George Sneath, Esq. (Messrs. Price, Waterhouse, and Co., Accountants), No. 44, Gresham Street; the Official Liquidator; and of Messrs. Frank Lewis and Co., Auctioneers, &c., 95, Gresham Street, E.C.

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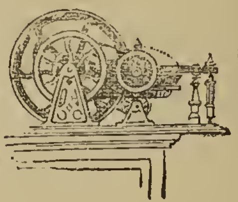

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THE CHEMICAL NEWS.

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THE
EFFECTS OF MAGNESIA ON VEGETATION.*

By MAJOR-GENERAL SCOTT, C.B., F.R.S.

Not among the least wonderful of the anomalies in the conduct of human beings is the persistence, in opposition often to their own interests, with which they adhere to a prejudice long after it has been overturned by experience. The erroneous views of the effects of magnesia on vegetation affords a notable instance. Eminent cultivators have given their testimony from actual practice of its favourable effects, yet the prejudice continues so strong that farmers will often carry other limes from a long distance, and at a much greater cost, rather than employ those made from dolomite. Its slower absorption of carbonic acid, or some defect in the mode of using it, had given in the first instance an unfavourable impression regarding magnesian lime, and this impression has been handed down from one author to another, and has been accepted by the unthinking agriculturist without question. Instances occur, even, in which men of science have contributed to the propagation of the error.

Now and again there has been some slight protest against the assumption that magnesian lime had qualities noxious to vegetation, but these protests have produced little effect, and the substance, though known to be an important constituent of plants, has seldom been made a component of manures. Thus, Prof. Johnston, in 1849, recommended the making of experiments both with caustic and mild or carbonate of magnesia, and stated that "In consequence of previous recommendations it (carbonate of magnesia) has been tried in numerous experiments by Mr. Gardner, at Barochan, in Renfrewshire, and by Mr. Main, at Whitehill, in Midlothian. This was never applied alone by these gentlemen, but always as an ingredient of mixed manures, in which it formed only a small proportion. These experiments therefore throw no light upon the special effects of this substance on our different crops and soils."

Thirty years after this we find Mr. Jamieson thus writing in the Report of the Proceedings of the Aberdeenshire Agricultural Association for 1878. Speaking of the essential ingredients of plants, he says that they "may benefit or injure plants according to the combination in which they are applied; thus chloride of lime and carbonate of magnesia are said to be plant poisons, although composed of ingredients which are beneficial in other forms, such as carbonate of lime, sulphate of magnesia, chloride of potash, &c." And again—"As to magnesia, it may be put down quite as a neglected substance in manures. Judging from examination of soils, its application is called for in many cases more urgently than most of the other essential ingredients."

It is true, also, that Messrs. Lawes and Gilbert introduced 16·3 per cent of sulphate of magnesia (=2 per cent of magnesia) into the mixed mineral manures employed in their experiments at Rothamstead, and small quantities were used in the trials of manures made in 1841 and 1842 in Scotland; but one of the most eminent authorities on the question of manures, M. Ville, whose work has been justly received with much appreciation by agriculturists in America and England as well as in his own country, does not think it necessary to introduce magnesia into manures at all, not because he does not consider it to be an essential element in vegetation, but because, as he states, "it is found in the soil naturally," and of course he means in sufficient abundance for the requirements of plants. How far the accuracy of this opinion is borne out by the facts he cites is, I think, open to question, and I therefore place the whole matter, almost in his own words, before you, and it will form an appropriate introduction to my paper.

M. Ville gives in his work the analysis, by Davy, of six samples of earth from different sources, "all renowned for their fertility," and states that all six possess the same degree of fertility. Now, Sir Humphry Davy, in his work on "Agricultural Chemistry," gives the analysis of three of them, and of these he says one was from a hop-garden, another from a "good turnip soil," and a third from "an excellent wheat soil." In none of the six samples, excepting that from the "hop-garden," did Sir Humphry Davy find any magnesia, and in that he found 0·7 per cent of magnesic carbonate (=0·33 magnesia). M. Ville gives also the analysis of a soil from Chalons-sur-Marne, by M. Rivot, which showed only traces of magnesia. At the same time they contained, with one exception (which had only 0·6 per cent of calcic carbonate), a fair proportion of lime, viz., from 4·7 to 57·2 of calcic carbonate. It is true that in the days of Davy methods of analysis were not so accurate as at present, and that magnesia might have been, and most probably was, present in some of the five samples in which it was not detected or noted by him, but the magnesic element must have been present in insignificant proportion in comparison with the calcic.

At the end of his work M. Ville gives tables for calculating the relative exhaustion of soils under different crops, but in these tables no mention whatever is made of magesia as an element abstracted from the soil by plants, though it is an undoubted fact that the seeds of peas, beans, rape, and wheat carry off a comparatively large proportion of it. In 100 parts of the seed crop of wheat there are 12 parts of magnesia and only 4 of lime, and in the straw of this plant the quantity of magnesia is about one-half that of the lime; yet M. Ville omits magnesia from his normal manure for cereals, though lime, in the somewhat soluble condition of sulphate, forms nearly one-half of the whole compound recommended for a wheat crop, and these facts appear the more remarkable when we examine the experiments made with the different mineral elements of plants by M. Ville. With reference to these experiments he says:—"This time a fixed and invariable quantity of nitrogenous matter was mixed with the [calcined] sand [soaked with distilled water] as a constant ingredient, and all the other mineral ingredients were added by turns except one. The experiments were repeated as many times as there were different mineral ingredients, in order that each might be excluded in its turn, the deviation between the crops obtained with the ten mineral ingredients and those in which they were reduced to nine being taken to indicate the degree of importance of the suppressed ingredients." . . . .

"Magnesia was submitted to the same method of exclusion. The defects were as disastrous as in the case of potash."

"There are some plants, particularly buck-wheat, on which the effects of this suppression are immediate, on wheat they are manifested a little more slowly, but are still very significant, and when magnesia is excluded from the soil the yield falls to about 123 grains instead of 337." . . . .

"The suppression of the lime produces a less sensible effect, the yield is then about 307 instead of 337." . . .

"Leaving the culture in calcined sand, I extended my investigations to various natural soils.

"On submitting them to the same experimental system we found that . . . the yield is maintained at the same level as when sulphur, silica, soda, magnesia, iron, and chlorine are added, which explains to you why I did not go further into the effects of those bodies. Experience therefore shows that the four ingredients—nitrogenous matter, phosphate, potash, and lime—are the only ones that need be admitted into manures (Ville, p. 153-5).

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* A paper read before the Chemical Section of the British Association, Swansea Meeting, 1880.

"I give the name, therefore, of normal manure to the mixture of phosphate of lime, potash, lime, and a nitrogenous material.

"In so doing I do not intend to deny the utility of the other ingredients; I exclude them from the manure because the soil is provided with them naturally." . . . .

"If we pass from these fundamental data to the function of each mineral ingredient in particular, the results are neither less expressive nor less explicit. The soil being provided with nitrogenous matter as a constant ingredient:—

	Wght. of Crop.
With all the mineral matter except phosphate,	*nil* grains
" " " " potash,	138 "
" " " " magnesia,	107 "
" " " " sol. silica,	123 "
" " " " without any suppression,	275—237 grns."

(P. 156—157.)

M. Ville's conclusions respecting magnesia, and indeed silica, appear to me to be far from convincing. If the above results can be said to demonstrate anything, they demonstrate that magnesia comes next to phosphate in importance as an element of mineral manures, and, as I shall show eventually, this is not far from the truth. I shall, however, now proceed to show that there are experiments recorded indicating that considerable advantage is derived by adding to very many soils more magnesia than they naturally contain, and we shall find, also, important testimony to its value where it naturally occurs.

Sir Humphry Davy, in opposing the notion of the noxious qualities of magnesian lime, and accounting for this prevalent and erroneous opinion, states that—

"Magnesia in its mild state—*i.e.*, fully combined with carbonic acid, seems to me to be always a useful constituent of soils. I have thrown carbonate of magnesia upon grass, and upon growing wheat and barley, so as to render the surface white, but the vegetation was not injured in the sligbest degree; and one of the most fertile parts of Cornwall, The Lizard, is a district in which the soil contains mild magnesian earth. The Lizard Downs have a short and green grass which feeds sheep, producing excellent mutton, and the cultivated parts are amongst the best of corn lands in the country." (Davy's "Agricultural Chemistry," pp. 299-300.)

Davy also found that wheat grew better in a soil with which he had mixed peat and magnesia than in either the "pure soil," or the pure soil and peat alone. It grew very well in the "pure soil," and nearly as well as with magnesia in the mixture of peat and pure soil, but peat often contains a notable quantity of magnesia. The ashes of the brown herbaceous peat in the neighbourhood of Troyes contain 14 per cent of magnesia, and those of a peat from the frontiers of Bavaria and Bohemia contain 3·5 per cent of it. Gelatinous silica and sulphate of lime are also frequent constituents of peat, and the fact, therefore, that the mixture of peat and pure soil gave better results than the pure soil, derogates little, if at all, from the evidence of improvement given by magnesia. Again, Morton, in in his "Encyclopedia of Agriculture" in opposing the erroneous opinions current against the use of magnesian lime, mentions that—"In the neighbourhood of Castellamonte and Baldissiro, the most luxuriant vegetation is met with in a soil which contains a very large quantity of magnesia, and in our own country many very fertile soils are found in the new red sandstone formation, which likewise is rich in magnesia."

John Donaldson, the author of a "Treatise on Manures," having been engaged in the cultivation of land in the immediate neighbourhood of Breedon magnesian rock, in Leicestershire, says:—"I had occasion to use considerable quantities of lime, and consequently had a fair opportunity of proving the quality of that rock for agricultural purposes." On two fields (the farm, he tells us, had been most miserably scoured and impoverished by the preceding tenants) he spread the magnesian lime at the rate of 200 bushels an acre, and in another field "a double allowance of lime, or 400 bushels, per acre . . . which, being both a large quantity and in a caustic state, would test the supposed noxious quality of the lime. In every case the green crops were good . . . one field was sown with barley, which yielded a most beautiful crop of 7½ quarters per acre, and the other produced 5 quarters of wheat; both very great crops when the exhausted state of the land was considered."

"When the wheat braided in November, the space which had got 400 bushels to an acre immediately showed a great superiority, which continued to the day of reaping, being much thicker on the ground, of a darker colour throughout the winter, and afforded more produce as the shocks were thicker in the ground, and discernible in the first view of the field. The succeeding crop of hay on that space showed an equal superiority, and for several years in succession."

"The same lime was used in the same quantity, of 200 bushels to an acre, and with the same beneficial results, without a single exception. On the headland, where the lime lay and where any damage might have been expected, there grew a very close and heavy crop of beet, with roots not equalled in size and weight. Many eminent cultivators join in the same opinion of magnesian lime derived from actual practice."—"A Treatise of Manures," by J. Donaldson, pp. 157-8.

This experiment is very instructive and conclusive, for on comparing the results obtained with the constituents of each of the various crops it will be observed that the beneficial results were in proportion to that of the magnesia which the ash contains. Though the green crops were good it is specially mentioned by Donaldson that the space which had the double allowance of magnesian lime, "*showed no difference on the turnip crop.*"

The following table, extracted from "How Crops Grow" (Eng. Ed. by Church and Dyer), gives the percentage of magnesia and lime in the ashes of the crops alluded to:—

	Magnesia.	Lime.
Turnips (mean of 3 sorts) ..	2·9	11·2
Hay	4·9	11·6
Barley—straw	2·4	7·6
" seeds	8·3	2·5
Beet	8·9	6·3
Wheat—straw	2·6	6·2
" seeds	12·2	3·1

We here see that Donaldson's results are exactly in accord with what would have been predicted from a consideration of the mineral constituents of the different crops. The turnip, containing the smallest proportion of magnesia, is not benefitted by the presence in the soil of a double proportion of that substance, probably the 200 bushels to the acre of Breedon lime supplied quite enough lime and more magnesia than was necessary for that crop; but in the case of wheat, where for the formation of the seed a very large supply of magnesia was necessary, the dose of 200 bushels to the acre was insufficient to enable the roots to find the requisite amount of magnesia for the full development of the crop, and yet, if, in this matter, we are to follow M. Ville's prescription, the soil naturally contains enough magnesia to supply all the wants of wheat, though analysis shows the presence of mere traces of it only, or none at all. Mr. N. Whiteley, Land Surveyor, the author of a treatise on "The Application of Geology to Agriculture," whilst receiving as well founded the common prejudice against the magnesian lime in its hot state, does full justice to the fertilising effects of magnesia in its mild condition. He says, speaking of land near St. Kevern, in Cornwall:—If we seek for a soil theoretically perfect it may be found in this formation. The large amount of magnesia which this fruitful soil contains (9 per cent) is worthy of observation." Of the magnesian limestone formation he says that "much of it is thin, light, and dry," and "we are prepared, therefore, to meet with a soil of medium quality. Much of the soil on the magnesian limestone is in arable culture, but from Standrop to Darlington the soil may be considered the best and richest grazing land in the north." Strong confirmation of the views I am seeking to establish, is to be derived from a consideration of the cases of soils, which are either abundantly supplied with, or are very deficient in magnesia.

In the following table are given the proportions of lime and magnesia found in both fertile and barren soils. I give the lime as well as the magnesia, because that substance is considered, very rightly, a most important ingredient of manures, although usually its proportion in soils is considerably in excess of the magnesia.

Proportion of Lime and Magnesia in 1000 *parts of very Fertile and Fertile Soils.*

	Exceptionally Fertile.						Fertile.			
	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.
Lime ..	26·0	9·3	12·6	61·5	12·29	42·1	12·9	7·4	5·3	5·6
Magnesia	9·5	11·6	8·8	8·8	10·82	6·1	5·7	5·3	4·9	3·1

No. 1. Is from the analysis of the celebrated black earth of Russia. This remarkable soil is "the finest in Russia, whether for the production of wheat or grass." It nourishes, on 60,000 square geographical miles, a population of more that 20 millions of souls, and yet "exports upwards of 50 millions of bushels annually." This very properly stands first in my table as it contains a very large amount of magnesia, and is the most fertile probably of any which the table includes.

No. 2. From an analysis by Sprengel of the soil of Nebstein, near Olmutz, in Bavaria, which had been cropped in 1847 for nearly 160 years successively, "without either manure or naked fallow."

No. 3. By Boussingault: a soil from Calvario, near Tacunga, Ecuador, S. America. It "possesses extraordinary fertility."

No. 4. "A very fertile alluvial soil from Honigpolder," analysed by Sprengel; no manure has ever been applied to it. The sub-soil contains to a great depth the same composition as the surface soil.

No. 5. Soil from Midlothian, analysed by Dr. Anderson. It "produces excellent wheat."

No. 6. From an analysis by Sprengel of a very fertile alluvial soil in East Friesland, formerly overflowed, but which had been, in 1863, cultivated for 60 years with corn and pulse crops *without manure.*

Nos. 7, 8, 9, 10. From soils at Göttingen, from near Hanover, from Alt-Arenberg in Belgium, and from a virgin soil on the banks of the Ohio, respectively. The analyses are by Sprengel.

I do not of course assume that the fertility of these soils is due entirely or chiefly to the magnesia they contain (the black earth of Russia, for instance, contains also more than 2 per cent of nitrogen), nor do I suppose that in very many cases the addition of magnesia, where the proportion of it is less, cannot be dispensed with, but I do say, seeing the disastrous effects which accompany its absence, as shown by Ville, the question merits the serious attention of agriculturists. This will be more apparent from the following table of—

Barren Soils, showing the Proportion of Lime and Magnesia in 1000 *parts of Soil.*

	11.	12.	13.	14.	15.
Lime ..	0·96	0·01	traces	2·9	3·2
Magnesia	traces	traces	0·12	0·7	1·3

No. 11. Refers to a soil which Dr. Sendtner characterises as the most sterile soil in Bavaria.

Nos. 12 and 13. Soils from the neighbourhood of Friesland; also barren.

No. 14. A very barren soil from Luneberg. This soil is wanting in many other elements besides magnesia, and is probably too rich in iron.

No. 15. Also from Luneberg, analysed by Sprengel, as also was the case with the three preceding numbers.

Here again I deprecate the supposition that I hold the barrenness of these soils to be solely, or even principally, due to the absence or scarcity of magnesia. Many of the other constituents of plants, not considered by Ville as essential to a manure, are also wanting, as well as some that he deemed to be indispensable.

Probably the opinion of Dr. Liebig may carry with it more weight with the generality of persons, than any of the foregoing evidence in favour of making a more extended use of magnesia in manures. In his "Natural Laws of Husbandry (pp. 257-8) he says, with reference to guano, the best probably of manures now in the market:—"If we compare the composition of the ashes of various seeds we at once see that the incombustible constituents of guano do not altogether replace the soil constantly carried off in the seeds.

"In 100 parts of seed-ash are contained:—

	Wheat.	Peas and Beans.	Rape.
Potash..	30	40	24
Lime	4	6	10
Magnesia	12	6	10
Phosphoric acid..	45	36	36

"Whereas guano ash contains in each 100 parts:—

Potash	1·56 to 2·03
Lime	34·00 to 37·00
Magnesia	2·56 to 2·00
Phosphoric acid	41·00 to 40·00

"The principal difference between the ash of guano and that of those seeds lies in the deficiency of potash and magnesia in the former. Agriculturists are generally agreed about the necessity of potash for vegetation, and that a supply is required by fields poor in that ingredient, or drained of it; but the question as to the importance of magnesia for seed formation has not as yet met with the same attention, and special experiments in this direction would be very desirable.

"The fact that much more magnesia is found in the seeds than in the straw unmistakably shows that it must play a definite part in the formation of the seed, which might, perhaps, be ascertained by a careful examination of seeds of the same variety of plants containing different amounts of magnesia. It is a well-known fact that the seeds of the several species of cereals, having the same proportion of nitrogen, do not always contain the same nitrogenous compounds; and it is possible that the nature of the latter, may, in the formation of the seeds, be essentially influenced by the presence of lime or of magnesia, so that the difference in the proportion of both of these alkaline earths may have a certain connection with the presence of the soluble nitrogenous compounds (albumen and casein), or of the insoluble gluten or vegetable fibrine."

To pursue this subject further in the direction indicated by Liebig is a task for which I feel myself unequal, but it encourages me to examine narrowly the records of experiments in magnesia has been made an element of manures, or has been tried alone, or in conjunction with an acid. Of experiments made with magnesia alone I know of no further instances of a definite and reliable kind than those of Donaldson, above quoted, but concerning experiments with sulphate of magnesia both alone and combined with other constituents, there are records to which I can refer for further proof of my argument. Before doing so, however, I wish to call attention to a fact which has a considerable bearing on the case. Both Liebig and Ville hold that farm-yard manure, excellent manure as it is, must be supplemented with mineral substances, if full value is to be given to it, and that additional phosphoric acid is necessary to make all of its nitrogen available for plant life. Now in 100 parts of farm-yard manure there is less than 0·14 per cent of magnesia, and of its mineral constituents the magnesia forms only 1·7 per cent. Donaldson tells us "the quality of earthy compost and of farm-yard manure is prodigiously improved by a mixture of seaweed. During the seasons of the seaweed coming on shore the farmers have heaps of dung or soil in readiness to receive the immediate benefit of the wrack; and these heaps, along with any lands which may be in a state fit to receive it, afford a ready application of this invaluable article. Farm-yard manure for turnips is improved by it almost beyond description . . and never fails to vindicate the expectations of its effects." (Donaldson, p. 123).

Now, what are the components of the ash in seaweed? No less than from 7 to 20½ per cent of magnesia are found in different kinds of it, and the ashes of the seaweed at the mouth of the Mersey contain upwards of 15 per cent. No doubt in the growth of the turnip crops the advantage is largely derived from the great amount of potash the seaweed contains, but of this substance the ash of dung itself yields upwards of 9 per cent, whereas it yields only, as stated, 1·7 per cent of magnesia. In the ash of dung, therefore,—

$$\frac{\text{potash}}{\text{magnesia}} = \frac{5\frac{1}{3}}{1};$$

in that of the turnip—

$$\frac{\text{potash}}{\text{magnesia}} = \frac{8}{1};$$

In seaweed—

$$\frac{\text{potash}}{\text{magnesia}} = \frac{2\frac{1}{2}}{1}.$$

It is reasonable, therefore, to suppose that the turnip if manured with seaweed as well as dung, derives more of the benefit from the magnesia added by the seaweed than from the potash thus supplied.

We will now proceed to examine some of the experiments made in 1842 and reported by Professor Johnston, which have been already briefly referred to.

Results of experiments with sulphate of magnesia:—

1. "On Yellow Turnips," by Mr. McLean Braidwood, Midlothian, 1842.

Farm-yard manure 30 carts produced per acre		24 tons
Do. with ½ cwt. of sulphate of magnesia mixed with it ..	" "	25 "

Or the increase gained by ½ cwt. of sulphate of magnesia was 4 per cent.

2. "On Yellow Turnips," by Mr. Fleming, Barochan, Renfrewshire, 1842. Variety, "Early Liverpool."

Nothing 1st plot	11·40
" 2nd "	12·85
Sulphate of magnesia, 1 cwt.	14·85

Here 1 cwt. of sulphate of magnesia alone gives an increase of upwards of from 12 to 30 per cent (mean 21 p.c.), but as the sulphate of magnesia was not tried in duplicate less reliance can be placed on this experiment.

3. "On Potatoes," variety Early American, carried out by Mr. Fleming, Barochan, in 1842.

Intended to test the comparative advantage of sulphate of magnesia when applied as a top dressing to the young plant, and when mixed with the manure at the time of its application.

As Top Dressing.

No.	Description of Manure.	Quantity of Manure per Acre.	Produced in Tons per Acre.
1.	Nothing but dung ..	40 c. yards	12·75
2.	Sulphate of magnesia	1½ cwts.	13·25
3.	Sulphate of soda ..	2 "	12·75
4.	Nitrate of soda.. ..	1½ "	16·00
5.	Nitrate of soda.. .. Sulphate of magnesia	1 " 1 "	22·50
	Manure mixed with Dung at time of Planting.		
6.	Farm-yard dung alone	35 c. yards	8·75
7.	Sulphate of magnesia	2 cwts.	11·35
8.	Sulphate of soda ..	2 "	8·00

These results are very interesting. It might be supposed that the increase in the crop was due to the sulphuric acid and not to the magnesia, but inasmuch as sulphate of soda gave no advantage, the effect is plainly not due to the sulphuric acid, unless we suppose the soda to have been injurious, which is not likely to have been the case with turnips and potatoes. An increase of sulphate of magnesia appears also to improve the crop; when 1½ cwt. of the salt was used, as a top dressing, with the dung, the increase was only 4 p. c.; but when 2 cwts. were used with the dung at the time of planting the increase was close upon 30 per cent. The result of this experiment appears, moreover, to confirm the view that the mixture of the ashes of seaweed and dung derives no unimportant advantage from the quantity of magnesia thus introduced into the plant.

4. "On Clover and Rye Grass cut for Hay," by Mr McLean Braidwood, Midlothian, 1842.

Nothing		125 stones per acre
Sulphate of magnesia, 1½ cwts...		290 " "
Gypsum	3 " ..	200 " "

Here the use of 1½ cwts. of sulphate of magnesia gave the enormous increase of upwards of 130 per cent, whilst 3 cwts. of gypsum (sulphate of lime) gave an increase of 60 per cent only. It will be of interest at this point to refer to the composition of the mineral constituents of the turnip, potato, clover, and hay as respects the proportions in them of lime and magnesia, for we shall then see that the advantage gained by the crops experimented on has been in general accordance with what might have been expected from the components of their ashes. I have given also the percentage of sulphuric acid for reasons stated below.

Percentage of Sulphuric Acid, Lime, and Magnesia in the Ashes of—

	Sulphuric Acid.	Lime.	Magnesia.
Turnips—the bulbs	13·60	13·60	5·34
" entire plants ..	12·52	23·27	3·09
Potatoes—tubers	13·65	2·09	6·28
" entire plants ..	12·52	13·11	5·55
Clover	3·33	32·80	8·40
Rye grass hay	3·25	6·50	4·01
" seed	3·24	19·24	5·51

The entire plants of the turnip and potatoe require in the ash 3·09 p.c. and 5.55 p.c. of magnesia, and clover and rye grass crops (taken in the average proportion of the ashes of their respective crops, viz., 400 lbs. to 220 lbs. per acre) require 7·3 per cent of that substance. The advantage gained by the use of sulphate of magnesia as shown by these experiments was—

	Per cent.	Magnesia in ash being—
For turnips	15⅓	3·09 (mean)
" potatoes..	30	5·55
" clover and rye grass	130	7·30

If this coincidence is accidental it is extraordinary Another point to which I wish to call attention is the fact that the advantage gained is manifestly largely due to the magnesia and not to the sulphuric acid only; for, where the advantage was the greatest by far, the sulphate of lime did not produce an equivalent effect, though the 3 cwts. of calcic manure carried to the soil upwards of six times the amount of sulphuric acid that the 1½ cwts. of the magnesic salt supplied; and though the latter is far more soluble than the former, gypsum is sufficiently soluble for all the requirements of plants.

A third point remains for remark, and that is the relative effect produced on potatoes in the above experiments of Mr. Fleming, of Barochan, by the use of dung, No. 1; of sulphate of magnesia, No. 2; of sulphate of soda, No. 3; of nitrate of soda, No. 4; and by the use of a mixture of nitrate of soda and sulphate of magnesia, No. 5. A comparison of these results can leave little doubt in the mind of the immense value of magnesia as an ingredient of a manure for potatoes. By the addition of 2 cwts. of sulphate of soda to the dung no advantage was gained; when 1½ cwts. of nitrate of soda were added to the dung the extra supply of nitrogen in its nitric acid increased the produce 30 per cent; but when 1 cwt. of sulphate of magnesia was added to the dung, together with 1 cwt. only of nitrate of soda, the amount of produce rose upwards of 76 per cent upon what was given by dung alone. Moreover, it is quite clear that as the 1½ cwts. of nitrate of soda alone gave an increase of only 30 per cent, and when sulphate of magnesia was added to the manure, the nitrate at the same time being reduced by 50 per cent, the produce rose to 76 per cent, more than one-half of the advantage was probably due to the sulphate of magnesia. I have before shown, and my conclusions are confirmed by these experiments, that the benefit is chiefly attributable to the magnesia, and not to the sulphuric acid combined with it. There is, indeed, one other possible supposition at variance with this conclusion, viz., that the 1½ cwts. of nitrate of soda was too large a quantity to use per acre, but all experience contradicts this idea.

The following are the results recorded by Prof. Johnston of the experiments on cereals as far as they affect our investigation. The quantities of manure and the crops are per acre—

With barley, common white—

		Bushels of Grain.
Nothing	3400 lbs. of straw and	47·25
1½ cwts. sulphate of soda ½ " " magnesia	3928 " " "	54·90

With oats, 2nd crop, after old lea, the manure applied as top dressing two months after sowing—

Nothing	2896 lbs. of straw and	54·00
16 cwts. rape dust	2592 " " "	44·96
1 cwt. sulphate of soda..	2792 " " "	38·56

With spring wheat after turnip—

Nothing	4056 lbs. of straw and	47·65
16 cwts. rape dust	4600 " " "	51·05
1 cwt. sulphate of soda..	3864 " " "	38·00

With winter wheat as top dressing—

Nothing	2560 lbs. of straw and	24·93
84 lbs. sulphate of magnesia .. .. 5 cwts. rape dust	3200 " " "	28·40

As the above crops were not the same and the quantities of manure employed differed, and were applied at different times, no exact results may be deducible from the above trials, but as the experiments were all with cereals we are able to draw general conclusions from them which are not unimportant as to the effects of magnesia as a manure, and

First.—As sulphate of soda when used alone proved in each case prejudicial, we may assume that at least it did not materially assist the result when mixed with sulphate of magnesia.

Second.—As rape dust when used alone gave only a certain increase to the crop, any further increase when used with sulphate of magnesia must be due to the influence of the latter substance, and

Third.—As rape dust used alone with oats *decreased* the crop of grain 16·6 per cent, and with spring wheat only increased the grain by 7 per cent, whereas when used in conjunction with 84 lbs. only per acre of sulphate of magnesia (=13·6 lbs. of magnesia), as a top dressing with winter wheat, the increase rose to nearly 16 per cent, it is reasonable to suppose that some portion, indeed a considerable portion, was due to the influence of the sulphate of magnesia, small as it was, and not to the rape dust as a single agent. So far as the rape dust improved the result it may be supposed to be attributable to the influence of the magnesia, to which substance Liebig, as we have seen, assigns a very definite part in the formation of the seed.

It must be borne in mind, in considering the question how far a soil is likely to be improved by an increase of the amount of magnesia in it, that crops remove from the soil the greater portion of their mineral constituents within a short space of time as compared with the whole duration of the existence of the plants, and that, therefore, it must be of the greatest moment that they should be able to gather them from the soil within this period. After a certain time a cultivated plant advances little in size and weight, although great changes occur in the distribution of its constituents.

Liebig says, with manifest truth:—"As a soil may contain far more potash or magnesia or lime than the crop may require, yet being diffused through a large quantity of earth, the roots may be unable to collect the ingredients fast enough to supply the growing wants of the plant. To such a soil it will be necessary to add a further portion of what the crop requires."

The last experiments to which I shall refer are those of Dr. Pincus, of Insterburg, which Liebig characterises as most important, both on account of the careful manner in which they were conducted and the conclusions drawn from them. Three plots of ground were selected lying close together, each of about ⅝ of an acre in extent, from the middle of a large clover field. The clover crop had a very promising appearance, and the plants were then about one inch high; one of the plots was manured with 1 cwt. of gypsum, the second with the same quantity of sulphate of magnesia, and the intervening plot was left unmanured. When all were in flower the clover was mown, and the following were the results:—

	Per ⅝ acre.
Without manure	21·6 cwts.
With gypsum (sulphate of lime).. ..	30·6 "
With sulphate of magnesia..	32·4 "

Or the gain with the use of the magnesic salt as a manure was just 50 per cent. It is to be observed that the stems were developed by both the sulphates much more than the leaves and flowers. The experiment also showed that there was no proportion between the quantities of sulphuric acid found in the crops and in those supplied by the two sulphates. The quantity of sulphuric acid severally in the two sulphates was 31·12 lbs. in the sulphate of magnesia and 44·18 in the sulphate of lime, which is about 6 : 8·8. The quantities of sulphuric acid in the crops obtained severally by sulphate of lime and sulphate of magnesia were as 6 : 8, and on the plot manured with sulphate of magnesia, which had received less sulphuric acid than the gypsum plot, the amount of vegetable matter was 8 per cent higher than on the latter.

Liebig from experiments made on arable soils came to the conclusion that dressing a field with sulphate of lime makes the magnesia in the soil soluble and distributable. An experiment made to test this conclusion showed that the contact of arable earth with the solution of sulphate of lime is attended by an actual substitution of magnesia for lime. If this notion be correct it points to a cheaper mode of supplying magnesia to the soil than to add sulphate of magnesia to manures, for it follows that the use of gypsum in conjunction with comparatively small doses of magnesian lime will effect all that is necessary in cases in which heavy carriage interferes with the use of an abundant supply of that substance.

In conclusion, though any one of the above proofs of the value and importance of magnesia in a manure may not carry conviction with it, yet, taken altogether, the evidences are overwhelming against the notion that soil naturally contains so much magnesia that an extra supply will be of little or no benefit. Moreover, there are strong grounds for supposing, as we have already indicated, that magnesia, like phosphoric acid, is not only an essential ingredient of plants and aids in their nutriment, but that it determines also the beneficial action of the other ingredients.

DETERMINATION OF THE FREE OXYGEN DISSOLVED IN WATER.

By J. KŒNIG and C. KRAUCH.

The authors, after referring to the researches of F. Tiemann and C. Preusse (*Ber. der Deutsch. Chem. Gesell.* for 1879, p. 1768), whose results they have not been able to confirm, enter upon an examination of the methods of Bunsen, of Mohr, and of Schützenberger and Risler. Their experiments lead to the following conclusions:—Reichardt's apparatus is not suitable for a quantitative determination of the gases in liquids. The circumstance that even when the access of air from without is cut off more air is still found than agrees with Bunsen's coefficients of absorption, and that a determination of the proportion of air dissolved in water effected with the apparatus of Jacobsen and Behrens yields more air, seems to point out that the absorption-coefficients of air for water, as calculated by Bunsen, are not valid under all conditions.

Mohr's method of determining oxygen in water yields invariably too high results—a conclusion exactly opposite to the one of Tiemann and Preusse. On what this difference depends the authors are not able to explain. If, however, Mohr's method is always exactly followed, it yields numbers which are relatively accurate, or, in other words, which are comparable among themselves. Waters which have been exposed to direct sunshine or to daylight yield more oxygen by Mohr's method as compared with the other two methods than the water of deep wells which has been for a considerable time excluded from sunlight.

Schützenberger's method yields results which fall below the truth. For absolutely accurate determinations of the oxygen in water neither Mohr's method not that of Schützenberger is suitable, the results of the former being too high and those of the latter too low. Where merely relative accuracy is required—as, *e.g.*, in the examination of well-waters—both methods may be used with equally good results. Mohr's method is suitable when single determinations have to be made at regular intervals, because the standard solutions required are more permanent and can be kept for a considerable time without change. Schützenberger's method is suitable when a great number of determinations of oxygen have to be made in succession in a very short time. The value of the sodium hydrosulphite solution should be determined for this purpose, not by means of a copper or iron solution, but by means of water containing a known quantity of dissolved oxygen.—*Zeitschrift fur Analytische Chemie.*

PROCEEDINGS OF SOCIETIES.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Physical and Mathematical Section.

Annual Meeting, April 27, 1880.

E. W. Binney, F.R.S., F.G.S., President of the Section, in the Chair.

The following gentlemen were elected Officers of the Section for the ensuing year:—

President—E. W. Binney, F.R.S., F.G.S.

Vice-Presidents—Joseph Baxendell, F.R.A.S., Alfred Brothers, F.R.A.S.

Treasurer—James Bottomley, D.Sc., B.A.

Secretary—Rev. Thomas Mackereth, F.R.A.S., F.M.S.

"*Colorimetry, Part V.* (*On the Absorption of Light by Turbid Solutions*)," by James Bottomley, D.Sc.

Media containing colouring matter may be divided into two classes, transparent and turbid. It might be considered that in their behaviour with regard to light they were wholly dissimilar. But the question arises, is not the difference between transparency and turbidity one of degree. Experience in the laboratory brings under our notice cases where matter is so finely divided as not to be separable from fluids by filtration, and showing but slight tendency to settle as a precipitate, and in some cases we have liquids which are apparently transparent, and yet are considered to hold solid matter in suspension. May not such examples be intermediate between solutions and cases where extremely fine particles are uniformly diffused through some transparent medium? If, then, we consider the passage from transparency to turbidity as a continuous one, and if for a transparent fluid we have established some law of absorption of light, may not the same law be applicable to a turbid solution? The subject seemed to me interesting both as a scientific enquiry and on account of its application to quantitative analysis.

Suppose we have diffused through a liquid some finely divided solid matter; the action of such a turbid solution on light will be twofold—it disperses light and it absorbs light. By reason of the first action we are made aware of the colour of the turbidity; by reason of the second, any object seen through the liquid seems of diminished distinctness.

As a typical case, take carbon diffused through water. In a paper which I read at the last meeting of the Physical and Mathematical Section I alluded to some attempts to obtain a soluble black, in order to make some experiments on the absorption of light. To assist my judgment as to the appearance which such a liquid should present, I had a cylinder containing a little carbon diffused through water. Weak diffusions of carbon in the colorimeter gave the same appearance when I looked at external white surfaces as I should have expected a liquid containing a soluble black in solution to give under the same conditions. A diffusion of carbon both disperses light and absorbs light. The dispersion gives rise to a greyish tint, due to the light coming from the innumerable particles of carbon. Owing to the absorption a white surface seen through the column of liquid seems of diminished whiteness. Hence, under ordinary circumstances, the light which comes to the eye has a twofold origin, part being transmitted light and part dispersed light. My first object was to dissociate these two phenomena. I therefore used cylinders which were covered with black cloth, admitting light by circular apertures at the bottom, 8 m.m. in diameter. In this way the dispersed light was almost wholly cut off. In some cases a feebly nebulous light could be seen round the apertures, but it was very slight, and did not interfere with experiments to determine the absorptive properties. With any attempt to explain by physical optics the analogy of the absorption of light by solutions and diffusions I have nothing to do, but on the supposition that there was continuity, I was led to expect that a function of the same form would express the intensity of the transmitted light in both cases. Also, independently of such considerations, it seemed probable from a reflection upon the mode of distribution or the particles throughout the mass of fluid, that the function would be the same as for transparent solutions. In the latter case, both experiment and reasoning from first principles concur in giving a formula $\Sigma\alpha\kappa$ for the intensity of light passing through a column t units long. As I have pointed out in another paper, this implies an expression of the form $\Sigma\alpha\kappa^{Q}$ denoting the connection between the transmitted light and the quantity of colouring matter. Therefore, we might expect a similar expression to hold in the case of diffusions (the term seems to me more convenient than to speak of turbid solutions). If such be the case, one consequence will be that, if we have two cylinders containing in equal bulks of water Q and Q' of solid matter in suspension, if we adjust the columns so as to obtain the same intensity of light when we regard external white surface, then the lengths of the columns will fulfil the condition $Qt = Qt'$.

The carbon that I used was lamp-black re-calcined in a covered platinum crucible. After being so treated it seemed blacker than it did before. Of this 0·4170 grm. was ground up in a mortar with 10 drops of a solution of gum. The contents of the mortar were then rinsed out with water, and diffused by shaking through 500 c.c. of water. This was poured into a cylinder; the length of the column was 22·5 c.m. After the lapse of twenty-four hours I drew off by a pipette the upper portion; the column so removed was 15·8 c.m. long. This carbon diffusion contained so much solid matter that I found it inconveniently strong for experiments, so as occasion required I made weaker diffusions, containing in 250 c.c. 5, 10, 20, or 40 of the stronger diffusion. Of these weaker liquids portions were taken and mixed with water so as to yield a bulk of 500 c.c. I stated above that the carbon was ground up with 10 drops of gum, so as to yield a smooth thick consistence. The amount of dry gum would be very small, nevertheless it had a remarkable effect in increasing the adhesion of the carbon to the water. When I had shaken up carbon alone with water it had sensibly subsided after the lapse of a few hours. After the addition of so small a quantity of gum the tendency to deposit is much diminished. The following are the details of some experiments. The strength of the carbon diffusion is given in terms of the number of c.c. of the strong carbon diffusions in 500 c.c. of water. The external white surface was a sheet of white paper. In all cases I have used the right eye only. The number under A denotes the mean of two trials got by pouring into the cylinder, and therefore likely to yield too low results. B denotes the mean of two trials got by pouring out of the cylinder, and therefore likely to give too high results. C denotes the mean of A and B. D denotes the length required by theory.

Standard diffusion, 1·2 c.c. in 500 c.c. of water,
length of column, 21·2.

Expt. I.—Comparison diffusion contains 1·8 c.c. in 500 c.c.

A.	B.	C.	D.
14·35	16·35	15·25	14·13

A second trial gave—

12·65	14·65	13·65	14·13

Expt. II.—Comparison diffusion contains 2·4 c.c. in 500 c.c.

10·6	11·25	10·93	10·6

Expt. III.—Comparison diffusion contains 3·0 c.c. in 500 c.c.

8·4	9·25	8·83	8·48

Expt. IV.—Comparison diffusion contains 3·6 c.c. in 500 c.c.

6·85	7·6	7·22	7·07

Expt. V.—Comparison diffusion contains 4·2 c.c. in 500 c.c.

5·6	6·1	5·85	6·06

Expt. VI.—Comparison diffusion contains 4·8 c.c. in 500 c.c.

5·1	5·55	5·32	5·3

Expt. VII.—Comparison diffusion contains 5·4 c.c. in 500 c.c.

4·1	4·5	4·3	4·71

On another occasion I got a nearer result, as follows:—

4·7	4·95	4·82	4·71

Expt. VIII.—Comparison diffusion contains 6 c.c. in 500 c.c.

4·13	4·58	4·36	4·24

Expt. IX.—Comparison diffusion contains 6·6 c.c. in 500 c.c.

3·98	4·25	4·11	3·85

Expt. X.—Comparison diffusion contains 7·2 c.c. in 500 c.c.

3·48	3·75	3·61	3·53

Expt. XI.—Comparison diffusion contains 7·8 c.c. in 500 c.c.

A.	B.	C.	D.
3·23	3·45	3·34	3·26

Expt. XII.—Comparison diffusion contains 8·4 c.c. in 500 c.c.

3·03	3·45	3·24	3·03

Expt. XIII.—Comparison diffusion contains 9 c.c. in 500 c.c.

2·9	3	2·95	2·82

Expt. XIV.—Comparison diffusion contains 9·6 c.c. in 500 c.c.

2·83	3·05	2·94	2·65

Expt. XV.—Comparison diffusion contains 14·4 c.c. in 500 c.c.

1·98	2·08	2·03	1·77

Expt. XVI.—Comparison diffusion contains 19·2 c.c. in 500 c.c.

1·6	1·58	1·59	1·32

Many of the above numbers are close approximations. I also tried columns of the lengths given by theory in all experiments, except by the ninth, where by an oversight I neglected to do so. The tints so obtained were in every case satisfactory, until I reached Experiment XIV. In this and in the succeeding experiments I thought that columns of the theoretical lengths gave tints slightly lighter than the standard diffusion. On some occasions I have wavered in my opinion that the theoretical column gave too light a tint in the fourteenth experiment. At another time I got the following result:—

Expt. XVII.—Comparison diffusion contains 9·6 c.c. in 500 c.c.

2·8	2·95	2·87	2·65

Here, again, the number under C is a little greater than that under D. I afterwards repeated the experiment with this variation—that the standard diffusion was contained in the comparison cylinder, and the comparison diffusion in the standard cylinder.

Expt. XVIII.—Standard diffusion contains 1·2 c.c. in 500 c.c.; length of column, 22·6.

Comparison diffusion contains 9 6 c.c. in 500 c.c.

2·85	3·07	2·96	2·82

On another occasion I got—

2·85	3·15	3·0	2·82

Here, again, there still remains a tendency to make the column a little longer than the theoretical.

I repeated the experiments in which the strength of the comparison diffusion is several times a multiple of the strength of the standard diffusion. The strength of the standard diffusion was the same as in the previous experiments, and the length of the column was 21·2.

Expt. XIX.—Comparison diffusion contains 9·6 c.c. in 500 c.c.

2·63	2·98	2·8	2·65

When I actually tried the theoretical column I thought that the tint was perhaps slightly lighter.

Expt. XX.—Comparison diffusion contains 14·4 c.c. in 500 c.c.

1·9	2·15	2·02	1·77

The tint given by the theoretical column I thought slightly lighter.

Expt. XXI.—Comparison diffusion contains 19·2 c.c. in 500 c.c.

1·55	1·63	1·59	1·32

The tint given by the theoretical column I thought slightly lighter.

Expt. XXII.—Comparison diffusion contains 24·0 c.c. in 500 c.c.

1·15	1·3	1·22	1·06

The tint given by the theoretical column I thought slightly lighter, but hardly distinguishable.

Expt. XXIII.—Comparison diffusion contains 28·8 c.c. in 500 c.c.

A.	B.	C.	D.
1·0	1·1	1·05	0·88

The tint given by the theoretical column I thought slightly lighter.

Expt. XXIV.—Comparison liquid contains 33·6 c.c. in 500 c.c.

0·88	0·9	0·89	0·76

The tint given by the theoretical column I thought slightly lighter.

Expt. XXV.—Comparison liquid contains 38·4 c.c. in 500 c.c.

0·73	0·83	0·78	0·66

The tint given by the theoretical column I thought slightly lighter; they were, however, very nearly the same.

This second series of experiments confirms the result of the first series that there is a slight departure from the rule, when the strength of one diffusion is several times a multiple of the other. Both series show a tendency to make the column in the comparison cylinder slightly too long. We must not hastily conclude that in these cases the theory is not applicable. It seems to me that we should expect such a result, for the medium is not perfectly transparent, and in one cylinder we have a column of this medium (water) several times a multiple of the length of the column in the other. This would require some slight compensation. In these experiments I have taken the lower level of the meniscus as the proper reading. In some of the stronger diffusions it was a little difficult to determine this exactly. On the whole, I think that the above experiments are favourable to the assumption that for a column of fluid containing finely divided carbon in suspension the relationship Qt = constant holds, if the intensity of the transmitted light remain constant. If we represent the above results graphically, and take as the theoretical curve the rectangular hyperbola $xy = 25{\cdot}44$, it will be seen that the results of the experiments do not depart far from the curve.

In a paper read at the last meeting of the section, I suggested a method for testing the assumed laws of the absorption of light. I also have applied the same reasoning and method of experiment to carbon diffusions. In one series of experiments I took a diffusion containing 1·934 c.c. in 500 c.c. The standard shades of grey used were one consisting of 10 grms. $BaSO_4$ and 0·012 of lamp-black—this I denote by Wa—the other consisted of 10 grms. of $BaSO_4$ and 0·048 of lamp-black—this I denote by Wb. The materials were well incorporated by shaking and grinding. To the powder I then added a little water so as to obtain a mixture of suitable consistence to be used as a paint. This was applied by a brush to pieces of cardboard so as to obtain uniform surfaces. These surfaces were then dried, the colorimeters employed were the same as in the last experiments. I looked at Wb through a column 3 c.m. long. I held the other cylinder Wa and endeavoured to get the same tint. The mean of two columns, one probably a little too long, and the other probably a little too short, gave 8·2 as the proper length. So I made the column of this length; I thought that the tints were the same. Now, if the law hold with regard to turbid solutions, if we increase both columns by the same length, the tints will again correspond. I took 4 c.m. as the common increment. The lengths of the columns were made 7 and 12·2; I thought the resulting tints equal. The lengths of the columns were made 11 and 16·2; I thought they were about equal, possibly Wa slightly lighter. My impression varied a little with the illumination of the surfaces. The lengths of the columns were now made 15 and 20·2; the tints seemed about equal, possibly Wa slightly lighter. The difference, if any, must be small, as is shown by the following experiment. Taking Wb, seen through a column 15 c.m. long, as the standard tint, I endeavoured to get the same tint with Wa seen through the other cylinder. For the lower limit I got 19·9 c.m., and for the upper limit 21·0 c.m., the mean of these is 20·45, not far removed from 20·2. Afterwards I thought a column of this length satisfied. I now made the lengths of the columns 19 and 24·2. I thought that the tints again corresponded.

Some experiments of a similar nature were made with a stronger diffusion; it contained 3·747 c.c. in 500 c.c. The tints of grey used were the same as in the last experiments. The standard tint at the commencement was Wb seen through a column 4 c.m. long. To get a similar tint with Wa one determination for the upper limit gave 6·5, and one determination for the lower limit gave 6·1. The mean of these is 6·3. A column of this length seemed to satisfy. The common increment is 3 c.m. The columns were made 7 and 9·3 c.m. long; the tints seemed to correspond. The columns were made 10 and 12·3 c.m. long; the tints again seemed to correspond. The columns were made 13 and 15·3 c.m. long; the tints seemed again to correspond. Finally, the columns were made 16 and 18·3 c.m. long; the tints again corresponded. In the last two experiments the greys obtained were very deep. I also made the following experiment:—I took two grey tints; one consisted of 10 grms. $BaSO_4$ and 0·042 grm. of lamp-black; the other consisted of 10 grms. $BaSO_4$ and 0·4003 grm. of lamp-black. These greys were made into a paint by the addition of a little water, and pieces of cardboard covered with them. They were then dried. These we may denote by $W\alpha$ and $W\beta$. I looked at $W\beta$ through a column 4·1 c.m. long, and endeavoured to get a similar tint with $W\alpha$ under the other cylinder. For the upper limit the column was 6·95 c.m. long, and for the lower limit 6·2 c.m. long; the mean is 6·57 c.m. I next altered the length of the column over $W\beta$ to 11·75. In the other cylinder for the upper limit the column was 14·9 c.m., and for the lower limit 14·8; the mean is 14·85. Hence we have—

$$W\beta K^{4\cdot1} = W\alpha K^{6\cdot57}$$
$$W\beta K^{11\cdot75} = W\alpha K^{14\cdot85}$$

By cross multiplication and elimination of $W\alpha, W\beta$

$$K^{4\cdot1}K^{14\cdot85} = K^{6\cdot57}K^{11\cdot75}$$

Theory requires the sums of the indices to be equal.

The sum on the right hand is 18·32, and on the left 18·95. The difference is, I think, not greater than what might be due to errors of observation.

CORRESPONDENCE.

A NEW HYDROGEN COMPOUND. (?)

To the Editor of the Chemical News.

Sir,—Having lately seen some interesting researches into the nature of hydrates conducted by Lt.-Col. W. A. Ross by means of the blowpipe, I venture to suppose these, and the way they are done, will be of equal interest to a numerous circle of readers.

It need hardly be mentioned that the investigation of the ultimate composition of hydrates is, from the very circumstances of the case, impossible by the ordinary solution processes of chemistry. An assay is needed. But, though slowly, the blowpipe method of analysis is forming its own separate chapters in the best works on chemical science, and deservedly. Great improvements have been made on the system of Plattner, and many are due to Col. Ross himself, whose two books, "Pyrology," and a "Manual of Blowpipe Analysis," are the standard England authorities on this branch of analytical chemistry.

The principal of the innovations which give their value to Col. Ross's system are the use of either boric or phosphoric acid as a fluid menstruum, instead of borax, or microcosmic salt; and the employment of an aluminium plate, instead of sticks of messy and obscuring charcoal.

It is at present a very unsettled point how hydrates are composed. Even "Miller's Chemistry" does not solve the whole problem. For instance, there is the case of anhydrous potash. The only theory which explains its production is that when water, symbolised by HHO, comes in contact with potassium K, an atom of hydrogen is displaced by an atom of potassium, the base; and the result is KHO, hydrate of potash. In a similar manner, if the hydrogen be dislodged from the hydrate KHO, we have KKO, anhydrous potash. The inference necessarily is that hydrates are not combinations of water, which is H_2O, and a base; but of a base and a substance, HO, which is still unrecognised. Col. Ross has written about this hypothetical substance under the title of chemical water, having had good grounds for supposing that it, and not sodium, gives the well-known D lines in the spectrum.

However this may be, boric acid is a very suitable vehicle for testing the properties of a lower oxide of hydrogen. In its fused form before the blowpipe, boric acid has the inestimable advantage of being anhydrous. Besides this, boron forms only this single oxide; and, finally, it is transparent, of nearly the same specific gravity as most vitreous masses, and soluble in boiling water.

In advance of the more detailed description which, it is understood, Col. Ross is furnishing to the Royal Society, I may state the process of estimating the oxide of hydrogen of a hydrate as shown me.

Col. Ross takes the purest silica (prepared by Dr. Schuchardt, of Görlitz), heats it to redness, and dissolves an accurately weighed quantity (about 0·037 grain) before the blowpipe, in a bead of pure borate of lime held in a platinum loop. He then notes the weight of the resulting silico-calci-borate bead. The silico-calci-boric bead is next surrounded with free boric acid in a fused state, and treated before the blowpipe. It is observed that the clear boric acid is at once clouded over, which would not be the case with borate of calcium alone, and becomes opalescent and semi-opaque.

The silico-calci-boric bead being, as found by Prof. Stokes, of Cambridge, insoluble, while fused boric acid is soluble, in hot water, is dissolved out and weighed. The result is, notwithstanding the loss of volatile opalescent matter, a very large gain in weight. This process of fusion in excess of boric acid, and subsequent extraction, is repeated four or five times, till there is no further opacity. When the silico-calci-boric bead is ultimately weighed it proves to have increased about 22 per cent.

Col. Ross can only account for the loss by opalescence, combined with an aggregate gain in the weight of the silico-calci-boric bead, on the assumption that the silica parts with the oxide of hydrogen it contains as hydrate, taking up an equivalent of boric acid instead. Hence the opalescence is most likely due to the appearance of HO, which Col. Ross has styled chemical water, and chemists have not yet isolated: the increase of weight to the absorption of boric acid.

But it has also been noticed that after the first exhibition of opalescence the calci-boric bead weighs much more than at the conclusion of the four or five reductions to which it is subjected. The probability, therefore, is that in addition to an oxide of hydrogen, HO, there is a compound of hydrogen and boron; hydroboric acid, in fact, given out in the later stages.

Hydrogen itself is not, according to the latest theory, a simple element, but more correctly a hydride of hydrogenium, symbol HH, these atoms being conjectured to exist in opposite electric states.—I am, &c.,

A. T. Fraser, Maj. R.E.

London, October 25, 1880.

EXPELLING AND COLLECTING GASES IN NATURAL WATERS.

To the Editor of the Chemical News.

Sir,—I beg to call your attention to the fact that the method of expelling and collecting the gases in natural waters described in the Chemical News (vol. xlii., p. 206) is not new. It was proposed by Reichardt (*Ann. f. Anal. Chem.*, vol. xl., p. 271), and is described in detail in Thorpe's "Quantitative Analysis," pp. 324—326.—I am, &c.

C. H. Bothamley.

Leeds, October 25, 1880.

THE SPECIFIC ROTATORY POWER OF CANE- AND INVERT-SUGAR.

To the Editor of the Chemical News.

Sir,—In a reply (Chemical News, xlii., p. 208) to my letter on the above subject, Mr. Allen says that I questioned his statement that "many authorities appear to have assumed that a solution of inverted sugar contained the same percentage as before inversion."

By referring to my letter you will see that the points I doubted were—that any authority should fail to take into account the increase of weight by inversion in calculating the specific rotatory power of invert-sugar, and that the value −25° was erroneous on that account.

In support of his statement Mr. Allen refers me to "Watts's Dictionary of Chemistry" (vol. v., p. 472), and to the new edition of "Miller's Organic Chemistry" (Part 3, Sect. I., p. 605); but as both quotations treat of the estimation of cane-sugar from *given rotatory powers of cane- and invert-sugar*, and *not the determination of* the specific rotatory power of invert-sugar, I cannot accept them as proofs of his original statement.

As I have already shown the origin of the value −25° given in "Watts's Dictionary," and as Mr. Allen's quotation from "Miller's Organic Chemistry" shows that the value −24·2 is also a *calculated and not an observed* value, it is evident that these numbers cannot be regarded as the result of assuming "that a solution of inverted sugar contained the same percentage as before inversion.—I am, &c.,

Alexander Watt.

Liverpool, October 26, 1880

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Note.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 11, September 13, 1880.

Certain Solar Phenomena observed at Nice.—L. Thollon.—This paper cannot be usefully abstracted without the accompanying figures.

Law of Electro-magnetic Machines.—J. Joubert.—The author concludes that the retardation is equal to one-eighth of the entire period. The intensity is constant and equal to the quotient by V_2 of the absolute maximum of intensity. The electro-magnetic work is proportional to the speed. The speed is in a constant ratio to the resistance.

Boro-duo-deci-tungstic Acid and its Potassium Salts.—D. Klein.—This acid does not lose the last traces of water till near a red heat. It becomes then insoluble, and is decomposed into a mixture of boric and tungstic anhydrides. The new acid is a hydrate of $10WO_3,Bo_2O_3$.

No. 12, September 20, 1880.

Odours of Paris.—Sainte-Claire Deville.—The author, having analysed a portion of the sub-soil taken at about the depth of 1 metre in the Rue Saint Jacques, concludes that the sulphur and the hydrocarbons present are due to the leakage of gas from the mains, to which he ascribes a sanitary and antiseptic action. He proposes that all excrementitious matter should be received in vessels of metal without ever coming in contact with the external air, and be conveyed away underground in metal pipes, heated to 100° or upwards, dried, and used as manure.

A New Experiment showing the Direction of the Rotation Impressed by Substances upon Polarised Light.—G. Govi.—If a very pure spectrum is produced with light polarised rectilinearly and made to traverse first a plate of rock crystal perpendicular to the axis and then an analyser, the spectrum is furrowed by one or more black bands, which are displaced on turning either the polariser or the analyser. The movement of the bands takes place from the red towards the violet, or from the violet towards the red (the analyser or the polariser being turned always in the same direction), according as the plate of rock crystal is dextro-rotatory or lævo-rotatory. In the direction of this movement we have therefore an indication of the direction of the rotation which the interposed substance impresses on the plane of polarisation.

Study on the Telluric Rays of the Solar Spectrum. L. Thollon.—The author remarks that a complete knowledge of the earth's atmosphere will not be attained until we have an exact and complete knowledge of the telluric rays, the elements which produce them, their variations of intensity according to the temperature, the hygrometric condition of the air, and the height of the sun above the horizon. For this purpose the telluric groups must be resolved into their simple elements, and separated from each other as well as from the other metallic rays; their positions on the spectrometric scale must be determined with all possible exactness; their variations of intensity and the attendant circumstances must be studied, and from these results the original element of each ray must be deduced, and the deduction must be experimentally verified. The author has determined the two first points for Angström's telluric groups, B, D, and α.

Liquefaction of Ozone and its Colour in the Gaseous State.—P. Hautefeuille and J. Chappuis.—Inserted in full.

No. 13, September 27, 1880.

Vapour-tension Manometer for Analysing Liquids and Measuring Pressures.—L. Perrier.—This apparatus does not admit of intelligible description without the accompanying illustration.

The Inventor of Binocular Telescopes.—G. Govi.—This invention appears to be due to D. Chorez, an optician of Paris, who made and sold these instruments in 1625.

No. 14, October 4, 1880.

Utilisation of Chamber Crystals.—C. Girard and A. Pabst.—The authors remark that the use of these crystals in the arts has hitherto been restricted to bleaching silks. They have succeeded in preparing the diazo compounds, amido-azo-benzol, and nitro-alizarin, by causing nitroso-sulphuric acid to act upon the corresponding amido derivatives, or upon aniline and alizarin. They mix the crystals with sodium chloride in a cast-iron apparatus, and the nitrosyl chloride evolved is received into a refrigerated hydrochloric solution of dimethyl-anilin for the preparation of nitroso-dimethyl-anilin. A similar mixture may also be applied to the oxidation and destruction of the gases and vapours given off in the treatment of fœcal matters.

Certain Thermometric Questions.—J. M. Crafts.—The author criticises the paper of M. Pernet (Sept. 6), and raises the question of the fixity of the interval between the points 0° and 100° depending on the permanence of the mean coefficient of expansion of glass between these limits.

Decomposition of Salts by Liquids.—A. Ditte.—The author has previously pointed out (*Comptes Rendus*, lxxix., pp. 915, 956, 1254) that the decomposition of a salt by water takes place according to laws quite analogous to those which govern the dissociation of bodies by the action of heat. These laws are equally applicable to the decomposition of salts by pure water, by saline or acid solutions, by the alcohols. They probably regulate in a general manner the decomposition of salts in the wet way whatever may be the solvent employed.

Journal für Praktische Chemie.

Nos. 11 and 12, 1880.

Combustion-Heat of Organic Compounds.—Dr. C. von Rechenberg.—The author determines the combustion-heat of cane-sugar, tartaric acid, naphthalin, starch, erythrodextrin, inulin, cellulose, gummic acid, maltose, milk-sugar, dextrose, dextrose containing crystalline water, lactose, mannite, dulcite, myristic acid, stearic acid, oxalic, malonic, succinic and citric acids, phenol, anthracen and anthraquinon, and the solution-heat of cane-sugar. Concerning the method employed in Prof. Frankland's researches ("On the Origin of Muscular Power"), he remarks that Stohmann has detected three essential errors: the want of any control as to the complete combustion of the substance employed; too high a production of heat in consequence of the combustion of a part of the copper apparatus, and the impossibility of completely dissolving the potassium chloride formed by the decomposition of the potassium chlorate, whence again an excessive degree of heat is registered.

Process of Saccharification by the Action of Dilute Sulphuric Acid upon Starch at Elevated Temperatures.—Dr. F. Allihn.—The conclusions at which the author arrives are, that the saccharification of starch by means of dilute sulphuric acid proceeds in general the more rapidly and completely the more concentrated the acid, the higher the temperature, and the longer. The quantity of the starch converted is proportional to the time of action up to 40 to 50 per cent. In the later stages the process proceeds more slowly, so that a complete conversion, even if possible under the given circumstances, is reached only after an exceedingly prolonged action. The causes of this retardation lies in the different resistance of the dextrins to dilute acids.

Specific Rotatory Power of Lactose.—Dr. E. Meissel.—The specific rotatory power of pure anhydrous lactose at $17{\cdot}5° = +88{\cdot}08$.

New Method for the Quantitative Determination of Arsenious Acid in the Presence of Arsenic Acid.—Leopold Mayer.—Taking as a point of departure the known property of arsenious acid to reduce ammoniacal solutions of silver at a boiling heat, the author bases upon it a convenient gravimetric method for the determination of arsenious acid along with arsenic acid. The common method of precipitating arsenic with magnesia solution as ammonio-magnesium arseniate is not free from sources of error, due to the solubility of the latter compound. The methods of H. Rose and Vohl are tedious and circumstantial. The author's method depends upon the following decomposition. If a solution contains in addition to arsenious acid no other substance which reduces the ammoniacal solution of silver at a boiling heat, the reduction takes place according to the following equation:—

$$As_2O_3+2Ag_2O=As_2O_5+2Ag_2.$$

After boiling for half an hour, the reduced silver separates out as a fine powder and is filtered off and weighed. The quantities of silver thus obtained agree exactly with the arsenious acid employed. The reduced silver must be washed with warm ammonia and water containing sal-ammoniac. If a portion of the silver is reduced in the form of a mirror on the sides of the glass it is dissolved off in nitric acid, precipitated as silver chloride, which is added to the main quantity of the silver for such small traces of silver chloride are reduced on ignition by the carbon of the filter. In applying this method for the determination of arsenious acid in presence of arsenic acid, the arsenious acid is determined by boiling with ammoniacal solution of silver. On the reduction of the silver, the arsenious acid passes into arsenic acid, which is determined along with the original arsenic acid. The quantity of the latter is found as difference.

Synthesis of Citric Acid.—E. Grimaux and P. Adam. —From the *Comptes Rendus*.

Reply to W. Hempel on the Salyicylic Acid Question.—E. von Meyer.—A purely controversial paper.

Antimoniferous Manganese Ore.—H. Reinsch.—The author has found antimony sulphide in quantity in the residue from the preparation of chlorine with hydrochloric acid and pyrolusite. The origin of the sample could not be ascertained. (Suppose such manganese ore had been used along with potassium chlorate for generating oygen gas?

Destructive Action of Woody Fibre upon Salicylic Acid—H. Kolbe.—The author points out that the casks in which salicylic acid failed to keep water free from taint were open. As soon, therefore, as the acid was consumed by the wood, septic matter was able to find entrance.

MISCELLANEOUS.

Newcastle Students' Field Club.—We learn with great pleasure that, in order to keep together the students who annually attend the various classes during the winter session, a club is being formed which will, during the summer months, organise Saturday afternoon excursions to localities in the district, "where scientific investigation and research can be combined with social intercourse." The idea is excellent, and if judiciously carried out will not merely keep the students together, but initiate them in the practice of observation in geology, mineralogy, botany, &c. Our informant, the honorary secretary of the new club, does not say with what institution the students in question are connected.

The Graham Medal.—We have much pleasure in drawing attention to the Triennial Medal, instituted by the Chemical Section of the Philosophical Society of Glasgow. It is a handsome gold medal (value about £12), and will be awarded at the end of the present session. The competition is an open one, no restriction whatever being imposed, the committee requiring no more than that the paper shall be original and not a mere compilation. For further information we would draw our readers' attention to our advertisement columns.

MEETINGS FOR THE WEEK.

Monday, Nov. 1st.—Royal Institution, 5. General Monthly Meeting.

Thursday, 4th.—Chemical, 8. "On the Compounds of Vanadium and Sulphur," by E. W. E. Kay. "Report on the Atmospheric Oxidation of Phosphorus, and on some Reactions of Ozone and Peroxide of Hydrogen," by C. T. Kingzett. "Action of Heat on the mixed Vapours of Benzene, Toluene," by Dr. T. Carnelly. "Bismuth and Bismuth Compounds: Part I., Oxides and Hydrates; Part II., Halogen Compounds," by M. M Pattison Muir, G. B. Hoffmeister, and E. E. Robbs. "On the Colour, Properties, and Relation of the Metals Copper, Nickel, Cobalt, Iron, Manganese, and Chromium," by T. Bayley. "Action of Diazo-naphthalin upon Salicylic Acid," by Dr. Percy Frankland. "Action of Organo-Zinc Compounds upon the Nitriles and their Analogues," by Dr. E. Frankland, F.R.S., and C. C. Graham. "Action of Zinc-ethyl on Benzoylic Cyanide," by Dr. E. Frankland and D. A. Louis. "On the Basic Sulphates of Iron," by Spencer Pickering. "Fourth Report on Researches in Chemical Dynamics," Sec. I. by Dr. C. R. A. Wright; Sec. II. and III. Dr. C. R. A. Wright and A. E. Menke. "On some Naphthalin Derivatives," by Dr. Armstrong and N. E. Graham.

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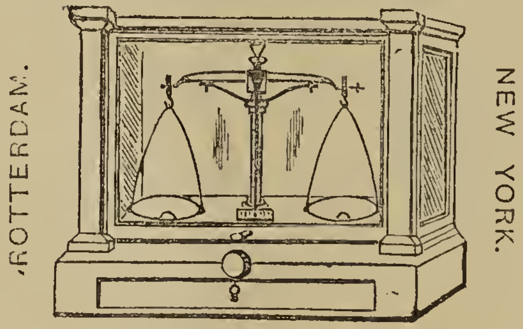

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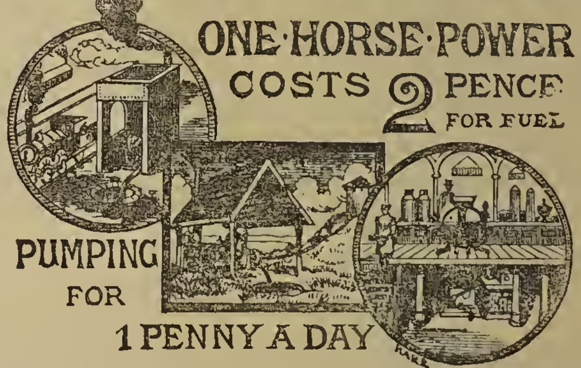

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THE CHEMICAL NEWS.

VOL. XLII. No. 1093.

ON THE
ETIOLOGY OF THE CARBUNCULAR DISEASE.*

By L. PASTEUR,
Assisted by Messrs. CHAMBERLAND and ROUX.

ONE of the diseases which cause the greatest destruction of cattle is the *carbuncular disease*, or *anthrax*. Almost all portions of this country suffer from it; some in a slight degree and others very heavily. The pecuniary loss from this disease is very serious in some localities, as, for instance, in the Department of Eure et Loire. Among the many herds raised there, there is hardly one which is not afflicted by it every year. Any farmer there considers himself fortunate, and even pays no attention to the disease, if his loss is not greater than from 2 to 3 per cent of the number of animals in his flock. This disease is known in all countries. In Russia it is particularly disastrous, and it is called the *Siberian plague*.

For a long time the belief has prevailed that the carbuncular disease is due to various incidental causes, such as the nature of the ground, of the water, of the fodder; the methods of breeding and of feeding. Every cause has been invoked to explain its spontaneous existence. Lately, however, the researches of Messrs. Davaine and Delafond, in France, of Pollender and Branell, in Germany, have called attention to the existence of a microscopical parasite in the blood of animals who have died of this disease. Moreover, rigorous researches have disputed the doctrine of the spontaneous generation of microscopical beings, and the effects of fermentations have been attributed to specific microscopical germs. From these causes, the idea has arisen that possibly animals suffering from carbuncular disease may have acquired its germs, which are the germs of the parasite, from the exterior world, and that there is not, properly speaking, any spontaneous origin to this disease. This opinion became still more definite when, in 1876, Dr. Koch, of Breslau, published that the bacteridia, in its vibrionary or cacillary shape, may be resolved into germ corpuscles or spores.

Two years ago, I had the honour of submitting to the Minister of Agriculture and to the President of the Council General of Eure et Loire, a project of research on the etiology of the carbuncular disease, which was accepted with alacrity. I also had the good fortune to find in M. Manoury, Mayor of the Village of St. Germain, near Chartres, an enlightened agriculturist, who had the kindness to allow me to establish on his farm a small flock of sheep, under the same general conditions that are usual in the Beauce for sheep penned in the open air. Moreover, the Superintendent of Agriculture very obligingly placed at my disposal two shepherd pupils of the School of Rambouillet, to watch and feed the sheep.

The experiments began in the first days of August, 1878. These consisted in feeding certain lots of sheep with lucern, watered with artificial cultivations of the bacteridia of carbuncular disease, containing the parasite and its germs. Without entering into details in this place, I will give the following results of our experiments:—

Notwithstanding the immense number of spores of bacteridia swallowed by all the sheep of one lot, many of them escape death, often after being visibly ill; others, in smaller numbers, die with all the symptoms of spontaneous carbuncular disease, after an incubation, which may extend to eight or even ten days, although towards the last the disease assumes those suddenly violent characters which have led some observers to think that the period of its incubation is very short.

The mortality may be increased in a marked degree by mixing with the food soiled by the germs of the parasite, bodies with sharp points, such as the pointed ends of thistle leaves, and the barbs of oats cut up into fragments about a centimetre in length.

It was important to ascertain if the *post mortem* examination of animals dying in these conditions would shows lesions similar to those which are observed in animals who die spontaneously in sheep folds or in open-air pens. It was found that the lesions in all cases were identical, and the nature of these lesions authorise the belief that the disease begins in the mouth and in the back part of the throat. The first observations of this kind were made in *post mortem* examination, conducted under our own eyes, by M. Boutet and by M. Vinsot, a young veterinary surgeon, and a graduate of the School of Alfort. Both of them have helped us with great zeal during all our experiments in St. Germain.

The idea that sheep which die spontaneously from the carbuncular disease in the Department of Eure et Loire are infected by the spores of the bacteridia of this disease mixed with their food, acquired more consistency in our mind from these examinations. But whence come the germs of these bacteridia? If we reject every theory of the spontaneous generation of this parasite, we must direct our attention to the animals buried under ground.

We must here explain what is done when an animal dies spontaneously from carbuncular disease. If there is an establishment in the neighbourhood for skinning animals, the body is taken there. If no such establishment is in the neighbourhood, or if the hide is of little value, as is the case with sheep, a grave is dug from 0·50 to 1 metre deep and the body is thrown in and covered over with earth. This grave is dug wherever the animal has died, or in some neighbouring field, if he dies in a stable. We may ask: What happens in this grave, and is there in it any cause for disseminating the disease? Many persons will answer in the negative, for Dr. Davaine has ascertained by accurate experiments that an animal who has died of anthrax cannot, after putrefaction, communicate the disease. Very recently numerous experiments have been made by one of the eminent professors of Alfort, a great partisan of the spontaneousness of all diseases. He has reached this conclusion: "That waters charged with the blood of animals who have died of carbuncular disease; that composts made by stratifying earth, sand, and stable manure with remains of bodies of dead sheep brought from Chartres, have never (by inoculation) caused the least symptom of carbuncular disease." (Colin, *Bulletin de l'Académie de Médecine*, 1879). But here we must take into account the difficulties of this research, difficulties of which M. Colin was entirely unaware. To take specimens of earth from the fields of the Beauce, and show in them corpuscles from one to two thousandths of a millimetre in diameter, capable of infecting animals with the carbuncular disease, this is in itself a difficult problem. However, by proper washings, and by making use of the susceptibility of Guinea pigs and rabbits to contract the carbuncular disease, something could be done if the parasites of this disease were the only ones in the earth. But the earth must contain an infinite multitude of microscopical germs of various species, and in the cultivation of these on a living animal, or artificially in vessels they interfere with one another.* During the last twenty years

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* Translated from *Comptes Rendus de l'Académie des Sciences* of July 12, 1880, p. 86, by P. Casamajor.

* I am led to believe that in this infinite quantity of germs is to be found the true solution of the nitrification which Messrs. Schlœsing and Müntz have shown to depend on fermentation. One day, if I remember rightly, in July, 1878, I received a visit from these excellent observers. They brought me little pellers from their nitrifying tubes, on which they had not been able to detect microscopic organisms. These, however, were full of germs. I do not believe that any special ferment, any body in the process of development (which would then have a contrary effect) causes nitrification; it is rather a physical effect of absorption and transportation of oxygen on the elements of ammonia by the innumerable germs in the earth, analogous to the influence of *Mycoderma aceti* on alcoholic liquids.

I have often called the attention of this Academy to the struggle for existence between microscopical beings. I may add that to isolate the carbuncular bacteridia from a portion of earth in which it may exist as germs, recourse must be had to special methods, whose application requires the most delicate attention. The action of air, of vacuum, changes in the nature of the media of cultivation, influence of variations of temperature; these are the means which must be used to prevent one germ from hiding the action of another. Any method of research which is not characterised by the most careful attention is powerless, and negative results only prove that, with the conditions in which the observations were made, the bacteridia did not show itself. The main argument presented by the eminent professor of Alfort is that the bacteridia disappears from the body of an animal as soon as it putrifies. This is an accurate statement, and the fact was known by those who flay and cut up the bodies of dead animals long before it was confirmed by Dr. Davaine. I have often heard these men, when handling the dead bodies of horses who had died of anthrax, when I put them on their guard against the danger they were running, say that there is no danger when the body is in an *advanced* state; the danger only exists when it is still warm. Although this fact is not strictly accurate, it agrees very well with what is true. In a previous investigation, published by M. Jaubert and myself, may be found the true explanation of the phenomenon. As soon as the bacteridia in its filiform state is deprived of air, if it is placed, for instance, in vacuo, or in carbonic acid, it resolves itself into granulations of great tenuity which are dead and innocuous. Putrefaction places the bacteridia precisely in these conditions of disaggregation. The germ corpuscles or spores do not go through the same process, as was ascertained by Dr. Koch. At any rate, as the animals at the time of their death only contain the filiform parasite, putrefaction must destroy it entirely. If this opinion was accepted as explaining the facts that take place in nature, we would only have an imperfect idea of the truth.

When a horse, a cow, or a sheep, which has died of the carbuncular disease, is buried in the ground, we may imagine that in most cases some blood finds its way out of the body, even if the animal has not been wounded. A habitual characteristic of this disease is that at the time of death, blood runs out through the nostrils, through the mouth, and even in the urine, which becomes red with blood. Besides, several days must elapse before the bacteridia is resolved into innocuous granulations by the gases free from oxygen which are produced by putrefaction. Meanwhile the excessive swelling of the dead body causes the liquids to run out through the natural openings, and through such ruptures as may exist of the skin and other tissues. The blood and other matters thus mixed with the surrounding portions of aërated earth are no longer in the same conditions as those of putrefaction, but rather in the conditions of artificial cultivation, suitable for the formation of the germs of the bacteridia. Does experiment confirm these preconceived ideas?

We have mixed blood from animals who had died of this disease with earth watered with yeast extract or with urine at the ordinary temperature of summer, and at such temperatures as are maintained by the putrefaction of dead bodies. In less than twenty-four hours, multiplication and production of germ corpuscles of bacteridia have taken place from the bacteridia in the blood. These germ corpuscles may afterwards be found in a condition of latent life, ready to develop, ready to propagate the disease not only after weeks of stay in the earth, but even after years.

But these are only laboratory experiments. We must ascertain what happens in fields exposed to the open air, and to all the alternations of dryness and moisture. In the month of August of 1878, we buried in a garden of the farm of M. Manoury, a sheep of his flock which had died of the carbuncular disease, as verified by a *post mortem* examination. Ten months after this, and also fourteen months afterwards, we took up earth from the grave, and we easily ascertained in this earth the presence of the germ corpuscles of the bacteridia, and, by inoculation on Guinea pigs, we caused their death by the carbuncular disease. Moreover, and this is a circumstance worthy of note, the same investigation was carried on with earth from the surface of the grave, and the germ corpuscles were found to exist, although the earth of the grave had not been disturbed in the interval. Finally, similar experiments were made with earth from graves in the Jura of the depth of two metres, in which had been buried the bodies of cows that had died of this disease in the month of July, 1878. Two years afterwards, which was quite recently, we have collected earth from the surface, and we have obtained deposits from it, which gave rise to the carbuncular disease. Three different times in this interval of two years we have obtained carbuncular disease from this same surface earth. We have finally ascertained that the germs on the suface of graves, in which animals are buried who have died of this disease, may be found after the operations of cultivation and after the gathering of crops. These last experiments were made in several places on the farm of M. Manoury. When these experiments were repeated on earth situated at a considerable distance from the graves, no carbuncular germs have been obtained.

I would not be surprised if, while I am speaking, doubts should rise in your minds concerning the accuracy of these observations. For how can the earth, which acts as a filter so thoroughly, allow microscopical germs to rise to the surface? Such doubts could easily find a justification in the experiments which M. Joubert and I have published. We have announced the fact that water of springs which rise from the earth, even from a moderate depth, are so entirely free from germs that they cannot produce a change in those liquids which are the most easily affected. The waters of springs, nevertheless, rise below portions of the ground through which rain-waters are constantly passing, even during centuries, and their tendency is to carry downwards the finest particles of the earth situated above these springs. There is certainly a great difference between such results and those to which I have called your attention, in which microscopical germs rise from below, even from great depths, in a contrary direction to the flow of rain-water. Here is certainly an enigma. The members of this Academy will certainly be surprised to hear the explanation of it. You may even be astonished that the theory of germs, but lately born from experimental research, has in store such unexpected revelations. Earth works are the carriers of germs, and it is to them that we owe it that the terrible parasite of carbuncular disease is brought to the surface from the depths of the earth, for it is in the little cylindrical agglomerations, and in the finer pellets voided by these worms, and deposited on the surface after heavy dews and after rains that we find the germs of the carbuncular disease, together with with many other germs. We may, by direct experiment, ascertain that it is to this agency that is due the transfer of the germs to the surface. If, in a volume of earth, in which spores of the bacteridia have been mixed throughout the mass, we leave a number of earth-worms for several days, we will, on opening their bodies, so as to carefully extract the earthy cylinders which fill their intestinal canal, find in these a great number of spores of the bacteridia.

If the loose earth at the surface of graves of animals who have died of the carbuncular disease contains the germs of the bacteridia, often in great quantities, they must originate from the disintegration, by rain-water, of the cylindrical excrements of earth-worms. The dust from this disaggregated earth is thrown on plants growing at the level of the ground, and, in this way, animals in the open air find in some pastures the germs of the germs of the carbuncular disease, and become infected exactly in the same way as those in our experiments who fed on Lucern, soiled by artificial cultivation of the bacteridia. These results lead us to meditate on the possible influence of the soil on the etiology of other disease, on the danger of cemeteries, and the usefulness of cremation!

Do not earth-worms carry to the surface of the ground other germs which may, to the worms themselves, be as harmless as those of the carbuncular disease, but which may be the cause of disease to man and to domestic animals? They are, indeed, constantly filled with germs of all kinds, and in all cases the germs of the carbuncular disease are found associated with those of putrefaction and septicemia.

As to the prevention of the carbuncular disease, it seems easy of accomplishment. Animals must never be buried in fields in which fodder is raised or in which cattle are penned. Whenever such soils can be found, preference should be given, for burying dead animals, to sandy or calcareous soils, in dry situations, as such soils are not favourable to the life of earth-worms. The eminent Chief of Agriculture, M. Tisserand, lately told me that the carbuncular disease is unknown in the region of Savarts, in Champagne. The absence of this disease may be attributed to the fact that in the poor soils of this kind, as in the case of the Camp at Châlons, the thickness of arable land is not greater than 0·15 to 0·20 of a metre, and the subsoil is a bed of chalk in which earth-worms cannot exist. In a soil of this kind, the burying of a carbunculous animal may give rise to many germs, which from the absence of earth-worms will remain at a depth in the ground where they are harmless.

It would be very desirable to have careful statistics stating, in given localities, whether the carbuncular disease is prevalent or not, and also stating the nature of the soil, whether favourable or not to the presence of earth-worms. M. Magne, Member of the Academy of Medicine, has informed me that in the Aveyron, in localities in which the carbuncular disease is found, the soil is argillo-calcareous, while in those in which the disease is unknown the soil is schistose and granitic. I have always understood that in these latter soils earth-worms do not abound.

I will take upon myself to close this communication with the assurance that, if agriculturists desire it, the carbuncular disease will soon be a thing of the past, because this disease is never spontaneous, and can only be found where it has been deposited, and where its germs have been disseminated by the innocent complicity of earth-worms; and, finally, that, in any locality, it will soon disappear unless the causes of its propagation are maintained.

On the proposition of M. Thenard, the Academy decides that the paper of M. Pasteur will be sent to the Minister of Agriculture and Commerce.

PROCEEDINGS OF SOCIETIES.

NEWCASTLE-UPON-TYNE LITERARY AND PHILOSOPHICAL SOCIETY.

October 20, 1880.

"*Electric Lighting*," by Mr. J. W. Swan.

The Lecturer commenced by saying that at the time of his last lecture on Electric Lighting, almost two years ago, the public mind was greatly perturbed by the question whether or not gas lighting was about to be superseded by electric lighting.

It was generally believed that a revolution-working discovery had been made, and you may remember, as a consequence, gas stock was much depressed in value. After a while, when the supposed wonderful discovery dwindled and paled in the daylight of scientific examination, the panic subsided, and then the current of feeling drove the other way, and in place of the hasty credulity which at first took possession of the public mind, there is now, I think, an equally unreasonable *unbelief* in the possibilities of electric lighting—the idea now widely prevailing that electric light, as a substitute for gas, is after all a delusion.

In tracing the progress of electric lighting during the last two years, the Lecturer endeavoured to give some reasonable *grounds* for forming an opinion on the question whether or not there is a prospect of electric light being made useful to us for lighting our streets and houses, and, therefore, to what extent it may probably compete with gas light.

After some introductory remarks on the general principles governing the production of powerful electric currents by the dynamo-electric machine, the lecturer said that it was practicable to develop 1 horse-power by the combustion of 2 lbs. of coal per hour, and to produce with this amount of motive power a current of electricity sufficient to give a light of over 1000 standard candles. That is to say, we can maintain for one hour an electric light more than equal to 1000 standard candles, or 66 large gas-burners, by the combustion of 2 lbs. of coal. That is a much more economical result than can be obtained by the voltaic battery, or any other means of generating an electric current at present known.

The method of storing up electricity on the principle of Planté's secondary battery was next alluded to, and the adoption of some such means as this was advocated to perform the same part in a general system of electric lighting that the gasometer performs in gas lighting.

After briefly describing the newest forms of thermopile, one of which, the Clamond, was said to have maintained four electric lights, each of 220 standard candles, by the combustion of 21 lbs. of coke per hour, Mr. Swan passed on to speak of the different ways of applying currents of electricity to the production of light in the following words:—

"All the various means of producing light by electricity that can make any pretension to practicability are divisible into two classes, namely:—

1st. Lighting by the electric arc.
2nd. Lighting by incandescence.

In lighting by the electric arc, there is a break or gap in the circuit which has to be bridged over by a sort of electric flame. In lighting by incandescence there is no break or gap at the point where light is produced, but a thin highly infusible and badly conducting solid substance is there interposed, which becomes white hot, and emits a light bright in proportion to the degree of heat produced in it.

Now, to fix in your mind precisely what I am talking about, I will first show you what I mean by the electric arc. This will enable you more easily to follow my remarks on the question of electric lighting by this method.

Here is an enlarged view of the electric arc,—here are the two carbon points, and here—between them—is formed the electric arc.

A powerful electric current, produced by a gas-engine and dynamo-electric machine, is supplied to these two pencils of carbon through thick copper conducting wires, and you see that a stream of flame is flowing or rushing between them. If the points are too much separated the light is lessened; by increasing the air-space between the points, resistance to the passage of the current is increased, and the current is consequently diminished, and when this diminution of current passes a certain limit the light is lessened. On the other hand, when the points are made to approach too closely to each other, the light becomes less for the opposite reason.

A certain amount of difficulty or resistance must be offered to the passage of the current in order to produce light, and to get the best effect the resistance must neither be too much nor too little.

When the points are made to touch, there is no longer the resistance of the air-space to be struggled through; the light consequently goes out.

Observe the alteration in the points; they burn away, and one faster than the other. Now as the production of a steady light demands that the points should be maintained at a constant distance apart, it is evident that under the complicated conditions of the case a nice regulation of the distance of the points to each other is a matter of extreme difficulty.

I will now ask for the current to be turned on to a lamp hung up aloft, in order that you may see how very nearly the difficulties of producing a steady arc light have been surmounted.

The particular lamp I am using for the purpose of my illustration is Crompton's lamp, of which here is a diagram. Properly regulated it feeds the pencils together almost continuously and with great precision.

That then, ladies and gentlemen, is "par excellence" *The Electric Light.* That is the form and style of electric light that inventors have for the last forty years struggled and battled with difficulty upon difficulty to render serviceable to the wants of man.

From the nature of the electric arc light you will readily perceive that it is a kind of light not suited to the lighting of dwelling-houses, nor shops, nor streets. It does not lend itself kindly to division nor extensive distribution. It will give you either a great deal of light or none.

For certain exceptional uses it is excellent. If you want a very brilliant illumination all centered in one focus —for a lighthouse, or signal, for example, or for a high-roofed railway station, or very large workshop, or open space—it is unquestionably the most economical and the best of all artificial lights.

It has already found for itself in England over 200 appropriate applications. Its use is continued and increased on the Thames Embankment. It is used in the reading-room of the British Museum; in the Picton Library, Liverpool; in a portion of the South Kensington Museum; at the Liverpool Road Railway Station; the Barrow Shipbuilding Works; the St. Enoch Railway Station, Glasgow; on the Promenade at Blackpool; and at the New Albert Docks, where, by means of Siemens's lamps, three miles of wharfs and quays are made almost as light by night as by day, and at a very moderate cost.

As an illustration of the economy of lighting by means of the electric arc under conditions suited to its use, I instance the case of the Alexandra Palace, where 2000 gas-lights, which consumed 26s. worth of gas per hour, are replaced by six of Mr. Crompton's lamps, giving a greater aggregate amount of light than the 2000 gas-burners, at a cost of 6s. per hour. In some instances the economy of the electric arc light is greater by half than in the case I have mentioned. Not only is it economical in such a case as that of the Alexandra Palace, but it produces an effect of general illumination overhead as well as upon the ground not producable by any other means.

But the interest which attaches to electric arc lighting is much lessened by the fact that the purposes to which that mode of lighting is suited are exceptional. We do not as a rule want the light of 1000 candles or more all in one place. What we do generally want is a number of small and steady lights, spread about in different rooms, and in different parts of those rooms. For electric light of this description we must search in another direction.

I said that there is *another way* of producing electric light; namely, by *incandescence*.

Lighting by incandescence is a branch of the subject which has a special charm for me, because I have bestowed upon it much thought and labour; and it is I believe *the* branch which will yield the largest crop of fruit. Electric lighting by incandescence is just as simple as arc lighting is difficult; all that is required is a material which is not a very good conductor of electricity, highly infusible, and which can be formed into a wire or lamina, and is either non-combustible in air, or, if combustible, does not undergo change in a vacuum. There are, so far as I know, just two substances that possess, in any sufficient degree for the purpose in question, the qualities I have specified.

The two substances are:—Platinum, or an alloy of platinum with iridium; and carbon. Platinum has the advantage over carbon that it is not combustible in air; it does not, like carbon, burn away if you make it white-hot; but it is very inferior to carbon in the degree of heat it will bear without fusion; and for producing light by incandescence it is essential to economy that the incandescent material should be capable of enduring an extremely high temperature, because the amount of light emitted by an incandescent substance increases in a more rapid ratio than the temperature.

When, for example, you have a piece of platinum wire or carbon red-hot it emits almost no light, but double its temperature by sending a double quantity of current through it, and it will yield much more than twice the light it did before.

It is therefore evident that the hotter the incandescent material can be made the less the light will cost per unit of power expended.

Iridio-platinum, comparatively with other metals, may be called extremely infusible, but compared with carbon it is nowhere. Carbon has in fact resisted without fusion the very highest degree of heat brought to bear upon it; and what that degree of heat is I can hardly estimate, it is so enormous.

But carbon has been found so difficult to deal with, on account of its ready combustibility (and some other troublesome properties which I will mention afterwards), that experimenters have bestowed much attention upon platinum and iridio-platinum as the incandescent material for electric lamps.

Mr. Edison was, I think, the last who attempted to utilise platinum in an electric lamp, and I think there can be no doubt that he obtained better results with platinum, and came nearer making a useful platinum lamp, than any experimenter in the same track who had gone before him.

Here is a view of Edison's platinum lamp.

This is the lamp of which so much was promised and expected in October, 1878, and which led, you remember, to the panic in gas shares. This lamp did not realise the hopes of the inventor.

(I will not re-kindle Mr. Crompton's Electric Sun, because I hope presently to show you some small lamps whose light would be absolutely drowned in that fierce radiance, as stars are by the light of day.)

Whilst Mr. Edison was endeavouring to produce a useful incandescent lamp by means of platinum, I was endeavouring to obtain the same end by means of carbon.

It had appeared to me for many years that if ever electric light was to become generally useful, it would, most probably, be by means of the incandescence of carbon. I had, long before the time to which I am referring, attempted to render this idea practicable.

As a matter of history, I will briefly describe an experiment which I tried about twenty years ago:—

I had a number of pieces of paper and card, of various forms and sizes, buried in charcoal in a crucible. This crucible I sent to be heated white-hot in one of the pottery kilns belonging to Mr. Wallace, of Forth Banks. From the pieces of carbonised card which I thus obtained I selected a long spiral; the ends of this I clipped between small blocks of carbon carried by uprights, and connected with conducting wires. A small glass shade was cemented over this mounted carbon spiral, and the air was exhausted by means of a very good air-pump, lent to me for the purpose of this experiment by the Rev. Robert Green, of Longhorsley. A good vacuum (according to the ideas that then prevailed) having been produced, I applied the wires of my battery (consisting of 10 cells of Callan's modification of Grove's Battery) with great expectation of a brilliant result: instead of this there was the most absolute negative presented to me; not a vestige of heat or light appeared in my long ringlet of carbonised paper. It was evident, and I immediately recognised the fact, that the electric current of the strength I was using would not go in sufficient quantity through so long a piece of carbon as I had taken. I therefore repeated the experiment with shorter carbon and a greater number of cells, and I obtained, under these altered circumstances, an extremely interesting result.

My carbon was in the form of an arch (this diagram will help my explanation), about one inch high and quarter of an inch wide. The ends of the arch were held in small clamps, with square blocks of carbon.

The air-pump having been worked, I had the pleasure of seeing that when contact with the battery of 40 or 50 cells was completed my carbonised paper-arch became red-hot, and it was evident that nothing more was wanted than a still stronger current to make it give out a brilliant light; but I had used up all the battery power at my disposal, and having reached this limit I contented myself with watching the behaviour of the arch, the engrossing question being—How long will it endure?

I noticed that the inner part of the arch was hotter than the outer part, and that—perhaps in consequence of this—the arch became bent on one side. This bending gradually increased, until at last the arch had so far curled down that the top was on a level with the clamps, and on coming in contact with the sole of the lamp it broke in two, and the experiment collapsed.

That, I confidently believe, was the very first instance in which carbonised paper was ever used in the construction of an incandescent electric lamp.

I am now speaking of twenty years ago, and at that time the Voltaic battery was the cheapest source of electricity known, and the means of producing high vacua were very much less perfect than they are now.

I laid my electric light experiments aside until about three years ago, when two things concurred to lead me to pursue the object afresh.

The discovery of the dynamo-electric machine had entirely altered the position of the question of electric lighting, shifting it out of the region of things scientifically interesting into that of things practically useful.

The Sprengel air-pump, too, had been invented, and with its invention we had been provided with a means of producing much higher vacua than could be produced by the old form of air-pump.

Mr. Crookes's radiometer experiments had shown us what a really high vacuum was, and how to produce it.

Mr. Stearn, of Birkenhead, an ardent scientific amateur, was so attracted by the extraordinary results Mr. Crookes had obtained by means of high vacua as to go with great enthusiasm into the same line of experiment, and he soon acquired such a knowledge of the Sprengel pump, and such expertness in its manipulation, as perhaps was only equalled by Mr. Crookes himself.

I had the good fortune to make Mr. Stearn's acquaintance, and that was the other one of the determining causes of my second attempt to solve the problem of electric lighting by the incandescence of carbon.

In the interval between the first and second periods I have mentioned, many attempts had been made by various experimenters to render practicable incandescent carbon lamps, but none were entirely successful.

Here is represented a variety of the most notable of these attempts. Some are vacuum lamps, and some have air admitted. Sawyer and Mann's lamp is filled with nitrogen. When the incandescent carbon is in air it burns away, and must consequently be renewed just as a candle must be renewed; it must also be thicker than would be necessary in a vacuum, and being thicker it requires a proportionally greater current to render it incandescent: both these circumstances are obviously against economy.

The André lamp is one of the best of this type. In this the supply of air is limited; still this lamp and all those lamps are lacking in simplicity and in economy.

In all the various attempts to utilise the principle of the incandescence of carbon in vacuo, two great difficulties had stood in the way, and baffled every attempt to overcome them. One was *the rapid wearing away, and consequent breaking, of the incandescent carbon*; and the other *the obscuration of the lamp by a kind of black smoke*. So uniformly did these phenomena present themselves that the idea was propounded and generally accepted that the blackening of the lamp-globes was due to volatilisation of the carbon under the action of the enormous heat to which it was subjected.

In Fontaine's work on electric light this passage occurs at page 180:—"Attentive examination of incandescent carbons, through a strongly coloured glass, has shown that they are not uniformly brilliant. They present obscure spots indicative of non-homogeneity and the position of cracks, which rapidly disintegrate the carbon. The vacuum never being perfect in the receivers, the first carbon is in greater part consumed. It would appear that, consequently upon the little oxygen contained in the lamp being transformed into carbonic acid and carbonic oxide, the carbon should be preserved indefinitely. But there is *then* produced a kind of *evaporation* which continues to slowly destroy the incandescent rods. This evaporation is, besides, *clearly proved* by *a pulverulent deposit of sublimed carbon, that we have found on the interior surface of the bells, on the several interior parts, rods, contacts, hammers, &c.*"

If this idea of the volatilisation of carbon were founded in fact, any further attempt to render incandescent carbon lamps *durable* by means of a vacuum were *mere waste of time*, and durable they *must* be if they were to be of any practical value.

Fortunately I did not accept as conclusive the experiments which seemed to show that carbon was volatile, and that the blackening of globes of incandescent carbon lamps was an inevitable result of the carbon being very highly heated. I know that the conditions under which, without exception, all previous experiments had been tried were such as did not allow to be formed anything approaching a perfect vacuum within the lamp. Screw fittings had invariably been employed to close the mouth of the lamp, and the ordinary air-pump to exhaust the air. Under such circumstances it was certain that a considerable residuum of air would be contained within it, and also that it would leak. Then, there had never been any thought given to the gas occluded in the carbon itself, and which, when the carbon became hot by the passage of the current through it, would be evolved; nor had sufficient care been taken *to make the resistance, at the points of fixture of the carbon, less than in the carbon to be heated to incandescence.*

It was evident to me that before any definite conclusion could be arrived at as to the question of the volatility of carbon, the cause of the blackening of the globes, and the wearing away of the incandescent rods, *we must first try the experiment of heating the carbon to a state of extreme incandescence in a thoroughly good vacuum* (such as Mr. Crookes had taught us how to procure), *and under more favourable conditions as to the contact between the incandescent carbon and the conductors supporting it than had hitherto obtained.*

Accordingly, in October, 1877, I sent to Mr. Stearn a number of carbons, made from carbonised cardboard, with the request that he would get them mounted for me in glass globes by a glassblower, and then exhaust the air as completely as possible. This delicate operation Mr. Stearn very kindly undertook and very skilfully carried out.

In order to produce a good vacuum it was found necessary to heat the carbon to a very high degree by means of the electric current *during the process of exhaustion*, so as expel the gas occluded by the carbon in its cold state, for otherwise, however good the vacuum was before the carbon was heated, immediately the current passed and made it white-hot, the vacuum was destroyed by the outrush of the gas pent up in the carbon in its cold state. In order to make a good contact between the carbon and the clips supporting it, the ends of the carbon were thickened, and, in some of the early experiments, electrotyping and hard soldering of the ends of the carbons to platinum was resorted to.

I will not weary you, however, with details, but simply say that the prescribed conditions having been rigorously complied with, it was found, after many troublesome experiments, that *when the vacuum within the lamp globe was good, and the contact between the carbon and the conductor which supported it sufficient, there was no blackening of the globes, and no appreciable wasting away of the carbons.*

Thus was swept away a pernicious error, which, like a lying finger-post, proclaiming "No road this way," tended to bar progress along a good thoroughfare.

It only remained to perfect the details of the lamp, to find the best material from which to form the carbon, and to fix this material in the lamp in the best manner. These points, I think, I have now satisfactorily settled; and you see the result in the lamp before me on the table.

It is a very modest looking affair, but its performance goes beyond its appearance. The carbon is extremely thin—a mere hair,—and how wonderfully strong and elastic it is I will endeavour to show you by means of the lantern.

This carbon unlike the carbon spoken of by Fontaine, in the extract I read to you, is quite homogeneous and almost flinty in hardness, and it becomes harder by use in the lamp; the longer and the hotter it is heated, the harder it becomes, What degree of hardness it will ultimately arrive at is an interesting question.

Here is a magnified view of the carbon ring in a state of incandescence; observe how absolutely uniform in brightness it is; that proves it to be homogeneous and foretells its durability.

Now I will show you how easily lamps of this kind are lighted, and how completely this form of electric light can be divided and distributed.

Is it not a pleasant light? It is not so white as the arc light, but yet a whiter light than gas. Colours are correctly seen by it, as this picture shows. But the great merit of this light consists in its not being in contact with air, and therefore there cannot possibly be the slightest air pollution caused by it. The rooms in which this light is used will be as pure by night as by day.

It is essential to economy in lighting by incandescence that the incandescent carbon should be very thin. The carbon I use is not one-twentieth the thickness of the thinnest of the carbons formerly employed, and therefore one-twentieth of the current, costing one-twentieth the price, will produce in my thin carbon the same degree of luminosity as twenty times more current will produce in such carbons as were used in those ancient lamps.

You will notice that in my lamp leakage is very thoroughly guarded against. The wire which passes through the glass not only having the glass fused around it where the wire and globe meet— but in addition to this, the wire is coated with glass almost up to the carbon. In this way the vacuum is preserved very effectually.

You have, of course, all heard that after Mr. Edison abandoned his platinum lamp as impracticable, he invented a new lamp in which carbonised cardboard is used.

Here is a diagram of Mr. Edison's carbon lamp, with its horse-shoe of carbonised paper. It is in some respects like mine, but latterly I have given up the use of carbonised cardboard, and am now using a material as much better than carbonised cardboard as carbonised cardboard was better than the material previously used. In an article which appeared in the February number of *Scribner's Magazine*, authenticated by a letter from Mr. Edison in the same publication, it is stated that Mr. Edison was the first to use carbonised paper; that is incorrect. And this also occurs after a description of the Sprengel pump used in exhausting these lamps:—"Mr. Edison's use of carbon in such a vacuum is entirely new." Now I daresay there are many here who will remember this little lamp, which I showed here two years ago in action. This lamp has exactly the same simplicity as my present lamp, being composed entirely of three substances, namely, glass, platinum, and carbon, and it was exhausted in precisely the same manner, and to the same degree, as that which Mr. Upton—no doubt in good faith but still in error—speaks of as "*entirely new*."

I do not mention these things in any way to disparage Mr. Edison, for no one can esteem more highly his inventive genius than I do. I merely state these facts because I think it is right to do so in my own interest, and in the interest of true history.

The complete seclusion of the light in this lamp from contact with air suggests its adaptability to coal mine illumination, and I earnestly hope that this may prove to be one of its uses.

But the great purpose to which a lamp of this kind is applicable is the lighting of your houses. In view of such an application—two all-important questions present themselves:—one, as to distribution, another, as to cost.

Can this light be divided, distributed, and measured as gas is divided, distributed, and measured? And at what cost? It is quite impossible in a brief lecture to discuss these questions exhaustively, but as far as is possible, in a few words I will answer them.

1st. Then, as to division and distribution, it has been asserted on very high authority that great loss necessarily attends the division of the electric light. To a certain extent this is true of lighting by the electric arc, but it is totally and absolutely erroneous of lighting by incandescence. *There is no less in dividing the electric light produced by this means.* Faraday has stated the law of the case in these words: "An electric current which will heat one inch of wire *white hot*, will also heat to the same temperature 100 inches, or an infinite length of the same wire." There is no question of the truth of this. Now, as it is only necessary, in order to maintain a given current, to increase the force which produces it in the same proportion as you increase the resistance to its flow, it follows that the cost of raising to a certain degree of incandescence a longer or shorter length of carbon or of maintaining a 10 candle-light or 100 candle-light will be exactly proportional to the light produced. You may even contemplate on this principle the economical production of an electric light as small as a rush-light. A certain unit of light may be established in an indefinite number of places, with no greater aggregate expenditure of power than that directly and simply proportional to the number of light.

With regard to distribution, I believe that it will prove to be practicable to light any large town—all Newcastle, for instance—by means of wires laid in the ground as gas-pipes are laid, and all branching from one centre, and conveying the electric current to lamps like this.

The lamps now lighted are supplied by a current coming from generators working at the far end of Mosley Street (a quarter of a mile away), and it would be just as easy by using a more energetic current—a current, as it were, under higher pressure—to maintain these several miles away, and for this purpose the conductors need not be large, not so large certainly as to make the distribution of electric current more costly than the distribution of gas.

For supplying large towns with electric light, Mr. Edison proposes to have a number of centres for the supply of electric power, perhaps a quarter of a mile apart, whence wires would be sent out in every different direction, distributing the current to the houses round about. His plan of distribution is this. He proposes to send out bundles of main wires from each of the centres of supply, and from these main wires to branch as many small wires into the houses as there are lamps to be lighted, each branch wire proceeding from a main wire to the place where the lamp is situated, and from thence to a return main wire.

Now although this plan has the great merit of simplicity, I do not think it will answer, except for very short distances.

When a number of lamps are grouped together in that manner, it is necessary that the individual lamps should offer a very high resistance to the current, for if each lamp does not offer an extremely high resistance to the passage of the current there must be great waste, a large proportion of energy being in that case spent in heating the conducting wires, instead of the carbon in the lamps.

Mr. Edison accordingly proposes to make his lamps of a very high resistance. He proposes to use for the incandescent material a form of carbon which offers a higher resistance than simple carbon in its compact state; but if carbon pure and simple is used, then I submit it had better be in as stable and condensed a state as possible, because in process of use it tends to consolidate, and it is undesirable that any change should take place in the lamp during use.

The resistance offered by a filament of carbon in its best state for incandescent lamps, as thin as it is safe to use in a lamp, and of a length sufficient to give, say, a light equal to one burner, or 10 standard candles (a unit of light which I think we must not go beyond in planning an extensive system of town lighting), will not offer so high a resistance as that which Mr. Edison has made the basis of his scheme of distribution.

With lamps of this resistance, the result would be that before many were bridged across from one main wire to another, as much or more work would be done in the conducting-wire as in the lamp. The only way of avoiding this waste of energy, without abandoning the idea of small units of light, would be either to employ enormously thick conductors, or have a very limited area supplied from one works.

I think the difficulty is capable of being surmounted in this way:—Instead of grouping the lamps as Mr. Edison proposes, each lamp being as it were a loop or bridge between two mains, I propose to string them in series—10, 50, or perhaps 100 lamps being all interposed in one and the same line. In this way every lamp would *add* to the resistance of the line, instead of, as in Mr. Edison's plan, *diminishing* its resistance. The waste of energy in the conducting wire would thus be avoided.

A copper wire, less than one-eighth of an inch thick, would supply current for one such series of (say) from 10 to 100 lamps, at 5 miles distance, with a very small percentage of loss; while to supply at the same distance a corresponding series on Mr. Edison's plan would demand copper conductors of such thickness as would certainly make the plan far too expensive; or if such thick conductor was not used, there would be an impracticably extravagant waste of energy in the wire. If even 50 per cent of the energy were expended in the wire, the size of the conductor required to transmit the current, say even 2 miles, would be far too great.

There is no way of escape that I know of from this dilemma, viz., that either we must make our unit of light larger than necessary for a very great many purposes, and so give up the idea of extensive division and extensive distribution, or, in order to gain these points, we must group the lamps in the manner I have proposed.

There are, no doubt, difficulties in the carrying out of my plan, but none that are not easily surmountable. For example, if 20, 50, or 100 lights were in a series, a break in any part of the line would extinguish all the lights. This danger can be met in two ways:—

I would have only one lamp belonging to a given line in one house, so that the extinction of such a line of lights as we are contemplating would not be a very serious mishap; but I would make such a mishap extremely unlikely to occur, by placing along with each lamp an automatic circuit closer. This would so act as to bridge over the gap made by the accidental breaking or failure of a lamp, and so prevent the extinction of the rest of the lamps in the series, while a fresh lamp was put in the place of the broken one,—a thing no more difficult, and probably not more costly, than the replacement of a broken gas-burner chimney or globe.

There is another difficulty occasioned by the variation of the current in proportion to the number of lamps in action.

What is required in this case is to maintain a uniform current in the line of lamps, whether 1 or 100 are alight. This can be accomplished by self-acting apparatus somewhat on the principle of the governor of the steam-engine, and which would automatically raise or lower the potential or pressure by steps of 100ths, according to the number of lamps in use.

I have also considered the question of measuring the current, and, if time allowed, I could show you that that could be done as easily as the measuring of gas.

Similarly, all other practical difficulties arising out of this method of distribution can be met, and being met we are at liberty to contemplate a great central works producing electricity by large steam-engines, and distributing it by means of wires to a whole town, exactly as gas is now distributed by gas-works.

I have already referred to the cost of electric light produced on the arc principle, and shown that when the circumstances are favourable to the employment of that method it is much more economical than gas-light.

The economy of lighting by incandescence has not been exemplified by so many instances of actual practical use. One thing is, however, *quite clear*, and that is, that *electric lighting by incandescence is an economical process*—it will be less costly than gas-lighting. That is conclusively demonstrated by the fact that 1000 feet of gas employed in working a gas-engine to develop an electric current, and used in my lamps, will yield more light than 1000 feet of gas consumed in the ordinary way in gas-burners. This room is now lighted by twenty of my electric lamps, and to produce the current which feeds them 160 cubic feet of gas per hour are being burnt in a gas-engine: before my lamps were kindled the room was lighted by seventy gas jets, consuming, I am told on good authority, 210 feet per hour. It is very evident that we have got more light out of the gas through the medium of electricity than was got from the larger quantity of gas which those burners consumed. Our conditions here are somewhat unfavourable to my light for a fair comparison, but from measurements carefully made—both of light produced and current required to produce it—I am warranted in saying that at least twice as much light will be produced by a certain quantity of gas used to generate an electric current employed in my lamps, than would be obtained from this quantity of gas burnt in gas-burners in the usual manner.

If that is so, then it is evident that when, instead of the motive power of gas, that of steam produced in the most economical manner is employed, this method of electric lighting will be very much less costly than gas lighting. I reckon that 40 lbs. of coal employed in raising steam to generate electricity is capable of producing in my lamps the effect of 1000 feet of gas burnt in gas-burners in the ordinary manner.

The economical part of the question is therefore, in my opinion, very favourable to electric lighting, and I think fully warrants me in anticipating an extensive substitution of electric light for gas light.

The great difficulty which till now has completely blocked the way to any general use of electric light was *the difficulty of division—that difficulty is now completely overcome*—by the method of producing electric light by the incandescence of carbon *in vacuo*, of which I have given you a practical demonstration to-night.

Now, ladies and gentlemen, if I have not exhausted my subject I certainly have exhausted your patience. I will weary you no more.

Eighty years ago Science gave us enlarged means of turning night into day. Since then not a little of our lifetime has been spent in gas-lit rooms, and it has been somewhat of a reproach to Science that she has not provided us with this larger measure of light without at the same time imposing on us the necessity of breathing a vitiated atmosphere.

To-day Science vindicates herself: henceforth we may make the long nights of our northern winter bright without any such sacrifice.

NOTICES OF BOOKS.

The Electric Light for Industrial Uses. By R. E. Crompton. London: Mansion House Buildings.

We have here an account of the various systems of electric lighting, with their respective advantages and defects, the area illuminated by each light, and the hourly cost. For estimating illuminating power he adopts the three standards proposed by Mr. Preece. In the first, any fine work may be carried on which usually requires a gas-jet at a distance of 2 or 3 feet. In the second, a newspaper can be read with comfort, and the third is rather better lighted than by a clear full moon, so that excavating, levelling, loading waggons, &c., can be carried on. A table is given showing how many square yards can be lighted up to each standard per lb. of coal burnt. It appears that the A Gramme machine lighted up 33 square yards to the first standard, 166 to the second, and 850 to the third. A B Gramme lighted 18 yards to the first, 100 to the second, and 4000 to the third. The different colours of the light produced —a point of capital importance in dye- or print-works— are incidentally mentioned. The single lights burned on the low tension currents of Siemens, Gramme, or Bürgin are said to be very white and pure, colours seen by the light being exceedingly true to sunlight. If the tension is increased and the quantity of the current diminished, several lights may be burnt in series, but the rays are often coloured purple, blue, or greenish, and the light is flickering and unsteady. A Gramme machine with a resistance of 1 ohm gives a current suitable for producing a stable arc ¼ inch long with a very pure white light. This arrangement appears to be recommended where colour and steadiness are important.

The Brush machines are described as capable of maintaining 16 arcs in series, but they are deficient as regards colour and steadiness. The penetrating power of the lights in foggy weather is not satisfactory.

Important information is given as to the choice of carbons. Those of Carre and Siemens are apt to give a colour to the light. Those of the Brush Company give an extremely white light, but the Company will not supply them for use in other systems. The best were produced by Gaudoin, but his secret died with him. Where motive power has not to be specially obtained, the cost of the A Gramme light and of the D^6 Siemens is given at 6d. per hour as a fair average. The B Gramme and D^2 Siemens average 10½d.

We hope that this pamphlet will subserve the extension of electric lighting.

Report of the Explosives Committee appointed jointly by the Royal Cornwall Polytechnic Society, the Miners' Association of Cornwall and Devon, and the Mining Institute of Cornwall. Truro: Lake and Lake.

The object of the Committee was to examine the nature, economy, efficiency, and safety of explosives, confining the enquiry to such as are in actual use or likely to be used in the mines of Devon and Cornwall. These appear to be gunpowder, Espir's explosive powder, gun-cotton, tonite, titanite, dynamite, blasting-gelatin, Liverpool cotton-powder. Concerning the composition of these agents the following information is given:—The gunpowder consists of charcoal 15 per cent, sulphur 15 per cent, and nitre 70 per cent. An especially strong gunpowder is also mentioned, the composition of which is not stated. Espir's powder consists approximately of 60 per cent nitrate of soda, 14 of sulphur, and 26 of hard wood sawdust. The gun-cotton is of the ordinary nature, but its percentage of trinitro-cellulose is not stated. Tonite is said to be gun-cotton combined, or rather mixed, with barium nitrate. Concerning the product which bears the misleading name "titanite" no information was obtained. Two kinds of dynamite were examined, No. 1 consisting of 75 per cent nitro-glycerin and 24 per cent "kieselguhr," whilst No. 2 contains less nitro-glycerin, and varying proportions of charcoal and a nitrate. "Blasting-gelatin," which is misnamed, as it contains no gelatin, is made of 7 to 10 parts collodion-cotton mixed with 90 to 93 parts of purified nitro-glycerin. Lastly, the Liverpool cotton-powder is said to be a mixture of gun-cotton with potassium nitrate.

From lack of funds the Committee made only two series of experiments to try the relative efficiency and economy of the explosives as used in hard ground and in driving levels for successive periods of not less than a week. In these series gun-cotton, titanite, and blasting-gelatin were not tried. In the first experiments the cost per foot of hole with common powder was ¾d., with compressed powder and Espir's powder 1d. each, Liverpool cotton-powder 2d., tonite 2½d., special strong powder 3d., and dynamite 3¼d. In the second series the total cost per foot ranged from 5d. to 1½d. with compressed powder to 8d. to 6¾d. with tonite.

The remaining labours of the Committee consisted in sending enquiries as to the respective merits and drawbacks of the different explosives, and in receiving replies, which are in many cases very conflicting. As to the relative cost of explosives for doing an equal amount of work, the majority of the answers seem to have been in favour of dynamite. One consumer said that he had known gun-cotton lose one-third of its strength when kept in a dry magazine, a result not easily intelligible. All the authorities except two agreed that the fumes from dynamite are the most injurious. The Committee point out that a serious accident—*i.e.*, that at Halkyn Level, April 10, 1878—was due to the published statement that "in close workings the absence of smoke or noxious gases renders the use of gun-cotton doubly valuable." A similar impression prevails as regards dynamite.

According to the official returns of H.M.'s Inspectors of Explosives, here quoted, the year 1878 was marked by 43 accidents, of which 4 are due to tonite, 2 to gun-cotton, 16 to dynamite, and 21 to gunpowder. These figures obviously throw no light on the relative danger.

It is much to be regretted that the Committee was not provided with the means to make an exhaustive series of experiments on all the explosive compounds and mixtures now offered for sale in England.

Fourth Annual Report of the Proceedings of the Aberdeenshire Agricultural Association. Season 1879—80.

Mr. Jamieson, F.C.S., Chemist to the Association, communicates in his report certain very interesting results. Thus, phosphates of lime "pure simple," differ little in their efficacy whether they are of animal or mineral nature, ground coprolite being substantially equal as a turnip-manure to ground bone-ash. A mixture of such ground coprolite with about one-third its weight of steamed bone-dust may, the author considers, be used with economy and advantage, though acting more slowly than certain other phosphates.

A second conclusion is that soluble phosphate is not superior to insoluble phosphate to the extent commonly supposed. Mr. Jamieson does not mean to say that soluble phosphate is hurtful or needless. He recommends it in small proportions along with the insoluble, but he estimates its superiority at nearer 10 than 100 per cent. Dr. Aitken, the chemist to the Highland and Agricultural Society, comes to a very similar conclusion. Thus the crop from a plot of land manured with insoluble phosphate weighed 19 tons 13 cwts. Similar plots dressed with dissolved coprolite yielded 22 tons 3 cwts., with dissolved Curaçoa phosphate 20 tons 17 cwts., and with dissolved bone-ash only 19 tons 7 cwts. Thus the average result of the dissolved over the undissolved phosphate is only 6 per cent.

The experiments of Dr. Voelcker, undertaken to test these views, are far from sustaining the conclusion expressed by the learned doctor that:—"Mineral phosphates scarcely produce a visible effect on vegetation." It is scarcely needful to say that if the Aberdeenshire results are found to be corroborated by further trials on other soils and under other climates, and as regards other crops besides turnips, both the manufacture of artificial manures and their analytical valuation must be greatly modified. The following, *e.g.*, is the scale of values adopted by the Kirriemuir Agricultural Association. The phosphate in minerals, or in mineral superphosphate, is valued, if insoluble, at 0 per unit, but if dissolved at 3s. 3d. In animal superphosphate the values are respectively 1s. and 3s. 7d, and in dissolved bones 2s. 9d. and 4s.

If soluble phosphate is found in practice to exceed the insoluble in value only by 6, 7, or 8 per cent, and has to be sold accordingly, it will scarcely be worth preparing. The author finds, further, that the phosphates of alumina and iron exerted little or no influence upon the crops. In the use of insoluble phosphates "it is of course essential that they should be applied in a fine state of division." Bones ground to ½, ¼, or even ⅛ inch cannot be fairly compared with superphosphate.

CORRESPONDENCE.

THE SPECIFIC ROTATORY POWER OF CANE- AND INVERT-SUGAR.

To the Editor of the Chemical News.

Sir,—Mr. Allen has done well to call attention to the discrepancies and confusion in the statements relating to the specific rotatory power of cane- and invert-sugar. I have, in my practice, occasion to polarise a large number of samples of sugar; during the last two years I have done several thousand analyses of this kind, but nearly always by the direct method. I have repeatedly tried to obtain results by the method of inversion (according to the specific rotatory power = −25), but in no instance have I been able to obtain correct results, although every precaution was used in the experiments. I at last gave up in despair, not knowing what to think, because the statements given in the text-books, and in special papers, on good authority, are so clear and definite. But Mr. Allen's paper brought up the matter again in my mind, and influenced me to make the experiments that are given below. I think it will easily be made clear that there is a thread of confusion running through much that has been written on this subject. Mr. Allen says in his paper:—"According to the observations generally accepted, the angular rotation produced on a ray of polarised light by a plate of quartz 1 millimetre in thickness is 24 angular degrees for the mean yellow ray or transition tint." If this is the true rotatory power of quartz, and if 12·26 grms. of sugar dissolved to 100 c.c. in water have a rotatory power equal to the plate of quartz when observed in a column 2 decimetres long, then the 16·26 grms. of sugar rotate through 24 angular degrees, and calculations based upon this assumption will give true results. But, as Mr. Allen states at the end of his paper,—"Broch has found the deviation produced by a thickness of 1 m.m. of quartz equal to 21·67 for the sodium ray, while Gerard and De Luynes take the same value as 21·8. Brown and Heron adopt the number 21·54. Tollens gives the number 66·5 as the value of S_D for cane-sugar solutions containing 10 per cent and upwards, a value which corresponds to 21·63. The mean of these four determinations is 21·66." I have made one experiment with pure sugar, and found the value to be 21·62, which agrees closely with the results quoted. The practice in graduating polariscopes seems to be this:—The maker takes a plate of quartz 1 m.m. thick, or a solution of pure sugar, and marks the point to which this rotates 100, and divides the intermediate space into 100 parts. This is a very different thing to taking 24 angular degrees and sub-dividing them into 100 parts, that is, if the quartz or sugar does not rotate through 24 degrees, but the same thing if it does.

I have had no opportunity of reading Tuschmidt's papers, but I have looked through that of Casamajor, and I think I am right in saying that he makes no reference to angular degrees, but only to the empirical degrees of Soleil, probably because his instrument was not graduated in anything but sugar percentages. But when we come to calculate the specific rotatory power of sugar, it is necessary to insert the angular degrees, and then the confusion comes in, because in most instances the number 24 has been used, which is, as we have seen, too high. The values for the specific rotatory power of cane- and invert-sugar must be multiplied by 0·90 (as Mr. Allen says) to give the correct results; this brings the value of cane-sugar to very near 66·45, instead of 73·8.

I have written on this point because of late, no doubt owing to these errors the practice of inverting sugars has much diminished, which is to be regretted. Although the graduation of Soleil's instrument does not depend upon angular degrees as far as the right hand portion of the scale goes, I am of opinion that in some instruments the left handed part of the scale does so depend, because, with a Soleil polariscope I had, the results were always considerably too low when the inversion method was employed. The following are the results of my experiments. The sugar used was a very fine sample of white candy, dried at 100° C.

(1.) 16·26 grms. dissolved to 100° C. without inversion gave +21°37′, +21°37′, +21°38′. This corresponds to 21·62°, and to a specific rotatory power 66·45.

(2.) 10 grms. dissolved to 100° C., after inversion with 10 c.c. of strong hydrochloric acid at 68·72 (boiling gives no higher results), gave—

$$-4^{\circ}49' \text{ at } 11^{\circ}\text{ C. } (=S_D - 22{\cdot}88)$$
$$-4^{\circ}43' \text{ at } 13{\cdot}5^{\circ}\text{ C. } (=S_D - 22{\cdot}44)$$
$$-4^{\circ}45' \text{ at } 13^{\circ}\text{ C. } (=S_D - 22{\cdot}56)$$

(3.) Another experiment gave—

$$-4^{\circ}46' \text{ at } 11{\cdot}5^{\circ}\text{ C. } (=S_D - 22{\cdot}64)$$
$$-4^{\circ}45' \text{ at } 12{\cdot}5^{\circ}\text{ C. } (=S_D - 22{\cdot}56)$$
$$\left.\begin{array}{l}-4^{\circ}40' \\ -4^{\circ}41'\end{array}\right\} \text{ at } 14^{\circ}\text{ C. } (=S_D - 22{\cdot}20)$$
$$\left.\begin{array}{l}-4^{\circ}36' \\ -4^{\circ}37'\end{array}\right\} \text{ at } 15^{\circ}\text{ C. } (=S_D - 21{\cdot}89)$$
$$\left.\begin{array}{l}-4^{\circ}33' \\ -4^{\circ}34'\end{array}\right\} \text{ at } 16^{\circ}\text{ C. } (=S_D - 21{\cdot}69)$$

(4.) In another experiment the results were—

$$\left.\begin{array}{l}-4^{\circ}40' \\ -4^{\circ}41'\end{array}\right\} \text{ at } 14^{\circ}\text{ C. } (=S_D - 22{\cdot}18)$$
$$-4^{\circ}39' \text{ at } 15^{\circ}\text{ C. } (=S_D - 22{\cdot}09)$$
$$\left.\begin{array}{l}-4^{\circ}35' \\ -4^{\circ}35'\end{array}\right\} \text{ at } 16^{\circ}\text{ C. } (=S_D - 21{\cdot}77)$$

The experiments were conducted with the usual precautions. A special tube surrounded by a water jacket was used to control the temperature. The polariscope employed was one of Schmidt and Hencke's direct rotation instruments, graduated in degrees as well as sugar percentages, and constructed for use with the sodium flame. These instruments are better, cheaper, and more accurate than the colour instruments, and are, unlike the latter, adapted for use with other substances than cane sugar. The results agree fairly well with those of Casamajor and Tuschmidtt when the correction is made for the angular degrees.—I am, &c.,

Thomas Bayley.

REPLY TO MR. WELDON.

To the Editor of the Chemical News.

Sir,—I take the liberty of requesting you to insert in the correspondence portion of your esteemed paper the notice that, after the approaching conclusion of my experiments, a reply will be given to the attacks of Mr. Weldon. I would further remark that it is not my intention to claim or to make use of the columns of your journal for a controversial expression.—I am, &c.,

JULIUS POST.

SOCIETY OF INDUSTRIAL CHEMISTS.

To the Editor of the Chemical News.

Sir,—Some six months ago I observed that you had a notice of a proposed Society of Industrial Chemists, but since that time I have been unable to learn that any further steps have been taken for the starting of the society. It appears to me, as a manufacturing chemist, that some such society is greatly needed, and I trust that the project has not been given up. I think that if started on a proper footing, it would do a great deal of good to the profession, and that it would command the support of the great proportion of chemists engaged in industrial pursuits.—I am, &c.,

NORTHERN CHEMIST.

Edinburgh, October 30, 1880.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

NOTE.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 15, October 11, 1880.

The Part Played by Time in the Formation of Salts.—M. Berthelot.—The part played by time in chemical reactions was formerly overlooked, or ascribed to the want of contact and of homogeneity, and in any case regarded as of little importance. It was especially brought under the notice of chemists in consequence of the author's researches (1854) on the synthesis of neutral fatty bodies and of polyatomic ethers, resulting from the union of acids and saccharine principles, compounds formed simply by the prolonged contact of the reacting bodies. These and subsequent researches show the distinction which exists between ethereal reactions, slow and progressive, even in homogeneous systems, and the saline reactions, the duration of which is so short in the majority of cases that it escapes our present means of measurement. Whenever an acid dissolved in water is caused to act upon a dissolved base or salt, or a dissolved base upon a dissolved acid or salt, or two dissolved salts the one upon the other, whenever the resulting products are equally soluble and form a homogeneous system, the reaction does not in general require for its completion any appreciable interval of time further than is necessary to effect the exact mixture of the two liquids. The author shows how slow chemical changes may be detected and measured by thermic methods. The chemical equilibrium so rapidly established in dissolved saline systems seems correlative with the electrolytic conductibility which characterises such systems.

Photophonic Experiments of Prof. A. G. Bell and M. Sumner Tainter.—A. Breguet.—This paper cannot be reproduced without the accompanying illustration. The question arises whether it is light or heat which is brought into play. M. Bell has placed before the pencil of rays a solution of alum and then a solution of iodine. In the former case the sonorous effects were little reduced in intensity, whilst in the latter scarcely any sound was heard.

Mechanical Actions of Light; Theoretical Consideration Serving to Interpret the Experiments of M. Graham Bell.—C. Cros.—Since light in passing from one medium to another of different density (separated from each other by a surface oblique to the direction of the rays), undergoes a deflection, we may infer that if the medium acts upon it, it must in turn act upon the medium. The author therefore affirms that:—(1.) Light tends to assimilate the density of the medium which it traverses to that of the medium whence it has issued. (2.) It tends to displace the transparent body in an opposite direction to the deviation which it undergoes. (3.) In case of reflection the reflecting body undergoes repulsion.

Distribution of Light in the Solar Spectrum.—J. Macé and W. Nicati.—In all cases the maximum intensity is in the yellow at a point very near to the ray D, in conformity with the generally received opinion. The intensity decreases very rapidly from this point on either hand, and becomes very feeble in the blue. The perception of the blue and the violet diminishes much more slowly with the decrease of illumination than does that of the less refrangible colours. From the extreme red to the green of the wave-length $0^{\mu}.5$, the law of the distribution of intensity remains absolutely the same, whatever may be the illumination. Even between eyes equally capable of distinguishing colours there are very sensible differences.

Vibratory Forms of the Circular Pellicles of Saposaccharic Liquid.—C. Decharme.—For the same diameter of a pellicle the numbers of nodals are inversely proportional to the corresponding lengths of the vibrating rod.

Place of Boron in the Series of Simple Bodies.—A. Etard.—Since boron was excluded from the carbon family by the researches of MM. Wœhler and Ste.-Claire Deville, it has not found its place in the classification of the elements. The author, founding his opinion on the atomicity of boron, which passes from 3 to 5 in the compounds BCl_3 and $BOCl_3$, assigns it to the phosphorus group. There will thus be the series N, P, As, Sb, B, &c., and B, Va, Nb, Ta, &c.

Propyl-acetal and Iso-butyl-acetal.—J. de Girard.—The author has obtained and analysed these two compounds.

Bulletin de la Société Chimique de Paris, No. 9, June 5, 1880.

Russian Chemical Society, Session of May 3/15, 1879.—M. Tougolessoff showed that the hydrocarbide $C_{10}H_{16}$ derived from diamylene is not identical with terebene.

MM. Boutlerow and Wischegradsky by causing alkalies to act upon cinchonine have obtained quinoline and a solid, which if again treated with acids is resolved into several acids and a volatile alkali.

M. Wischnegradsky described the results of his researches on the constitution of quinoline.

A paper was communicated on behalf of M. Krestownikoff, on the aldehyd of β-chloro-propionic acid.

M. Maretnikoff brought before the Society a memoir on β-chloro-butylic aldehyd.

MM. Narkownikoff and Krestownikoff communicated a paper on homo-itaconic acid.

M. O. Miller gave an account of the products of the dry distillation of calcium phthalate.

M. L. Chickoff described the result of his experiments on the constitution of milk. He considers it as an emulsion of fat in a special liquid formed of a mixture of albuminoid matters, sugar, &c. The emulsion requires the presence of a certain quantity of free fatty acids.

M. Tawildaroff has not succeeded in forming triethylglycerin by heating acrolein with absolute alcohol in presence of acetic acid.

MM. Beilstein and Jawein describe a new method of separating manganese and iron, based on the fact that, in the cold, iodine throws down all the manganese as peroxide from a solution of the two metals in an excess of potassium cyanide.

M. Wroblewsky proposes to separate orthoxylen from its isomers by means of its acetylic derivatives.

M. Maltschewsky read a note on the preparation and properties of aniline dithionate.

M. Sokoloff communicated analyses of the water of the Oka and of the springs which supply the town of Nijny Novgorod.

M. Tchelzoff sent a note on the determination of nitrogen in the explosive ethers of nitric acid.

M. Boutlerow described his researches on isotributylen.

MISCELLANEOUS.

Royal Institution of Great Britain.—The Lecture Arrangements (yet incomplete) for the ensuing Season (before Easter) will include the Chrismas Course by Prof. Dewar; and Courses by Profs. Tyndall and Schäfer, the Rev. William Haughton, the Rev. H. R. Haweis, Mr. H. H. Statham, Mr. Reginald S. Poole, and others. Friday Evening Discourses will probably be given by Mr. Warren De La Rue, Professor Tyndall, Sir John Lubbock, Sir William Thomson, Dr. J. Burdon Sanderson, Dr. Andrew Wilson, Dr. Arthur Schuster, Mr. Alexander Buchan, Dr. W. H. Stone, Dr. W. J. Russell.

Society of Arts.—The First Meeting of the One-Hundred-and-Twenty-Seventh Session of the Society is announced for the 17th November, when the Opening Address will be delivered by F. J. Bramwell, F.R.S., Chairman of the Council. Before Christmas there will be four Ordinary Meetings, in addition to the Opening Meeting, at which the following papers will be read:—November 24th—"Barry's Influence on English Art," by J. Comyns Carr. December 1st—"The Photophone," by W. H. Preece. December 8th—"London Fogs," by Dr. A. Carpenter. December 15th—"The Use of Sound for Signals," by E. Price Edwards. The following papers are down on the list for reading after Christmas:—"Buying and Selling: its Nature and its Tools," by Professor Bonamy Price; on this evening Lord Alfred S. Churchill will preside. "The Participation of Labour in the Profits of Enterprise," by Sedley Taylor, M.A., late Fellow of Trinity College, Cambridge. "The Gold Fields of India," by Hyde Clarke. "Flashing Signals for Lighthouses," by Sir William Thomson, F.R.S. "The Present Condition of the Art of Wood-carving in England," by J. Hungerford Pollen. "Ten Years' Experience of the Working of the Trade Mark Act," by E. C. Johnson. "Trade Prospects," by Stephen Bourne. "The Manufacture of Aërated Waters," by T. P. Bruce Warren. "The Compound Air Engine," by Col. F. Beaumont, R.E. "Improvements in the Treament of Esparto for the Manufacture of Paper," by William Arnot, F.C.S. "Deep Sea Investigation, and the Apparatus used in it," by J. G. Buchanan. "The Discrimination and Artistic Use of Precious Stones," by Professor A. H. Church. "Indian Agriculture," by W. R. Robertson. Five Courses of Lectures are announced under the Cantor bequest:—First Course—Five Lectures on "Some Points of Contact between the Scientific and Artistic Aspects of Pottery and Porcelain," by Professor A. C. Church. Second Course—Three Lectures on "Watchmaking," by Edward Rigg, M.A. Third Course—Four Lectures on "The Scientific Principles involved in Electric Lighting," by Prof. W. G. Adams, F.R.S. Fourth Course—Three Lectures on "The Art of Lace-making," by Alan S. Cole. Fifth Course—Three Lectures on "Colour Blindness and its Influence upon Various Industries," by R. Brudenell Carter. The two Juvenile Lectures, for children of Members, during the Christmas Holidays, will be by G. J Romanes, F.R.S., on "Animal Intelligence." The arrangements for the "Indian," "Foreign and Colonial," and "Chemical and Physical" Sections will be announced after Christmas.

MEETINGS FOR THE WEEK.

Monday, 8th.—Royal Academy of Arts, 8. "The Action of Light, Heat, Moisture, and Air upon Oils, Turpentine, and Resins, by Arthur H. Church, M.A.
— Royal Geographical, 8.30.
— Medical, 8.30.

Tuesday, 9th.—Anthropological Institute, 8. "On Anthropological Colour-Phenomena in Belgium and elsewhere," by Dr. J. Beddoe. "On different Stages in the Development of the Art of Music in Prehistoric Times," by Mr. J. F. Rowbotham. "On Neolithic Implements in Russia," by Prince Paul Poutiatin.
— Medical and Chirurgical, 8.30.
— Civil Engineers, 8.
— Photographic, 8.

Wednesday, 10—Microscopical, 8.

Thursday, 11th.—Royal Academy of Arts, 8. "The Changes to which Painting Grounds are Liable," by Atrhur H. Church, M.A.
— Mathematical, 8.

Friday, 12th.—Quekett, 8.
— Clinical, 8.
— Astronomical, 8

Saturday, 13th.—Physical, 3. "On the beats of Mistuned Consonances of the Form $h:1$," by R. H. M. Bosanquet. "Note on Prof. Exner's paper on Contact Electricity," by Profs. Ayrton and Perry "On Action at a Distance," by Walter R Browne.

THE CHEMICAL NEWS.

VOL. XLII. NO. 1094.

THE CONSTITUTION OF AMIDO-, AZO-, AND DIAZO- COMPOUNDS.

By S. E. PHILLIPS.

(ABSTRACT.)

THERE are few departments of organic chemistry which possess a greater interest than the azo-colouring-matters, which have quite recently threatened to become an extended rival to the beautiful coal-tar colours. Their constitution has long been a puzzle, and modern attempts can scarcely be regarded as at all settled or satisfactory. We may note three main lines of thought, as devoted to their interpretation—all endorsing the modern delusion that tri-atomic nitrogen replaces three atoms of hydrogen as a common basis in these endeavours; it is also, and correctly, agreed that the initial or typical reaction is to be found in the action of nitrous acid upon aniline.

(1.) Hofmann's view is that "2 molecules of aniline are linked together, in which 3 atoms of H are replaced by 1 atom of N," yielding azo-diphenyl-diamine (in old notation), $C_{24}H_{14}N_2+HNO_4-4HO=C_{24}H_{11}N_3$.

(2.) P. Griess, regarding this body from a benzene rather than an aniline point of view, considers it to be a diamine combination of diazo-benzene and amido-benzene. That the reaction consists of 2 parts or stages is commonly known, seeing that we may so vary the reaction as to obtain either diazo-benzene by itself, or in combination with its menstrua, amido-benzene.

$$\left.\begin{array}{l}\text{Diazo-benzene} = C_{12}H_1.(NH_2)HN \\ \text{Amido-benzene} = C_{12}H_5 \quad H \quad HN\end{array}\right\} = C_{24}H_{11}N_3.$$

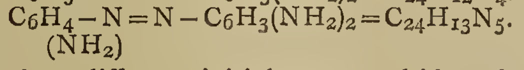
By very many chemists it is called diazo-amido-benzene.

(3.) Dr. O. N. Witt, fully accepting the terms and ideas involved in the 1st reaction by which diazo-benzene is produced, yet strongly regards the 2nd stage as resulting in what he calls amido-azo-benzene.

Starting from the hypothetical body of double atomic weight he has—

$C_{12}H_3,H_2N$ = azo-benzene = (a)
Aniline-yellow = amido-azo-benzene = (b)
Chrysoidine = diamido-azo-benzene = (c)
Phenylene-brown = triamido-azo-benzene = (d)

(a) $C_6H_5-N=N-C_6H_5 = C_{24}H_{10}N_2$.
(b) $C_6H_5-N=N-C_6H_4(NH_2)=C_{24}H_{11}N_3$.
(c) $C_6H_5-N=N-C_6H_3(NH_2)_2=C_{24}H_{12}N_4$.
(d)



$$\underset{(NH_2)}{C_6H_4}-N=N-C_6H_3(NH_2)_2=C_{24}H_{13}N_5.$$

Viewing these different initial terms and ideas, in connection with their interminable consequents in a very extended series of azo-colours, each preserving a certain numerical conformity to the facts of analysis, one may well feel a certain degree of humility in presuming to give another interpretation to such a complex field of chemical research.

It is, however, some consolation to find that upon viewing matters from the new or original point of view of old chemistry, and eschewing all modern hypotheses, that in an eclectic sense we find the views of Griess and Hofmann to be two isomeric sides of the probable truth.

This essay consists of two parts—1st, a wide endeavour to analyse the constituent elements, and to define the uncombined azo- and amido- bodies involved; and 2nd, to estimate upon old principles of ammonia condensation, apart from any new or special hypothesis, the modes of their combination to monamine, diamine, and triamine colouring bases.

Amides are defined as of two kinds—1st, the ordinary amides, so well understood by which hydrates with ammonia lose 2HO, while the *radical* replaces one H, in the ammonia type, reactions so familiar in the production of—

Ethylamide, $C_4H_5, \quad H_2N$
Acetamide, $C_4H_3O_2,H_2N$, &c.

2nd, other amides, whose genesis is not so clearly understood, where the same materials lose 2H instead of 2HO, the radical becoming more oxidised in proportion.

	By 1st Reaction.	By 2nd Reaction.
Phenol gives	$C_{12}H_5,H_2N$	$C_{12}H_5O_2,H_2N$
Acetic acid gives	$C_4H_3O_2,H_2N$	$C_4H_3O_4,H_2N$, &c.

The modern view is that the 2nd series are acid bodies, with amidogen replacing one A of any given acid, and they are all notated with the supposed character of real acids, the notation ending in (COOH).

Acetic acid, $CH_2.H.COOH$
Amido-acetic acid, $CH_2(NH_2).COOH$, &c.

Our aim is to completely refute this interpretation, by a wide survey of the facts of their essentially amide or *basic* characters.

The alleged salts of these so-called acids are often referred to, but it is easy to prove that they are no salts at all, but metal replacements in the amido base. As well might we say that zinc amide is a salt, and that ammonia is its acid. It is painful to note how large a portion of modern chemistry is confused in this manner.

A complete refutation of the modern imido-theory is similarly attempted, and marvellous indeed it is to see how plain and simple facts can be distorted by special or wanton hypothesis. Tabular views of typical amido-bodies are given as thus where $A=(NH_2)$.

Benzene, $C_{12}H_5,H$ (a) – Benzoic acid, $C_{14}H_5O_4,H$ (b)

	Amido.	Diamido.	Triamido.
(a)	$C_{12}H_5H_2N$	$C_{12}H_5AHN$	$C_{12}H_5AAN$
(b)	$C_{14}H_5O_4,H_2N$	$C_{14}H_5O_4,AHN$	$C_{14}H_5O_4,AAN$, &c.

It may be noticed that the ordinary amide of benzoic acid is $C_{14}H_5O_2$. An amide, or amido-body, is in every case a replacement of one H by amidogen.

An azo-body is in every case precisely the same thing, under a more reducing action, by which 2H of the radical are eliminated. This proposition is apparently true in the sense of a universal law, as upon a wide survey we have found no exceptions. Meanwhile a tabular view of some of the typical cases are given in comparison with their amido-analogues, as thus—

	Benzene (a)	Phenol (b)	Benzoic acid (c)
	Amido.	Azo.	Diazo.
(a)	$C_{12}H_5,HN$	$C_{12}H_3,H_2N$	$C_{12}H_1,AHN$
(b)	$C_{12}H_5O_2,H_2N$	$C_{12}H_3O_2,H_2N$	$C_{12}H_1O_2,AHN$
(c)	$C_{14}H_5O_4,H_2N$	$C_{14}H_3O_4,H_2N$	$C_{14}H_1O_4,AHN$, &c

A further list is briefly traced in their individual characters and histories, and the same principle is applied to both tri- and tetra-azo-bodies, with only one uniform result. All such amido- and azo-bodies can be traced as weakly amido- or ammonia-bases, in their varied saltic combinations, and, while the facts of analysis are quite independent of old or new modes of representation, it becomes an early enquiry of the second part, why most of these bodies should be found in modern manuals of double atomic weight. Two reasons for, or explanations of, this dire confusion are given as an introduction to the second part.

1. An *illogical* interpretation of the Avogadro Law.
2. A want of clear views in regard to the laws of ammoniacal dedoublement or condensation.

"If we can interpret the whole series, as embracing nothing new beyond the normal laws of condensation of the ammonia type, where our alphabet of the constituents, Part I., can be clearly and distinctly traced, not only in the compounds, but in the materials employed for their production, it will add a force and value to their apprehension, which no atomic doubling or molecular hypotheses can upset or destroy."

Here follows a brief chemical history of some of the principal compounds, including the monamine variety, where the fertile substance diazo-benzene combines with phenols, acids, and other hydrates, in modes and ways fully made plain in the previous history of chemical research.

Of the diamine series, we only refer to chrysoidine in this connection, as we have given it a position in Dr. Witt's initial series, where a numerical regularity might captivate attention. However plausible on paper, it has the misfortune of being like melid-acetic acid, which had no acetic acid in its genetic materials, and is no acid at all, being a definite glycolyl-melamine base; so in the case of chrysoidine there is no azo-benzene, whatever in its genetic materials, and every chemist must know that as aniline-yellow is or may be formed *directly* by combining diazo-benzene with amido-benzene, precisely so it is with chrysoidine, where the amido-body is replaced by diamido-benzene. The Griess view of the chrysoidine triplet is therefore thus—

Aniline-yellow = Diazo- (and) amido-benzene, (*a*)
Chrysoidine = Diazo- (and) diamido-benzene, (*b*)
Phenylene-brown = Diazo- (and) triamido-benzene, (*c*)

$$(a)\quad \left.\begin{matrix} C_{12}H_1AHN \\ C_{12}H_5HHN \end{matrix}\right\} = C_{24}H_{11}N_3.$$

$$(b)\quad \left.\begin{matrix} C_{12}H_1AHN \\ C_{12}H_5AHN \end{matrix}\right\} = C_{24}H_{12}N_4.$$

$$(c)\quad \left.\begin{matrix} C_{12}H_1AHN \\ C_{12}H_5AAN \end{matrix}\right\} = C_{24}H_{13}N_5.$$

It only remains to say that any student who may wish to canvass broad views on this aspect of chemical philosophy will be quite welcome to a perusal of the paper now so briefly abstracted.

16, Greenwood Terrace, St. John's, Hackney.

HEATING BY GAS.*

Cooking in Private Families, and use in Workshops.

By THOS. FLETCHER, F.C.S.

It is not my intention to-night to go deeply into the theory of gaseous fuel; to do so would take more time than is at my disposal. The figures I shall give are approximately correct, quite sufficiently so as a guide to consumers. Unfortunately I am not in a position to give the practical working values of the gas of high illuminating power as used in Scotland; my knowledge of this comes to me from others, and from what I can learn it would appear to be about one-sixth worse as a fuel than the same quantity of London gas. This is not owing to its actual inferiority, but is caused simply by the greater difficulty in burning so as to obtain a compact flame of high temperature, such as is necessary for an economical gaseous fuel; and without setting up my opinion as absolutely reliable, in the absence of sufficient practical experience, I think that as gas is now so largely used as a fuel, the Scotch gas companies would find it to their advantage to reduce their quality to the average English standard, reducing the price also in proportion. At present it is like a break in a railway gauge, and causes endless trouble and annoyance to both makers and users of gas apparatus for heating purposes, checking most seriously the consumption of gas in Scotland, and doing considerable damage to the gas companies; the gas used for heating being almost entirely wanted when the gas works are at present almost standing idle, *i.e.*, in the day time, and in summer more than winter. The gas companies who do not seriously consider the advisability of taking every possible means to increase the consumption of gas for heating purposes are simply ignoring their most important business and neglecting their most valuable and profitable customers. I would even go beyond this, and make for all purposes a gas of about 14 candle power as a maximum, and sell it, as it very well could be sold, at about 1s. 10d. per 1000 cubic feet (which, by the way, I am informed is the present price in Leeds). This would be cheap, perfectly suited for all purposes, both lighting and heating, and would be very much more largely used than the present gas. If the gas companies have any great future to look to it is from the use of gas for heating purposes, which can be cheaply made in large quantities at a time when the works are almost standing idle. Those who objected to the little additional heat given off from the lighting burners could well afford the expense of ventilating out of the reduction in their bills, and both makers and consumers would be benefited. At present the use of gas as a fuel is hampered in every way by the sleepy let-well-alone system of manufacturers with a monopoly, having their customers under their thumbs. The only reply I ever obtained to any remarks about creating a business in gas for cooking is "Ah, we are doing very well; look at our profits last year." These profits, bear in mind, are the result of excessive prices, in the face of a wasteful and costly system of working. If the Crystal Palace Gas Company understand their business, and we may suppose they do, it is the business of a gas company to test and know all the cooking and heating apparatus, to judge and select for their customers, knowing positively the best, and to fix the apparatus on hire, as they do gas meters, at a low rate, publishing and distributing constantly pamphlets explaining the advantages of gas heating and also its disadvantages, giving the whole case honestly and fairly. The united gas companies could afford well to offer a high premium for the best and most concise pamphlet on gas cooking, and to print and distribute it by the million. It is their duty to do so, and they would combine both duty and profit. It is not the business of any maker of gas cooking and heating apparatus, who naturally states his own side of the case and kindly overlooks all his weak points, even if he knows them.

In England a mixture of about 75 per cent cannel and 25 per cent good coal is commonly used for gas making. This produces from each ton 13 cwts. coke, 13 gallons tar, 25 gallons ammonia liqour, and 10,000 cubic feet of gas of about 18 candle power. The gas, if reduced to the solid state again, would weigh about 350 lbs. Allowing this as a fuel at a value of 1s. 2d. per cwt., which is about the average cost of the material used to produce it, we get the actual fuel value of coal gas as about 4½d. per 1000 cubic feet. Its cost at the gas works, delivered into the mains, including all expenses, is about 1s. 2d. per 1000 cubic feet.

Gasoline, benzoline, and petroleum, which are all hydrocarbons, and very similar in composition to coal gas, may be taken as fuel as equal to coal gas in value. A little calculation shows them to be worth as fuel about 10½d. per gallon, coal gas being 3s. 6d. per 1000 cubic feet—one gallon being equal to about 250 feet of gas. To get this duty from the petroleums it is necessary that they shall be burnt in a state of vapour. When burnt in spray or as a simple flame in a lamp with a wick it is exceedingly difficult in practice to obtain a fair proportion of the work out of them, and they become very expensive. When burnt as vapour, such as is produced from benzoline or gasoline in any of the well-known air gas apparatus, or in the simple little generator I will show you at the conclusion, there is no practical difference between the cost of these and of coal gas as fuel, and my remarks will apply in almost every point equally to coal gas, and the vapour or air gas made by passing air through gasoline or spirit petroleum. The generator which I shall use at the conclusion is simply a tin box divided lengthways, so as to secure a long passage for the air through a number of cotton screens, saturated with spirit petroleum. If I use

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* A Paper read before the Philosophical Society of Glasgow, October 22, 1880.

the more volatile gasoline instead of spirit petroleum the supply of vapour from a generator of about 1 cubic foot capacity, is equal to a coal gas supply through a 1¼ or 1½ inch main at ordinary pressures, and will work two of my largest furnaces—each melting 28 lbs. of steel—at the same time. This generator, being specially designed for blast furnaces, has no blowing apparatus, and is therefore unfit for use except where a supply of air under pressure is available.

It will be at once seen that gas and petroleum, at present prices, can never approach coal or coke as a competitor for large or continuous work where the cheaper fuel can be burnt without great waste. When, however, we come to the cooking for private families, and almost the whole of the heating work required in small workshops, the conditions are completely altered, and gas fairly used becomes a very economical fuel.

Take as an example our own cooking for a family of eleven. The cooking each morning is done in about 10 to 15 minutes, frequently in less than 10 minutes. The dinner, including roast joint, pastry, two or three different vegetables, sauces, &c., requires one burner only for 1½ hours, and three others from 10 to 30 minutes, say an average of 30 minutes for four burners. Tea and supper may be taken at a maximum of 10 minutes each. We here have about an hour during which the burners are in use, at a total cost of about twopence per day; if we had a fire it would probably be burning 15 or 16 hours, burning almost the whole time many times more fuel than would be necessary to do treble the work required.

The same remark applies to general workshop use. To braze a joint a fire has to be kept going the whole day, or a man has to spend perhaps 20 minutes lighting a forge, whereas with gas he could do his work in two minutes. If 3 lbs. or 4 lbs. of metal is required for a casting, a large fire has to be lighted, burning probably 20 lbs. of coke and requiring one or two hours before the crucible can be put in. After melting, the fire has to burn itself out. With gas the metal can be melted in 10 to 15 minutes, with a consumption of about eight or nine cubic feet of gas, the actual cost of fuel being far less than coke, and the value of the workman's time saved being more than ten times the cost of both coke and gas put together. In a foundry where pots are in the furnaces all day gas would be, as compared with coke, excessively costly; and in cooking for large hotels and institutions, where the work is almost continuous and on a large scale, gas is far too costly a fuel for the bulk of the work; but both in the hotel and in the large workshop or foundry, gas, although not the main fuel, is most valuable, and in many cases absolutely necessary as an assistant.

To have gas fuel at command is to be always the master of the situation, as it is ready for instant use when the fires are out of condition or extinguished, and hardly a day passes when gas in both places would not be a cheap fuel for some purposes.

In my lecture before the Society of Arts I stated that I obtained in a perfectly arranged water heater one gallon of boiling water by the consumption of about 1½ cubic feet of gas. This statement has been pretty severely handled by the theorists who are supposed to know all about it. They say it is above the theoretical maximum possible. I find no error in my results after repeated experiments, and there are possible reasons why the practical maximum may exceed the present theoretical maximum, which would explain the difference in results. If the theorists would put their aprons on and work with their hands as well as their heads it would be better for both theory and practice, and the "practical maximum" of use the theorists are at present would be considerably increased.

I am frequently asked, "Is gas a cheap fuel as compared with coal?" This is not easy to answer. For family cooking it is, I think, rather cheaper than coal. In saving of work and time in cleaning and attention it pays for itself in most cases ten times over. I know in our own case the absence of gas for cooking would entail our having an extra servant, and a greatly increased wear and tear caused by the dirt and mess of fires. In the routine work of my own laboratory the absence of gas for odd work would entail an additional expense in wages of nearly 50 per cent, but in addition to this, my special work, which in fact has created all the gas apparatus I have ever devised, could not be carried on at all without gaseous fuel. I refer to the production of some special alloys of the rarer metals which can only be produced with certainty under the most exact conditions of temperature and time. For my own work all furnaces and burners must be as exact in results as a chemical balance, and must be under the most perfect and instantaneous control; they are, in fact, instruments of precision working with the certainty of a chronometer. Here we have a business of some importance, the very existence of the good reputation of which is absolutely dependent on gaseous fuel.

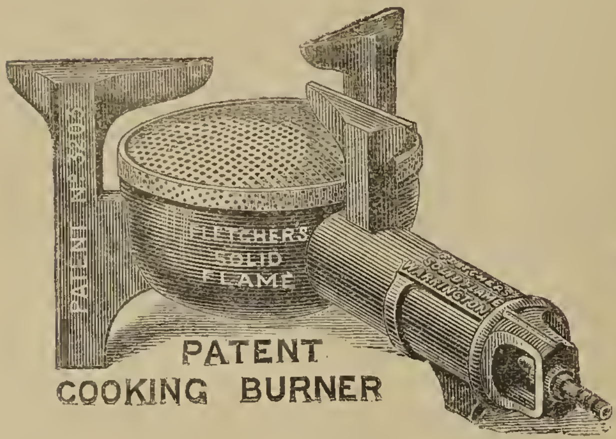
For cooking in ordinary families, for workshops where soldering, brazing, melting small quantities of metals in crucibles, &c., are required, and as an accessory in larger workshops, hotels, and public institutions, I consider gas fuel an absolute necessity for convenience and economy. It cannot be approached by any other fuel known. At the same time we must not forget that on account of its cost we cannot afford to waste it by using imperfect apparatus, and it pays better to throw away a faulty burner than to attempt to continue its use and make shift with it.

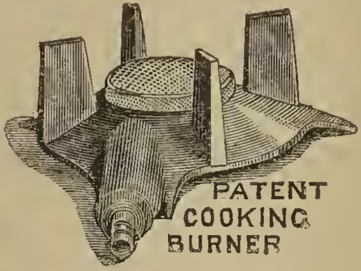
The question of the possibility of a good gas fire is frequently raised. I know many cases where gas fires have been fixed, and I know none where they have remained in use twelve months. They are far too costly in fuel, and I do not see how a good gas fire is to be made at a cost of less than 1d. to 1½d. per hour. Of course a small poor fire can be made for less, but it is practically useless, even to look at. If a gas fire is to be used I have no possible doubt that the proper material to be heated is most certainly not asbestos or fire-clay, but loose open balls of iron wire. It burns away slowly, but is very easy and cheap to replace, and gives off far more radiated heat than any substance I have yet met with, although iron is one of the worst radiators of heat known. The reason of this is simply that iron takes up heat much more rapidly than asbestos or fireclay, and becomes very much hotter.

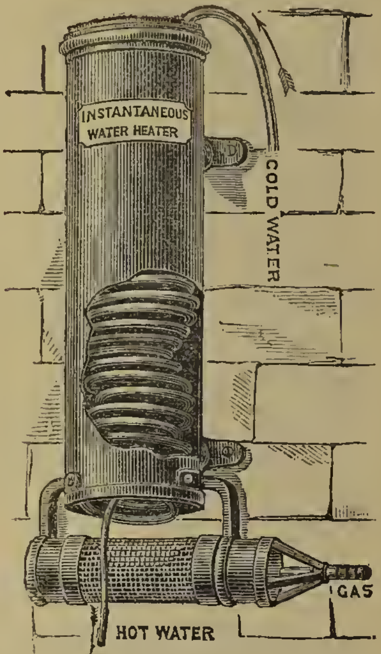
With regard to gas stoves, and giving my own personal opinion, the old style, be they reflectors or non-reflectors, are a horrible abomination, not to be tolerated by anyone whose nose and feelings are in anything like working order. I have a stove in use which is really a boiler with hot water pipes heated by gas, and which I shall again refer to; but the convected heat from warm metal, whatever the temperature of the metal, is to me so decidedly disagreeable, that it would be only a matter of pure necessity that would make me bear the annoyance. Still, speaking of my own feelings I greatly prefer, both for comfort and economy, to heat any room by gas by keeping the ordinary lighting burners at work without a glass globe. The radiated heat from these is, to myself, beyond comparison preferable to the convected heat from a surface of warm metal. You will clearly understand I speak now only for myself and my own feelings, having a strong dislike to any and every stove for gas or solid fuel, and also to the system of heating by hot water pipes when it can be avoided.

I have here a new form of oven for gas cooking, which so far as our own experience goes, is apparently destined to rule the pattern and principle of the gas ovens of the future. Using a line of solid flame so as to distribute the heat equally, the bottom of the oven is first heated. The whole of the radiated heat from the bottom is used underneath for toasting bread, grilling chops and steaks, making Yorkshire puddings, roasting potatoes, apples, &c. The hot air is then, after doing its work underneath, passed in a layer round the sides of the oven inside in such a manner that the products of combustion, although inside the oven, pass up round the sides and under the top so as not to come

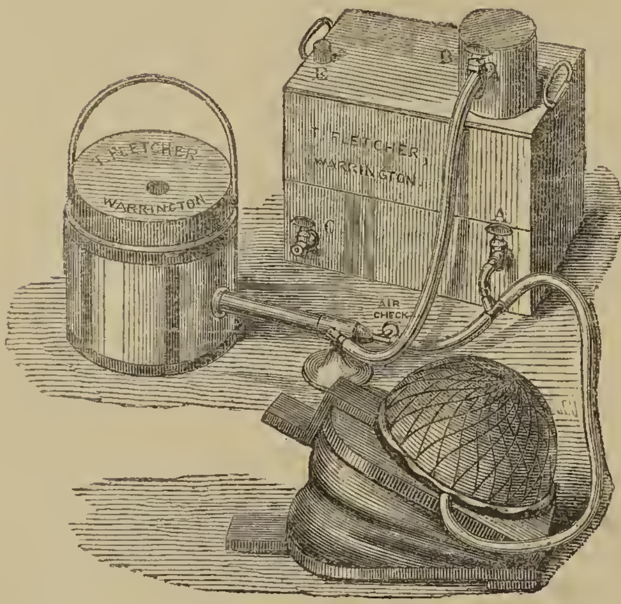
APPARATUS USED TO ILLUSTRATE THE LECTURE.



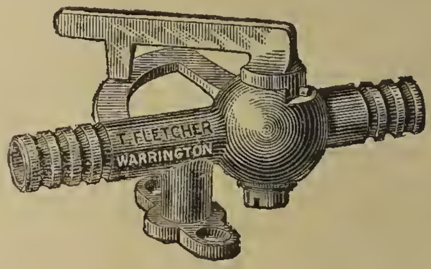


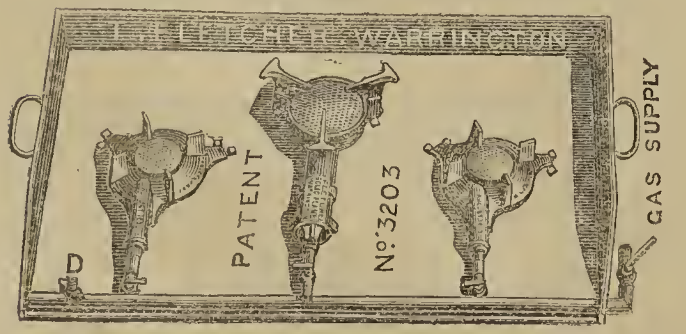


Instantaneous Water Heater, for Scullery and Bath-room.

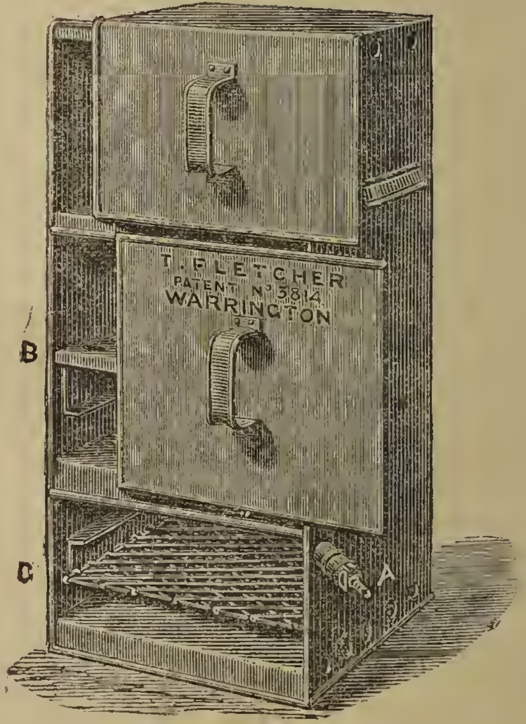


Crucible Furnace for Gas or Petroleum Vapour, showing Gas-making attachment in use.





Boiling Bench.



Triple Oven, requiring only one Gas Burner.

into contact with the food. Here meat can be roasted, not baked, to perfection, also pastry, bread, and all cooking which requires browning and a dry sharp heat with plenty of ventilation. After doing its work here the heat is again taken into an upper oven in a precisely similar manner, but has become rather too moist to brown and roast well. This upper oven is specially adapted for rice puddings, stews, and work of this class. On the top of the oven, and also round the sides, plates and dishes can be warmed.

With the assistance of burners for boiling, this oven, which is about as simple and cheap in form as an oven well could be, will do in the most perfect manner everything which can possibly be done with a first-rate open fire range, and as it is fully hot in half a minute after lighting the gas, and therefore always ready for anything and everything at half a minute's notice, it overcomes the one great objection to a fire range. The one I show you, which measures 14 by 12 inches on the bottom, and costs on an average one penny for three hours' work, will do the whole of the cooking, boiling excepted, for a family of 12 or 14 people.

There is one purpose for which gas is frequently required, the heating of small greenhouses. The fumes of gas, burnt or unburnt, are seriously injurious to plants. My own tropical stove and orchid house is a very small one, too small to heat steadily with coke, and I have in it 40 ft. of 4½ inch pipe heated by gas. The burner is placed in a boiler inside the house, but is lighted from the outside, and the flue runs inside the water pipes for a distance of 20 ft. The products of combustion leave the house over the lighting hole at a temperature of from 75° to 85°. The house is well built and well glazed. To keep a minimum temperature of 60° in the summer and 50° in the winter costs about £10 per annum. To keep a minimum of 40° would cost about 25s. This is a further proof of the heavy cost of continuous work with gas; at the same time I could not do the same work with any other fuel, as the house is separate, and cannot be heated from the other boiler which is worked with coal, and which does ten times the work at a smaller cost. With gas it may be taken as an average that a rise of one degree in temperature costs about one penny for twenty-four hours in a plant house 12 ft. square 12 ft. high.

I will now proceed with my experiments and will show you the fusion of cast-iron in a crucible which, starting all cold, will require seven or eight minutes. I will then replace the crucible with one containing pure nickel, which I will melt in about 25 minutes. I may say that the fusion of pure nickel is a feat rarely attempted, and still more rarely successful in any furnace except the injector which I shall use. For the sake of rapid working I will only melt about half a pound in a small crucible. The heat required is so tremendous and the loss by radiation is so rapid the instant the crucible is removed from the furnace that I doubt whether I shall be able to pour it before it sets hard in the crucible, and I dare not exceed the heat required to melt the nickel, as no crucible will stand a higher temperature. If I fail to pour it I will cool the crucible and show you the fused mass, which you will see me put in the crucibles as small lumps. The lumps I use have been fused and poured in a larger bulk in a furnace precisely similar to the one I use, but of a larger size. Whilst these rather noisy operations are going on I will show you the difference between a solid and a hollow flame, both having the same appearance, by putting my bare finger in the centre of the hollow flame, my hand being protected from the outer film of flame by a wet cloth. I will then put a ball of gun-cotton in the centre of the hollow flame, and afterwards explode it by making the flame solid.

I asked my wife to be my head cook for to-night, intending to give you a practical illustration of cooking on a variety of substances; but she refused, and as you see here I am, the unhappy victim of a monopoly, as she, like the gas companies, has no fear of opposition and does as she likes. We must, therefore, omit our intended extensive supper: I offer no apologies; we are all present the victims of this monopoly, and we must make the best of it. To assert my independence I will, however, roast you a small joint, bake a fruit pie, and cook a fish underneath, so that the vapours from the fish will have to pass completely round both the roasting meat and the pie. If the fish flavours either, no doubt someone present will be sharp enough to discover it.

I wished to show you the actual working of a four-course dinner for twelve people, *i.e.*, soup with dry toast, fish, joint with three vegetables, sauces, pastry, custards, &c.; warming about 50 plates and dishes, and boiling three or four gallons of water for washing up, all of which we can easily do with the oven and three small boiling burners now before you. When I was a bachelor, and before I came under petticoat government, I never dreamt of such a thing as a four or five-course dinner, more especially when, as often happened, I had to cook it myself; there is no telling what one may come to when somebody else has to do the cooking.

ON THE PRESENCE OF COPPER IN SULPHATE OF SODA CRYSTALS.

By Dr. PHIPSON.

The coarse sulphate of soda crystals obtained, from tanks where copper is extracted by the wet process in Cornwall, have given me, besides a very large amount of chloride of sodium and much iron,—

Oxide of copper	0·62
Oxide of lead	0·15
Tungstic acid	0·10
Silver	trace
Arsenic	trace

The comparatively large amount of copper in these crystals is worthy of notice.

ON THE PRESENCE OF NICKEL IN CORNISH MANGANESE.

By Dr. PHIPSON

Some splendid specimens of Cornish oxide of manganese have yielded me notable amounts of oxide of nickel with little or no cobalt. Upwards of 0·1 per cent metallic nickel was obtained in two analyses, with a mere trace of cobalt.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, November 4, 1880.

Prof. H. E. Roscoe, President, in the Chair.

The following certificates were read for the first time:—J. O. Arnold, A. E. Barclay, A. C. Cockburn, P. H. Cathcart, T. Farrington, P. F. Frankland, A. E. Garrod, R. Holliday, A. Jesseman, J. A. Macfarlane, W. B. Mason, R. T. Plimpton, C. H. Sharples, F. M. Sexton, B. H. Thwaite, J. M. Wyborn.

The President congratulated the Society on having thirteen papers down for reading on the first evening of the session.

During the reading of the first paper, which was communicated by the President, Dr. Frankland took the Chair.

"*On the Sulphides of Vanadium*," by W. E. Kay. Berzelius describes in his original research on vanadium two sulphides, VS_2 and VS_3; from the researches of Roscoe it follows that their real formulæ should be $(V_2O_2)S_2$ and $(V_2O_2)S_3$. The author has repeated the experiments of Berzelius, analysed the compounds formed, and investigated the whole subject. He also gives details of the method employed to estimate the sulphur, *i.e.*, ignition in a hard glass tube in a stream of oxygen, the products of combustion passing successively through two Liebig's potash bulbs, the first filled with bromine water, the second with distilled water. Berzelius obtained his compounds by precipitation, and, one of them, by a dry process. The substances formed in the wet way are oxy-compounds, but do not seem to have any definite composition. The dry process, *i.e.*, heating V_2O_3 to redness in sulphuretted hydrogen, forms, curiously enough, V_2S_3, which gives the same increase in weight over that of the V_2O_3, as $(V_2O_2)S_2$; so that if Berzelius had but estimated the sulphur in the product instead of being satisfied with the increase of weight, the true constitution of the vanadium compounds would probably have been ascertained by him. The author has obtained and studied three sulphides, V_2S_2, V_2S_3, and V_2S_5. V_2S_2 is obtained by reducing V_2S_3 in a current of hydrogen, as glistening black scales, or a blackish powder; it absorbs oxygen with avidity. Hot strong sulphuric acid gives a greenish yellow solution; hot strong nitric acid oxidises it with violence; colourless ammonium sulphide gives a splendid purple solution; it is acted upon but slightly by caustic alkalies. V_2S_3 is prepared as above, or by the action of SH_2 or CS_2 at a red heat on V_2O_5. In its properties it resembles V_2S_3, but is oxidised less readily. V_2S_5 is prepared by heating a mixture of S and V_2S_3 in a sealed tube to 400° for three hours; it resembles the other sulphides, but is readily dissolved by caustic soda. The author remarks the inferior stability of the sulphides as compared with the oxides, V_2S_5 when heated giving off sulphur and forming V_2S_3, whilst V_2S_3 in an atmosphere of hydrogen is reduced to V_2S_2.

"*On the Action of Zinc Ethyl on Benzoylic Cyanide*," by E. Frankland and D. A. Louis. The cyanide was gradually added to zinc ethyl diluted with three times its volume of dry ether, and the reaction completed by heating in a water-bath. The product of the reaction, an amber-coloured jelly, was decomposed, and then extracted with alcohol. Tufts of colourless needles were obtained which gave on analysis $C_{24}H_{19}NO_2$. The authors propose the name benzcyanidin; the yield is small, about 3 per cent of the cyanide. Another substance boiling between 200° and 220° was obtained, but the authors did not succeed in purifying it so as to procure a liquid with a constant boiling-point. It was oxidised with bichromate and dilute sulphuric acid, when propiophenone, $C_9H_{10}O$, was obtained, which the authors believe was formed directly by the action of zinc ethyl on the benzoylic cyanide—

$$2\begin{cases}C_6H_5\\CO(CN)\end{cases} + ZnEt_2 = 2\begin{cases}C_6H_5\\COEt\end{cases} + Zn(CN)_2.$$

Much zinc cyanide was also formed. The halogen nature of the compound radical cyanogen is thus shown to extend to its replacement by an electro-positive hydrocarbon radical.

"*On the Action of Zinc Ethyl on Cyanogen*," by E. Frankland and C. C. Graham. The zinc ethyl rapidly absorbs the cyanogen, and is finally converted into a solid mass. This mass was heated to 120° in an oil bath, and furnished a colourless liquid, which was freed from traces of zinc ethyl and re-distilled. It then boiled at 96°: analysis indicated the formula C_3H_5N, and it had all the properties of propionitrile. The solid residue remaining after the distillation, consisted almost entirely of zincic cyanide. The reaction is therefore—

$$2\begin{cases}CN\\CN\end{cases} + ZnEt_2 = 2Et(CN) + Zn(CN)_2.$$

It is to be noted that a separation of carbon from carbon occurs, accompanied by a simultaneous union of carbon to carbon.

"*Report on the Atmospheric Oxidation of Phosphorus, and some Reactions of Ozone and Peroxide of Hydrogen*," by C. T. Kingzett. The author commenced with a historical *résumé* of previous researches by Schönbein, Andrews, Marignac, Corne, Boehe, Leeds, McLeod, and himself. The author in the present research employed a series of glass bottles stoppered, each fitted with two glass tubes ground into holes in the stopper, thus avoiding altogether the use of cork and india-rubber. He concludes that both ozone and peroxide of hydrogen are produced by the oxidation of phosphorus, the peroxide of hydrogen remaining almost entirely in the water in which the phosphorus is oxidised. The ozone passes on in the air current, and after washing and drying with strong H_2SO_4, was estimated by the amount of iodine liberated in an acidified solution of potassium iodide. A large number of experiments were made. In several cases the proportion of peroxide of hydrogen to the ozone produced was approximately 1 to 2. The ozone was entirely destroyed at 240°, and by passing through oil of turpentine; it was, however, not destroyed by passing through a ten-volume solution of hydric peroxide. Ozone when it acts on oil of turpentine gives rise to the same peroxidised compound as is formed by the absorption of oxygen. The turpentine which has absorbed ozone, when treated with water, produces an equivalent amount of peroxide of hydrogen. Similar observations are recorded with ether. As regards the estimation of peroxide of hydrogen, the author finds that the ordinary process with potassic iodide and starch may be much shortened by employing a large excess of dilute sulphuric acid. When it is estimated by the amount of oxygen evolved with potassium permanganate, the author advises that no acid be added, as a mixture of potassic permanganate and sulphuric acid gradually gives off oxygen until all the permanganate is decomposed.

After a few remarks from the President and Dr. Armstrong,

The Secretary read a paper "*On Bismuth and Bismuth Compounds*," by M. M. P. Muir, G. Bernard Hoffmeister, and C. E. Robbs. The preparation of the various oxides, hydrates, and halogen salts is discussed; previous work is criticised and experimentally examined. The oxides and hydrates certainly known to exist are Bi_2O_2, Bi_2O_3, Bi_2O_4, Bi_2O_5, $Bi_2O_3H_2O$, $Bi_2O_3,2H_2O$ and $3H_2O$, $Bi_2O_4H_2O$ and $2H_2O$, $Bi_2O_5H_2O$, and possibly $Bi_2O_2H_2O$ and $2H_2O$, and $Bi_2O_53H_2O$. The relative stabilities towards heat and reducing agents of the oxides, and towards heat of the hydrates, are discussed. The action of chlorine and bromine on the oxides is described. The hydration of the oxides in moist air is measured: $Bi_2O_52H_2O$ is shown to be the hydrate into which the others tend to pass. BiI_2 is much more stable, and more indifferent to reagents, than the corresponding bromide or chloride. There is apparently no tendency to form complex oxy-iodides. By the action of HF on Bi_2O_3, BiF_3 and BiOF are formed. The following equations represent the final distribution of salts in each reaction:—

$$\begin{cases}1.\ Bi_2O_3 + 2HCl + xH_2O = 2BiOCl + (x+1)H_2O.\\2.\ Bi_2O_3 + 6HI + xH_2O = 2BiI_3 + (x+3)H_2O.\end{cases}$$

$$\begin{cases}1.\ 3BiOCl + xHI + x'H_2O =\\\qquad\qquad 2BiI_3 + BiCl_3 + (x-6)HI + (x'+3)H_2O.\\2.\ 4BiOI + xHCl + x'H_2O =\\\qquad\qquad 3BiCl_3 + BiI_3 + (x'+4)H_2O + HI + (x-9)HCl.\end{cases}$$

An attempt is made to condense the leading relations between oxides and hydrates in structural formulæ, in all of which the bismuth atom is represented as trivalent.

"*On the Colour, Properties, and Relations of the Metals Copper, Nickel, Cobalt, Iron, Manganese, and Chromium*," by T. Bayley. These metals form a group of elements with atomic weights progressing from 52·2 to 63·00. They are characterised by salts, corresponding to their normal oxides, which in dilute solution possess strongly-developed and characteristic colours. Standard solutions were prepared containing 5 grms. of each metal per litre. The iron was oxidised with nitric acid after solution in dilute sulphuric acid; the other solutions contained excess of both of these acids. By mixing 20 parts of the copper solution, 7 of the iron, and 6 of the cobalt, a colourless liquid was obtained, appearing, however, slightly grey from the partial suppression of the white light passing through; the colour equivalents of cobalt, iron, and copper are therefore respectively 20, 7, and 6, the colour equivalents being the numbers expressing the quantities of the metals required to produce an optically neutral mixture. The colour coefficients are the reciprocals of the colour equivalents reduced to terms of the coefficient of copper as unity. The colour coefficient expresses the specific chromatic power of the element in the particular combination. These numbers vary slightly with the temperature: thus, the above neutral solution (at 18°) when boiled appears to be yellow. By mixing iron and copper we can simulate exactly a nickel solution. A copper solution is complementary to a mixture of iron and cobalt, and to the reflection from a surface of copper. The mean wavelength of the light absorbed by iron, nickel, cobalt, and copper increases as the atomic weight increases. The mean wave-lengths of the light absorbed by nickel and cobalt are nearly identical. Generally the affinity of the metals of this group for oxygen increases with the colour coefficients. The coefficients are—Cu, 1; Ni, 1·05; Fe, 2·85; Co, 3·33; Cr, (?); Mn, (?). The oxides studied are the so-called normal oxides, *i.e.*, neither protoxides nor peroxides. The following table exhibits the colour properties of these elements as studied up to the present time, elements with solutions of similar colour being placed in the same colour:—

Yellow.	Orange.	Pinkish Red.	Violet.	Blue.	Green.
Fe	7Fe+6CO	Co	20Cu+6CO	Cu	20Cu+7Fe
Mn (?)	Copper reflection	Cr	Cr mixtures	Cr	Cr
	Mn (nearly)	Mn			(Ni nearly)

The following combinations give grey, partially opaque, liquids:—Cobalt, iron, and copper, copper solutions and copper reflections, chromium (blue) and copper reflection; copper, iron, and chromium (red); copper, iron, and manganese (red): chromium (green) and chromium (red); chromium (green) and cobalt. The author has specially examined only the first of these combinations. He gives diagrams showing the absorption-spectra of cobalt, copper, and iron, the solutions being of equivalent chromatic strength, *i.e.*, equal volumes give when mixed a grey liquid.

"*On the Action of Diazo-naphthalene upon Salicylic Acid*," by P. F. Frankland. Although numerous azo-bodies have been prepared, yet only a few thereof are compounds of the oxy-benzoic acids, and in none has the position of the phenyl-azo group been ascertained. The author's object was therefore twofold—the preparation of a new azo compound and the determination of the place of entry into the diazo group into the benzene nucleus of salicylic acid. The diazo group was introduced in the form of diazo-naphthalen, naphthyl-azo-salicylic acid being formed. This substance, on treatment with tin and hydrochloric acid, yielded naphthylamin and amido-salicylic acid. The latter was then converted by means of its diazo compound into iod-salicylic acid, which by its melting-point, reactions, and conversion into oxy-salicylic (quinol-carboxylic) acid, proved to be the iod-salicylic acid already known. The diazo group of the diazo-naphthalin thus attached itself to the benzene ring of the salicylic acid in the para position to the hydroxyl, and in the meta position to the oxatyl. In the preparation of naphthyl-azo-salicylic acid an amorphous brown body was obtained, insoluble in water. This proved to be a mixture of two substances—one soluble in alkalies, with the formula $C_{10}H_8NO$, the other insoluble in alkalies, $C_{30}H_{33}N_5O_2$.

"*On the Basic Sulphates of Iron*," by Spencer Pickering. The author has investigated the numerous sulphates said to exist, but of the existence of fifteen of them he can find no proof, there being apparently but one definite chemical compound.

"*Fourth Report to the Chemical Society on some points in Chemical Dynamics*," by C. R. A. Wright, E. H. Rennie, and A. E. Menke. The authors have determined the rate at which reduction of certain metallic oxides is effected under conditions varying as to temperature, but otherwise constant, with the view of finding out how far abrupt alterations in the rate occur when a higher is reduced to a lower oxide, the heat of formation of which differs from that of the higher oxide. They show that these alterations are readily distinguishable at any rate at certain temperatures. Thus, samples of ferric oxide in different states of physical aggregation were reduced under constant conditions by hydrogen and by carbon oxide: curves were then plotted, the amounts of reduction being taken as ordinates and the times as abscissæ. It was seen that as soon as one-ninth of the oxygen had been removed (so that the partially reduced oxide had the composition Fe_3O_4) a marked slackening in the rate of reduction was always observed, particularly at lower temperatures, a sort of angle being thus produced in the curve. No such angle exists at points representing either removal of one-third of the oxygen (reduction to FeO) or any other stage of reduction. The results with carbon oxide are complicated by its tendency to bring about re-oxidation,

$$Fe_xO_y + CO = Fe_xO_{y+1} + C.$$

The effect of this is to diminish the rate of reduction; nevertheless it was found almost invariably that the rate of reduction of carbon oxide was, *cæteris paribus*, greater than that of hydrogen, as was expected from the greater evolution of heat in the former case. Manganese dioxide was examined in the same way, with similar results: a distinct angle was exhibited by the curve when the composition was Mn_3O_4, and another angle when it was MnO. The reducing action of both hydrogen and carbon oxide then ceases up to a bright red heat. In every case the angle indicates a decrease in the rate of reduction, and that consequently the heat of formation of the lower oxide is greater than that of the higher oxide. From the present and previous experiments the authors deduce the general law that two comparable cases of reduction, in which the heat evolutions differ from another, the case in which the heat evolution is algebraically the greatest (*i.e.*, the greatest evolution or least absorption) is the case in which (*a*) the length of time requisite to produce a given amount of reduction at a given temperature, is the least, (*b*) the amount of reduction produced in a given time at a given temperature is the greatest, and (*c*) the temperature requisite to produce a given amount of reduction in a given time is the lowest. Certain apparent minor exceptions to this law have been noticed, and are explained by the non-identity of conditions, *e.g.*, when carbon oxide reduces ferric oxide, the action is retarded by the tendency, above alluded to, of carbon deposition; and, again, when the oxide is coated with a film of reduced substance the carbon oxide diffuses much more slowly than hydrogen under similar circumstances, the reducing action of carbon oxide being thus much retarded. Again, when metallic copper is oxidised a similar retardation takes place, owing to the formation of a film of oxide. The general law stated above may be represented graphically by constructing curved surfaces about three rectangular axes, the co-ordinates of any point on a surface representing time, temperature, and amount of reduction, the surface representing the values obtained with the substance, the reduction of which is accompanied by the greatest heat evolution, always lies outside.

"*On some Derivatives of Naphthalene* (1. *On β-naphthol Sulphonic Acid.*)," by H. E. Armstrong and W. C. Graham. The authors were led on certain theoretical grounds to re-examine the β-naphthol sulphonic acids prepared respectively from β-naphthol, and from the so-called naphthalene, β-disulphonic acid, by fusion with caustic potash. They have prepared and examined various salts, and conclude that Ebert and Merz were right in asserting the identity of the two products. They have also studied the action of bromine on β-naphthol sulphonic acid.

"*On Acetyl-ortho-amido-benzonic Acid*," by P. D. Bedson and A. J. King. Acet-ortho-toluidine was first prepared by heating ortho-toluidine with glacial acetic acid for several hours, and re-crystallising from aqueous alcohol; it was thus obtained in white needle-shaped crystals, melting at 107° to 109°. This compound was oxidised by the addition in small quantities at a time of potassium permanganate, the whole being warmed in a water-bath. The MnO_2, &c., was filtered off, and the acid precipitated directly by concentrated hydrochloric acid, or as a lead salt with lead acetate. The authors have studied the acid and some of its salts. The physical and chemical properties are given in the original paper.

A paper by Messrs. Cross and Bevan "*On the Formation of Sodium Hydrate from the Sulphate*," was taken as read.

The Society then adjourned to November 18, when the following papers will be read:—"Notes on the Oxides of Manganese," by Spencer Pickering; "Aluminium Alcohols (Part I., Their Preparation by Means of the Aluminium Iodine Reaction)," by J. H. Gladstone and A. Tribe.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, October 19, 1880.

E. W. Binney, F.R.S., F.G.S., President of the Section, in the Chair.

The following communication from Dr. R. Angus Smith, F.R.S., was read:—

In relation to my paper on the word Chemia, Mr. Wm. Simpson informs me that Dr. Muir, in "Sanscrit Texts," vol. v., p. 402, refers to the Rig Veda, where Kam is represented as Eros. Dr. Muir also says that Kama is distinctly identified with Agni, the Sanscrit for fire. This gives the word a firmer basis in the East than I found for it. He also adds that Wilford, in "Asiatic Researches," identifies Cæma or Kama of India with the Chemia or Chemi of Egypt. I shall add this to my paper, which is being printed in full. It is one of the proofs of early connection of Aryan and Semitic people both in language and thought; but this is a subject that belongs to others to speak of. I hear of other connections with the far East in the word Chemia, but having begun to argue this view of the case, others may advance it. When I say begun, I merely sought to connect old links of thought, and other old links may be found lying about in many places.

"*Additional Note on a Theory of Mixed Opaque Colours*," by James Bottomley, D.Sc.

Professor Reynolds, F.R.S., said that he had been able to get a barometer tube free from air by first washing the tube with water, and introducing the mercury while the tube was wet, and then leaving the tube in an inverted position for several days. The water absorbed the air, and floating up between the mercury and the glass left the tube dry, full of mercury, and free from air. He hoped at the next meeting to report some further experiments on the suspension of mercury by its cohesion in a tube 90 inches long.

"*Some Endeavours to Ascertain the Nature of the Insoluble Form of Soda Existing in the Residue left on Causticising Sodium Carbonate with Lime*," by Watson Smith, F.C.S., Assistant Lecturer on Chemistry in the Owens College, and Mr. W. T. Liddle.

In the following are given the results of an enquiry (yet in progress) which were obtained towards the close of last session, in the Laboratory of Owen's College, by Mr. W. T. Liddle in conjunction with myself, and with the occasional co-operation of Mr. H. Rimmer. The present inquiry was the final step after a series of exercises in the technical examination of some alkali products by the two gentlemen named, kindly furnished by Messrs. Gaskell, Deacon, and Co.

Hargreaves (Chem. News, 387) and Kynaston (*Chem. Soc. J.*, 11, 135) have noticed the occurrence of soda in an insoluble form in the crude soda (black-ash) of the alkali works, but they only speak of alumino-silicates and silicates of sodium, and in these early papers mentioned do not appear to imagine any other insoluble compound present in which soda may be practically lost to the manufacturer.

Dr. C. R. A. Wright published a paper (appearing in *Journ. Chem. Soc.*, 1867, p. 407) in which he distinctly shows (1) that soda in an insoluble form does exist in black-ash treated with water, in process of lixiviation, and (2) that though it may partly exist as alumino-silicate, yet undoubtedly it exists in some other form, and most probably as a double sodium calcium carbonate.

To show the importance to the soda manufacturer of this loss in insoluble sodium compounds left behind in the black-ash and waste, Wright states that it forms the largest item of the several individual losses, making up the total 20·24 per cent loss out of 100 parts Na_2O as salt-cake occurring in the practical conversion of salt-cake to soda-ash. Wright tabulates this as follows:—

Previous to lixiviation of the black-ash:—	
Undecomposed sodium sulphate	3·49
Insoluble sodium compounds............	5·44
Vaporisation, &c., of sodium compounds..	1·14
During and after lixiviation:—	
Soluble alkali left in vat waste	3·61
Leakage and losses in soda-ash process ..	6·56
Total loss per cent	20·24

In experiments tried with samples of black-ash Wright showed that on prolonged boiling (6 hours) with water, the insoluble sodium compound in the black-ash residue was decomposed, and yielded a sodium salt in solution capable of neutralising acid. On taking soda waste and submitting this to prolonged boiling with water, only 3·81 out of 5·08 per cent of the insoluble soda, calculated as Na_2CO_3, were extractible, and he considers that this difference from his experience with the black-ash is due to the influence of the other sodium salts present on the insoluble compound in the case of the black-ash. Wright also cites the well known fact, that on causticising sodium carbonate solutions with quicklime, the calcium carbonate formed retains a considerable portion of sodium in an insoluble form, and adds, that most probably in the case of the black-ash a double sodium calcium carbonate is formed, either in the furnace or on addition of water to the crude soda. In further proof of this view he mentions the case of some experimental charges for black-ash, in which an unusual excess of limestone was used. Wright examined the resulting product to see if more of this insoluble compound were formed, and found there was.

Mactear (Chem. News, Feb. 2, 1872, p. 55) makes the interesting discovery that "oxidised alkali waste yields on lixiviation almost all the soda contained in the waste." The soda thus rendered soluble occurs in the solution as sulphate. Mactear says "The chief loss in the soda process is that which occurs during lixiviation of the ball soda. This loss is in part represented by the insoluble and soluble compounds left in the waste. The former sometimes amounts to 3 or 4 per cent of the soda, and the amount is increased as the silica and alumina of the raw materials increase."

Wright has shown that an increase of limestone in the black-ash mixture will also increase the amount of the insoluble soda compound.

Scheurer-Kestner (*Comptes Rendus*, Nov. 11, 1872) confirms Wright's views. He proves conclusively that an increase of chalk in the black-ash mixture causes a proportionate increase in the amount of insoluble soda compounds left in the waste. "The excess of chalk employed is converted into lime, and when the crude soda is lixiviated with water, the lime while becoming hydrated reacts upon the sodium carbonate, and thereby renders a portion insoluble in water." According to Scheurer-Kestner's experiments, the lime may retain even as much as from 4·75 to 4·95 per cent of soda Na_2O.

With regard to our own experiments, we first operated upon some samples of soda waste, with the view of determining the soluble and insoluble soda therein contained.

After titration and digestion with water of about 60° for an hour a sample of waste yielded us 0·22 per cent of alkali as Na_2O soluble in water. On solution of the residue in acid, a gravimetric determination of the residual and therefore insoluble soda gave 2·18 per cent Na_2O, hence the waste contained of total Na_2O—2·40 per cent.

Another sample analysed by Mr. Rimmer gave as

Soluble Na_2O	—0·31	per cent
Insoluble „	1·91	„
Total	2·22	„

Wright found in an average sample of a fortnight's soda-waste as

Soluble Na_2O	—2·07	per cent
Insoluble „	0·91	„
Total	2·98	„

In the difference noticeable between Wright's results and ours, as regards the insoluble soda, it is possible his own explanation for the fact that continuous boiling with water will extract the insoluble soda from black-ash, but will not from soda waste, may here hold good, for it will be noticed his soda waste contains considerably more soluble sodium salt than ours does.

We now turned our attention to the soda left behind in the lime sludge remaining as a residue in the process of causticising sodium carbonate solutions. In the sludge taken as a sample of many tons lying outside the causticising plant of a works after suitable draining, the total Na_2O extractible by water was found to be 2·62 per cent. Calculated roughly into dry residues this would represent 3·84 per cent.

Now according to several careful analyses made some years ago, the amount of soda existing in lime mud in the insoluble form averages about 2½ to 3 per cent. on the mud. If we add this to the above figure 2·63 per cent for the soda soluble in water, we get an approximate 5 per cent of total soda. In alkali works where black-ash is made, this soda is not lost, the mud, drained and dried as far as possible, being mixed with the black-ash charges, and worked into ball soda, and thus it is kept in constant circulation, instead of being lost. The loss entailed thereby is one of heat, and hence of fuel, in converting water into steam in the black-ash furnace.

In order to have some object to aim at, we commenced the next step by assuming the existence of such a double sodium calcium carbonate, as Wright believes is formed, under the circumstances already named; and, as the simplest mode of representing such a compound, we took the liberty of giving the formula

$$Ca\Big\langle\begin{matrix}CO_3Na\\CO_3Na\end{matrix}$$

We then made an attempt to prepare this so far hypothetical salt. But before proceeding to this, we will just refer to some experiments we made with the object of ascertaining with some degree of precision under what circumstances the insoluble sodium compound is formed.

Solutions or caustic soda and sodium carbonate were prepared; the form had a specific gravity of 1·09 and contained 6·52 per cent Na_2O; the latter contained 5·985 per cent Na_2O.

(I.) A quantity of precipitated $CaCO_3$ was diffused in water and boiled then with 20 c.c. of standard caustic soda for 15 minutes. After filtering and washing till the filtrate was no longer alkaline, it was found by titration with normal hydrochloric acid that no soda had been retained by the calcium carbonate.

(II.) No soda was retained either, when instead of the precipitated $CaCO_3$ finely powdered marble was used.

(III.) A quantity of calcium carbonate (precipitated) was now boiled for a long time with 20 c.c. of the sodium carbonate solution of known strength and with the addition of water. No soda was retained.

(IV.) The above experiment was repeated with finely powdered marble, with like negative results.

(V.) A quantity of milk of lime was now taken, and boiled with 25 c.c. of the Na_2CO_3 solution. After filtering, washed with 500 c.c. of hot water, removed lime, filtered, washed, evaporated to dryness, ignited, dissolved in water, and titrated, 1·286 per cent Na_2O was retained by the calcium residue.

(VI.) A quantity of milk of lime taken, and to it were added 25 c.c. of caustic soda solution with some water. The whole was boiled for some time—0·05 per cent Na_2O retained. Some of the sodium hydrate becoming accidentally carbonated might account for this.

This lime mud residue of (V), washed as above till the filtrate ceased completely to react alkaline, was washed into a flask and a current of CO_2 was passed through for a long time, to endeavour to decompose this insoluble compound. In this way only 0·078 per cent of the soda (Na_2O) was extracted.

We now attempted to prepare some of the double sodium calcium carbonate in the following manner:—

A quantity of pure sodium carbonate solution (somewhat concentrated) was mixed with about three times its volume of clear lime water, and this mixture was heated to boiling. The precipitate was allowed to settle, was filtered, and washed with hot water till the filtrate ceased to manifest the slightest alkalinity to test paper. It was then dried in the water-bath. When the lime water was added to the sodium carbonate solution, the precipitate of carbonate which came down had the floccular appearance of alumina freshly precipitated, but on standing for about half an hour without heating, it became crystalline in appearance. The dried precipitate under the microscope was distinctly crystalline, being apparently composed of minute rhombohedra.

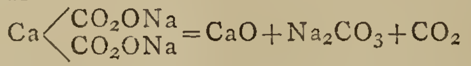
In subsequent experiments we found the microscopic appearance to vary, sometimes rhombohedra mixed with minute prisms making their appearance.

On analysis we found that the crystalline powders (they were only just perceptibly crystalline to the naked eye) contained according to two experiments:—

$CaCO_3$		Na_2CO_3
(1)	(2)	
67·65%	97·90%	2·46%

It would seem probable from this that the major reaction, so to say, is that converting sodium carbonate into hydrate, calcium hydrate passing into *pro ratâ* carbonate, but a minor reaction also occurs by which a small quantity of a double carbonate is formed. It is all the more certain that such a compound actually is formed from the fact that the precipitates obtained as just mentioned (and this experiment has been repeated many times) were in every case distinctly crystalline powders; we never detected the smallest amorphous particle with the microscope. Now it is hard to imagine any difficulty in removing sodium carbonate by continued washing with boiling water in excess, from a powder consisting of distinct crystals of calcium carbonate. We can then best account for the presence of the alkali by considering it as having formed itself, a crystalline and insoluble compound with calcium carbonate, this double crystalline carbonate being mixed in small quantity with the superabundant calcium carbonate.

Another experiment was now tried, to prove indirectly if such a double carbonate were present in the crystalline precipitated powders we obtained as already described. We reasoned thus: "If such a double compound exist here, strong ignition ought to decompose it, driving off carbon dioxide from the lime, but leaving sodium carbonate intact," thus



$$Ca\left\langle\begin{matrix}CO_2ONa\\CO_2ONa\end{matrix}\right. = CaO + Na_2CO_3 + CO_2$$

This experiment was tried with a small quantity of the crystalline precipitate. It was well ignited in a platinum crucible, and the resulting mass was treated with some dilute alcohol, which extracted easily a quantity of the soda, showing a strongly alkaline reaction to test-paper. We intend to repeat this experiment quantitatively, and to determine thus the amount of soda extracted.

One point becomes pretty clear by these experiments, viz., that the materials lime and sodium carbonate in contact with water give rise to the formation of this insoluble sodium compound. Also that it is not a case of mere cohesive retention of soda by the lime mud, for our experiments show that until a definite and suitable chemical reaction between the members of the mixture sets in, no appreciable amount of soda is retained, but that when such reaction sets in, in the condition of nascent state and therefore unstable equilibrium, in which the various constituents momentarily find themselves, a major and normal reaction takes place, and also this minor reaction to a small extent—giving us a small yield of the insoluble sodium compound.

The experiments we have yet in view with the crystalline precipitate prepared as mentioned, and also with ordinary lime mud, are (1) the effect of long boiling with water to see if thus the insoluble compound is decomposed, as with Wright in the case of the black-ash; (2) boiling with water containing certain salts in solution, such, *e.g.*, as sodium sulphide. We hope by obtaining a closer knowledge, at all events, of the properties and behaviour of this singular compound, to find at length, peradventure, a practical and ready means for extracting it, and thus doing a service to the alkali manufacturer.

THE AMERICAN CHEMICAL SOCIETY.*

The September Conversazione was held on Thursday, September 23, 1880.

Dr. **Leeds** exhibited a number of new compounds obtained during the course of a research upon the action of hyponitric anhydride upon organic bodies. The most important of the substances obtained thus far had been those derived from benzene and naphthalene. The former dissolves large volumes of hyponitric anhydride, more especially in the cold, and increases greatly in bulk. After long-continued standing the liquid loses its dark red colour, and is found to contain a large amount of bitter almond oil. An abundant crop of crystals of oxalic acid slowly settles out, and this method, though very tedious, is the best one, since, on attempting to distil off the liquid portions, a quantity of dark red oil is formed, which greatly interferes with the subsequent operations. By boiling with water, the unaltered benzene and mono-nitro-benzene are driven off, and on concentrating the solutions thus obtained, a mass of yellow fern-like crystals separates out, which, on recrystallisation from chloroform, proves to be picric acid. In the distillate from the benzene after nitration, two bodies had been obtained which had been separated by means of their different behaviour towards solvents; the one soluble in alcohol, from which it crystallises in slender prismatic needles of a faint reddish yellow colour; the other insoluble in alcohol but soluble in benzene, from which it crystallises in brilliant lemon-yellow prisms. Only the former had as yet been obtained in quantity sufficient for analysis, its formula being C_6H_4O, monoxybenzene. When naphthalene is similarly treated it rapidly liquefies, with the formation of an abundant deposit of needle-shaped crystals of mono-nitro-naphthalene. On exhausting the nitrated mass with water, evaporating the filtered solutions to dryness, and subliming, long flat pearly needles are given off, which on re-sublimation had a melting-point of 131°. Their analyses having given irrational formulæ, they were re-crystallised from hot benzene. On cooling, minute fern-like crystals separated out, and afterwards, on long standing, another crop, which on re-crystallisation formed large monoclinic prisms, perfectly colourless, and of a high vitreous lustre. Both of these bodies proved to be new substances, the former having the composition, $C_{10}H_8O_4$, tetroxy-naphthalene; the latter, $C_{10}H_4O_4$, naphtho-diquinone. Besides the mono-nitro-naphthalene, α-dinitro-naphthalene was obtained by appropriate treatment of the nitrified substance, before its exhaustion with water.

A specimen of paratoluic acid was likewise exhibited, obtained by submitting cymene to the action of hyponitric acid. It was obtained by exhausting the nitrated cymene with boiling water, a small amount of α-nitro-cymene passing off in the distillate at the same time. As yet the action upon anthracene had not been completely studied, but a number of specimens of the anthraquinone, the principal product obtained when anthracene is treated with hyponitric anhydride, were exhibited to the Society.

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* Communicated by Arthur H. Elliott, F.C.S., Recording Secretary.

CORRESPONDENCE.

WELDON MUD.

To the Editor of the Chemical News.

Sir,—An expression in Dr. Post's letter in the Chemical News, vol. xlii., p. 233, may lead some of your readers to suppose that, in the replies which I have had occasion to make to certain papers of his, I have overstepped the limits of fair controversy. Those replies consist simply of a letter published by yourself some months ago, and a paper read before the Newcastle Chemical Society on the 25th March last. The former your readers have seen; but as very few of them can have seen the latter, you will perhaps permit me to say that it deals simply with the published results of Dr. Post's analyses of Weldon mud, showing that they are not in accordance with the results of manufacturing experience, and that if Dr. Post does not find in the product made in the Weldon oxidiser all that part of the CaO unquestionably put into the oxidiser which has not been employed to decompose $MnCl_2$ or other chlorides, either his analysis must be inaccurate, or CaO must have been annihilated. To point out to him that dilemma is surely not to make "attacks" on Dr. Post.—I am, &c.,

Walter Weldon.

Rede Hall, Burstow, November 6, 1880.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Note.—All degrees of temperature are Centigrade, unless otherwise expressed.

Bulletin de la Société Chimique de Paris.

No. 11, June 5, 1880.

Perforation of Zinc Cisterns and Corrosion of Lead Pipes by Water.—X. Rocques.—The author observes that the plates of zinc cisterns are corroded, not uniformly, but in certain well-defined places. The cause of this inequality is the electric current, which is set up between the purer portions of the metal and those more alloyed. Zinc, lead, and copper are attacked very slowly by ordinary water and by saline solutions in general (chlorides, bicarbonates). The corrosion is more rapid if there are several metals in contact. The presence of nitrogenous matters and ammonia accelerates the action, especially in case of zinc. The phenomena display their greatest activity in presence of oxygen. This is the case at the surface where the metal is alternately in contact with water and air. The deposits formed are chiefly silicates and carbonates of lead, zinc, and copper.

Density of the Vapour of Iodine.—J. M. Crafts and F. Meier.—Already noticed.

Formation-Heat of Ammonia.—M. Berthelot.—Already noticed.

Formation-Heat of the Oxides of Nitrogen.—M. Berthelot.—Already noticed.

Observations on a Paper by MM. Vincent and Delachanal on Pure Methyl Cyanide.—A. Gautier.—The physical constants characterising methyl cyanide given by MM. Vincent and Delachanal are almost identical with those published by the author in 1868 and 1869. (*Annales de Chimie*, Series 4, xvii., p. 103.)

Tetric and Oxy-tetric Acids and their Homologues.—E. Demarçay.—This memoir does not admit of useful abstraction.

Detection and Determination of Heavy Mineral Oils, Resin, and Fatty Oils in the Oils of Commerce.—A. Rémont.

Gazzetta Chimica Italiana.
Anno 10, Fasc. 2, 1880.

Physico-Chemical Analysis of Arable Soils.—Prof. Fausto Sestini.—The author concludes from the experiments instituted at an "Agricultural Station," that the method of Schlœsing (communicated to the Academy of Sciences in 1874) is the only one which renders it possible to determine sufficiently well the proportion of clay in cultivated soils. The liquid may without prejudice to the exactness of the operation be allowed to settle for twelve hours, instead of twenty-four as proposed by Schlœsing. The number of washings should be increased from six to twelve, using a small pencil to detach entirely the clay from the sand. Neither the clay nor the sand as thus separated are of constant chemical composition, and it would be preferable to call them clayey matter and sandy matter.

Succinine.—Dr. Angelo Funaro and Leobaldo Danesi.—The authors give $C_7H_{10}O_5$ as the formula of pure succinine.

Colouring Bases Derived from Furfurol.—Ugo Schiff.—The author has examined the behaviour of diphenylamine, meta-nitraniline, para-nitraniline, toluendiamine, and benzidine with furfurol, and has obtained a number of coloured compounds. Diphenylamine unites with furfurol in the proportion of two mols. of the base to one of furfurol. The diphenylamine employed was previously freed from aniline and toluidine. At the heat of 150° there was obtained a brown oily liquid, which at 0° congealed into a crystalline mass. The addition of hydrochloric acid gives a compound of a magnificent bronze colour, which dissolves in alcohol with a very intense crimson.

Certain Compounds of the Myristic Series.—Dr. F. Masino.—The author has examined myristine, myristic acid, myristamide, and myristolic acid.

The Crystalline Form of Nitroso-thymol, Lapacic and Cuminic Acids.—Dr. Ruggiero Panebianco.—Of no value without the accompanying diagrams.

Formation of the Fatty Matter and Ripening of the Olive.—Dr. Angelo Funaro.—The author gives tables showing the respective weights of the pulp and kernel of the olive at different stages of growth, of the average bulk of the pulp and kernel, and of the proportions of moist-dry matter, and matter soluble in ether both of the pulp and in the leaves.

Analysis of Four Samples of Water from Turin.—Ad. Lieben.

Analysis of Four Waters from Turin.—Stanislao Cannizaro.—These papers, the former of which is very voluminous, enter upon the general question of the analysis of water from a sanitary point of view, and discuss the value of various methods.

Revue des Industries et des Sciences Chimiques et Agricoles.
No. 27, 1880.

Analysis of the Incrustations formed in Triple-Action Apparatus, and its Removal.—The apparatus in question is used in the manufacture of beet-root sugar. The deposit varies not merely in different works, but in different parts of the apparatus, and may contain 60 per cent of silica. It is removed by boiling soda-lye and milk of lime in the apparatus.

Pyrogenous Chemical Products Derived from Wood.—A. Bresson.—This paper is the continuation of a controversy, rather commercial than scientific in its objects.

On Lead and its Salts with Regard to Public Health.—Dr. Rœser.—An account of the trades—much more numerous than is commonly thought—in which workmen are exposed to the action of lead, and of the maladies to which they are consequently liable. It appears that the mechanical uses of lead are scarcely less dangerous than its chemical applications.

Chemisches Central-blatt.
No. 25, 1880.

On Glykosamine.—G. Ledderhose.—Glykosamine is a decomposition product of chitine, for which latter the author proposes the formula $C_{15}H_{26}N_2O_{10}$. Two mols. chitine take up 6 mols. of water and form 3 mols. acetic acid and 4 of glykosamine.

MEETINGS FOR THE WEEK.

Saturday, 13th.—Physical, 3. "On the beats of Mistuned Consonances of the Form *h*:1," by R. H. M. Bosanquet. "Note on Prof. Exner's paper on Contact Electricity," by Profs. Ayrton and Perry. "On Action at a Distance," by Walter R. Browne. "Thermo-dynamic Relations of the Thermal and Ergonal Constituents of Heat in Liquids and Gases."
Monday, 15th.—Royal Academy of Arts, 8. "The Chemical and Physical Changes Involved in the Several Processes of Painting," by Arthur H. Church, M.A.
— Medical, 8.30.
Tuesday, 16th.—Civil Engineers, 8.
Wednesday, 17.—Society of Arts, 8,
— Geological, 8.
— Meterological, 7.
Thursday, 18th.—Royal Academy of Arts, 8. "The Conservation and Restoration of Pictures," by Arthur H. Church, M.A.
— Royal, 4.30.
— Royal Society Club, 6.30.
— Chemical, 8. "Notes on the Oxides of Manganese," by Spencer Pickering. "Aluminium Alcohols (Part I, Their Preparation by means of the Aluminium Iodine Reaction)," by J. H. Gladstone and A. Tribe. "On the Synthetical Production of New Acids of the Pyruvic Series (Part II.)," by Edward Moritz.

TO CORRESPONDENTS.

A. T. F.—The "Note on Hydroxyl" is not suitable for insertion, being a suggestion only, quite at variance with known chemical laws.

Answer to W. R. E. Grantham.—(1) Dr. Lunge's "Manufacture of Sulphuric Acid and Alkali," J. Van Voorst, Paternoster Row. (2) "Destructive Distillation," by Dr. E. J. Mills; Van Voorst. This subject requires a whole library if our correspondent wishes to enter into all its branches.

THE CHEMICAL NEWS.

VOL. XLII. No. 1095.

ON THE
MEASUREMENT OF CHEMICAL AFFINITY.*

By C. R. ALDER WRIGHT, D.Sc. (Lond.),
Lecturer on Chemistry in St. Mary's Hospital Medical School.

SOME half century or so ago, affinity was defined in the text-books as a species of attraction exerted between the particles of bodies of dissimilar or different kinds when brought together, whereby there are produced not mere aggregations of particles of similar characters due to cohesion (such as would result were the particles of the same kind), but new groups of particles constituting bodies different in character from the original substances. Thus, on breathing into lime-water, the carbonic acid of the breath was said to unite with the lime, forming carbonate of lime, in virtue of the peculiar affinity which the particles of lime and of carbonic acid have for one another respectively. It was then known by experiment that occasionally one substance could displace another from chemical combination; *e.g.*, copper is thrown down by iron from blue vitriol solution, silver by mercury from lunar caustic, and lead by zinc from sugar of lead, forming "trees." This class of action was explained by saying that the affinity of sulphuric acid for iron was greater than that for copper; and similarly in the other cases, the phrase "elective affinity" being employed to indicate the apparent preference of the sulphuric acid for the copper, of the nitric acid for the mercury, of the acetic acid for the zinc, and so on. Similarly it was known that two salts would often mutually decompose one another, *e.g.*, that carbonate of potash added to nitrate of lime would form carbonate of lime and nitrate of potash, and this was explained by supposing that the affinity of carbonic acid for lime plus that of nitric acid for potash, jointly exceeded the sum of the affinities of carbonic acid for potash and nitric acid for lime; so that the former (referred to as *Divellent affinities*) being in excess of the latter (*Quiescent affinities*), decomposition of the first two compounds and simultaneous recombination of the resulting acids and alkalies in a more stable form necessarily resulted.

It was evident to the thinkers of that epoch (to quote from one of them, Dr. Henry) that "the determination of the precise forces of affinity would be an important step in chemical philosophy, for its phenomena would then be reduced to calculation, and we should be able to anticipate the results of experiment." Although very much yet remains to be done in this field before anything like a sufficiently wide and extended basis for such predictions is obtained for chemical phenomena generally, yet during the last few decades a good deal has been accomplished tending in this direction.

The establishment of the principle of the conservation of energy, with the experimental portion of which work the name of Joule is indissolubly connected, affords the primary means of making such determinations. In the course of the various researches hitherto made on the development of energy (to outward appearance) by chemical actions, the proposition has been abundantly verified that in all such instances there is no real creation of force or increase in the total energy of the universe, but that each body met with in nature has, so to speak, stored up in it a supply of latent or potential energy (somewhat after the fashion in which heat is rendered latent in melting ice or boiling water), and that the actual energy obtained by means of a chemical change (*e.g.*, when coal is burnt under the boiler of a steam engine) is derived from the lessening of the store of potential energy contained in the chemicals employed (the fuel and the atmosphere); so that the development of energy may be put in the form of the equation—

$$A+B+\ldots\ldots=C+D+\ldots\ldots+W$$

or otherwise— $\Sigma(A)=\Sigma(B)+W$

where A and B . . . are respectively the amounts of potential energy latent in the materials employed, jointly $=\Sigma(A)$; and C, D, ... are respectively the amounts of potential energy latent in the resulting products, jointly $=\Sigma(B)$, W being the amount of energy which is converted from the potential into the actual state during the chemical change. Just as the principle of the indestructibility of matter inculcates that the sum of the masses of the reacting bodies is equal to the sum of the masses of the products, no matter what the variation in the ways in which they are associated, so the principle of the conservation of energy tells us that the sum of the actual and potential energy of the materials is equal to that of the products, so that whatever actual energy is apparently gained is really obtained at the expense of the potential energy of the system.

Of course, if this proposition is universal it must equally embrace the case where W has a $-$ sign, that is, when the performance of a chemical change causes an apparent disappearance of energy (*e.g.*, an absorption of heat); in short, if $\Sigma(A)=\Sigma(B)+W$, $\Sigma(B)=\Sigma(A)-W$, so that the inversion of a given chemical change requires the increase of the amount of potential energy of the system by an amount precisely equal to that by which it is decreased by the original change, the increase and decrease of potential energy being each precisely equal to the decrease and to the increase of the actual energy in the inverted and original changes respectively; *e.g.*, in the burning of a given weight of hydrogen and oxygen, together forming liquid water, a conversion of potential into actual energy takes place to a definite extent, usually producing the visible effect of heat evolution. In order to "unburn" the water (to use Dr. Odling's phrase), *i.e.*, to decompose it into the original hydrogen and oxygen, just as much actual energy must become potential, causing a precisely equal heat absorption. The determination of the "precise forces of affinity," referred to by Dr. Henry as so desirable, or, at least, the determination of their resultant in any particular case, is then made by determining the amount of energy which passes from the actual to the potential state, or *vice versa*, during the performance of a chemical change occurring with known masses of reagents.

Hence it results that there are two principal ways in which the extent of conversion of actual into potential energy, or *vice versa*, in any given chemical change may be measured, viz., the determination of the numerical value of W in the chemical change referred to by the equation representing the energy-changes—

$$\Sigma(A)=\Sigma(B)+W,$$

in which potential energy becomes actual; and the determination of the same numerical value in the converse chemical change referred to by the equation—

$$\Sigma(B)=\Sigma(A)-W,$$

in which actual energy becomes potential.

Hitherto most of the measurements that have been made have been performed by the first method, for it is found experimentally that reactions in which actual energy becomes potential as the nett result, on the whole occur very rarely, if at all, at ordinary temperatures at any rate; and when they do apparently occur, in many cases the apparent absorption of heat is demonstrably due to change of physical state of some one of the substances involved in the change; so that the measurements have practically been made by bringing together known masses of materials, causing them to undergo chemical change, and finally measuring

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* Opening Address to the Owens College Chemical Society. October 29, 1880.

by means of the calorimeter the quantity of heat liberated as the nett result of the actions taking place.

Upwards of thirty years ago, however, Joule, and also Woods, experimentally indicated the possibility of applying the second method to the same general purpose, the energy rendered latent or potential being derived not from a spontaneous chemical change, but being subtracted from that which a given electrical current would otherwise yield; *i.e.*, they measured the amount of heat-absorption taking place when certain compounds are decomposed by the electric current (electrolysed) by determining the difference between the heat actually generated, and that which the current would have generated had all the work done by it been performed as heat and none as chemical decomposition. Very shortly afterwards Sir William Thomson worked out the mechanical theory of electrolysis on these principles, and advanced the theoretical aspect of the question to a point beyond which but little progress has since been made. It results from his demonstrations that it is just as convenient to measure energy in terms of electromotive force as in terms of heat, the two measurements being convertible. By pursuing his line of argument and working it out practically, I find that the experimental measurement in terms of electromotive force may be made to yield information about the internal mechanism of a given chemical change, which the calorimetric determination by the first method is unable to give, whilst it equally gives the value of the energy corresponding to the nett result of the total changes ensuing (*vide infra*).

By comparing together the results of determinations of the heat evolved in chemical changes made by Andrews, Favre and Silbermann, Berthelot, Julius Thomsen, and many others, but more especially the two last-named, a large body of information is gradually being accumulated, the importance of which is daily increasing; indeed, it seems highly probable that "thermo-chemistry" will eventually become more important from a theoretical point of view than any other branch of chemistry, and that the fundamental facts of the science will be shown by-and-bye to be natural consequences of the thermal qualities of bodies. As examples it may be noticed that wherever an "elective affinity" is exerted, as when copper is displaced by iron, lead by zinc, &c., the end result of the transaction is an evolution of heat, *i.e.*, such changes only occur when the *heat of combination of the displacing body is greater than that of the body displaced.*

In many cases the energy of combination of analogous elements with another element (measured per equivalent weight of each) is found to decrease in the equivalent increases; for instance in the synthesis of the following compounds:—

Formula.	Weight of Substance in Grms.	Heat Evolution.
HCl	36·5	+22,000
HBr	81	+ 8400
HI..	128	− 6000
H_2O	18	+68,000
H_2S	34	+ 4500
H_2Se	80	− 5400

But this is by no means an invariable rule. Thus certain tellurion compounds produce a greater heat evolution than the corresponding selenion compounds, whilst aqueous iodic acid develops more heat than aqueous bromic acid during synthesis. The combination of cyanogen with potassium evolves less heat than that evolved by the combination of either chlorine, bromine, or iodine with potassium, although these latter follow the same order as the heats of combination of hydrogen with chlorine, bromine, and iodine respectively.

According to Thomsen the quantities of heat given out in many reactions are related to one another after the same fashion as the quantities of chemical substances that combine with one another, *i.e.*, their ratio is a simple multiple of a constant ratio; thus, in many cases he finds that the heat evolution is a multiple of a number lying close to 18,000. Using Thomsen's notation [where the comma means combination, so that (H_2,S,O_4,aq), for instance, means combination of 2 grms. of hydrogen, 32 of sulphur, 64 of oxygen, and an unlimited amount of water of solution], the following specimen numbers may be quoted (*Berichte Deut. Chem. Ges.*, 1873, 239):—

$$(N_2O_2,O\ ,aq) = 2 \times 18{,}170$$
$$(N_2O_2,O_2,aq) = 3 \times 18{,}214$$
$$(N_2O_2,O_3,aq) = 4 \times 18{,}235$$

$$Hg_2Cl_2,aq,Cl_2 = 2 \times 18{,}537$$
$$2FeCl_2,aq,Cl_2 = 3 \times 18{,}270$$
$$SnCl_2,aq,Cl_2 = 4 \times 18{,}469$$
$$SO_2,aq,Cl_2 = 4 \times 18{,}477$$

$$Mn_2O_7aq,5H_2O_2aq,2SO_3aq = 10 \times 18{,}924$$
$$Mn_2O_7aq,5H_2O_2aq,4HClaq = 10 \times 18{,}218$$

$$(Cu,O,SO_3,aq) = 3 \times 18{,}705$$
$$(Cd,O,SO_3,aq) = 3 \times 18{,}094$$
$$(Pb,O,SO_3,aq) = 4 \times 18{,}888$$
$$(Fe,O,SO_3,aq) = 5 \times 18{,}772$$
$$(Zn,O,SO_3,aq) = 6 \times 18{,}077$$
$$(Mg,O,SO_3,aq) = 10 \times 18{,}092$$

$$Br_2aq,H_2 = 3 \times 18{,}551$$
$$Cl_2aq,H_2 = 4 \times 18{,}441$$
$$H_2O_2aq,H_2 = 5 \times 18{,}335$$

And similarly in other cases, a numerical value near to 18,000 being uniformly an exact divisor of the heat-evolution during the synthesis of a substance, even when belonging to very different classes of compounds in different cases respectively.

What I more particularly propose to bring before you to-night is the general result of a large amount of my own personal work in this field of research carried on during the last few years, and in many respects at present only in a partially developed and incomplete state; and I am the more tempted to do so because whilst the field is very wide, the labourers in it are but few, and I am persuaded that a rich harvest of results will reward patient investigation in the domain, and that if by putting before you some ideas concerning certain of the directions in which experiments have been or may be made, I can induce any of you to take up this line of research, both the investigator himself and science generally will be benefitted.

One class of experiments then, on which I have bestowed a large amount of time and attention is the investigation, in conjunction with Messrs. Luff and Rennie, of the circumstances attending certain chemical changes of the "elective affinity" class (*i.e.*, represented by the reaction, $AB + C = A + BC$, where C displaces A from combination with B), which do not take place at the ordinary temperature, but do at temperatures a little elevated, but still readily measurable. The final result of these observations is that a distinct correlation exists between the temperature, the rate at which the action proceeds under constant conditions, and the nett amount of heat generated (or absorbed) by the reaction as a whole. The substances chiefly investigated hitherto have been the metallic oxides and certain reducing agents which will remove oxygen from them (*e.g.*, Fe_2O_3 or CuO, and H_2 or CO), and the general law obtaining in such cases may be put thus:—The greater (algebraically) is the heat-evolution (*i.e.*, the greater the actual evolution or the less the absorption), the lower is the temperature requisite to bring about a given amount of reduction in a given time (under constant conditions), the shorter the time requisite to effect a given amount of reduction at a given temperature, and the greater the amount of reduction brought about at a given temperature in a given time: from which it results as a particular case that a given reducing agent will act to a just measurable extent in a given time (*i.e.*, will just visibly begin to produce reduction) at a lower temperature with a metallic oxide, the reduction of which is accompanied by a greater (algebraic) heat-evolution, than it will on another which evolves less heat during reduction; or that a given metallic oxide will be reduced to a just perceptible extent at a lower temperature by a reducing agent evolving more heat during its union with O, than by one evolving less heat.

The above general law may be expressed graphically thus:—Let the time, the temperature, and the amount of reduction be measured off respectively along three rectangular coordinates, and let the points thus obtained be connected so as to form a curved surface; then the surface corresponding to the lesser heat-evolution under constant conditions always lies inside the other surface.

In actual experiment this is not absolutely always the case; but in the few instances where the normally underlying surface projects above the other one, the cause is always found to be an inequality of the conditions. For instance, when hydrogen and CO act on Fe_2O_3 the actions are not absolutely identical; in each case there is an action of the form—

$$Fe_2O_3+3X=3Fe+3OX,$$

but with the CO there is also a tendency to a secondary reaction—

$$Fe_x O_y + CO = C + Fe_x O_{y+1}$$

where the CO acts as an oxidising agent, which of course diminishes the total nett reducing action of the CO. This interfering action has no parallel with hydrogen; its effect (with one particular kind of Fe_2O_3 examined) was to make a small extent of the surface corresponding to the CO lie inside that corresponding to the H, instead of outside as should be the case in accordance with the law, and as actually was the case with the great majority of the surfaces. Again, CO diffuses less rapidly than H_2 and H_2O vapour than CO_2, so that when reduction has gone on to a certain extent and the particles of substance are coated exteriorly with a film of reduced metal or lower oxide, the penetration through this film of the hydrogen and the removal of steam are both carried on more rapidly relatively than is the case with CO and CO_2; and hence the rate of reduction by CO is somewhat retarded relatively to that of the hydrogen, the effect of which is in certain cases that the hydrogen surface instead of underlying the carbon oxide surface, reaches above it.

A peculiar phenomenon is sometimes noticeable in the earlier stages of a given reaction, viz., a gradual acceleration of the rate of reduction by a process of "chemical induction" precisely analogous to the "photo-chemical induction" studied by Dr. Roscoe in conjunction with Prof. Bunsen. This is most distinctly marked at lower temperatures, and at certain particular temperatures causes the curious result that the amount of action during considerable periods of time (from several hours to days or even weeks), after first bringing together the substances in question, is so small as not to be measurable, but by-and-bye a notable action becomes manifest, the rate of change continually increasing: so that a "period of incubation," so to speak, exists during which the rate of action is too small to be measurable, and after this is over action becomes apparent and measurable. *Cæteris paribus*, the duration of this period of incubation is less the higher the temperature, becoming too brief for measurement at temperatures above a certain limit, and so prolonged as to be practically infinite (at any rate lasting over years) at temperatures below a certain lower limit.

I have recently obtained some interesting results (at present unpublished) by studying, in connection with Messrs. Rennie and Menke, the nature of the curved surfaces obtained during the reduction of the higher oxides of metals which form more than one oxide. When the different oxides of a given metal are formed with about the same amount of heat evolution (per constant quantity of oxygen added on)—*e.g.*, with copper as metal—the curve traced out for any given temperature as reduction proceeds exhibits no abrupt alteration in direction at any point. At first it is concave upwards whilst induction is proceeding, and subsequently it becomes concave downwards when the quantity of reduced substance accumulates to such an extent as to retard the action by covering up mechanically the as yet unreduced, or imperfectly reduced, portion, the point of contrary flexure varying with circumstances, and being lower down, *cæteris paribus*, with CO than with hydrogen, owing to the greater diffusive power of the latter enabling it to penetrate the superficial layer of reduced metal more readily.

But if the heat of formation of the higher oxide is less than that of the lower ones (measured per constant quantity of oxygen added on) the direction of the curve becomes notably altered in the direction of retardation of rate of action when the higher oxide has lost so much oxygen that the composition of the mass is sensibly that of the lower oxide. Thus in the case of manganese dioxide an "angle" in the curve line is noticeable when the mass is so far reduced as to be sensibly Mn_3O_4; and when the mass has the composition MnO reduction at temperatures below a bright red heat stops altogether. When ferric oxide is thus examined an "angle" always exists in the curve at a point representing the composition Fe_3O_4: but none is found at any other point, from which it results that the heat of formation of all oxides of iron up to Fe_3O_4 is sensibly the same per equal amount of oxygen consumed; but the heat of formation of Fe_2O_3 is less; just as the heat of formation of MnO is greater than that of Mn_3O_4, which appears to be greater than that of Mn_2O_3, MnO_2, or any other higher oxide. It is comprehensible, therefore, why intense heating of any higher oxide of manganese, *per se*, may reduce it to Mn_3O_4, but no further, the action stopping at that point, because Mn_3O_4 is more stable as regards tendency to split up by heat than higher oxides. Similarly iron, when oxidised at very high temperatures, burns to Fe_3O_4, and not Fe_2O_3, for the same reason. Presumably, if Fe_2O_3 were very highly heated, *per se*, it would evolve oxygen and become Fe_3O_4.

Another branch of the subject to which I have given attention is that of the nature of the deductions to be drawn from calorimetric determinations in the field of organic chemistry. It has been shown by Berthelot, Thomsen, and Hermann that by comparing together the heats developed during the combustion to CO_2 and H_2O of various organic products, and of the amounts of solid carbon and gaseous hydrogen contained therein, it becomes possible to deduce the amounts of heat that would be generated during the synthesis of the various organic bodies from solid carbon and gaseous hydrogen and oxygen. Thus, in the case of alcohol, experiment shows that when 1 grm. of alcohol is burnt to CO_2 and H_2O, thus—

$$C_2H_6O+3O_2=2CO_2+3H_2O,$$

there are developed about 7000 calories, or on burning 46 grms. 322,000 are developed, whilst on burning the carbon present and the hydrogen separately there would be developed by the first $24\times8000=192{,}000$ calories, and by the second $6\times34{,}000=204{,}000$.

Hence it results that the synthesis of alcohol from 24 grms. of solid carbon, 6 of gaseous hydrogen, and 16 of gaseous oxygen must be accompanied by an evolution of heat to the extent of $204{,}000+192{,}000-322{,}000=74{,}000$ calories.

Similarly, the synthesis of 32 grms. of methylic alcohol from 12 carbon, 4 hydrogen, and 1 oxygen is accompanied by the evolution of—

$$12\times8000+4\times34{,}000-32\times5300=62{,}400,$$

where 5300 is the heat of combustion of 1 grm. of methylic alcohol. Hence the transformation of 32 grms. of methylic alcohol into 46 of ethylic alcohol, by adding on 12 grms. of solid carbon and 2 of gaseous hydrogen (symbolically represented by the replacement of H by CH_3) would be accompanied by an evolution of $74{,}000-62{,}400=11{,}600$ calories. Could this replacement, or rather addition, be effected by a single reaction at the ordinary temperature, so that we should start with methylic alcohol, carbon, and hydrogen, and end with ethylic alcohol, this amount of heat evolution could be observed by means of the calorimeter. Using Thomsen's symbols, this synthesis would be written CH_4O,C,H_2.

In order to make results of this sort comparable a uniform standard of comparison is necessary. The calculations that I have made, based on a particular standard (*Phil. Mag.*, December, 1874), have given the following results, the standard being such that the bodies supposed to be synthesised from C, H, and O are regarded as vaporous at a temperature $t°$ (which for convenience is taken as 100°) under the constant pressure of 760 m.m., the constituent carbon being solid, and the oxygen and hydrogen gaseous, and previously reduced to the same volume at $t°$ and 760 m.m. as would be occupied by the synthesised vapour: so that the calculation amounts to this:—A given quantity of solid carbon and compressed hydrogen and oxygen are supposed to be suddenly transformed into the vapour of a compound occupying the same bulk, and the heat developed in the transformation is measured. By comparing together the amounts of heat development in various such cases, reckoned on equal bulks of vapour (for convenience on such a bulk as would, if reduced to 0 and 760 by the ordinary rules, occupy 22·32 ,litres, a bulk which for convenience may be spoken of as 2 *metropneums*, and the weight of which is 1 *metrogramme, vide Phil. Mag., loc. cit. supra**), information is gained as to the amount of heat absorption or evolution that would accompany the "operations" (as B. Brodie terms them), of conferring upon a given bulk of space weight, and communicating to it the properties characteristic of one kind of matter (*e.g.*, methylic alcohol vapour), and of further communicating to it additional weight and other properties (*e.g.*, ethylic alcohol vapour). That is, the operations in the examples taken are symbolised by firstly synthesising $CH_3.OH$, and then transforming this into $CH_3.CH_2.OH$, this latter operation being one which may be conveniently termed *hydrocarbonous methylation*, to distinguish it from *hydroxylic methylation*, which would be represented by the transformation of $CH_3.OH$ into $CH_3.O.CH_3$.

From the calculations of this kind that I have made, based on the experimental data furnished by Andrews, Favre and Silberman, Regnault, Kopp, J. Thomsen, and others, it results, amongst other things, that:—

(1.) The operation of hydrocarbonous methylation is always accompanied by heat evolution.
(2.) The operation of hydroxylic methylation is always accompanied by heat absorption.
(3.) Conversion of a group—CH_2—into the group —CO—is accompanied by heat evolution.
(4.) Any operation causing heat evolution gives rise to a body of higher boiling-point than the one operated on, and *vice versa.*

As an example of (1) and (4) the members of all true homologous series (such as methylic and ethylic, &c., alcohols) *boil at gradually increasing temperatures*, and the formation of each member by methylating the previous one *gives rise to heat evolution*. As an example of (2) and (4), the formation of a methylic ether of an alcohol or acid *gives rise to heat absorption, and to lowering of boiling-point.* Water on its successive conversion into methylic alcohol and methylic ether affords a good example of this, each hydroxylic methylation absorbing heat and lowering the boiling-point; whilst conversion of methylic alcohol into ethylic (hydrocarbonous methylation) evolves heat and raises the boiling-point. Conversion of hydrogen into marsh-gas evolves heat, *ergo*, marsh-gas should have a higher boiling-point, *i.e.*, be more readily condensable to the liquid state.

In the case of (3) and (4), an acid is formed from a primary alcohol with *evolution of heat and great increase in boiling-point.* Conversion of water into free oxygen by replacement of H_2 by O, however, is accompanied by heat absorption and lowering of boiling-point, so that replacement of H_2 by O does not necessarily always evolve heat, unless associated with carbon, just as hydrocarbonous and hydroxylic methylation are different.

The third and chief class of experiments on which I have worked recently, and am still engaged, is that of the carrying out into practice and further development of the electrochemical method of Joule, Woods, and Sir W. Thomson, referred to above. This method is based on the following principles:—Faraday showed that (with moderately strong currents, at all events) the passage of a given quantity of electricity through an electrolyte always produces an equivalent amount of decomposition no matter what the nature of the electrolyte,* so that the weight of matter decomposed n grms., say, varies as Q, the quantity of electricity passing, and as a the equivalent of the body; whence n varies as Qa, and hence $n = Q\,a \times \chi$, where χ is a constant independent of the equivalent or of the quantity of electricity.

Now the total work done during the passage of a quantity of electricity Q from one point to another, the potentials of which differ by E, is E Q. Suppose that in the electrolysis all the work is done as chemical decomposition and none as heat evolution; then if H is the heat evolved during the synthesis of 1 grm. of electrolyte from the products of decomposition, n H J represents the work done, whence—

$$n\,H\,J = E\,Q = E\frac{n}{a\chi},$$

or,

$$E = H\,a\,\chi\,J = H_1\,\chi\,J.$$

Where H_1 is the heat evolved during the synthesis of a grms. (1 grm. equivalent) of electrolyte. So that the energy corresponding to the synthesis of a grm. equivalent of electrolyte may be expressed either by $H_1\,J$, or by $\frac{E}{\chi}$: that is, just as J represents a factor by which a heat value must be multiplied to obtain the corresponding energy value, so $\frac{1}{\chi}$ represents an analogous factor by which an E.M.F. value must be multiplied to obtain the corresponding energy value: or, in other words, just as work—*i.e.*, affinity—may be expressed in terms of heat units, so it may also be expressed in terms of E.M.F., the first value being transferable into the other by multiplication by the factor $\chi\,J$. To χ I propose to apply the term *electro-chemical constant*; its numerical value on the C.G.S. system, electromagnetic units, is 0·000105, that of J being 41,500,000, or $41{\cdot}5 \times 10^6$ (Joule's water-friction value). That is, one heat unit (grm. of water raised 1°) represents $41{\cdot}5 \times 10^6$ C.G.S. work units = 41·5 megalergs: whilst one C.G.S. work unit represents 0·000105 C.G.S. units of E.M.F. equal to $1{\cdot}05 \times 10^{-12}$ volts. (1 volt. = 10^8 C.G.S. unit): so that 1 heat unit corresponds to $41{\cdot}5 \times 10^6 \times 0{\cdot}000105 = 4357{\cdot}5$ C.G.S. units of E.M.F., or to 0·000043575 volt.; and 1 volt. corresponds to 10^8 C.G.S. E.M.F. units—

$$= \frac{10^8}{41{\cdot}5 \times 10^6 \times 0{\cdot}000105} = 22949 \text{ heat units.}$$

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[*] The term "metropneum" is used to indicate the particular value of the expression—

$$11160 \times \frac{273+t}{273} \times \frac{760}{p}$$

for any given temperature t and pressure p, representing the bulk in cubic centimetres that 1 grm. of hydrogen would occupy at that pressure and temperature. The term "metrogramme," representing the weight in grms. of two metropneums of the particular vapour referred to, hence indicates more conveniently the mass sometimes referred to as a "gramme-molecule," or as the "weight of two volumes of vapour."—*Vide* also CHEMICAL NEWS, July 18, 1873, p. 25.

[*] Various experimenters—*e.g.*, Faraday himself and Favre—have considered it possible for very feeble currents of electricity to traverse an electrolyte without producing decomposition; *i.e.*, they supposed that conduction without electrolysis can take place. This has been denied by others. I have recently rigorously examined the point, with the result of finding that *a given quantity of electricity passing always does produce the same equivalent amount of decomposition no matter how slowly it passes*, *i.e.*, no matter how minute the current is made.

In actual practice a current never does decompose an electrolyte without also developing heat; but if this heat be estimated by a calorimeter and called h, the total work done in passing between two points of potential difference, E, during the decomposition of a grm. equivalent of substance will be—

$$\frac{E}{\chi} = (H_1 + h)J,$$

whence $H_1 \chi J = E - h \chi J$. If, then, e be the E.M.F. representing the chemical work done during electrolysis—

$$e = H_1 \chi J = E - h \chi J.$$

Hence the affinity between the final products of an electrolyte (*i.e.*, the work done in resolving it into these final products) is readily determinable by determining the difference of potential E subsisting between the electrodes, and the heat evolved as such, h, during electrolysis of a grm. equivalent. In this way I have found (in conjunction with Mr. Rennie) that the affinity of oxygen and hydrogen gases to form liquid water at 15° is almost exactly 1·5 volts.

It is noticeable that the accuracy of this determination depends on the accuracy with which J is measured, and on that with which the electrical standards from which E is deduced are determined, that is, finally, on the determination of the B.A. unit of resistance. When Joule's water-friction value for J is taken, and the B.A. unit, there is found to be a discrepancy of about 1·5 to 2 per cent between the results as to affinity of H and O in water got by the electrolytic method and by direct combination; whilst the two results agree absolutely if the B.A. unit is supposed to be 1·5 to 2 per cent in excess of the true value through errors unavoidably made during its determination; and hence our experiments coincide with those of Joule and other observers, from whose work it results that if Joule's water-friction value for J be exact, the B.A. unit is really too large to the extent of 1·5 to 2 per cent. On the other hand, certain other observers' work indicates that the true value of J is somewhat higher than Joule's water-friction value, and that the B.A. unit is undervalued, not over estimated; whence fresh authoritative determinations (of the B.A. unit especially) are desirable.

The amount of heat, h, which is actually generated in an electrolytic cell depends upon two things: firstly, the amount of resistance, R, which is exerted by the electrolytic fluid, the effect of which is to cause the development of a quantity of heat equivalent to an amount of work expressed by C^2Rt (C=current, t=time in seconds during which it flows), in accordance with the law established by Joule and known by his name: and, secondly, the heat that is generated by secondary changes that ensue in the voltaic cell. The first quantity can readily be determined; so that finally the heat corresponding to the secondary changes is known (in terms of E.M.F.) by subtracting from E (the difference of potential between the electrodes) the value of e (the E.M.F. representing the work done in breaking up the compound into the final products), and also the E.M.F. representing C^2Rt, that is CR (since $Q = Ct$); that is, if e' be the E.M.F. representing the work done by secondary changes,—

$$e' = E - (e + CR), \text{ or}$$
$$e + e' = E - CR.$$

Now the quantity $e + e'$ is what is generally spoken of by physicists as the "Counter E.M.F. set up during electrolysis," or sometimes as "polarisation" (which latter term, however, is often also used in other senses), and admits of easy measurement by electrical means, so that by means of this measurement, combined with the determination of e (either by direct synthesis and measurement of heat evolved, or by the electro-chemical method just described), the value of e' can be determined.

All existing data show that the value of e' is variable with the conditions of the experiment. I am engaged in studying the effects of the variations produced by altering the current strength, the size of the electrodes, their material, the strength and nature of the electrolytic fluid, the temperature, &c.; and as the result of my own experiments at present made (not yet published), and those of previous investigators, am led to the following general theorem.

When a voltaic current is made to cause electrolysis, it would, were no interfering causes at work, split up the electrolyte into certain definite primary (nascent) products of such a nature as to evolve heat during their transformation into the final or secondary products (possibly into free atoms in the first instance, which would unite by a secondary action into molecules), so that a constant E.M.F. would be requisite to decompose the body into the nascent products, the numerical value of which would exceed the E.M.F. requisite to decompose it into the final products.

But, owing to the attractive action exerted by the electrodes, the products of electrolysis are more or less condensed, and in consequence the end result of the electrolysis is to decompose the electrolyte only partially into the nascent products, the remainder being evolved in a condensed form; in the case of a solid product in the form of the ultimate resulting solid modification; in the case of a gas into a quasi-liquefied or quasi-solidified form, such as that exhibited by the film of gas condensed on a surface by surface attraction or occlusion (the energy of which condensation is very considerable). Finally, therefore, the E.M.F. $e + e'$ actually requisite to break up the electrolyte is expressed by the formula, $e + e' = E_2 - n(H_1 + h_1)\chi J$, where H_1 is the heat evolved in transforming a grm. equivalent of nascent products into the ultimate products, and h_1 that evolved in the condensation of a grm. equivalent of the ultimate products (when gaseous) into the quasi-liquefied form, n being the fraction of the total products (viewed as 1) which is not developed in the nascent state, but is in the condensed form, and E_2 the value which $e + e'$ would have, were all the products evolved nascent, and consequently $= e + H_1\chi J$. Hence, the expression—

$$e + e' = E_2 - n(H_1 + h_1)\chi J$$

may be written—

$$e + e' = e + H_1\chi J - n(H_1 + h_1)\chi J,$$

whence—

$$e' = [(1 - n)H_1 - nh_1]\chi J,$$

the superior limit of e' (when $n = 0$) being $H_1\chi J$, and the inferior limit (when $n = 1$) being $-h_1\chi J$, and hence being 0 in the case of a solid product, when $h_1 = 0$. When h_1 is not $= 0$, *i.e.*, when gaseous products are evolved (such as oxygen, hydrogen, or both together as when water is decomposed), any, the smallest, rate of flow of electricity will set up a "polarisation," of amount capable of being reduced indefinitely, near to 0, by reducing the current sufficiently; and hence it results that the value of e', which is then more nearly at the lower limit than it can be under any other circumstances, must then approach indefinitely near to $-e$, so that at the actual limit e' is at least as great numerically as e; that is, the degree of energy expended in condensing the evolved gases to the quasi-liquefied or quasi-solidified form is as great as, if not greater than, the energy requisite to break up the electrolyte into the final products. Thus, in the case of water the sum of the energy of condensation of oxygen and hydrogen jointly on non-oxidisable electrodes (carbon, gold, Pt, &c.) is at least equal to that represented by 34,000 calories. In the case of copper sulphate where oxygen is the only gas evolved, the energy of condensation of the oxygen on Pt is apparently not less than that represented by about 28,000 calories, the heat that would be evolved during formation of a grm. equivalent of $CuSO_4$,aq from Cu, gaseous oxygen, and dilute H_2SO_4.

It is impossible to determine directly the value of e' at the other higher limit when $n = 0$; but its value can be approximately determined by a method, the principle of which is the same as that used by Crova in his experiments on "polarisation," viz., by plotting as a curve the corresponding values of current and "polarisation" obtained in a great many instances, calculating the equation to the curve (a logarithmic one), and thence deducing the limiting value of the ordinate when the abscissa becomes infinite. To do this accurately requires an enormous amount of laborious work in order to overcome difficulties and avoid sources of error. The first rough approximation in this direction that I have obtained (conjointly with Mr. Rennie), indicates that the highest value of e' for water is at least as great as e, *i.e.*, that the energy requisite to convert 1 grm. of ordinary hydrogen and 8 of ordinary oxygen gases jointly into the allotropic modifications or "nascent forms" of these bodies, in which they are primarily evolved by electrolysis, is at least 34,000 calories, of which probably the major half is due to the hydrogen. Admitting that the "nascent forms" are separate atoms, and the final products molecules, it results that the heat development in transforming atoms of hydrogen into molecules thereof, together with that in transforming atoms of oxygen into molecules of oxygen, is not less than the heat developed during the union of these molecules of H and O to form molecules of liquid water.

INTRODUCTORY LECTURE TO THE COURSE OF METALLURGY, AT THE ROYAL SCHOOL OF MINES.

Session 1880-81.

By W. CHANDLER ROBERTS, F.R.S.,
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The distinguished metallurgist who has held this lecturership since the foundation of the Royal School of Mines concluded the introductory lecture he delivered more than a quarter of a century ago[*] by pointing out, to the students who were then beginning their course, that "in proportion to the success with which the metallurgic art is practised in this country, will the interests of the whole population, directly or indirectly, in no inconsiderable degree be promoted." This is a fact that none of his students are likely to forget.

Looking back on the actual advance of this country during the past thirty years, and remembering that the success with which any manufacturing art is practised must bear a direct relation to the way in which it is taught, we cannot but feel how greatly this development of metallurgical knowledge must have been influenced by Dr. Percy's labours. During this period the conditions under which metallurgy is practised have changed considerably; for the field of knowledge has so widely extended, the scale on which operations are conducted is now so great, and the mechanical appliances they involve are so varied and complicated, that while the interest of our subject is deepened its difficulty is gravely increased.

On the other hand, the preliminary training of the students before entering the school has much improved, although it still falls short of that required on the Continent—in France, for instance, where admission to the higher Mining Schools, such as the Ecole des Mines, is only granted to those who have either passed through the Ecole Polytechnique, graduated in science, or have passed a rigorous examination. While not advocating a too comprehensive preliminary training, I am none the less glad to see from the class lists that I am fortunate in having to lecture to students, many of whom have already distinguished themselves in chemistry, physics, and mechanics.

In turning to the history of metallurgy, more especially in its relation to chemical science, it is easy to be led away by the charm of the antiquarian riches of our subject into devoting too much time to this kind of literary research: I may remind you, however, that much of what is both interesting and full of suggestion, even at the present day, is to be found buried in the treatises by the old writers whose work we inherit and continue.

Primitive metallurgical processes are referred to in some of the oldest known historical records; naturally, therefore, the development of metallurgy as a science must have been long preceded by its practice as an art, an art for which a place has even been claimed among the religious systems of antiquity.[*] The earlier literature of the subject consists mainly of descriptions of processes; but it is well known that chemistry was to a great extent built up on a metallurgic basis, and Black's singularly advanced definition of Chemistry as the "effects produced by heat and mixture"[†] might well be applied to Metallurgy. The library of Leyden contains a papyrus which has been described by M. Reuvens,[‡], and is considered by Kopp[§] to be the oldest known chemical MS., its date being possibly as early as A.D. 200. It treats generally of metallurgical matters, and the purification of gold and silver is frequently mentioned. But of all the phenomena of our subject probably none have more contributed to advance the science of chemistry than those bearing upon the relations between oxygen and lead; indeed the interest attaching to the mutual behaviour of these two elements is so great that I propose devoting a few minutes to its consideration, more especially as I am anxious to indicate the influence of an ancient process on the scientific views of the present day.

When lead is melted with free access of air, a readily fusible substance forms on its surface. This substance may be allowed to flow away, or, if the metal is contained in a suitable porous receptacle, the fusible oxide sinks into this containing vessel; in either case the oxidation of the lead affords a means of separating it from precious or inoxidisable metals if any were originally present in the lead. The above fact has been known from remote antiquity, and the early Jewish writers allude to it as old and well known. They clearly show, for instance, that lead can be removed from silver by being "consumed of the fire," while the silver is not affected. That the Greeks knew and practised the method is abundantly proved, if only by certain specimens of gold and silver now in the adjoining museum, which were recently discovered by Dr. Schliemann. The Arabians investigated the subject; for passing to Geber,[||] the greatest of the early chemists (he died in 777), we find a remarkable account of cupellation; he also describes the conversion of lead into a fine powder by calcination with much clearness, and he noticed the fact that after calcination the mass has "acquired a new weight in the operation." I think his subsequent observations on the reduction of altered metals from their "calxes" show that he knew the weight to be increased: in any case it is interesting to observe that his work was, in a sense, quantitative. He, moreover, was cognisant of the fact that two different substances may be produced by heating lead in air, and he assumed that "in the fire of calcination a fugitive and inflammable substance is abolished." The alchemists refer continually to the subject, and "deliver themselves,"—as Roger Bacon said, in his "Speculum Alchimæ,"—"in the enigmas and riddles with which they clouded and left shadowed to us the most noble science." In the middle of the sixteenth century the truly accomplished metallurgist Biringuccio,[¶] con-

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[*] *Records of the School of Mines*, vol. i., pt. 1 (1852), p. 127.

[*] Rossignol, "Les Métaux dans l'Antiquité" (1863).

[†] Lectures by Joseph Black, M.D., vol. i., p. 8 (Edin., 1803).

[‡] Reuvens, "Lettres à M. Letronne" (Leide, 1830), quoted by Prof. Ferguson in an Address to the Glasgow Phil. Soc. (1876), p. 19.

[§] *Beiträge zur Geschichte der Chemie*, 1869.

[||] The Works of Geber, translated by R. Russell (1686), pp. 74, 78, 220, 234.

[¶] "Pirotechnia" (Vinegia, 1540), translated into French by T. Vincent (Rouen, 1627), p. 41.

temporary of Paracelsus and Agricola, seems to have been specially attracted by the phenomenon in question, and he remarks :—“ If we had not lead we should work in vain for the precious metals, for without its aid we could not extract gold or silver from the stones containing them . . . The alchemists also,” he said, “ make use of it in their operations, calcining it by itself or with other substances ; but,” he goes on to observe, “ the calcination, conducted in a reverberatory furnace is accompanied by a marvellous effect, the result of which should not be passed by in silence ; for lead thus treated increases 10 per cent in weight, and, considering that most things are consumed in the fire, it is remarkable that the weight of lead is increased and not diminished.” Although he subsequently gives evidence of much accurate knowledge of practical metallurgy, his views as to this particular phenomenon were hardly in advance of Geber’s ; but we may claim Biringuccio as an early metallurgist, who knew the facts, and recognised that they were theoretically important. It was not until nearly a century later (1630) that a French chemist, Jean Rey,[*] stated that the increase in weight came from the air. The problem attracted much attention in England ; and it is not a little interesting that among the very first experiments recorded by our own Royal Society is a metallurgical series relating to the weight of lead increased in the fire on the “ copels ” at the assay office in the Tower, the account being brought in by Lord Brouncker in February, 1661.[†] Subsequently, in 1669, John Mayo showed that the increase in weight of calcined metals was due to a “ spiritus ” from the air.[‡] Boyle heated lead in a small retort,[§] and attributed the increase in weight, as Lemery also did,[||] to his having “ arrested and weighed igneous corpuscles.”[¶]

I need hardly point out how important this calcination of lead was considered by those who defended the Phlogistic theory in regard to chemical change, a theory which, for more than a century, exerted so profound an influence on scientific thought ; as, however, we owe it to a metallurgist, Becher, I cannot dismiss it without a few words. [Here is the book in which his views were first embodied, the “ Physica Subterranea ” (1669), and here is his other celebrated work, the “ Alphabethum Minerale ” (1682).]

According to his still more famous pupil Stahl, the litharge produced by the prolonged calcination of lead in air is lead deprived of its phlogiston; but he and his followers were indifferent to the fact that when lead is burnt the weight of the resulting mass is greater than that of the original metal, and were content to insist that the burnt lead had lost its inflammable principle—that is, Phlogiston. Lavoisier, as we know, overthrew the old phlogistic theory by showing that a chemical combination takes place, resulting in an augmentation of weight which represents the exact weight of the gaseous body added. At the same time it should be remembered that the phlogistic chemists made a great step in advance, as was admitted by J. R. Mayer in his memoir on the mechanical theory of heat[**] and Odling, discussing the experiments on the oxidation of lead, has more recently pointed out[*] that an error has arisen in consequence of the same word being used in a different sense at different periods of time : chemists, in fact, now substitute the words potential energy for phlogiston ; or, as Dr. Crum-Brown has well observed[†], we recognise “ that no compound contains the substances from which it was produced, but that it contains them *minus* something. We now know what this something is, and can give it the more appropriate name of potential energy ; but there can be no doubt that this is what the chemists of the seventeenth century meant when they spoke of phlogiston.” It will thus be evident that the main aim of chemical investigation down to the end of last century was the explanation of calcination, combustion, or oxidation, and that lead was especially useful in solving the problem.

I might, perhaps, add that the *absorption* of oxygen by molten litharge has furnished M. Ste.-Claire Deville,[‡] a physicist and metallurgist, with an important step in the argument as to dissociation, and thus connects the history of the metal with the great advance on the borderland of chemistry and physics in modern times, to which I shall constantly refer.

The above remarks will, I trust, be sufficient to show that conclusions of the utmost importance in the history of chemical theory were based on a very ancient metallurgical process ; but I have also selected lead as an illustration, because, in the gradual development of the knowledge derived in the first instance from its metallurgy, there is much that is typical of the mutual relation of theory and practice that still prevails.

When Dr. Percy began his teaching he considered at some length the kind of assistance that other sciences might be expected to render our subject, considered as a manufacturing art ; and this at the time was necessary for two reasons :[§] first, because he was “ able to adduce from his own observation several striking cases in illustration of the advantage of the application of science to practical metallurgy ; and, second, because the practice of metallurgy, so far as relates to magnitude of operation, having been developed to an unparalleled extent in this country in the absence of specific public instruction on the subject, it was necessary to justify the providing of such instruction.”

The absence of accurate knowledge on the part of those engaged in metallurgy was lamented as long ago as 1700, in an “ Inaugural Dissertation of Pyrotechnical Metallurgy,” delivered, on the 25th of March of that year, in the University of Magdeburg ; no less a person than the great supporter of the theory of Phlogiston, George Ernest Stahl, presided, and the lecturer was Fritschius, who said[||]—“ If in any part of the working of metals there is commonly more owing to experience than reason, truly it is in fusion or melting ; . . . nevertheless if the reason be asked why the business succeedeth well in this way, but in another doth not succeed at all, you have no solid answer, but only that most general one, which is most commonly false, viz., that one fire is stronger and another weaker, and so insufficient.” It is just a century since Bishop Watson, Professor of Chemistry and Regius Professor of Divinity in the University of Cambridge, pointed out[¶] that “ the improvement of metallurgy, and other mechanic arts dependent on chemistry, might best be made by public establishment of an Academy, the labours of which should be destined to that particular purpose ;” and the School of Mines, thus foreshadowed, was established in 1851, its principal object being to “ discipline the students thoroughly in the principles of those sciences upon which the operations of the miner and metallurgist depend.”

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[*] “ Essays de Jean Rey (reprinted in Paris, 1777), p. 64.

[†] “ MS. Register Book of the Royal Society.”

[‡] *Tractatus quinque Medico-Physici*, p. 25 *et seq* (Oxonii, 1674).

[§] “ Collected Works,” vol. iii. (1744), p. 346.

[||] *Cours de Chymie* (1675), 2nd English Edition (1686), p. 107.

[¶] I am indebted to my friend, Prof. Ferguson, M.A., of the University of Glasgow, whose eminence as a historian of chemistry is well known, for several interesting additional facts in connection with the calcination of metals. After referring to Eck (1489). Glauber (1651), and others, he writes :—“ One of the most curious passages I know is in the ‘ Hippocrates Chemicus ’ of Otto Tachen, or Tachenious, a German who lived at Venice and published his book there in 1666. He describes how lead, when burnt to minium, increases in weight. This incease he ascribes to a substance of acid character in the wood used for burning, and then, by a very curious course of argument, based on the saponifying powers of litharge, makes out that lead is of the nature of or contains an alkali, which combines with the ‘ occult acid of the fat.’ This is a curious anticipation of a very modern classification which brings lead into relationship with the alkalies and alkaline earths, as well as of Chevreul’s investigations.”

[**] “ Bemerkungen über die Kräfte der unbele Natur,” *Liebig’s Ann.*, xlii. (1842), p. 233.

[*] *Proc. Roy. Inst.* (1871), vol. vi., p. 323.

[†] *Edin. Roy. Soc. Proc.*, v. (1866), p. 328.

[‡] “ Leçons sur la Dissociation,” 1864.

[§] *Records of the School of Mines*, vol. i., pt. 1 (1852), p. 128.

[||] “ Pyrotechnical Metallurgy,” by J. C. Fritschius, of Schwartsburg (translated in 1704), p. 203.

[¶] “ Chemical Essays,” 2nd edition (1782), vol. 1., p. 47.

Our honoured founder, Sir Henry de la Beche, in his Address at the opening of the School of Mines,* said:—"We still too frequently hear of practical knowledge, as if in a certain sense opposed to a scientific method of accounting for it, and as if experience, without that advantage, was more trustworthy than the like experience with it." Such remarks might, with truth, be made at the present day; but it should nevertheless be remembered that many metallurgical works are successfully conducted in this country by so-called practical men. I do not mean the kind of man so forcibly described by Mr. Bramwell† as one "whose wisdom consists in standing by, seeing, but not investigating, the new discoveries which are taking place around him . . . . the aim and object of such a man being to ensure that he should never make a mistake by embarking his capital or his time in that which has not been proved by men of large hearts and large intelligence;" nor do I mean the man who accepts no rule but the rule of thumb; but I do mean practical men possessing technical knowledge of a high order, whose careful observation enables them to use the results of past experience in dealing with circumstances and conditions analogous to those they have met with before, and with which long practice has made them familiar. It would be difficult to overrate the value and importance of such knowledge as theirs, and, when we remember the scale on which smelting and other operations are carried on, it will be obvious that this kind of knowledge can only be gained in the works, and not in the laboratory or lecture-room; for, however careful the metallurgical teaching here may be, it can only be practical in a limited sense. At the same time it must be borne in mind that a man trained to scientific methods starts with the enormous advantage of being able to deal with circumstances and conditions that are new to him, and with which, therefore, he cannot be said to be "familiar." The technical skill that time and opportunity can alone give him will then rest on a solid basis. I repeat, however, that I am anxious at the outset to guard against undervaluing the teaching of experience unaided by reasoning that we should recognise as scientific; for it is only necessary to witness such operations as the roasting of a large mass of ore on the bed of a furnace, or the forging of many tons of iron under a steam-hammer, to appreciate the value of the subtle skill of sight and touch on which success depends.

I have thus ventured to trace the relation between scientific and technical men, as hitherto there have been misunderstandings on both sides, or, as Dr. Williamson so well observes‡:—"Men of detail do not sufficiently appreciate the value and usefulness of ideas, or of general principles; and men of science, who learn to understand and control things more and more by the aid of the laws of nature, are apt to expect that all improvements will result from the development and extension of their scientific methods of research, and not to do justice to the empirical considerations of practical expediency, which are so essential to the realisation of industrial success in the imperfect state of our scientific knowledge."

While it is no longer necessary to justify the scientific teaching of metallurgy, as Dr. Percy did, it is as important as ever that the true relation of theory and practice should be clearly understood. It rarely happens that a process can be transferred from the laboratory to the works without important modifications; and we must remember that metallurgy is a manufacturing art, and that, when the truth of a theory has been demonstrated, a dividend has to be earned; this would indeed often be difficult without the aid of the technical man. Practical men have, however, ceased to undervalue science, and the most practical body of men in the world, in the best sense of the term, the Iron-masters of this country, on whom its prosperity so largely depends, formed themselves ten years ago into an Iron and Steel Institute, many of the members of which possess high scientific attainments and are distinguished for scientific research.

Let us turn, then, to the advice given us by those who are accustomed to deal with metals on a large scale. Mr. I. Lowthian Bell stated in his address as President of the Institute in 1873*:—"If we would avoid the failure of what may be designated unscientific practice, or the failure of impracticable science, we must seek to combine commercial intelligence with a knowledge of those natural laws which form the only trustworthy groundwork of the complicated processes in which we are engaged."

Dr. Siemens† said in 1877:—"It is not many years since *practical* knowledge was regarded as the one thing requisite in an iron-smelter, whilst *theoretical* knowledge of the chemical and mechanical principles involved in the operations was viewed with considerable suspicion;" and he adds, with reference to the teaching of the School of Mines and of a general Technical University:—"But it must not be supposed that I would advocate any attempt at comprising in its curriculum a practical working of the processes which the student would have to direct in after-life. . . . . Let technical schools confine themselves to teaching those natural sciences which bear upon practice, but let practice itself be taught in the workshop and in the metallurgical establishment."

The President for 1879, Mr. E. Williams, a most eminently practical man, and one of the founders of the prosperity of the great Cleveland iron district, urged‡ "educated intellectual young men, who now hang listlessly about the professions, ... to break through the absurd old prejudice against seemingly rough work," in order that they may act as scientifically trained managers.

I have thus appealed to authorities, because my own practical work has been mainly confined to a limited branch of metallurgy. I say limited, for, although, on looking into the matter, I find, to my surprise, that I have during the last ten years been responsible for the fineness of 330 tons of gold and 740 tons of silver, this, though of a total value of 47 millions sterling, is a comparatively small bulk of metal, and the operations through which it passes are seldom complicated; but I am none the less convinced that in metallurgical works generally, as in a Mint, the work can only be efficiently conducted by taking advantage to the utmost extent of the aid that science has to offer, a Mint only differing from other works by the extraordinary care and vigilance which must be exercised to ensure accuracy and avoid loss in dealing with the precious metals. Even this difference is less marked than formerly, and, as attention to minute details is becoming more and more essential to the profitable conduct of works, my experience in this respect will be useful to you.

On the Continent, as is well known, the government inspectors and engineers are far more numerous than in this country, where both the extraction and utilisation of metals are left largely to private enterprise. One result of this official influence is that it enables students in the mining schools to visit and report on works as part of their educational course. I hope that it may be possible to arrange for systematic visits of students to manufactories in this country. I have already received courteous promises of aid from several owners, and the wide distribution of our own men throughout the country will doubtless help to render this easy.

As regards the actual training in the school, I believe that our utmost efforts should be devoted to giving the students a thorough acquaintance with scientific methods and metallurgical principles, furnishing them at the same time with as many well-ascertained *facts* as possible. Here I may, perhaps, be permitted to quote a few words

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* *Records of the School of Mines*, vol. i., pt. 1 (1852), p. 20.
† "British Association Report, Brighton" (1872), p. 238.
‡ "A Plea for Pure Science" (Inaugural Lecture, University College, London, 1870).

* *Journal of the Iron and Steel Institute* (1873), No. 1, p. 12.
† *Ibid.* (1877), No. 1, p. 7.
‡ *Ibid.* (1879), No. 1, p. 24.

from Prof. Huxley's[*] address at Birmingham on Friday, as they bear so directly on our subject; he said, "What people call applied science is nothing but the application of pure science to particular classes of problems. It consists of deductions from those general principles, established by reasoning and observation, which constitute pure science. No one can safely make these deductions until he has a firm grasp of the principles; and he can obtain that grasp only by personal experience of the processes of observation and of reasoning on which they are founded."

In one important branch of metallurgy—assaying—the teaching in the School is thoroughly practical, and the operations you may in future be called upon to conduct will not differ from those taught in this laboratory. The teaching will, I am glad to say, be now specially entrusted to my friend Mr. Smith, the value of whose instruction, in my own case, I gratefully acknowledge.

It can hardly be questioned that, until the School of Mines was established, the metallurgical success and reputation of this country rested to a remarkable extent on the exceptional skill of its technical men. I think, therefore, we may be fairly asked to consider whether the metallurgical teaching of the School has been justified, and how far advance has been due to trained scientific thought.

(To be continued.)

CORRESPONDENCE.

ELECTRIC *VERSUS* GAS LIGHTING.

To the Editor of the Chemical News.

Sir,—In reading the very clear and comprehensive lecture on the subject of Electric Lighting (Chemical News, vol. xlii., p. 227), I was greatly pleased to find that so competent an authority as Mr. J. W. Swan is convinced of the fact that electric lighting by means of incandescent carbon is more economical than lighting by gas. In Mr. Swan's illustrations, however, I do not see that the original cost of the steam-engine (or gas-engine) and of the dynamo-electric machine has been taken into account. Besides the original cost of machines, there is the important matter of depreciation: the wear and tear of the current generators must be especially great. Further, the wages of the staff of engineers and workmen, and the cost of the distributing plant, will be no mean items. On page 231 Mr. Swan takes the gas at its price when delivered to the consumer; while the electric light is estimated to cost nothing more than the gas used by the engine. Probably when the above corrections have been made the cost of the electric light will still be less, or not more, than that of gas. If so, the days of gas lighting are clearly numbered; for—leaving out of consideration the greater beauty and whiteness of the light emitted by the incandescent carbon—the fact that it will neither heat the air of the room in which it is used, nor contaminate it by replacing its oxygen by the products of combustion, give the latter such an unquestionable and valuable superiority, that the education of the public only is necessary to bring it into general use.—I am, &c.,

C. B. Caswell.

13, George Road, Edgbaston, Birmingham,
November 9, 1880.

THE SPECIFIC ROTATORY POWER OF CANE- AND INVERT-SUGAR.

To the Editor of the Chemical News.

Sir,—With your kind permission I intend ventilating the questions and points brought forward by Mr. Allen, as it seems to me not desirable that sugar chemists should accept such statements apodictically. In regard to the specific rotatory power of a substance dissolved in an inactive liquid we have to remember that [α] is not constant, and varies more or less with the number of active molecules in solution. It requires, therefore, a whole series of optical observations for different concentrations, and such have up to now been made only for cane-sugar (Tollens, Schmitz), for dextrose (Tollens), lactose (Meissl), and for milk-sugar (Hesse). All these observers applied sodium light for their experiments. In looking through the tabulated results of Tollens and Schmitz we find, as the nearest amounts to the normal weight for Soleil's saccharimeter:—

15·01 gr. made up to 100 c.c. at 20° C. $= 19{\cdot}94^{x^\circ}$ (Tollens), and 18·175 gr. made up to 100 c.c. at 20° C. $= 24{\cdot}13$ (Schmitz).

If, then, we accept Broch's statement, that a quartz of 1 m.m. in thickness rotates the sodium light for $21{\cdot}67^{x^\circ}$, we may use the proportions—

$$19{\cdot}94 : 21{\cdot}67 = 15{\cdot}01 : (x = 16{\cdot}31) \text{ and}$$
$$24{\cdot}13 : 21{\cdot}67 = 18{\cdot}175 : (x = 16{\cdot}32),$$

and this answer shows very nearly the actual amount required as the normal weight for Soleil. Moreover, Schmitz has, from his own results and those obtained by Tollens, calculated a formula for the degree of concentration, viz., $C = 0{\cdot}75063\,\alpha + 0{\cdot}0000766\alpha^2$, and for $\alpha = 21{\cdot}67$, $C = 16{\cdot}302$ gr. in 100 c.c. at 20° C. The difference between this number and 16·35 is therefore not very great, and the latter is certainly near enough for practical purposes. Besides, for scientific observations a whole number (like 10 grs.) is much more convenient.

Before proceeding with the next question, I may say that I did not take as a basis for my calculations the data given for the transition tint, because they are not precise enough. Biot states that 1 m.m. of quartz rotates 24^{x°, while Broch declares it to be $24{\cdot}5^{x^\circ}$; and as for the rotation caused by solutions of sugar, Montgolfier calculates—

$$(\alpha)_D : (\alpha)j = 1 : 1{\cdot}129,$$

while Weiss considers the right proportion (for solutions from 5 to 19 grs. in 100 c.c.) to be as 1 : 1·034. Now, even if we accept 24 as the proper angle, which is or was the concentration when obtaining $(\alpha)j = 73{\cdot}8$?

We may now consider the statements made concerning invert-sugar. Clerget found that 16·35 grs. of cane-sugar, after inversion and cooling to 0° C., deviate the light for −44 divisions of Soleil's scale. Tuchschmidt, by a series of careful observations, confirmed this (finding −44·16), and Casamajor merely repeated partly what Tuchschmidt has explained before him.

As 100 divisions of Soleil equal—

$$21{\cdot}67^{x^\circ},\ -44{\cdot}16d. = -9{\cdot}5693^{x^\circ},$$

and which rotation is caused by a layer of 200 m.m. of 17·21 grs. invert-sugar (derived from 16·35 grs. cane) in 100 c.c. Hence 17·21 grs. : $9{\cdot}5695^{x^\circ} = 10 : (x = 5{\cdot}560^{x^\circ})$, and from this we obtain, by multiplying with 5, the specific rotation $(\alpha)_D = -27{\cdot}8$ at 0° C. and for our special concentration. But according to Clerget, as also Tuchschmidt, the rotation decreases steadily with the rise of temperature, and the latter observer shaped the respective law into the following formula:—$(\alpha)^t{}_D = -27{\cdot}8 + 0{\cdot}32\,t$; therefore, for $t = 20^\circ$ C. $(\alpha)_D$ equals $-21{\cdot}4$, and for $t = 14^\circ$ C. $(\alpha)_D$ becomes $-23{\cdot}32$.

Mr. Allen speaks also of the specific rotation of lævulose, and to calculate this we have to remember that, according to Tollens, $(\alpha)_D = 53$ for solutions of dextrose containing up to 14 grs. dissolved in 100 c.c. at 20° C. (Soxhlet considers it to be slightly less, viz., 52·5.) As invert-sugar consists of equal parts of dextrose and lævulose we have—

$$-\frac{L}{2} + \frac{D}{2} = -21{\cdot}4$$

and $L = -95{\cdot}8 = (\alpha)_D$ at 20° C., and for a solution containing about 8·5 grms. in 100 c.c. For the temperature of 14° C. $(\alpha)_D$ is calculated to equal $-99{\cdot}64$.

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[*] *The Times*, October 2 0.

A series of exact observations of the rotation of lævulose have not yet been made, and as our knowledge of the optical properties of this sugar are incomplete, Mr. Allen cannot expect to find full or clear information about it in a "Dictionary" or a general treatise on chemistry.—I am, &c.,

J. Steiner.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Note.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 16, October 18, 1880.

Saccharine Matters Contained in the Fruit of the Coffee Tree.—M. Boussingault.—The author has operated upon 6·4 kilos. of alcohol, in which the ripe fruits had been brought over, and 9·030 kilos. of fruit taken out of the alcohol. He obtained a total yield of 92 grms. mannite, 364·4 grms. inverted sugar, and 98·6 grms. cane-sugar. He does not consider that the pulp of the coffee berry could be utilised as a source of alcohol.

Photophone of MM. Graham Bell and Sumner Tainter.—A. Breguet.—This paper cannot be reproduced without the accompanying illustration.

Spectroscopic Studies of the Sun, Conducted at the Observatory of Paris.—L. Thollon.—The author remarks that the sun has entered upon a period of activity, and he describes and figures certain luminous protuberances, to one of which he ascribes a height of more than 100,000 kilometres.

Vibratory Forms of the Circular Pellicles of Saposaccharic Liquid (Continuation).—C. Decharme.—The author, in this second portion of his memoir, treats of the position of the nodals for each system.

Presences of Cerium in the Coal Measures of the Basin of St. Etienne.—M. Mayençon.—The cerium, probably accompanied by didymium and lanthanum, was found in certain kidney-shaped nodules of iron-spar from the new mine of Ferouillat, near Béraudière.

Les Mondes, Revue Hebdomadaire des Sciences. No. 5, June 24, 1880.

Ultra-violet Rays.—J. L. Schönn.—The author gives the position of the ultra-violet rays of the spectra of cadmium, zinc, thallium, calcium, indium, magnesium, iron, and aluminium. His apparatus is well adapted for the study of absorption spectra; a column of water of 10 centimetres contained between two plates of quartz absorbs the greater part of the ultra-violet rays, whilst a block of very pure ice of 21 centimetres does not sensibly absorb the rays of cadmium in this portion of the spectrum.

Simple Method of Observing the Phenomena of Diffraction.—V. D. Vorak.—The rays reflected by a heliostat are concentrated by two lenses. In the focus is placed a diaphragm with a very small aperture, and the luminous glass is received on a screen. In this glass are placed the bodies whose shadows are to be studied.

Remarks on the Experiments of Mr. Crookes—Leonhard Weber.—For electrodes sealed into the glass the author substituted metallic armatures applied to the surface of an exhausted receiver and connected with the poles of an induction-coil. The only difference is that the currents, being alternately in a contrary direction, the effects indicated by Crookes at the negative pole were een simultaneously at both poles with the same intensity.

New Air-Thermometer.—M. Miller.—The apparatus is constructed of an iron support fitted at its upper part with two tubulures, in which are fixed the two tubes of the manometer. The closed branch bears a thread of opaque glass, soldered in its side at the point where the large tube joins the connecting tube.

The Magnetic Apparatus of M. Edard.—Among other electric or magnetic appliances for the treatment of various diseases is mentioned a magnetic sand, which M. Edard imports from the Isle of Bourbon, and which has been subsequently found near Morbihan. Its application is said rapidly to revive diseased plants.

July 1, 1880.

At London, in Canada, there is said to be a girl possessing remarkable electric powers. On joining hands she can give a strong electric shock to fifteen or twenty persons, and she attracts the magnet. If she wishes to take up a knife the blade rushes to her hand, and a packet of needles folded up in paper hangs suspended from the ends of her fingers.

The Automatic Electric Lamp of M. Jamin.—The apparatus cannot be intelligibly described without the accompanying woodcut.

The Theory of the Battery.—M. Pellat.—A voltaic battery is the seat of electromotor forces, and it is a source of energy, but there is no necessary coincidence between the place of origin of these two quantities. The energy is due to the chemical activity, whilst the electromotor forces have their seat in the different contacts, especially those of dissimilar metals.

L'Orosi, Giornale di Chimica, Farmacia, e Scienze Affini. No. 2, 1880.

Giuseppe Orosi and his Works.—A biographical notice of an Italian chemist and pharmaceutist, from whose name this journal derives its puzzling title.

Reciprocal Relation between the Atomic Weights of the Elements and their Properties (Continued).—Dr. Dario Gibertini.—This extensive memoir may be regarded as a survey of the researches of Mendelejeff and Lothar Meyer.

Mean Composition of the Salt Waters and the Various Products of the Brine Springs of S. Leopoldo, near Volterra, and the Theory of the Preparation of Salt as there Practised (Continued).—Augusto Brasseur.—Not suitable for abstraction.

Characteristic Distinctions between Human Blood and that of other Animals.—Dr. Vincenzo Peset y Cervera.—On mixing the blood in question with a little bile there are formed in the mass, crystals not exceeding 0·003 metre in size. These crystals, according to the author, may be distinguished thus:—Those of man are right rectangular prisms; those of the horse, cubes; of the ox, rhombohedrons; of the sheep, rhombohedric tablets; those of the dog, rectangular prisms; those of the rabbit, tetrahedrons; of the squirrel, hexagonal tables; of the mouse, octahedrons; of common poultry, cubes modified at their angles, &c.

No. 3, 1880.

Giuseppe Orosi and his Works.—Dr. Angiolo Funaro.—A continuation of the biographical notice begun in the last number.

Thermic Equilibrium of Chemical Actions.—Dr. Donato Tommasi.—The author proposes the solution of the following problem:—When any compound such as can be oxidised and reduced is submitted to a chemical action, which is at the same time oxidising and reducing, in what manner will such compound behave, being in presence of two forces equal and opposite? The paper is to be continued, and does not appear to admit of useful abstraction.

Reciprocal Relations between the Atomic Weights of the Elements and their other Properties.—Dr. Dario Gibertini.—A continuation of the summary of the results of Mendelejeff and L. Meyer.

On some Causes which Hinder or Facilitate the Precipitation of Manganese Hydrate by Ammonia.—Giulio Puliti.—The author first examines the influence of ammoniacal salts in particular, and of ammonium chloride in particular. He finds that the precipitation of manganese from its solutions by means of ammonia may be partially or totally hindered by sal-ammoniac. Heat renders the sal-ammoniac more efficacious. In hot liquids the precipitation of manganese may be completely prevented if the metal meets with this reagent in the proportion of 1 : 150. He next examines the action of the metals which are capable of precipitation as hydrates by means of ammonia. He finds that iron, aluminium, and chrome facilitate the precipitation of manganese.

Mean Composition of the Salt Waters and the Various Products of the Brine Springs of S. Leopoldo, near Volterra.—Augusto Brasseur.—A continuation from the last number.

No. 4, 1880.

Giuseppe Orosi and his Works.—Dr. Angiolo Funaro.

Formation of Glucosides of a Complicated Structure.—Ugo Schiff.—From the synthesis of helicine effected by A. Michael, the author considers that some derivatives of this base may be considered as glucosides of three constituent groups, comparable in their constitution to phlorizine.

Thermic Equilibrium of Chemical Actions (Conclusion).—Dr. Donato Tommasi.—The author here answers the question raised in the first portion of his memoir :—When any substance is submitted to two chemical actions equal and opposite, the one which will evolve the greater number of calories will be produced in preference provided that the said reaction can be begun. Of two chemical reactions that which can be set up most easily will be produced by preference although it develops less heat than the other.

Reciprocal Relations between the Atomic Weights of the Elements and their other Properties.—Dr. Dario Gibertini.—A continuation from Nos. 2 and 3.

Mean Composition of the Salt Waters and other Products of the Brine Springs of S. Leopoldo, near Volterra.—Aug. Brasseur.—Continued from the last two numbers.

Utility of Boric Acid in Vinification.—Prof. A. Herzen.—The author maintains that boric acid does not hinder, but favours the alcoholic fermentation, and that it impedes the conversion of wine into vinegar.

Correspondenz-Blatt des Vereines Analytischer Chemiker.

No. 11, June 1, 1880.

Detection of Picric Acid in Beer.—Dr. H. Fleck.—The author evaporates 500 c.c. of the beer to a syrup, mixes with ten times its volume of absolute alcohol, filters off the precipitate, washing it as well as possible, and evaporating the alcoholic filtrate to dryness. The residue is extracted with water at a boil as often as the liquid becomes coloured, evaporates to dryness, and extracts the residue with ether. The ethereal extract contains the picric acid almost pure.

Quantitative Determination of Magenta in Coloured Meat.—Dr. H. Fleck.—A somewhat circumstantial process based upon the extraction of the colour with amylic alcohol.

Remarks on Mr. Tatlock's Letter.—G. Weiss.—The author considers the solvents recommended by Mr. Tatlock too powerful and unsafe. It is not sodium chloride which interferes with the accurate determination of potassa, but sodium, calcium, and magnesium sulphates.

Proportion of Water in Superphosphates (Concluded).—The author calls attention to the hygroscopic character of superphosphates made from guanos damaged by sea-water.

Value of Precipitated and Reverted Phosphates.—It is maintained that ferric and aluminic phosphates and bicalcic phosphates are equal in their effects to soluble phosphates containing equal quantities of phosphoric acid, and that upon sandy, peaty, calcareous, and marl soils the phosphoric acid soluble in citric acid is decidedly preferable in practice to that soluble in water. Raw Peruvian guano is generally preferable to such as has been rendered soluble with sulphuric acid. Ammonium carbonate, nitrate, and phosphate, and compounds analogous to ammonia, like certain nitrogenous and readily decomposable organic compounds are preferable to ammonium sulphate.

No. 12.

Use of Bromine in the Analysis of Sulphides.—E. Reichardt.—Bromine oxidises sulphur and sulphides very rapidly. Iron pyrites require to be very finely pulverised and a prolonged action is required. Copper pyrites are dissolved very rapidly if an excess of bromine is used, which is easily expelled by a gentle heat. The sample is placed in a small flask, covered with a little water, and the bromine is added. A gentle heat is sometimes necessary towards the end. One part of sulphur requires about 15 parts of bromine. Bromine water is especially adapted for destroying sulphuretted hydrogen and dissolving recently precipitated sulphides.

Behaviour of Carbonic Acid with Nessler's Reagent and Ammonia.—Th. Salzer.—A solution of acid ammonium carbonate or a dilute solution of sal-ammoniac mixed with water containing carbonic acid or with sodium bicarbonate, if mixed drop by drop with Nessler's reagent gives a yellow precipitate, which disappears on agitation without imparting the slightest colouration to the liquid. Not until the free carbonic acid has been saturated by the addition of caustic potassa or of an excess of the reagent, is a permanent yellow colouration produced.

No. 13.

Occurrence of Copper.—Dr. W. Hadelich.—The author has detected and determined copper in the soil of a churchyard, in portions of exhumed bodies, in wheat flour and in water.

Behaviour of Sugars with Solutions of Copper and Mercury.—F. Soxhlet.—From the *Journal f. Praktische Chemie.*

Detection of Phosphorus in Urine in a Case of Acute Poisoning.—It is recommended in cases of poisoning with phosphoric acid, to submit the urine to special examination.

Adulterations of Saffron.—A. Meyer.—Saffron is sophisticated with muscular fibre, the flowers of *Calendula officinalis*, safflower, *Crocus vernus*, *Punica granatum*, fragments of sanders-wood, glucose, glycerin, oil, chalk, and heavy-spar.

No. 14.

Determination of Carbonic Acid in Beer and Artificial Mineral Waters.—Dr. Bering.—This paper cannot be reproduced without the accompanying woodcut.

Determination of the Cinchona-Alkaloids by Polarisation.—Taken from Landolt's *Des Optische Drehungsverœmgen Organischer Substanzen*, and not susceptible of useful abstraction.

Analysis of Certain American Tantalites.—W. J. Comstock.—From the Chemical News.

Determination of Theine in Tea.—Fifteen grms. tea are repeatedly extracted with boiling water till completely exhausted ; the liquid is filtered, evaporated to the consistence of an extract, mixed with 2 grms. calcined magnesia and 5 grms. powdered glass and completely dried.

The residue is ground to powder, treated three or four times with 60 c.c. ether, and the ether is allowed to evaporate. The crystals of theine are re-dissolved in a little chloroform and the solution allowed to evaporate to dryness in a tared capsule.

Water in Superphosphates.—The author recommends that portions of 10 grms. should be heated for two hours in the water-bath in shallow porcelain capsules. He mentions that gypsum slowly loses three-fourths of its crystalline water even below 100°, and part of the remainder at temperatures little above 100°. Free phosphoric acid, if heated with gypsum, expels sulphuric acid even in the water-bath. Acid calcium phosphate is decomposed in the water-bath into free phosphoric acid and insoluble phosphates.

Chemisches Central-blatt.

No. 25, 1880.

Action of Cane-sugar on Silver Oxide.—E. Salkowski.—Cane-sugar produces the finest silver mirrors if a little soda-lye is added, though no inversion is occasioned.—*Zeit. f. Physiol. Chem.*, 4, 133.

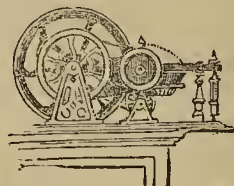
MEETINGS FOR THE WEEK.

Monday, 22nd.—Medical, 8.30.
—— Royal Geographical, 8.30.
—— Society of Arts, 8. "Some Points of Contact between the Scientific and Artistic Aspects of Pottery and Porcelain," by A. H. Church, F.C.S.
Tuesday, 23th.—Civil Engineers, 8.
—— Medical and Chirurgical, 8.30.
Wednesday, 24.—Society of Arts, 8. "The Influence of Barry upon English Art," by J. Comyns Carr.
Thursday, 25th.—Royal, 4.30.
—— Philosophical Club, 6.30.
Friday, 22th.—Quekett, 8.
—— Clinical, 8.30.
Saturday, 27th.—Physical, 3. "On Refraction Equivalents," by Dr. J H. Gladstone, F.R.S. "On the rate of Loss of Light from Phosporescent Surfaces," by Lieut. L. Darwin, R.E. "On Minor Applications of Electromotors," W. H. Coffin.

TO CORRESPONDENTS.

Enquirer.—"Select Methods in Chemical Analysis," published by Longmans.

A. E. Scott.—The iridescence is probably due to sulphur present as an impurity; if, however, you will send a short note of particulars, we shall be able to say more about it. The iridescent colour seen on ancient glass admits of a satisfactory explanation.



THE CHEMICAL NEWS.

Vol. XLII. No. 1096.

ON THE ATOMIC WEIGHT OF BERYLLIUM.

By T. S. HUMPIDGE.

The correct number to be assigned to the weight of the atom of beryllium has been a matter of discussion amongst chemists for very many years. It has been held by many that some number about equal to 9·2 would be this correct value, and by others that it was rather represented by the number 13·2. According to the former school the formula of the only oxide which this metal forms becomes BeO, and according to the latter Be_2O_3. Berzelius, who is well known to have supported the latter view, based his opinions upon the analogies which the beryllium salts show rather with those of aluminium than with those of magnesium and the allied metals. As a matter of fact these analogies remain as strong as ever they were, but something more than analogy is required to definitely decide the atomic weight of an element. It is true that it has been shown that many compounds of beryllium might be written in a simpler manner if the formulæ BeO were accepted as that of its oxide.[*] But this was as little definite proof for the former supposition as the analogies discovered by Berselius were for the latter. It was therefore necessary to adapt some well-accepted method for the determination of the atomic weight. After Mitscherlich's theory of isomorphism became generally known endeavours were made to establish the correct formula of the beryllium compounds upon a more substantial basis. It had been already remarked that no double sulphate of beryllium and potassium or ammonium corresponding to the alums existed, as well as that beryllium sulphate did not crystallise with the sulphate of the magnesium group of metals. Debray showed,[†] on the other hand, that the crystalline form of beryllium oxide agreed equally well with that of zinc oxide and with that of alumina. The analysis of some specimens of noble beryl from Elba had also shown that they contained varying proportions of beryllium oxide and alumina, having at the same time the same general composition as ordinary beryl. With these contradictory results it was absolutely necessary to adopt some other means than isomorphism, in order to obtain a correct answer to the problem before us. For this purpose either the vapour density of some volatile compound of beryllium or the specific heat of the metal was required to be determined. As far as I am aware, no determination of the vapour density of those few compounds of beryllium which are volatile at a sufficiently low temperature (beryllium chloride, &c.)[§] has been made, although Victor Meyer, has promised us such a determination. With regard to the second method, determinations of the specific heat of metallic beryllium by two different chemists, using entirely different methods for the preparation of the metal and different apparatus for the determination of its specific heat, have been made known within the last few years. The first of these was made by Professor Emerson Reynolds,[‖] who prepared the metal from the chloride by means of sodium, using a calorimeter of his own devising for the determination of the specific heat. Prof. Reynolds, who employed platinum vessels for the reduction of the chloride, does not seem to have made any analysis of the metallic product which he obtained, to ascertain whether it fairly represented metallic beryllium. This method for the preparation of metallic beryllium was first employed by Wöhler,[*] and the metal obtained by him was apparently largely contaminated with platinum. That platinum vessels really are attacked during the reaction has been later confirmed by other investigators, *e.g.*, MM. Nilson and Pettersson, to whose work we have immediately to refer. Prof. Reynolds has also furnished us with no details of the other physical properties of his metal except that they agreed generally with those of the metal obtained by Debray; neither are details given of the manner in which the determinations of the specific heat were carried out. His determination of the value required gave 0·642, which, with an atomic weight of 9·2 for the metal would give an atomic heat of 5·9064, sufficiently near to the normal atomic heat (6) required by Dulong and Petit's law. The atomic weight of beryllium was therefore fixed by these experiments at 9·2. Almost immediately afterwards (1878) the second determination of the specific heat was made by MM. Nilson and Pettersson.[†] These investigators found at once that beryllium prepared by the reduction of the chloride in platinum vessels contained far too much of this metal to admit of its employment for a determination of its specific heat. They therefore employed another method for the preparation of the metal, which consisted in placing layers of molten beryllium chloride and sodium in a very thick iron tube, screwing up the tube, and heating to redness in a wind furnace. The beryllium was obtained as small partially fused crystals, some of them being 2 mm in diameter. The determiations of specific heat were made by Bunsen's ice-calorimeter, the only modification employed being that of Schuller and Wartha, by whom the external vessel of the instrument is covered with a thick layer of ice instead of being entirely immersed in pure snow. The metal prepared by these chemists was by no means pure, but they determined its exact composition and allowed for the impurities in their determinations. The percentage composition of the metal they obtained was:—

Beryllium	87·09
Beryllium oxide	9·84
Iron..	2·08
Silica	0·99
	100·00

The specific heats of the two latter impurities had been previously accurately determined, and they made a fresh determination of that of beryllium oxide. Deducting then the relative specific heats of these impurities from that actually found, the true specific heat of beryllium was obtained. The mean of their experiments, carried out between 100° and 0°, gave the specific heat of the metal as 0·4079, which required the atomic weight of 13·8 to make the atomic heat 5·693—a number nearly approaching the normal value.

In attempting to form a judgment of these two determinations, which are entirely at variance with one another, simply upon their intrinsic methods, we must remember, firstly, that the metal prepared by Prof. Reynolds was in all probability very impure by admixture of platinum, which would tend to lower its specific heat, and that the apparatus he employed, though perhaps capable of accurate measurements, could scarcely be considered to be so exact as the ice-calorimeter of Bunsen. Secondly, it must not be forgotten that although M. Nilson was careful to determine the impurities which the metal prepared by him contained, and to allow for their relative specific heats, it is by no means clear that silica in combination in a silicate of beryllium, it must have existed in the impure metal, would really possess the same specific heat as when in the free state. Other things being equal, the method he employed for the determination of the specific heat, together with the precautions to which he attended so carefully,

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[*] Audejeu, *Ann. d. Phys. u. Chem.*, lvi., 3.

[†] *Ann. de Chim. et d. Phys.*, xliv., 37.

[§] *Phil. Mag.* [v] iii., 38.

[‖] Organic compounds of beryllium, *e.g.*, Be_2Me_6, Be_2Et_6, &c., if they exist would probably be volatile bodies.

[*] *Pogg. Ann.*, xiii., 577.

[†] *Ber. Berichte*, xi., 381; *Wiedermann's Ann.*, iv., 554.

give his determination a decided preference over that of Prof. Reynolds to all unprejudiced minds.

Unfortunately the atomic weight of beryllium had been previously almost generally accepted as 9·2, and the determination of Reynolds came as a welcome confirmation to the common view. It had been especially noticed by Mendeleeff and Lothar Meyer, in the construction of periodic tables of the elements, that beryllium must possess the atomic weight of 9·2 and so be a divalent element, in which case it received a correct position in the tables and became the first member of the magnesium group; while if its atomic weight were 13·8 its position would fall between carbon (12) and nitrogen (14), which did not at all correspond to that of any other metallic element. The determination of Reynolds was therefore accepted without any attempt at criticism of the manner in which he arrived at his results. This was even the case at least in this country, after the publication of MM. Nilson and Pettersson's detailed account of their experiments.

As soon as Nilson had published his results he was at once attacked by Lothar Meyer, who attempted to show that the atomic weight of beryllium found by the former chemist was one entirely incompatible with the position of the element in the periodic series. As we have above remarked it would come between carbon and nitrogen, in which place no trivalent metallic element with a high melting-point could be introduced; and while attaching importance to the determinations of Nilson, he was of opinion that if these determinations were correct, the equivalent weight of beryllium must be lower then that generally accepted, and its atomic weight must lie between that of boron (11) and carbon (12). To this he added the observation that if the atomic heat of the oxygen contained in the beryllium oxide were compared with that contained in alumina, &c., the former, according to Nilson's atomic weight, would be much lower than it should be. The specific heat of the oxygen contained in an oxide is, as is well known, obtained by substracting from the molecular heat of the oxide the atomic heat of the metal which it contains; thus, if *s* represents the specific heats of the oxide and metal, *m* the molecular or atomic weight, and A the atomic heat of oxygen, then:—

	m.	*s.*	*s.m.*	A.
Al_2O_3 ..	102·8	0·217	22·3	
Al_2 ..	54·8	0·214	11·7	$\frac{10·6}{3} = 3·5$
			10·6	
Fe_2O_3 ..	160·0	0·168	26·9	
Fe_2 ..	112·2	0·114	12·7	$\frac{14·2}{3} = 4·7$
			14·2	
Be_2O_3 ...	75·6	0·247	18·7	
Be_2 ..	27·6	0·408	11·3	$\frac{7·4}{3} = 2·5$
			7·4	

And he concluded by pointing out that there might be some constant error running through all Nilson's determinations, which would tend to make all his results too low. The attack of Lothar Meyer was followed up by a remark from Brauner[*] that beryllium might belong to the same class of bodies as carbon, silicon, and boron, the atomic heats of which are too low to agree with Dulong and Petit's law except at very high temperatures. Whence it followed that if the determinations mere made at a sufficiently high temperature the specific heat might be sufficiently high to agree with the atomic weight 9·2.

Nilson replied to Lothar Meyer's criticism almost immediately,[†] in which he stated the objections which might be urged against the determination of Reynolds to which we have already referred. He also showed that every precaution had been taken in his experiments, and that no such error as Lothar Meyer imagined—an error equal to one-half the reading—was at all possible. With regard to the low value given to specific heat of oxygen in beryllium oxide by his determination (2·47), he remarked that if the molecular heat of alumina as determined by Neumann were taken (20·3), the atomic heat of the oxygen it contained became 2·83, or very little different from that found for the oxygen in beryllium oxide; and if again Regnault's number (22·3) were taken, the atomic heat of the oxygen became 3·5, or very much less than that of the other sequioxides (about (4·6). He was of opinion that beryllium showed far more analogy with the metals of the cerite and gadolinite metals than with those of the magnesium or of the aluminium series.

So remained the subject for two years; but within the last few months Nilson and Petterson have again taken it up.[*] They published, in the first place, the results of several careful determinations to establish the equivalent weight of beryllium more accurately. Employing the sulphate and igniting to redness, they obtained a mean value of 4·542 (O = 7·98) with a very small error between any two determinations. This slightly lowered the atomic weight, making it 13·65, though still not lower than that of carbon (12). They also made further determinations of the atomic heat of beryllium at higher temperatures, and showed that at 300° it only became 6·90, while that of iron was 7·09, the increase being at about the same rate. Further experiments on the molecular heat of the oxide and on that of alumina, scandia, and other of the rare earths, revealed the fact, that the numbers so obtained agreed very well with one another, and that the atomic heat of the oxygen contained in the first two elements was almost the same. The following is an abstract of one of their tables:—

	m.	*s.*	*m. s.*	A.
Be_2O_3 ..	75·32	0·2471	18·61	
Be_2	27·32	0·4246	11·60	$\frac{7·01}{3} = 2·34$
			7·01	
Al_2O_3 ..	102·8	0·1825	18·78	
Al_2	54·8	0·2143	11·74	$\frac{7·04}{3} = 2·35$
			7·04	
Sc_2O_3 ..	136·0	0·1530	20·81	
Sc_2[†] ..	88·0	0·1454	12·80	$\frac{8·01}{3} = 2·67$
			8·01	

They also made determinations of the molecular volume and molecular heat of the anhydrous sulphates of beryllium, aluminium, and the rarer metals of the earths, of which the following are some of their results:

	m heat.	*m* volume.
Beryllium sulphate	62·37	129·07
Aluminium ,, ...	63·59	126·50
Yttrium ,, ...	61·60	178·80
Erbium ,, ...	64·48	168·57
Scandium ,, ...	62 42	145·80

Showing that in all these examples the molecular heat is nearly the same, and that, for the first two, the molecular volume also corresponds with one another exceedingly well, while in the following the variation is not excessive. It was thus quite evident to the authors that the formula to be ascribed to beryllium oxide must be Be_2O_3 and not

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[*] *Ber. Berichte*, xi., 872.

[†] *Ibid.*, xi., 906.

[*] *Ber Berichte*, xiii., 1451.

[†] Atomic heat of scandium was taken = 6·4.

BeO. Finally, these chemists refer to a remark of Mr. T. Carnelly, who had calculated that the melting-point of beryllium chloride would lie between 547° to 600° if its formula were $BeCl_2$, and between 50° to 100° if it were Be_2Cl_6. He found the melting-point to be about that indicating the former composition, and thence went on to say, that Nilson and Pettersson's determination of the specific heat must be wrong. Nilson and Pettersson remark that the author of this astounding statement based upon such slight evidence must possess a very limited knowledge of the metals which make up Mendeleeff's third group. With the exception of aluminium, little or nothing is known of the melting- or boiling-points of the halogen compounds of the metals contained in this group, and it is obviously inconsistent to base analogies which are to extend to a whole group or series upon such insufficient data. They were convinced that enough halogen compounds would be found within this group which would not agree in their melting- and boiling-points with those of aluminium. It appeared to them strange that Carnelly should attach so little importance to their careful determinations, and should, at the same time, place such implicit confidence in the periodic law.

The *Berlin Berichte* (xiii., 1780) contains a further reply to Nilson and Pettersson's last results by Lothar Meyer. This chemist still bases his arguments upon the periodic law, and from the fact that the specific heat of beryllium rises with increase of temperature more rapidly than that of iron, silver, and platinum; he concludes that the metal belongs to the same class as boron, carbon, and silicon, as had been suggested by Brauner, although neither of these three elements were metals, and although he had previously admitted that the specific heat of the metals as a general rule is normal. But he fails to bring forward any experimental proof of his opinion to contravert that of Nilson and Pettersson.

Such, then, is the present position of this important problem. No chemist who has really made a study of the subject could accept Professor Reynolds's results as they at present stand even when supported by the periodic law. We may, therefore, lay them on one side, especially as this investigator has, as far as I am aware, not made any attempt to combat the results of the experiments of Nilson and Pettersson.[*] It follows that we have on the one hand, the careful determinations of these two last-named chemists on the specific heat of beryllium, and their results of the molecular heat and volume of the oxides and sulphates of beryllium, aluminium, and other metals which argue strongly for the acceptance of the atomic weight 13·65. While, on the other hand, theoretical chemists persist that Mendeleeff's periodic law has shown itself of such value in the science, and has been tested by numerous coincidences (*e.g.*, the discovery of the predicted ekaluminium-gallium, and equabor-scandium) that its guidance ought to be accepted absolutely. These chemists would assign the number 9·2 to the atomic weight of beryllium. If we except the fact that the specific heat of beryllium does increase with rise in temperature more rapidly than that of other corresponding metals, we have nothing to prevent our acceptance of Nilson and Pettersson's *experimental* results, based upon the well-established law of Dulong and Petit, but the *theoretical* conceptions arising from a consideration of Mendeleeff's periodic law. I should be loth to disparage the very important service which this latter law has rendered to chemical science in late years, but it must not be forgotten that we have here to deal with a fact which directly contradicts the position which it assigns to beryllium as the first member of the magnesium series; and the question arises, shall we accept the fact or the theory? I would remind my readers that if the theory is accepted and the fact rejected, they are in the position of the supporters of some of the earlier chemical theories, when once they found that a theory which had for a considerable time satisfied their wants was no longer sufficient to explain new facts, they preferred to retain the theory and to explain the facts as best they could. I am not arguing for the rejection of the periodic law, but only wish to show that if facts are discovered which are incompatible with it, it must of necessity receive some modification. Already it is insufficient to explain the physical differences between nickel and cobalt and between their compounds; and the elements with high atomic weights, including the rarer elements of which so many have been lately discovered, are simply placed in a chaotic state at the end of the table.[*] We cannot, therefore, reject facts for the sake of a new theory, which may be said to be but in its infancy.

It is, however, most important that the experimental results of Nilson and Pettersson should be confirmed by some independent observer—that the specific heat of beryllium should again be determined. The metal ought to be capable of preparation in a purer state than they were able to procure it, and thus one source of error would be removed. In the meantime we cannot do otherwise than accept their determination of the atomic weight (13·65), writing beryllium oxide Be_2O_3 and the other compounds with corresponding formulæ.

INTRODUCTORY LECTURE
TO THE
COURSE OF METALLURGY,
AT THE
ROYAL SCHOOL OF MINES.

Session 1880-81.

By W. CHANDLER ROBERTS, F.R.S.,

Associate of the Royal School of Mines, Lecturer on Metallurgy in the Royal School of Mines, Chemist of the Mint, One of the Secretaries of the Physical Society of London.

(Concluded from p. 257).

Of all the metallurgical operations conducted in this country, those connected with iron are, of course, the most important. The production of pig-iron alone in the United Kingdom has increased from two million seven hundred thousand tons in 1852, to six million two hundred thousand tons last year, a maximum slightly in excess of this figure having been reached in the year 1872. Now the Bessemer process, the first patent in connection with which was taken out in 1855, has reduced the cost of steel from £50 to £6 per ton, and has changed the whole aspect of the iron and steel manufacture; indeed, the success with which this process alone is conducted may almost be regarded as an index of our national prosperity. Notwithstanding the almost universal depression of trade during the last few years, the outturn of steel has been steadily increasing; and it is estimated that in 1879 this country produced nearly a million tons in the Bessemer converter, double the entire produce of the remainder of the world in the year 1870 by the same process.[†] The outturn of Bessemer steel in America has, however, advanced with still more rapid strides; for last year she actually produced, with far fewer converters, ninety-four thousand tons more than this country. It will be evident, therefore, that every improvement effected in this process is of truly national importance, and I would briefly refer to the greatest that has been introduced in recent years.

In 1855 the fact was established that pig-iron from the blast-furnace contains the greater part of the phosphorus originally present in the ore. Dr. Percy pointed out that

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[*] A short paper was read by Professor Reynolds at the Brit. Ass., 1878, on "Double Salts of Beryllium," in which he mentioned that the metal was obtained by decomposing the double fluoride of potassium; the specific heat of the metal was 0·642.

[*] Lothar Meyer's *Moderne Chemie*—table at end.

[†] *The Times*, December 31, 1879.

phosphorus is not eliminated in a sensible degree in the Bessemer process, as it is in the old process of puddling; and he stated that if the Bessemer process is to be "generally applicable in this country, it must be supplemented by the discovery of a process of producing pig-iron sensibly free from sulphur and phosphorus, with the fuel and ores which are now so extensively employed in our blast-furnaces."[*] The problem, so far as it relates to the elimination of phosphorus, has received the attention of many of the first metallurgists in this and other countries"[†] but the practical application of basic linings in the Bessemer converter is the outcome of Dr. Percy's teaching; for Mr. S. G. Thomas was a student of the School of Mines, and his partner, Mr. Gilchrist, is an Associate. Mr. Snelus is also an Associate, and Mr. Riley long worked in the metallurgical laboratory. The process not only gives hope that it will be possible to utilise the large quantities of ore in the well-known Cleveland district, but is also widely practised with success on the Continent.[‡] It is probable, therefore, that the large deposits of ore in the basin of the Saar, and those of Lorraine and Luxembourg, which in extent are equal to the Cleveland district, while containing a much greater amount of phosphorus, will now be available. During a recent visit to the Hoerde Works in Westphalia, where I witnessed the operation, Herr Massenez, the director, told me that 10,000 tons of "Thomas-Gilchrist" metal have already been produced there since the adoption of the process a few months ago.

The metallurgy of Copper presents several features of peculiar interest. Mr. Thomas Gibb (a student of my own year), whose work in connection with the extraction of copper by wet methods has been most important, reminds me that in 1863 the wet processes for extracting copper were insignificant; now over forty thousand tons of copper are annually produced by their aid; much has also been learnt as regards other branches of its metallurgy.

In the case of Lead, improvements have been introduced in the mode of purification that have proved to be of considerable importance. But these, as well as the modifications in the treatment of other metals, will require to be considered in some detail in the course of my lectures, and I would only now allude to the growth of the platinum industry in recent years as affording a striking instance of the application of scientific methods to metallurgy. In 1851 the amount of platinum separated from its associated metals and employed in the arts was probably not more than one-tenth of that now extracted. We owe the development of the use of this metal mainly to the eminent metallurgists, Messrs. Johnson, Matthey, and Co., a well-known member of which firm was a student in Dr. Percy's laboratory. They early adopted the classical method of Deville and Debray; and by its aid now supply about 80 per cent of the total amount of this metal annually employed in the world, in addition to other metals of the platinum group, which they alone have isolated in quantity.

I had intended to indicate the metallurgical work done by the more prominent men who have been associated with the School, but I found that it would not be possible, in the brief time at my disposal, to do justice to such as Bauerman, Dick, Hackney, Pearce, Riley, Willis, and others, whose labours have placed them so high in the ranks of English metallurgists. You will, however, as the Course proceeds, have opportunity of becoming familiar with their names.

In referring to the past teaching of the school, I must remind you of the importance of rigorous and minute inorganic analysis; and it is the more necessary that I should do so from the fact that the peculiar charm of organic research appears, as has been already pointed out by Prof. Abel,[*] to lead the younger chemists, to "underestimate the value and importance, in reference to the advancement of science, of the labours of the plodding investigator of analysis." I am satisfied, however, that, if we bear the traditions of the Chemical and Metallurgical laboratories of the School of Mines in view, we are not likely to underrate the importance of analytical work; and much conclusive evidence as to the value of the teaching of the past thirty years is afforded by the labours of the accomplished analysts who have, from time to time, worked under Dr. Percy's direction.

The direct influence of the School on the success with which metallurgy has been practised in this country has been most marked, and would alone afford an answer to the question whether the possession of high scientific attainments is generally advantageous to the successful conduct of metallurgical works. It must not be forgotten that our subject is constantly receiving valuable aid from branches of science other than chemistry; and this can hardly be better shown than by the growing importance of physical research in connection with metallurgical problems. I would incidentally remind you that it is the more important for us to consider this, because special attention was directed to the question in the evidence given before the Royal Commission on Scientific Instruction,[†] whose recommendations will, it is to be hoped, extend the influence of the School of Mines.

In connection with this branch of our subject a most prominent position must be given to the production of high temperatures, as it will be obvious that we have principally to consider the reactions of the elements when under the influence of heat. In the first half of the present century temperatures higher than the melting-point of zinc were not known with any degree of certainty; but in 1856[‡] M. Henri St.-Claire Deville pointed out that chemistry at high temperatures, that is to say, up to the blue-white heat at which platinum volatilises and silica fuses, remained to be studied. Since then, in conjunction with M. Troost, he has given us certain fixed points, such, for instance, as the boiling-points of cadmium and zinc; and Deville's researches on dissociation have entirely modified the views generally entertained in regard to the theory of combustion. Indeed, we owe so much to this illustrious teacher, that the best homage we can offer him will be to work in the directions he has indicated. M. Stas has proved that it is perfectly easy to distil even large quantities of silver from one lime crucible to another,[§] a fact which has been taken advantage of by Mr. Lockyer and myself in some experiments on the absorption-spectra of the vapours of certain metals at high temperatures.[||]

As regards scientific advance of a more essentially practical character, the gradual discovery of the fact that in certain cases fuel can be best employed if it be previously converted into gas, and the recognition of the advantages to be derived from a preliminary heating of the gases and the air, has led to the wide adoption of the regenerative system, by which the waste heat of the furnace is utilised for heating the incoming air or combustible mixture of air and gas necessary to effect the required operation. Dr. Siemens has thus shown us how to economise fuel to a vast extent, it being now possible to produce a ton of steel by the use of 12 cwts. of small coal, instead of 3 tons of coke required to melt it in the old form of furnace. By the command of high temperatures, moreover, he has developed new processes in the metallurgy of iron, which are resulting in the replacement of the old "cinder-mixed" wrought-iron by "cinder-free" ingot iron and steel.[¶] The degree of heat attainable by the regenerative furnace

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[*] "Metallurgy"—Iron and Steel (1864), p. 819.

[†] M. Gruner, *Annales des Mines* (1869), t. xvi., p. 199.

[‡] M. Gruner, *Annales des Mines*, part 1 (1879), p. 146. H. von Tunner, *Zeitschrift der berg- und hüttenmännischen Vereins für Steyermark u. Kärnten*, xii. Jahrg., Mai-Juni, 1880. Herr J. Massenez, *Engineering*, vol. xxx. (1880), p. 198.

[*] "British Association Report, Plymouth" (1877), p. 44.

[†] Report, vol. ii., "Minutes of Evidence," p. 86 (1874).

[‡] *Ann. Chim. et Phys.* [3], t. xlvi, p. 182; *Comptes Rendus*, t. xc. (1880), p. 773.

[§] "Sur les Lois des Proportions Chimiques" (1865), p. 37.

[||] *Proc. Roy. Soc.*, vol. xxiii. (1875), p. 344.

[¶] Akerman, *Journal of the Iron and Steel Institute*, No. 2 (1878), p. 360.

is, however, limited to the temperature of dissociation of carbonic acid and aqueous vapour, so that the temperature never exceeds about 2600° C.; but during the present year[*] Dr. Siemens has employed the far greater heat of the electric arc for the fusion of steel and platinum.[†] Bearing in mind the interest excited by recent experiments on the effect of intense heat on bodies now considered to be elementary, we may expect physicists to look to us for aid in developing the methods of employing high temperatures.

We shall have to consider very carefully the physical work which groups around the metallic alloys. It has long been known that the union of two or more metals produces a result which often differs more in physical properties from either of its constituents than they do from each other. There is strong evidence that metals combine in chemical proportions; and we may turn with advantage to the class of alloys, certain of which have been in use from prehistoric times—the alloys of copper and tin. Here are two copper-tin alloys, one containing 61·7 per cent of copper, and the other 68·2 per cent. They correspond respectively to the formulæ $SnCu_3$ and $SnCu_4$; they only differ by 6·5 per cent, and yet their physical properties are in certain important respects totally distinct. This essential difference in their properties produced by a small difference of composition brings me to one very distinctive feature of metallurgy, the enormous influence exerted on a large mass of metal by a trace of another metal or metalloid—that is, by a quantity so small that it appears to be out of all proportion to the mass in which it is distributed.

I think it may safely be asserted, that in no other branch of applied science has the operator to deal with quantities that are at once so vast and so minute; and the Course will not have proceeded far before you will recognise this fact.

It may be that the trace to be extracted is alone of value—as, for instance, the few grains of gold that can be profitably extracted from each ton of a material, which, though containing only one part of gold in five millions by volume, is thereby entitled to be regarded as an auriferous deposit that can be profitably worked; or it may be the minute percentage of a metalloid which must be extracted, in order that the physical properties of a large mass of metal may not be entirely altered.

Let me give you a few instances of the influence of small traces of metal.

The presence of only 1-300th per cent of antimony in a mass of molten lead, the surface of which is exposed to the air, will cause it to be rapidly oxidised, while a similar mass of lead of equal surface, but free from the minute quantity of antimony, will be but slowly acted upon; and it has been shown by the late Mr. Baker, of Sheffield, a distinguished student of the School, that 7-1000ths per cent of copper is detrimental to the lead employed in the manufacture of white lead.

The presence of 1-20th per cent of lead or certain other metals in standard gold will render a bar an inch thick so brittle that it may readily be broken by a slight rap with a hammer. Less than ½ per cent of iron in metallic copper will reduce the electrical conductivity by about 60 per cent, while a far smaller quantity will render it quite unfit for manufacture into telegraph cables, or for other electrical purposes.

In 1866, Graham showed,[‡] by experiments with which I had the privilege of being connected, that the presence of occluded gases in metals often exerts a marked influence on their molecular structure. In the case of iron he urged that metallurgists should study the effects of occluded gases, more especially carbonic oxide, the weight of which, according to his experiments, could not exceed the 1-15 per cent of the weight of iron in which it was present. The significance of such facts is now under consideration by a Committee of the Institution of Mechanical Engineers;[*] and the question of the presence of gas in steel, either occluded or retained in the form of bubbles, is further being investigated by Chernoff,[†] Müller,[‡] and others.

There are many cases in which the malleability of metals is seriously compromised by the presence of oxygen, either dissolved or in a combined form. The addition, however, of a minute quantity of a metal or metalloid capable of combining with the oxygen without itself exerting an injurious influence on the metal produces a most marked beneficial effect. Thus, if 7-100ths per cent of phosphorus in the form of phosphide of copper be added during the operation of "poling" copper, its action is stated to be most serviceable, although only 3-100ths per cent is actually retained by the metal.[§]

It is, however, in the metallurgy of Iron and Steel that the influence of minute traces of impurity is most marked; and on this subject it will be necessary to enter into considerable detail during the course of lectures, but I must now confine myself to a few instances only. I have already referred to the importance of the elimination of phosphorus in the manufacture of malleable iron and steel. It is known that a minute quantity of this elementary body may be highly injurious to the metal, even if the relation between the carbon and manganese is favourable. I would refer, for instance, to the tests made on certain plates of Swedish puddled iron exhibited in the Paris Exhibition of 1878, which were found to have a far higher resistance to fracture by impact than certain other plates compared with them; and yet analysis proved that the main difference between them lay in the fact that the good plates contained only 2-100ths per cent of phosphorus, whereas the inferior plates contained 1-10th per cent more.[||]

Carbon, it is well known, gives to iron fusibility, and renders it capable of being cast in moulds. The results of very many experiments appear to show that the presence of 15-100ths per cent of carbon converts iron into steel, rendering it capable of being slightly hardened; beyond 1½ per cent of carbon the metal ceases to be malleable, and it is known as cast-iron.

The influence of carbon on the tensile strength of steel is very remarkable. Mr. Willis, the able chemist of the Landore Siemens Steel Works, tells me that two samples under identically favourable conditions as to their content of sulphur and phosphorus, but containing 15-100ths and 18-100 per cent of carbon respectively, will differ by 6 tons per square inch in breaking-strain, or by an increase in the latter case of 27 per cent. But carbon is far from being the only metalloid that exerts a marked influence on the physical properties of iron. Manganese, for instance, is generally considered to act in a manner similar to carbon; and Willis has shown[¶] that an ingot of a certain variety of steel containing no manganese will break into pieces at the first blow of the hammer, whereas a similar ingot containing 8-100ths per cent of that metal will forge readily. The influence of phosphorus has already been referred to; and during the Course we shall consider together the manner in which the character of iron and steel is modified by the presence of minute quantities of sulphur, silicon, and other metalloids, as well as of a few metals. The problems to which they individually give rise are very complicated; but when several are present

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[*] *Engineering*, vol. xxix. (1880), p. 478.

[†] Figures convey but little impression as to such high temperatures; but it may be mentioned that Dewar has given 7000° C. as approximately the temperature of the electric arc ("British Assoc. Report," 1873, p. 466), and, according to Rosetti, the true temperature of the sun can hardly be less than 10,000° C., or more than 20,000° C. *Phil. Mag.* [5], vol. viii., p. 550 (1879).

[‡] *Phil. Trans.*, 1866, p. 438.

[*] "First Report of the Committee on the Hardening, Tempering, and Annealing of Steel," 1879.

[†] "On the Structure of Cast-Steel Ingots," Translated for the Institution of Mechanical Engineers by W. Anderson, C E. (1879).

[‡] *Berichte der Deutschen Chemischen Gesellschaft*, 1879, No. xii., 93. *Glaser's Annalen für Gewerbe und Bauwesen*, August, 1880, p. 138.

[§] W. Weston, *Phil. Mag.* [4], l., p. 542.

[||] Akerman, *Journal of the Iron and Steel Institute*, 1878, No. 2, p. 363.

[¶] *Journal of the Iron and Steel Institute*, No. 1 (1880), p. 92.

together the question becomes so involved that it will be long before we can hope for a complete solution.

Dr. Fleitmann, of Iserlohn, has recently shown[*] that nickel, which breaks under the rolls, may be made perfectly malleable by the addition of a little over 1-10th per cent of magnesium, and at the Düsseldorf Exhibition I saw thin sheets of large dimensions produced by his method. M. J. Garnier has also shown[†] that malleability may be ensured by adding 3-10ths per cent of phosphorus.

A fact of much interest in relation to the assay of gold has been discovered by Dr. A. D. van Riemsdijk, of Utrecht,[‡] who has shown that the presence of 1-100th per cent of certain metals of the platinum group may be detected by the fact that the cooling mass of molten metal does not "flash" or pass through the remarkable state known as "superfusion."

I must not further multiply instances, but will only mention that M. Nyst, of the Brussels Mint, has lately found that the presence of 15-100 per cent of silicon in standard gold will so affect its molecular grouping as to render it possible for a thin strip to bend by its own weight, as zinc would, in the flame of a candle.

The growing importance of physical research in connection with metallurgy is shown by the fact that physical methods are now constantly appealed to by those interested in metallurgy, more especially in the case of iron and steel. We are told, for instance, that the hardness of steel may be correctly inferred from a numerical determination of its coercive force;[§] it is sought to establish the actual nature of the change in the mode of existence of the carbon in steel that accompanies hardening by determining its thermo-electric properties;[||] and the hope is held out[¶] to us that the time will soon come when boiler makers will electrically test their plates, possibly by the aid of the induction-balance, just as they now test them for ductility and tenacity. I can only add the expression of a belief that this powerful weapon of molecular research which Prof. Hughes has given us will yield good results in the hands of some of you.

The varied materials now used in the Bessemer and Siemens-Martin processes have greatly complicated what is known as ingot metal; and the enormous increase in recent years in the use of certain varieties of steel has rendered it necessary to scrutinise more rigorously the analytical and other methods of estimating the amount of such elements as manganese, chromium, and silicon, the presence of which we have seen produces such remarkable effects. Many skilful observers have, therefore, as might have been expected, sought the aid of the spectroscope.

Its use for determining the point at which decarburisation has ceased in the Bessemer converter was pointed out by Dr. Roscoe,[**] followed by Lielegg, Wedding, and Marshall Watts;[††] and more recently Messrs. Parry and Tucker,[‡‡] of the Ebbw Vale Works, have shown that the influence of mere traces of certain impurities on the working of iron renders it very desirable to add spectroscopic observations to the ordinary chemical analysis. In 1874 Mr. Lockyer and myself made experiments on the quantitative analysis of certain alloys by means of the spectroscope;[§§] and in the following year Sir John Alleyne[||||] described a method for estimating small quantities of phosphorus in iron and steel by its aid, which possesses much interest.

The result of mechanical tests are even more important. Not long since the appearance of the fracture of a sample of metal was considered to afford trustworthy and sufficient evidence as to its nature and properties; but such rough methods have given place, in the hands of Kirkaldy and others, to the rigorous physical and mechanical investigation to which metals must now be submitted as a matter of ordinary routine. The results, tabulated or plotted into curves, which mark the influence of each constituent or impurity, form permanent records of the greatest value.[*]

The most careful mechanical and physical tests do not, however, necessarily supply all the information required. The complex varieties of iron and steel now used seem, as M. Lan has pointed out,[†] to be specially liable to what may almost be called a molecular mobility, in virtue of which, when subjected to shocks or vibrations of sufficient intensity, a structural re-arrangement is set up; of their behaviour under these circumstances the previous static tests by traction can give no indication.

It has only been possible for me to indicate the more important conditions affecting the successful practice of Metallurgy. I have traced the relation between technical and scientific workers; but there is yet another condition of somewhat recent growth. The enormous scale on which operations are now conducted renders it more necessary than formerly for those engaged in metallurgical enterprise to seek the aid of capitalists. The result is that a large share in the control of many important works falls to the non-scientific members of the Board of Directors, men of high commercial ability, but whose knowledge of the importance of scientific work is necessarily limited. It is true that they may recognise the necessity for scientific aid in the works with which they are connected, but they are too often unconscious of the labour and difficulty that are involved in the attainment of accurate scientific knowledge. I am convinced, however, that facts are gradually compelling them to recognise that the value of a metal may entirely depend on whether it does or does not contain a trace of impurity, and that the exact method of treatment to be adopted depends much on the character of the materials employed; they will therefore examine more carefully than they have hitherto done the qualifications of men to whom important duties are entrusted, and will insist that the services of only adequately trained metallurgists shall be secured.

I shall have to direct your attention to the minute care with which details affecting commercial interests are now investigated;[‡] and your success will further depend on the facility with which you are able to use the "tools of thought" furnished by chemistry, physics, and mechanics. Whether you will ever possess the tact and judgment necessary to direct such works as Dowlais with an army of ten thousand people, obviously depends on personal qualifications which I can but little influence.

I venture to hope that you will, by original research, add to the general advance of science, for, as the late Prof. Clifford has reminded us, what has often proved to be the most useful parts of science have been investigated for the sake of truth and not for their usefulness.

Dr. Percy found Metallurgy practised in this country mainly as an empirical art. He may well feel, to borrow the words of an old writer, that in his hands "the business of Metallurgy and Essaying has not only been illustrated but also improv'd, amended and enrich'd;" for his works contain a record of its progress, his teaching and researches have secured it a scientific basis, and he has trained a body of scientific workers, in whose hands the immediate future of metallurgy to a great extent rests.

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[*] *Deut. Chem. Gesel.* (1879), vol. xii., p. 454. English Patent Specification, No. 5126 (1878).

[†] *Comptes Rendus*, xci. (1880), p. 331.

[‡] Chemical News, vol. xli. (1880), p. 126; *Ann. Chem. et Phys.* [5], tome xx., p. 66.

[§] Tréve and Durassier, *Comp. Rend.*, t. lxxx. (1875), p. 799. Wattenhofen, *Journal of the Iron and Steel Institute*, 1879, No. 1, p. 305.

[||] Barus, *Phil. Mag.* [5], vol. viii., p. 341.

[¶] W. H. Johnson, Chemical News, vol. xlii. (1880), p. 70.

[**] *Proc. Lit. and Phil. Soc. Manchester*, 1863, iii., p. 57.

[††] *Phil. Mag.* (4), xxxiv., p. 437.

[‡‡] *Journal of the Iron and Steel Institute*, 1880, No. 1, p. 163.

[§§] *Phil. Trans.*, clxiv. (1874), p. 495.

[||||] *Journal of the Iron and Steel Institute*, 1875, No. 1, p. 62.

[*] V. Deshayes, "Classement et Emploi des Aciers" (Paris, 1880); also *Bull. Chem. Soc.*, tome xxxi. (1879), p. 166. H. Lebasteur, "Les Métaux á l'Exposition de 1878" (Paris, 1878).

[†] M. Lan, "La Métallurgie á l'Exposition de 1878" (Paris, 1879); also *Ann. des Mines*, Mai-Juin, 1879.

[‡] In illustration of this see an exhaustive mathematical paper on the values of iron ores by Prof. A. Habets: Cuyper's *Revue Universelle des Mines*, tome i. (1877), p. 504.

Bearing in mind how much the progress of our science means to England, I cannot but be conscious that, in attempting to continue this work, I undertake a grave responsibility.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, November 18, 1880.

Prof. H. E. Roscoe, President, in the Chair.

It was announced that a ballot for the election of Fellows would take place at the next meeting, December 2.

The following certificates were read for the first time:—S. Harvey, J. W. James, W. E. Kay.

Mr. **Spencer Pickering** then communicated a lengthy paper entitled "*Notes on the Oxides of Manganese.*" Various samples of oxides of manganese were obtained, and heated to various temperatures until their weight was constant. The results obtained showed that these oxides sometimes lost and sometimes gained in weight; at other times their weight remained constant. This loss and gain of oxygen does not appear to bear any relation to the degree of oxidation of the oxide. It appears to be greatly dependent on the amount of water present in the sample. The greatest loss is found to occur in those oxides which were examined soon after they were prepared. The oxides of manganese appear to undergo a molecular change on being kept in air for any length of time, whereby their behaviour on heating may be entirely reversed, a gain of oxygen taking place instead of a loss. This molecular change is not accompanied by a variation in composition. When small quantities of manganese oxides are heated in a platinum dish over a Bunsen burner, the oxide obtained was remarkably constant in composition when the gas was at the day pressure: with a larger supply of gas less constant results were obtained.

The **President** said that the oxides of manganese seemed sometimes to lose and sometimes to gain in weight when heated to 100°. The circumstances which governed this alteration he hoped the author would eventually succeed in determining. He should like to ask if any special tests had been made for the presence of organic matter, and whether any specific gravities of the oxides had been taken.

Dr. **Wright** asked if the absence of other metallic oxides had been secured, as in his researches great care was found to be necessary in this respect.

Mr. **Pickering** replied that the pure oxides used in his research, after solution in acid and precipitation with ammonium sulphide, gave no fixed residue on evaporation with sulphuric acid. Some of the specimens were very light and some very heavy powders. The presence of organic matter could not account for a gain in weight.

Prof. **McLeod** said that it would be interesting to know if any oxygen was given off at ordinary temperatures in a Sprengel vacuum.

The **President** then called upon Dr. **Gladstone** to read a paper by himself and Mr. A. Tribe, entitled "*Aluminium Alcohols (Part I., Their Preparation by Means of the Aluminium-Iodine Reaction).*" In 1876 the authors showed that either aluminium or aluminium iodide singly is without action on absolute alcohol, but that together they decompose that liquid with facility. The resulting bodies are hydrogen and two new organic aluminic compounds, aluminic iodo-ethylate $(C_2H_5O)_3I_3Al_2$, and aluminic ethylate, $Al_2(C_2H_5O)_6$. It was also shown that one molecule of the iodide suffices to bring about the combination of very many atoms of aluminium with the oxy-radical of the alcohol, and that the amount of hydrogen set free is equivalent to that of the metal which disappears. In the present communication the authors have studied more completely the composition and properties of the ethylate, and have applied the aluminium-iodine reaction to the preparation of analogous aluminium derivatives of other alcohols. 20 c.c. of alcohol, 2 grms. of aluminium-foil, and 1 grm. of iodine are heated in a flask to 100°: the decomposition proceeds with tolerable rapidity until the metal has disappeared. The flask is then heated to 300° until distillation at that temperature ends. It is then connected with a receiver, and the apparatus exhausted of air, the product in the flask melted and distilled under diminished pressure. The authors have prepared and examined aluminic methylate, aluminic ethylate, aluminic propylate. No reaction took place when iso-propyl alcohol was used, although the iso-iodide and the zinc iso-propide are less stable than the corresponding normal compounds. Aluminic isobutylate and aluminic amylate were prepared without difficulty. Aluminic acetylate was formed, but was to a great extent decomposed by the temperature necessary for its formation by this reaction. On trying the reaction with allyl alcohol aluminic allylate was apparently formed, but could not be isolated by distillation *in vacuo*. Aluminic phenylate, cresylate, and thymolate were prepared, but could not be distilled; they were, however, obtained tolerably pure by heating the respective alcohols to about 200° with the theoretical quantity of aluminium and enough iodine to start the action, and then pouring off the semi-fluid product. The hydrogen of benzylic alcohol can be replaced by aluminium, but the compound formed is completely decomposed when heated *in vacuo*. No aluminium compounds of ethene alcohol or propenyl alcohol were obtained. The aluminic alcohols are solid, non-crystalline, greyish white bodies, and are more or less decomposed on heating at the ordinary atmospheric pressure, but those of the methyl series distil at reduced pressure, and have the property of remaining fluid below their fusing-points.

In reply to Dr. Armstrong, Dr. **Gladstone** said that an attempt had been made to determine the vapour-densities of these bodies, but up to the present time had not been successful.

Mr. W. H. **Perkin** then gave an account of the artificial production of indigo, and prepared some before the Society. He stated that through the kindness of Dr. Caro he was enabled to exhibit some specimens of artificial indigo, and to demonstrate its preparation. We are indebted to Prof. Adolph Baeyer for the discovery of the process. The starting-point is practically toluene, C_7H_6O; indigo, therefore, may be classed among the coal-tar colours. From this hydrocarbon cinnamic acid is prepared by a process which was suggested by Mr. Perkin not long ago, and which at the time did not seem likely to be so soon turned to a practical application. The next step is the conversion of the cinnamic acid into its nitro-derivative by the action of nitric acid. Two isomers can be thus formed, the ortho and the para: the ortho is the only one which is useful in this process. Fortunately, by employing certain precautions, this ortho body can be obtained as the chief product of the reaction. On exposing this nitro body to the vapour of bromine ortho-nitro-dibrom-hydro-cinnamic acid is prepared. By treatment with caustic soda ortho-nitro-propiolic acid is obtained, which, on reduction by grape-sugar in a solution rendered alkaline by an alkali or an alkaline earth, is converted into indigo. The steps in the process are therefore:—Toluene, C_7H_6O; dichloride of benzyl, $C_6H_5,CHCl_2$: cinnamic acid, $C_9H_8O_2$; ortho-nitro-cinnamic acid, $C_9H_7(NO_2)O_2$; ortho-nitro-dibrom-hydro-cinnamic acid, $C_9H_7Br_2O_2(NO_2)$; ortho-nitro-phenyl-propiolic acid, $C_9H_5(NO_2)O_2$; and finally, indigo, $C_{16}H_{10}N_2O_2$. Prof. Baeyer has also discovered a second process by heating ortho-nitro-phenyl-oxacrylic acid to about 110°, but practically this process is not so good as the one just mentioned. The artificial indigo is identical with the natural product in every respect: it can be reduced to form an indigo-vat, and gives a sulpho-acid soluble in water, which dyes silk. The colouring matter may be made in the fibres of the fabric by passing them through, or printing them with a mixture, $C_9H_5(NO_2)O_2$, alkali, and grape-sugar, and them steaming or heating them for a few minutes. Every effort is being made on the Continent to produce the colouring matter commercially, and there is but little doubt as to the ultimate and speedy success of these endeavours.

The President, in thanking Mr. Perkin for his interesting demonstration, commented on the very superior facilities which existed in Germany in the technical institutions and the large manufactories for working out such questions as the above. In one large works no less than thirty chemists were employed, at liberal salaries, working at subjects connected with chemical technology. If such arrangements existed in this country we should have nothing to fear from competition.

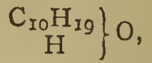
"*On the Absorption-spectrum of Ozone*," by W. N. Hartley. The author has photographed, by means of the light afforded by the coil and Leyden jar (which also served to ozonised the air), the absorption-spectrum. The absorption-band stretches from wave-length 285 to 233 millionths of m.m.: the air contained 1 vol. of ozone in 2000 of oxygen. Great care was taken to exclude chlorine and oxides of nitrogen. The mean wave-length of the rays intercepted by ozone is 256 millionths m.m. From this the author deduces the mean rate of vibration of the molecule of ozone as 1231 billions per second.

"*On the Probable Absorption of the Solar Ray by Atmospheric Ozone*," by W. N. Hartley. The author refers to the recent experiments of Cornu on the shortening of the solar spectrum in the ultra-violet portion. This observer has photographed the spectrum at different altitudes. He finds that the longest spectrum was obtained at the greatest height at mid-day; the shortening is therefore due to the atmosphere. From various considerations the author concludes that it is highly probable that the absorbing substance in the atmosphere is the ozone. The author attributes the blue tint of the atmosphere to a great extent to ozone. The blue colour cannot be due to the aqueous vapour nor the oxides of nitrogen. Some further experiments on this interesting question are promised.

"*On the Synthetical Production of New Acids of the Pyruvic Series* (*Part II., Isobutyryl and Butyryl-formic Acids*)," by E. Moritz. The author has prepared and studied the following bodies:—Isobutyryl-formic amide, di-isobutyryl-dicyanide, and isobutyryl-formic acid. The acid was prepared by treating the polymeric cyanide with hydrochloric acid, and subsequent boiling in a water-bath. If the mixture of cyanide and hydrochloric acid be poured into cold water before boiling oily drops form at the bottom, which subsequently solidify. Two substances were isolated from this deposit, one soluble in sodic carbonate fusing at 187°, the other insoluble, fusing at 207° to 208°. The author has also prepared and studied butyryl-formic amide, dibutyryl-dicyanide, and butyryl-formic acid. In conclusion, the author states that an improved method for preparing the acid-radicle cyanide is necessary for the success of further investigations.

"*On the Ancient Alum Well at Harrogate*," by R. H. Davis. This old well was noticed by Dr. Garnett in 1791 as containing alum and iron. Up till 1870 the well seems to have been forgotten; in that year, however, it was brought to light again during some excavations made for increasing the supply of sulphur-water. It is a pale reddish brown water, strongly acid to litmus, and very astringent to the taste. The well is surrounded with sulphur-wells, and is of comparatively superficial origin. The author gives an analysis of the water in grains per gallon:—$Fe_2(SO_4)_3$, 78·76; $FeSO_4$, 69 33; $Al_2(SO_4)_3$, 89·47; $CaSO_4$, 56·91; $MgSO_4$, 57·38; K_2SO_4, 3·14; Am_2SO_4, 2·19; NaCl, 33·9; and SiO_2, 3·27. Total residue, 397·25.

"*On Peppermint Camphor*," by M. Moriya, of Tokio. This paper forms No. IV. of the contributions from the University Laboratory of Tokio, and forms part of a thesis presented by the author on graduating in chemistry. In 1862 Oppenheim concluded that this camphor was a mono-atomic alcohol,—



$$\left.\begin{matrix}C_{10}H_{19}\\H\end{matrix}\right\}O,$$

and named it Menthol. In 1876 Beckett and Wright showed that menthol was connected with the aromatic and paraffin series. The sample examined by the author was obtained from Yonezawa. Its specific rotatory power was $-59·3°$; sp. gr. at 15°, 0·89; melting-point, 37° to 35°: distils over at 210° to 212°; crystallised in long needles; soluble in absolute alcohol, ether, CS_2, chloroform, and glacial acetic acid. By the action of bichromate and sulphuric acid at 120° for ten hours a colourless oil was obtained, $C_{10}H_{18}O$, boiling at 204° to 205°; sp. gr. 0·9032; insoluble in water; soluble in ether, alcohol, and chloroform. By the action of nitric acid an explosive nitro-body, probably $C_{10}H_{19}NO_2$, was obtained, yielding an amide, $C_{10}H_{19}NH_2$. By treating the camphor with twenty times its volume of fuming nitric acid a yellow liquid is obtained, which on evaporation yielded a white crystalline substance, soluble in water, which contained no nitrogen. Analysis indicated the compound to be normal pyro-tartaric acid. Bromo-menthol, $C_{10}H_{19}Br$, was also prepared. By the action of zinc chloride menthene was obtained. By freezing the oil of peppermint obtained by distilling the plant and separating the crystals, and subsequently fractionally distilling, a liquid was obtained, which the author concludes to be the liquid which holds the camphor in solution. It has the composition $C_{10}H_{18}O$. In a note to the paper Prof. Atkinson suggests constitutional formulæ for menthol founded on the above researches.

The Society then adjourned to December 2, when a ballot will be held for the election of Fellows, and the following papers will be read:—"Communications from the Laboratory of the University College, Bristol," by W. Ramsay; "On the Specific Volume of Chloral, by Laura Maude Passavant, Brown Scholar at the Yorkshire College; "On the Formation of Carbon Tetra-bromide in the Manufacture of Bromine," by J. W. Hamilton.

PHYSICAL SOCIETY.

Saturday, November 13, 1880.

Prof. W. G. Adams in the Chair.

Mr. Bosanquet, of St. John's College Physical Laboratory, Cambridge, read a paper "*On the Nature of the Sounds which occur in the Beats of Consonance.*" From mis-timed octaves and twelfths he found that when the beats of the harmonics are cleared away each beat consists entirely of variations in the intensity of the lower notes. He gave the mathematical theory of these beats, and likewise of the curves given by the harmonograph. He also described an ear-tube for using in connection with a resonator. It is difficult to get definite results with a resonator unless the passage from the latter to the ear is closed to sound. The ear-tube consists of a copper pipe bent into a sickle shape to gird the face so that the ends may enter the ears into which they are screwed, plugging them close. The sound is led from the resonator to the middle of this bent pipe by a flexible india-rubber tube, and thence to the ears.

Mr. Brown read a paper "*On Action at a Distance.*" He drew attention to the fact that though Newton disbelieved in action at a distance, he did not pronounce whether the medium was material or immaterial. Mr. Brown showed that the hypothesis of a material medium was encumbered with difficulties, since, among other reasons, direct contact could not explain gravity, projection of small particles from one body to another could not explain attraction, and Le Sage's theory of corpuscles (as modified by Mr. Tolver Preston) required an enormous degree of porosity in masses of matter. The nature of magnetism and vibrations was also discussed by the author.

Mr. J. Macfarlane Gray read a paper "*On the Mechanical Nature of the Forces called Attraction*," and gave grounds for attributing them to the pressures of a universal material ether of a gaseous nature. The paper was long, and had to be in part left unread. The hypothesis held by Mr. Gray is remarkably confirmed by numerical results obtained by him.

Professor Cottrell threw some doubts on Mr. Gray's results, on the score that numerical coincidences were not always safe ground for basing theoretical deductions on.

Mr. Gray stated that in the parts of the paper which had to be skipped, Prof. Cottrell's objections were answered. He also pointed out that Mr. Brown in his criticism of the gasiform ether had not taken into account the important condition that the particles of ether have volume.

Professors Perry and Ayrton read a "*Note on the Contact Theory of Herr Exner*," recently brought before the Academy of Sciences, Vienna. They showed that Exner's experimental results disagreed with the concordant results of several independent experimenters, namely, Kohlrausch, Hankner, and Ayrton and Perry. They concluded that Herr Exner's experiments were inaccurate. They further argued that Exner's second and later paper, so far from being a disproof of the contact theory of electromotive force as now received, is in reality a proof of it.

Dr. Wright stated that he will read a paper on this subject soon; and Prof. Reinhold said that Herr Exner had since corrected some of the results of his earlier papers on contact electricity.

Prof. Minchin, of Cooper's Hill Engineering College, exhibited a New Photo-electric Cell. This consists of a vessel of water containing a little acid carbonate of calcium, and two tinfoil plates. When a beam of lime-light was allowed to fall on one of the plates a powerful current was set up in the cell, as seen by the deflection of a galvanometer connected in circuit with the plates. When a red glass screen intercepted the beam the effect was very slight. Prof. Minchin had begun his experiments with fluorescene, but found "hard" water, containing this salt of lime, do equally well. The cell possesses this advantage—that the current it gives soon decreases in the light. When first the light falls on it the exposed plate is positive, but it soon changes to negative. Prof. Minchin had tried the cell in place of a selenium one in the photophone, but with unsatisfactory results.

CORRESPONDENCE.

THE SPECIFIC ROTATORY POWER OF CANE- AND INVERT-SUGAR.

To the Editor of the Chemical News.

Sir,—In his last letter (page 221) Mr. Watt demurs to my statement of the nature of his doubts, and defines them for himself. With regard to your correspondent's first point, in which he questions "that any authority should fail to take into account the increase of weight by inversion," I can only refer him again to the two notable instances adduced in my former letter. Whether Mr. Watt "accepts" them or not, there can be no doubt that they are distinct cases in which it has been assumed that a solution of invert-sugar contained the same percentage as before inversion. They are not the only published cases of the sort, and the private correspondence which has arisen from my paper shows that the same omission has been pretty widely practised.

I certainly thought that I had sufficiently explained that I was not prepared to adhere to my suggestion—for it was nothing more—that the value $-25°$ was erroneous *because* the increase of weight by inversion was ignored. Anyhow, I now state definitely that I think Mr. Watt has shown that such was not the origin of the erroneous value. But the erroneous calculations remain as facts, whatever their origin, and clearly prove my statement that the increase of weight is not always taken into account.

I may point out that the value Sj or $[\alpha]j = -26°$ at 14, for invert-sugar, was based on the facts that a solution of cane-sugar which rotates $+100$ divisions before inversion will read -37 divisions at 14° C. after inversion; and that the specific rotatory power of cane-sugar is 73·8° for the transition tint. I by no means contend for the accuracy of these data, but I protest against chemists playing fast and loose. They must not with the same breath accept the above data as facts and yet give $-25°$ as the value of Sj for invert-sugar at 14° C.

But there is another view of the subject to which my attention has been further called since the commencement of this correspondence, by a valuable private communication from Mr. A. Watt. In my paper, the conclusions were deduced from published observations made with instruments employing the transition-tint. It is unfortunate that whilst Soleil's instrument is rapidly becoming superseded by polarimeters employing sodium light, very few observations have been published of the rotatory power of *invert*-sugar, as determined by the latter instrument. Again, in English works, the polarimeters employing sodium light have hitherto received very meagre attention, and in some of the most recent English researches on sugars, as those of O'Sullivan and Brown and Heron, the rotatory powers are all determined by instruments using the transition-tint. But the transition tint is not a ray of definite refrangibility, and is even said to vary with the idiosyncracy of the observer. Further than this, it is well known that the relation of Sj to S_D is not the same for tartaric acid as it is for quartz, and although saccharine solutions are stated to follow the same law as the latter, it is by no means certain that this is true for all descriptions of sugar. On all these accounts it is preferable to make observations of specific rotation by sodium light, and in recording them the strength of the solution employed should always be stated. The importance of this factor is well shown in the following determinations by Hesse,* employing solutions of cane-sugar of various concentrations:—

Grms. of Sucrose per 100 c.c.	Value of S_D.
1	67·95
2	67·39
3	67·05
6	66·67
10	66·50
20	66·45

These results are in each case the mean of several independent determinations. Beyond a concentration of 10 c ($c \leqq$ grammes per 100 c.c.) the decrease is pretty regularly 0·005° for each 1 c. The value of Hesse for the specific rotation of cane-sugar agrees with that of Tuschmidt,† who obtained 66·42 (apparently for somewhat concentrated solutions), and Backhoven,‡ who obtained the same number. Schmitz, again, has found 66·42 and 66·53 as the value of S_D when $c = 10$;§ and, lastly, Tollens gives $+66·48°$ as the correct value for S_{10D} in the case of cane-sugar.‖ These closely corresponding results afford overwhelming evidence in favour of a number closely approximating to $+66·5°$ for cane-sugar in solutions containing from 10 to 20 grms. per 100 c.c. It must be remembered, of course, that this is the *apparent* specific rotation for the concentration in question, the *absolute* value of S_D for cane-sugar being according to Tollens $+63·90°$, and according to Schmitz $+64·16°$.

Some of the values for the ratio $S_D : Sj$ were given in my paper. Although those given appear to agree fairly well among themselves, the discrepancy between them becomes more apparent if they be used for calculating a constant

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* *Annales de Chemie*, clxxvi., 95.

† *Journ. f. Prakt. Chem.*, [2], ii., 235.

‡ *Ibid.*, page 277.

§ *Ber. der Deutsch. Chem. Gesell.* 1877, p. 1414.

‖ *Ibid.*, p. 1403.

number for S_D into its equivalent in Sj. Thus if 66·5 be accepted as the value of S_D for cane-sugar, then the following numbers will be obtained on using the factors of various authorities for Sj:—

S_D. ×	Factor.	=	Sj.	Authority.
66·50 ×	1·129	=	75·08	Landolt.
	$\frac{24}{21\cdot67}$	=	73·65	Broch.
	$\frac{24}{21\cdot8}$	=	73·21	Girard and De Luynes.
	$\frac{24}{21\cdot54}$	=	74·09	Brown and Heron.
	1·091	=	72·55	Calderon.
	1·049	=	69·96	Weiss.

The factor of Weiss may safely be ignored. That of Calderon is remarkable. It is deduced from determinations made by him in Berthelot's laboratory with the view of revising that chemist's value for Sj (=73·8). Calderon found, for 10 to 20 per cent solutions of cane-sugar, $S_D = 67\cdot1°$ and $Sj = 73\cdot2°$.* Brown and Heron do not state the grounds of their adoption of the ratio employed by them. The mean of the first three results is $Sj = +73\cdot98°$, a value practically identical with 74·0. The mean of the first five results is $Sj = +73\cdot72°$.

The foregoing conflicting results of determinations of specific rotatory power may be advantageously pondered by those observers who are never tired of telling of the extreme accuracy with which polarimetric readings may be made.

Since my last letter appeared and this one was commenced I am glad to see that Mr. T. Bayley (page 233) has published some interesting determinations of the value of S_D for invert-sugar. If he or some other chemist having extensive practice in polarimetric observations will determine the value of S_D for invert-sugar under various conditions as to the *concentration* of the solution, proportion and strength of acid employed, and time and temperature employed for inversion, instead of blindly following the prescribed directions, it will be doing essential service.—I am, &c.,

Alfred H. Allen.

Sheffield, November 8, 1880.

Postscript.—The above letter was written and in type before that of Mr. J. Steiner appeared in your columns. It will be apparent that Mr. Steiner and myself have independently been writing to much the same effect. The fact that the correspondence should have led to the publication of such a valuable letter as that of Mr. Steiner is sufficient proof that the subject required ventilation.—A. H. A.

November 22, 1880.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Note.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 17, October 26, 1880.

Application of Selenium to the Construction of a Photo-electric Regulator of Heat for Baking Stained Glass.—P. Germain.—The author has taken advantage of the different influences exerted upon the electric resistance of this body by the different rays of the spectrum.

Certain Modifications Undergone by Glass.—J. Salleron.—The author often meets with well-made thermometers, the indications of which are erroneous to 8° or 10°, or more. Such changes occur at printing ink works, where oils are heated for several days to 270°; in glycerin works, and with rectifiers of benzol. Glass is not merely modified when heated to 300°; it undergoes true deformations at far lower temperatures. Thus the hydrometers used in sugar works, which are often exposed for a considerable time to temperatures of 95°, are affected. After an immersion of some days they are completely modified; their weight decreases, and they become erroneous to the extent of 7° or 8° B. [In many chemical works it has been found necessary to submit all hydrometers used for hot liquids to a weekly comparison with a standard instrument.—*Ed.* C.N.]

No. 18, November 2, 1880.

Heat of Formation of the Ethers Formed by the Hydracids.—M. Berthelot.—The author's experiments relate to the hydrochloric, hydrobromic, and hydriodic ethers of methyl and ethyl.

Heat of Formation of Carbon Disulphide.—M. Berthelot.—This compound is formed from its solid elements with absorption of heat, but it is probable that if formed from gaseous carbon and sulphur, heat would be liberated.

Application of the Photophone to the Study of the Noises taking place on the Surface of the Sun.—A. Graham Bell.—On visiting the observatory of Meudon, at the invitation of M. Janssen, Mr. Graham Bell examined with much care the large photographs which are being made there for the study of the solar surface. M. Janssen having informed him that he detected movements of a prodigious rapidity in the photospheric matter, Mr. Bell had the idea of employing the photophone for the reproduction of the sounds which these movements must necessarily produce on the surface of the sun. M. Janssen approved of the idea, and requested Mr. Bell to attempt its realisation at Meudon, placing all the instruments of the observatory at his disposal. The weather being very fine on Saturday last (October 30), Mr. Bell came to Meudon to attempt the experiment. A large solar image of 0·65 metre in diameter was examined with the selenium cylinder. The phenomena were not sufficiently decided to be regarded as successful, but Mr. Bell does not despair of succeeding on further examination. M. Janssen suggested that the chance of success would be much greater if in place of directly interrogating the solar image where the variations which are produced, though responding to considerable changes on the sun's surface, are not sufficiently rapid even in the most powerful instruments to cause the production of sounds in the photophone, a series of solar photographs of one and the same spot, taken at sufficient intervals to obtain well-marked variations in the condition of the spot, might be passed with a suitable rapidity before an object glass, which would give conjugated images upon the selenium apparatus. This would be a means of condensing into a time as brief as could be desired the variations which in solar images are much too slow to give rise to a sound. M. Janssen has placed himself at Mr. Bell's disposal to provide him with solar photographs suitable for carrying out this idea, and the latter has sent M. Janssen the photophonic apparatus requisite. It has appeared to M. Janssen that the idea of reproducing on earth the sounds caused by great phenomena on the surface of the sun was so important that the author's priority should be at once secured.

Oxidation of Mannite.—J. A. Pabst.—Mannite yields on oxidation a tribasic of the formula $C_6H_8O_9$.

Ferments of the Albuminoid Matters.—M. Duclaux.—These ferments are ærobian, anærobian, or both. If cultivated in milk they transform the caseine into soluble albuminoids. That which the ærobian forms effect by a slow and regular action, the anærobian accomplish by disengaging carbonic acid and hydrogen, a portion of

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* *Comptes Rendus*, lxxxiii., 393.

which becomes hydrogen sulphide and phosphide, the mass taking a putrid taste and smell.

Gazzetta Chimica Italiana.
Anno 10, Fasc. 3 and 4, 1880.

Ulmic Materials Produced by the Action of Acids upon Sugar.—F. Sestini.—The formulæ ascribed by Mulder to the ulmic products which had been dried at from 140° to 165° before being submitted to combustion are not admissible, since, at temperatures above 100°, these bodies lose a notable quantity of volatile matter, and in particular of formic acid. The ulmic substances obtained by the action of dilute sulphuric acid upon sugar, and which may be called sacchulmine, appear in the form of minute yellowish brown globules. On treatment with a cold aqueous solution of caustic potassa, sacchulmine gives off an acid principle derived from the action of sulphuric acid upon glucose. The ulmic matter (sacchulmine) insoluble in cold alkaline liquids is derived directly from saccharose. In the ulmification of sugar there is evolved a considerable quantity of volatile acids, especially formic acid.

Certain Derivatives of β-Chloro-butyric Acid.—Luigi Balbiano.—This paper is chiefly devoted to an examination of β-amido-butyric acid, and of the action of aniline upon β-chloro-butyric ether.

The Diffusion and the Physiological Condition of Copper in the Animal Organism, declared for the first time by Bartolomeo Bizio and vindicated by Prof. Giovanni Bizio.—The author seeks to prove that his father, Bartolomeo Bizio, was the original discoverer of the normal occurrence of copper in the animal economy.

Chemical Constituents of Stereocaulon Vesuvianum.—Emanuele Paterno.—The question is raised whether the succinic acid obtained from this lichen is a product of the decomposition of atranoric acid, which, however, on treatment with baryta, furnishes a product totally different.

Correspondenz-Blatt des Vereines Analytischer Chemiker.
No. 15.

Detection of Honey Sophisticated with Starch-Sugar.—A. Planta.—The author determines the grape-sugar present in honey before and after inversion. In pure honey, the proportion of pre-existing grape-sugar is from 63 to 71, whilst in sophisticated samples it is 29 to 37. But the starch-sugars of commerce contain a much higher proportion of pure grape-sugar than he assumes.

Determination of Sulphur in the Products of the Iron-Manufacture in Ores and Fuel.—A. Kollert.—From the *Chem. Central-Blatt.*

Simple Method for Determining the Temporary Hardness of Water.—A. Wartha.—The most accurate method is to titrate at a boil in a silver capsule with normal hydrochloric acid, using as indicator solution of alizarin or extract of logwood. As a more convenient method when travelling, &c., the author uses a tube 30 to 40 c.m. in length, closed and rounded at one end, and with a lower mark showing 10 c.c. From this mark, upwards towards the mouth, the tube is graduated in 0·1 of a c.c. For use, the tube is filled up to the lowest mark with water in question and a little piece of filter-paper is added which has been previously saturated with extract of logwood and dried again. This gives the water a violet colour. Centinormal hydrochloric acid is then added from a dropping bottle till the colour of the liquid approaches an orange. The tube is then closed with the thumb and well shaken. Carbonic acid escapes, and the liquid grows red again, Acid is again added, and the tube is again shaken till a single drop of acid changes the colour of the liquid to a pure lemon yellow. The quantity of centinormal acid used is then read off on the graduated tube, and can be calculated into calcium carbonate.

MISCELLANEOUS.

Adulteration of Food.—The following was cabled from New York on the 15th November inst.:—The First Prize of 500 dols., offered by the National Board of Trade of the United States of America, for the best Essay and Draft of an Act to prevent injurious Adulteration and regulate the Sale of Food without imposing unnecessary burdens upon commerce, has been awarded to Mr. G. W. Wigner, F.C.S., F.I.C., Honorary Secretary of the Society of Public Analysts, &c., of London, England.

Chemical Constitution of Milk.—Dr. N. Gerber.—Caseine is not a homogeneous albuminoid, but a mixture of albumen and protalb-bodies which appear as transition-stages in peptonisation. In the milk globules has been found an albumenoid which constitutes the serum. In the curd are met with an albuminous body identical with the stromæi alb-compound of the globules, a body which Danilewsky and Radenhausen name orroproteine and two series of peptones. Hence it is no longer proper in milk-analysis to speak of caseine and albumen, but rather of albuminates.—*Correspondenz-Blatt,* 13, 1880.

NOTES AND QUERIES.

Analysis of Oils.—Would you kindly inform me of the best treatise on the analysis of oils, tallow, and lubricants generally, and where such a book may be procured ?—R. T.

Preventing Iron from Rusting.—In making some photographic experiments I find that iron and steel do not rust when immersed in solutions of caustic soda and caustic potash, but cannot discover any explanation of the fact in books to which I have access. Would you kindly, therefore, in your answers to correspondents, next issue, favour an earnest student with the reason of the phenomenon, and say whether any other solutions protect those metals from oxidation. I cannot understand why the alkali in the solution prevents the oxygen in the water acting on the iron or steel.—A.B.

MEETINGS FOR THE WEEK.

Monday, 29th.—Medical, 8.30.
—— Society of Arts, 8. "Some Points of Contact between the Scientific and Artistic Aspects of Pottery and Porcelain," by A. H. Church, F.C.S.
Tuesday, 30th.—Civil Engineers, 8.
—— Royal, 4.30. (Anniversary.)
Wednesday, Dec. 1st.—Society of Arts, 8. "The Photophone," by Prof. Alexander Graham Bell.
—— Geological, 8.
—— Pharmaceutical, 8.
Thursday, 2nd.—Chemical, 8. "Communication from the Laboratory of the University College, Bristol," by W. Ramsey. "On the Specific Volume of Chloral," by Laura Maude Passavant. "On the Formation of Carbon Tetra-bromide in the Manufacture of Bromide," by J. W. Hamilton. Ballot for the Election of Fellows.
Friday, 3rd.—Geologists' Association, 8.

TO CORRESPONDENTS.

D. E.—Consult some standard work on Quantitative Analysis.

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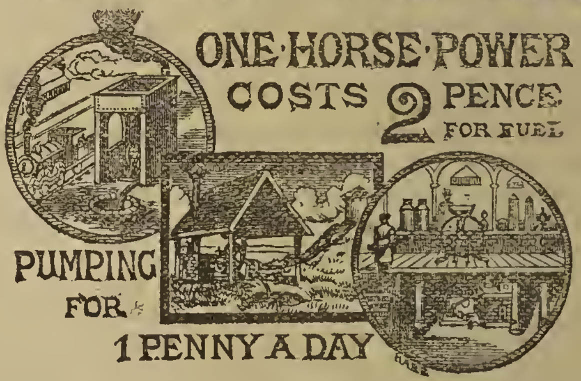

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THE CHEMICAL NEWS.

Vol. XLII. No. 1097.

NOTE ON THE ATOMIC WEIGHT OF GLUCINUM, OR BERYLLIUM.

By J. EMERSON REYNOLDS, M.D., F.R.S.

The present position of this much discussed question has been stated so clearly by Mr. Humpidge in the Chemical News, vol. xlii., p. 261, that, for the purposes of this note, I shall partially adopt his account of the results of my earlier experiments and those of MM. Nilson and Pettersen, and proceed to give the details asked for concerning my first observations. In doing so, however, I have no intention to refer to my later results, which will be published in full in due course, or to enter upon the discussion of the side questions raised by L. Meyer, Brauner, and others, although I think Mr. Humpidge underrates the importance of Meyer's criticism of MM. Nilson and Pettersen's last results.

I may remark in passing that the latest experiments of the Swedish chemists afford some ground for a suspicion I have long entertained, that in the earlier experiments on both sides we have possibly dealt with two forms of the same substance. The literature relating to glucinum contains the records of a particularly large number of differences of opinion amongst chemists of equal skill and ability; it may be that they but indicate some real differences in matter.

Preparation of Pure Glucina or Beryllia.—The material operated upon was the product obtained from Donegal beryl by the well-known method of fusion with alkaline carbonate and subsequent treatment with hydrochloric acid; separation of most of the silica by evaporation, extraction of the earths again with acid, addition of chlorine water, and precipitation of all the iron and a small proportion of the earthy hydrates by ammonia carefully added. Aluminic and glucinic hydrates were then precipitated from the iron-free filtrate by excess of ammonia, and the mixed hydrates, after thorough washing, were digested for a week with a large excess of nearly saturated cold solution of ammonium carbonate. The glucinic solution was then filtered from the undissolved alumina, distilled to dryness, and the residue thoroughly washed with boiling water, and heated to redness. The glucina thus obtained still contained silica and alumina, and the method followed in its purification was that recommended by Dr. Wolcott Gibbs,* and is the only process known to me that affords a perfectly pure product. It consisted in fusion of the glucina with excess of double fluoride of hydrogen and potassium in a large platinum dish. The product was extracted with boiling water and the solution filtered through paper supported by a platinum cone; the filtrate was received in a platinum dish, and it then deposited crystals of the double fluoride of glucinum and potassium. The aluminium was left on the filter in the form of fluoride of aluminium and potassium. The potassio-glucinic fluoride was purified by recrystallisation, and the pure hydrate obtained from it in the following way:—The pure salt was heated in a platinum dish with pure sulphuric acid until all hydrofluoric acid was expelled, and along with it every possible trace of silica; the residual sulphate was then poured into water, precipitated by ammonia, thoroughly washed, and dried. In this way the perfectly pure oxide was obtained that was employed in my experiments.

Conversion of Oxide into Chloride.—The dried hydrate obtained as above was mixed with half its weight of sugar charcoal, and then heated strongly in a platinum crucible. The residue was mixed with oil free from lead, and made into balls, which latter were heated to redness in a platinum crucible. The somewhat friable lumps thus obtained were transferred to a porcelain tube, and heated to full redness in a current of washed and dried chlorine gas. The pure white chloride was tested in the following way:—A portion was added to water acidulated with hydrochloric acid, and, when all dissolved, a saturated solution of ammonium carbonate was added in excess. The precipitate first formed soon dissolved in excess, and no cloud appeared on standing; therefore alumina had not been taken up from the tube. The absence of silica was proved by evaporating the aqueous solution of the chloride to dryness, heating to 150° C., and taking up the residue with hydrochloric acid in which it was completely soluble.

Reduction of the Chloride.—A platinum crucible was lined as completely as possible with the pure chloride; a mixture of the latter with one-third of its weight of clean metallic sodium in small pieces was then introduced, covered with a layer of pure chloride, and a piece of lime used as a lid for the crucible. The temperature was not allowed to rise so high as to liquefy the mass, and on removal of the material from the crucible those portions that had been in contact with the sides were rejected. The rest was then placed in a small lime crucible, covered with pure sodium chloride, and heated strongly. On breaking the crucible a small coherent mass was obtained after washing away all soluble matter. There was great loss in this operation, as much of the finely-divided glucinum, or beryllium, had been carried into the porous lime; hence it would be very difficult and costly to prepare even 2 or 3 grms. in this way. The semi-fused mass obtained in this operation was carefully cleaned from adherent oxide, and was then reduced by trimming under a magnifier to a little over 2 decigrms. The small piece of beryllium thus prepared, and used in the determination to be presently described, was certainly free from aluminium and silicon. The importance of excluding the latter is evident when it is remembered that beryllium can unite with a large proportion of silicon,* and is said even to reduce the latter from silicates. As stated in my paper I suspected the presence of platinum in the metal, and after the publication of the results I dissolved my sample of metal in hydrochloric acid, when a minute proportion of a dark powder was left, which proved to be platinum. The total weight of beryllium then at my disposal was so small that an estimation of the platinum in it was out of the question. The beryllium solution was tested for iron, and it gave a very faint reaction for that metal. I much regret that it did not occur to me until long after the solution of my metal to examine it for any possible alloy with sodium.

Characters of the Metal.—The mass when burnished showed a somewhat silvery lustre. When air was removed it sank in a solution† whose specific gravity = 2. Its specific gravity was therefore greater than 2, but very slightly higher, as it did not sink in a solution whose specific gravity was 2·13. It did not seem to decompose water in the cold and on standing for a short time. It was soluble in dilute hydrochloric acid, and was attacked by potash. Therefore, so far as could be ascertained with the small quantity at my disposal, the metal agreed in characters with that previously obtained by Debray. I did not observe any trace of crystalline structure in the material. The sample of metal that possessed these characters was exhibited at the meeting of the last British Association at Glasgow.

First Determination of Atomic Heat.—It is not quite correct to refer to my experiments as estimations of specific

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* *American Journal of Science* [2], vol. xxxvii., p. 356.

* As much as 20 per cent of silicon.

† Of mercuric iodide in potassic iodide.

heat. All that I have made with beryllium aimed at the determination of the weight of the metal that contained at 100° C. the same quantity of heat as a centigramme atom of pure silver at the same temperature. The specific heat was then *calculated* from the data obtained.

I may state here that when I began my experiments with glucinum, or beryllium, any mental bias of which I was conscious was in favour of the atomic weight 13·8 rather than 9·2, owing chiefly to the frequent occurrence of 3Be in formulæ which represented the metal as a diad with the lower atomic weight. But in the first comparative calorimetric experiments described below, 9·2 centigrms. were taken as the mean between the equivalent of the metal (4·6) and 13·6, and therefore a convenient weight to begin with. My paper contains a sufficiently full description of the apparatus in which the above comparison was made: it was essentially a spirit-thermometer with a test-tube sealed into the bulb. I have lately improved the little instrument, and made a very convenient *atometer;* but this form will be described in another communication. The measurements I shall now give were obtained with the first apparatus made.

Three metals were compared in the instrument, namely, pure silver, pure aluminium, and the beryllium, which latter I had at least endeavoured to obtain in a pure condition. When a centigrm. atom of silver was heated in the steam-bath used by Bunsen for the same purpose, and dropped into the tube by the calorimeter, which contained 1 cubic centimetre of water, the thread of mercury in the graduated stem of the instrument rose through 122 units of the arbitrary scale. The silver was removed, as in Bunsen's excellent method, the metal again heated to the same temperature, and again dropped into the calorimeter, which had meanwhile been gradually cooled to the original starting-point. Four experiments thus made gave the following expansions:—

Expt. 1	122
,, 2	120
,, 3	125
,, 4	124
Mean	122·7

A pair of similar experiments was made with 27·5 centigrms. of aluminium, under the same conditions as regards temperature and starting-point on the scale. The following numbers were obtained:—

Expt. 1	127
,, 2	130
Mean	128·5

In the case of the smaller mass of aluminium there was probably less loss of heat in transference to the calorimeter, hence the higher value obtained. A corresponding difference is observed in the case of magnesium.

Another pair of experiments was made with 9·2 centigrms. of the beryllium. The readings were:—

Expt. 1	120
,, 2	116
Mean	118

These numbers seemed sufficiently to distinguish between the weights 9·2 and 13·8, for if the latter were the true atomic weight, the expansion caused by 9·2 c.grms. of metal ought not to exceed some 80 measures of my scale.

If we take the atomic heat of silver as 6·15, that of beryllium would be as nearly as possible 5·91.

It has been objected, however, that the comparison of weights so widely different is unsatisfactory, and there is no doubt that the experiments show a tendency to exaggeration of atomic heat in the case of aluminium. But this objection is diminished in force if aluminium be taken as the standard, and its atomic heat be represented by the number 5·84. On this scale, even accepting the highest reading for aluminium and the lowest for beryllium, the atomic heat of the latter is 5·2.

I have no desire to press these results beyond their legitimate bounds, nor do I wish to depreciate the value of the determinations made by MM. Nilson and Pettersen, for those only who have had much experience with beryllium and its compounds can fairly estimate the difficulties surrounding the investigation and the perplexing character of the results often obtained. At the same time I do not think that chemists are likely to accept as decisive a determination of specific heat made with a metal containing nearly 13 per cent of impurity! Although corrections were applied for these impurities, we have no evidence as to their condition in the specimen. Any impurity in my metal was likely to *lower*, not to raise the specific heat.

The question arises, however, whether there may not be some connecting links between the two groups of experiments: in other words, whether we may not have dealt with the same matter, but in different states of aggregation. Nilson and Pettersen's metal was in distinct crystals: mine did not exhibit any crystalline structure. Now, as a rule, to which there are comparatively few exceptions, the specific heat is lower in the crystalline than in the uncrystalline form, and even difference of crystalline form is marked, as in the case of tin, by change of specific heat. This will appear from the following table:—

Element.	Specific Heat.
Sulphur crystals	0·1776
,, recently fused	0·2025
Carbon—animal, black, pure	0·2608
,, wood charcoal	0·2415
,, graphite	0·2018
,, diamond	0·1468
Selenium—"metallic"	0·0761
,, vitreous	0·1031
Tin—cast	0·0559
,, allotropic	0·0545

The wide differences observed in some of these cases are in the same direction as are those between the beryllium results: it is at least possible that the cause may be similar in all cases. However, this question cannot be definitely settled until we succeed in preparing a good lump of homogeneous beryllium in a pure state. Until this is accomplished, and the question finally decided, we are likely to keep to our respective atomic weights for beryllium.

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MATERIALS CONTAINING TANNIN USED IN JAPAN.*

By I. ISHIKAWA,
Graduate of the University of Tokio.

Numerous methods have been proposed for the purpose of estimating tannin, but none are free from objections, and few of them give really trustworthy results. That method which has been received with most favour is Löwenthal's, which is based upon the oxidation of the tannin by means of a solution of potassic permanganate. The tannin is found to be completely oxidised when a certain weight of indigo-carmine added has lost its colour, and this acts as an indicator. Knowing the amount of permanganate required to oxidise the indigo-carmine alone, the amount required to oxidise tannin and gallic acid, and other things present, is found by difference. The tannin is then removed by means of gelatin, and the number of cubic centimetres of potassic permanganate still required is deducted from the total amount, the difference being a measure of the amount of tannin in solution. In using this method it is evident that the amount of permanganate equivalent to a certain weight of pure tannin must be accurately known. This equivalent was determined by

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* Abstract of a thesis presented on graduating, July, 1880.

Neubauer, who employed tannin from nut-galls, and found that a litre of permanganate of potassium solution, which required 6·3 grms. of oxalic acid to completely decolourise it, used 4·157 grms. of tannin. Oser, who used tannin from oak-bark, found that a litre of permanganate of the same strength as above required 6·236 grms. of tannin, so that nut-gall tannin decolourises 1½ times as much permanganate as oak-bark tannin. Löwenthal's process is, therefore, at present only applicable to the determination of the two tannins above mentioned, and if employed for the assay of other tannin materials the results only give the amount of tannin in terms of one or other of the two just mentioned.

In examining the series of tannin matter used in Japan, I have employed Löwenthal's method, and the percentage numbers express the amounts of tannin in terms of the tannin of Japanese gall-nuts. As it was not certain that the permanganate equivalent of the tannin contained in them was the same as that contained in the European variety, I first carefully determined the amount of permanganate necessary to oxidise a given weight of the purified tannin.

Preparation of the Tannin.—A quantity of powdered Japanese gall-nuts was placed in a percolating apparatus, and treated with a mixture of alcohol and ether the percolate afterwards separating into two layers. The upper ethereal layer was carefully decanted, and the aqueous solution was evaporated on the water-bath. The residue was treated with hot water, and filtered from some insoluble matter, and from the filtrate the tannin was fractionally precipitated by acetate of lead, the first and last portions being rejected. The tannate of lead was then decomposed by means of an insufficient quantity of oxalic acid, as recommended by Löwenthal. The solution finally obtained was evaporated first over the water-bath, afterwards *in vacuo*. The tannin thus obtained was a light porous mass of a pale yellow colour.

The potassic permanganate solution was made so that 1 c.c. exactly corresponded with 1 c c. of a solution of oxalic acid containing 6·3 grm. $C_2H_2O_4.2aq$ in 1 litre. The indigo solution was prepared by treating indigo with fuming oil of vitriol, diluting with water and filtering.

Process.—1·2 grms. of tannin were dissolved in 250 c.c. of water. Of this solution an aliquot portion was added to a large volume of water containing a known amount of indigo solution and sulphuric acid. The permanganate solution was added drop by drop to this mixture until the deep blue colour of the indigo changed to a clear golden-yellow. A separate determination of the amount of permanganate required to decolourise the indigo alone was made, and this number was deducted from the total number above obtained.

The following are the details:—25 e.c. of the indigo solution and 10 c.c. of sulphuric acid were diluted with water, and required, in five separate determinations, 21·25, 21·15, 21·13, 21·20, 21·10 c.c. of permanganate; average, 21·166 c.c.

A similar mixture, with the addition of 10 c.c. of the above tannin solution, was examined. Required in five experiments:—32·73, 32·67, 32·60, 32·70, 32·70; average, 32·68. Required for the tannin, therefore, 32·68 − 21·166 = 11·514 c.c.

One litre of the permanate solution thus corresponds with 4·1688 grms. of tannin. This number is sufficiently near to that (4·157 grms.) obtained by Neubauer.

Determination of the Tannin in Barks.—Besides the solutions already mentioned, two others are required—1st. A solution of gelatin. 25 grms. of gelatin, well swollen in cold water, were melted over the water-bath, and diluted to 1 litre with a saturated solution of sodic chloride. 2nd. A saturated solution of sodic chloride. This solution contained in 1 litre 25 c.c. of oil of vitriol. The assay is then conducted as follows:—5 to 10 grms. of the rasped bark are exhausted with boiling water, and the solution when cold made up to half a litre. Of this solution 10 or 20 c.c. are taken according to the richness in tannin, mixed with a considerable volume of water containing known amount of indigo solution and sulphuric acid, and the amount of permanganate required to decolourise the solution ascertained. The next step is to precipitate the tannin from another portion of the solution, and to estimate the effect of the gallic acid and other impurities upon the permanganate. 100 c.c. of the tannin solution are mixed with 50 c.c. of the salted gelatin: after well stirring, 100 c.c. of the salt solution are added, and the mixture allowed to stand over night. After filtering, an aliquot portion is mixed with indigo, and tested with permanganate as before. In presence of salt the action of the permanganate on gelatin may be neglected. Allowing for the indigo added in both cases, and deducting the number of c.c. of permanganate used in the second experiment from that used for the same number of c.c. of tannin solutions before precipitation, the amount of tannin present is found by using the equivalent before determined.

Enumeration of Tannin Materials.

1. *Kibushi, or Gall-nuts.*—This stands first in importance. It is also known as *fushi*, and in old writings as *mimi-fushi*, and is the excrescence produced on the *Rhus semialata* by the puncture of an insect. According to the botanist, Ranzan, the tree grows wild in the plains and among mountains, but it is only upon the latter kind that the excrescence is found. The best product is found on trees growing in moist places. The form of the gall-nut is very irregular, and its surface is covered with small bosses. It is hollow, and contains inside the eggs of the insect. It varies in size, from about half an inch to one-and-a-half inches in length, and somewhat less in breadth. The thickness of the walls ranges between 1 and 2 millimetres. In texture it is hard and horny, but frequently parts of it are quite friable, and of an opaque white appearance. When freshly gathered its colour is yellow, but on keeping it assumes a grey colour, and its surface is of a velvety appearance.

In the Tokio market only the Japanese product is now met with, but about eight or nine years ago considerable quantities were imported from China. I was fortunate enough, on a visit to Kiyoto, to obtain a specimen of the Chinese variety, the percentage of tannin in which is given at the end. In appearance it is quite different from the Japanese; it is larger, and its surface is wanting in the projections characteristic of the Japanese gall-nut. It is either spherical or oblong, harder and stronger in texture, and the thickness of the walls is three or four times greater than that of the Japanese.

The gall-nuts found on the markets of Osaka and Kiyoto come from the neighbouring provinces of Kii, Bichû, Tango. In Tokio we find gall-nuts from Chichifu and other districts. The galls are gathered in July, August, or September. They are plucked, placed in wooden tubs, and boiling water added, which is left for half an hour; after which they are removed, and dried in the sun for three or four days. From the analysis it will be seen that the percentage of tannin varies considerably. The dyers usually prefer the old kinds, which they say are stronger. In Kiyoto they are kept in storehouses for years before being used. The old varieties are more brittle, and yield darker decoctions. In the specimens from Kii it will be seen that the older kind contains 65·26 per cent, while the younger variety has only 60·9 per cent tannin.

The reactions given by a solution of the Japanese gall-nut are identical with those given by Aleppo galls.

2. *Yasha-Bushi* and *Han-no-ki.*—Yasha-bushi (alder fruits), frequently called *Yasha* only, is the fruit of the *Alnus firma*, which grows mainly in the provinces of Totomi, Idzu, Kai, and Iwaki. It is sold by the name of the province from which it comes, as, for example, Idzu Yasha. Some confusion appears to exist as to the species of *Alnus* from which the fruit is obtained. That which is sent to the market, and which contains most tannin, is the species above-mentioned, *A. firma*, but the fruit of the *A. maritima* (or Han-no-mi) is used to a small extent. The bark of this species is also used.

The decoction of this fruit is dark, and contains a large amount of resinous matter, and gives a dirty yellow precipitate with acetate of lead. The original solution gives a greenish black precipitate with ferric salts, but after partial purification it yields a bluish black precipitate. The analyses of two specimens given show that it contains much less tannin than the gall-nuts, but it is richer than most of the barks.

3. *Yobaihi* or *Shibuki*.—This is the bark of the Yamamomo-no-ki (*Myrica rubra*), which grows in many parts of Japan—Kii, Josa, Isé, Totomi, Idzu, and other warm provinces: that brought from Loochoo is, however, most highly esteemed. The thickness of the latter is two or three times as much as that found in this island, and, as will be seen, the percentage of tannin is greater. Besides the tannin, which gives a bluish black precipitate with ferric salts, the bark contains a body giving a yellow precipitate with acetate of lead. This dissolves in water, more easily in alcohol, and still more so in ether. By this means it can be obtained free from tannin.

4. *Zakuro*, Pomegranate rind.—The *Pumica granatum*, the fruit of which is known as the pomegranate, grows largely in the province of Kii, and is imported thence into Tokio. As sold it forms hard pieces of a beautiful yellow colour on the broken surfaces. The decoction gives a bluish black precipitate with ferric salts, and a yellow one with acetate of lead. The yellow body is soluble in ether, and can be extracted by that reagent. The rind is seldom used alone, but generally in conjunction with gall-nuts, for producing blacks on cloth, the presence of the yellow being considered by dyers to improve the appearance.

5. *Mangrove Bark* (Jap. Tangara) and *Betel-nut* (Binroji) are imported in small quantities from China and the south of Asia, but their use is extremely limited. The amount of tannin in one specimen of betel-nut is given in the table.

6. *Kashiwa Kawa* (oak-bark).—This, the bark of the *Quercus dentata*, is sometimes known amongst tanners by the same name as is given by dyers to the bark of the *Myrica rufra*, *i.e.*, *Shibuki*, and some confusion is therefore liable to arise between. The bark found in Tokio comes mainly from the provinces of Shimodzuke and Sasami, and from the mountainous districts of Musashi, the latter variety being highly esteemed. Although it contains only a small percentage of tannin—calculated as gallo-tannic acid—it is the cheapest and most abundant material found in Japan. It is mainly used in tanning and for giving strength and durability to fishing-nets. The bark of the *Quercus cuspidata* is also used. It is called Shii-Kawa.

7. *Kaki-no-shibu*.—The next material to be mentioned is not used in dyeing at all, but only as an antiseptic. This is a liquid called *Kaki-no-shibu*, and is the juice of certain varieties of the *Persimmon*, the fruit of the *Diospyros kaki*. The edible *Persimmon*, when quite ripe, is very sweet to the taste, but when unripe is very astringent: other varieties even in the mature condition do not lose their astringency. By drying in the sun, the skin being first removed, even these varieties become quite sweet, the tannin apparently disappearing completely. The *Kaki-no-shibu* is always prepared from the latter variety. The fruit is gathered early in the summer, and is beaten in stone mortars. It is next transferred to wooden tubs, in which the mass is covered with water, and allowed to stand for half-a-day. After this it is filtered through a straw bag, and the milky juice thus obtained is known as the best *shibu*. The residue in the bag is treated once more in the same way. The juice is milky at first, but it soon grows darker when exposed to the air and light, and then a thin film forms on the exposed surface. As sold it is a light or dark grey liquid holding minute particles of solid matter in suspension. It is strongly acid towards litmus-paper, gives a copious white precipitate with gelatin, and has a very unpleasant smell. When allowed to stand it deposits gummy particles.

The applications of this liquid are numerous. Whenever a substance is to be rendered unchangeable in the air it is washed over with the juice, and then becomes permanent and much stronger. It is especially used for giving strength and durability to paper, which is applied in Japan to a great many more purpose than in Europe.

The first question which may be asked is, how the juice causes the material covered with it to become stronger and more durable? In one specimen of the juice which I examined the total solid matter amounted to 64·6 grms. in 1 litre, of which more than one-half was tannin. The antiseptic properties of tannin are well known, but it does not seem that the presence of the tannin is sufficient to account for the great change in the character of the paper which actually takes place. Besides, paper painted with other tannin solutions does not present the same properties. Nor does the effect appear to be due to the presence of albuminoids, which, slowly combining with the tannin in the pores of the paper, might form a kind of leather. The juice contains nitrogen equal to 0·18 per cent of the total residue, which would correspond to 1·1 per cent of albuminoids; but this supposition seems improbable.

During these experiments the following facts were observed, and they suggest an explanation, which I give later:—

(1.) The blackening of the milky juice is only effected in an open vessel, its colour remaining unchanged when left in closed vessels. In this respect it resembles lacquer, as observed by Mr. Ishimatsu (Chemical News, vol. xxxix., p. 113).

(2.) By exposure to the air in a flat vessel the colour becomes darker, and a membraneous film forms over its surface. The film is almost insoluble in water and alcohol, and it is not perceptibly attacked by boiling with dilute sulphuric acid.

3. When left in closed bottles with only a small space above it the thin film is formed, but when this has been made to sink by shaking no further formation of film takes place.

4. When the film formed in an open vessel has been removed a fresh one forms, but the production of fresh films becomes slower and slower.

5. Of the suspended matter the coarse particles settle to the bottom, but the finer ones remain suspended in the form of an emulsion. It is not cellulose, for it is insoluble in an ammoniacal solution of cupric oxide. It is but slightly soluble in alcohol, 3·4 per cent of the dried substance being dissolved by 97 per cent alcohol. It has a greyish colour, but when dried becomes black, and forms a hard mass. The properties are those of a gum resin.

6. In addition to the above-mentioned facts, the dealer stated that the juice loses its peculiar property when exposed to the air for a few months.

Taking the above facts into consideration, it is probable that the following is the true explanation of the action of this liquid upon paper and such bodies:—

(*a*.) The strength gained by the paper is due to the formation on its surface of the film above described.

(*b*.) The formation of the film is due to the presence of the small particles of gum-resin held in suspension, which, on oxidation, becomes solid and darker in colour, like lacquer. Hence the dark colour which paper so treated always possesses.

(*c*.) The tannin present acts as an antiseptic.

The peculiar smell of the juice appears to be owing to the presence of butyric acid. About half-a-litre of the liquid was distilled, 200 c.c. being collected. It had a slight acid reaction and a strong odour. Calcic carbonate was added; the liquid was filtered and evaporated to small bulk, then left *in vacuo* over oil of vitriol. A thin film appeared on the surface, and increased till it covered the whole of the liquid. The vessel was then taken out and heated; a fresh amount of solid matter appeared, as would happen if the solution contained calcic butyrate. As time pressed the liquid was merely dried up at 70° C., then heated to 140° C., by which it lost 8 per cent of its weight. 0·5190 grm. of the powder dried at 140° C. was ignited, and

0·1425 grm. of lime was obtained, equal to 19·61 per cent of calcium :—

	Obtained.	Calculated for $(C_4H_7O_2)_2Ca$.	Calculated for $(C_4H_7O_2)_2Ca.H_2O$.
Ca.. ..	19·61	18·69 p.c.	—
H_2O ..	8·00	—	7·76 p.c.

It is probable, therefore, that the volatile acid is principally butyric acid.

1. *Kibushi* (nut-galls)—

	Percentage of Tannin (Gallo-tannic acid).
Chinese	77·38
Awa (1 year old)	64·85
Minabe in Kii (2 years old)	58·82
Kii (1 year old)	60·9
Kii (8 years old)	65·26
Tango	60·44
Suruga	65·3
Musashi..	67·7

2. *Yasha-bushi* (fruit of *Alnus firma*)—

Idzu Yashi	27·53
Totomi	25·32

3. *Shibuki* (bark of *Myrica rubra*)—

Haibara, in Totomi	11·66
Tosa	10·55
Loochoo	14·96

4. *Zakuro* (rind of *Punica granatum*)—

Kii	20·36

5. *Binroji* (betel nut) .. .. 18·03

6. *Kashiwa Kawa* (bark of *Quercus dentata*)—

Inner bark	7·4
Outer bark	2·64

PROCEEDINGS OF SOCIETIES.

PHYSICAL SOCIETY.

Saturday, November 27, 1880.

Prof. W. G. Adams in the Chair.

New member—H. C. Jones, F.C.S.

Prof. **Graham Bell** exhibited his photophone, and explained the apparatus employed by Mr. Summer Tainter and himself for transmitting sounds by a beam of light. The form in use consists of a metal plate or mirror, vibrated by the sound, and reflecting a beam of light to a distance, where it is focussed on a selenium cell in circuit with a telephone and battery. The light undulates in sympathy with the vibrations of sound, and alters the resistance of the selenium in accordance with the vibrations, thereby reproducing the sound in the telephone. The electric light used was too unsteady to give articulate speech; but by means of a rotating disc perforated round its rim with holes the light could be occulted in such a manner as to give an audible note in the telephone. Different varieties of receivers were described, some of which have not yet been tried. One of these consisted in varying the rotation of the plane of polarisation of the polarised beam. A plan for transmitting the beam consists in making the vibrating plate vary the supply of gas to a jet or manometric flame. The farthest distance speech has been heard by photophone is 800 feet, but, theoretically, it should operate better the greater the distance between the mirror and selenium. On interposing a sheet of hard rubber in the ray the invisible rays passing through it conveyed the sounds in a lower degree, and sounds can be heard by replacing the selenium receiver by discs of different materials, such as hard rubber, metal, &c., and simply listening at them. All substances appear to possess the power of becoming sonorous under the influence of varying light. Hard rubber, antimony, zinc, give the best effects; paper, glass, carbon, the worst. Even tobacco smoke in a glass test-tube held in the beam emitted a note, as also did crystals of sulphate of copper. When hard rubber was simply made into the form of an ear-tube and held in the beam the audible effect was also produced; and, in fact, when the beam was focussed in the ear itself, without any other appliance whatever, a distinct sound could be perceived.

Prof. **Adams** thanked Prof. Bell in the name of the Society, and called on

Mr. **Shelford Bidwell**, who exhibited a lecture-photophone, in which the reflector for receiving the light was discarded, and the beam focussed on the selenium by a lens. The two lenses used cost only 25s., and the beam was sent 14 feet. The selenium cell was made by spreading melted selenium over sheets of mica, and then crystallising it by heat. For mica Prof. Bell recommended microscopic glass. The resistance of the cell was 14,000 ohms in the dark, and 6500 in the light. Speech was distinctly transmitted by this apparatus.

Mr. J. **Spiller** thought that since selenium probably alloyed with brass and the baser metals, it would be better to use gold and silver for the cells; but Prof. **Bell** said that he preferred brass, since (perhaps for the reason that Mr. Spiller gave) it yielded the best results.

Dr. J. H. **Gladstone** read a paper "*On the Specific Refraction and Dispersion of Isomeric Bodies*," an extension of his paper of last June. He concluded that the dispersion of a body containing carbon of the higher refraction is very much greater than that of a body containing carbon of the normal refraction, 5, and that isomeric bodies which coincide in specific refraction coincide also in specific dispersion.

NEWCASTLE-NPON-TYNE CHEMICAL SOCIETY.

General Meeting, October 28, 1880.

Mr. R. C. Clapham, the Retiring President, in the Chair.

Mr. **Clapham**—My time of office has come to a close, and it gives me very great pleasure to introduce to you Mr. Swan, who has already made for himself a name in connection with electric lighting. It is an honour to us to have him for our President.

Mr. **Swan** now took the Chair.

Mr. **Swan**—I have to thank you for the honour you have done me in electing me as your President, though I am conscious that you might easily have chosen some one who would have fulfilled the duties of the office much more ably than I shall do. Our first business is, I think, the confirmation of the minutes of the last meeting, which I will now ask the Secretary to read.

The minutes of last meeting were now read and confirmed.

The Statement of the Treasurer (Mr. Pattinson) was read and adopted.

The Committee's report was then read and adopted.

PRESIDENT'S ADDRESS.

Ladies and Gentlemen,—In making me your President you have conferred upon me an honour which I very highly appreciate, but you have at the same time imposed on me a certain measure of duty, which, if not very onerous, is yet of a kind which I feel could have been better discharged by some other member. However, I thank you heartily for the honour you have done me, and so far as I am able I shall, with your kind help, do what I can to serve the interests of the Society.

It has been to a certain extent customary for your Presidents on the occasion of the first meeting of the session to deliver what is termed an inaugural address, and this inaugural address has for the most part taken the form of an abstract or *resumé* of the results of recent chemical research. This has been so agreeable a practice that I feel some large compensation will be demanded of me if I venture to break through the pleasant custom. Under ordinary circumstances nothing, I can assure you, would have been more to my liking than to have been your leader in the customary excursion to those regions where the latest and fairest explorations have been made, and to have shared with you the pleasure of a momentary glimpse into the newly opened-out country; but pleasant as this would have been, I think the present is an occasion when I may usefully deviate from this usage.

You are aware that I have lately been lecturing on Electric Light. For the purpose of illustrating that lecture I had conducting wires carried from the Lecture Room to Mosley Street, where I have a gas engine and dynamo-electric machines. Having those wires so near to this room, and not being certain that I can retain them in position for many days, it seemed to me, that as there are several applications of an electric current (such as I can command through these wires) more or less bearing on chemical processes, that I should probably be anticipating your wishes, and doing that which you would prefer, if, instead of delivering an inaugural address of the usual type, I were to occupy a portion of the evening by making one or two demonstrations in which the current from the dynamo-electric machine could be utilised. I have therefore had the conducting wires brought into this room. I propose to use the current in the exhibition of Jamin's Blowpipe and Siemens's Electric Furnace. Dr. Siemens has kindly lent me the furnace which he himself used—probably the one furnace of the kind in existence.

Before I proceed to these demonstrations allow me to congratulate you on so auspicious an opening of the session as this largely-attended meeting constitutes. The Society is, I think I may venture to say, in a healthier condition at this moment than at any former period of its existence. It has recently been strengthened both in the number and vigour of its members by coalescence with the Tyneside Chemical Society, a union from which I think we may anticipate much advantage in the furtherance of the objects of the Society.

Thanks to the College authorities, and especially thanks to Professors Marreco and Herschel, who have most hospitably allowed us as it were to make their "house our home," we have been relieved of all expense in respect of our place of meeting, and very much in consequence of this most generous treatment we are free from debt, or rather, I should say, our assets exceed our liabilities. I think I may also congratulate you on the prospect there is that during the session our Society will be made the organ of some valuable contributions to chemical technology. I have handed over to our Secretary (Mr. Dunn) a paper on "Bleaching Powder" by our esteemed honorary member, Professor George Lunge. I have, moreover, received through Mr. Pattinson an interesting communication from Mr. Mond with reference to the progress of alkali manufacture by the ammonia process, and I trust that there may also be forthcoming a corresponding paper from Mr. Stead bearing on the progress and development of that other great local chemical industry, the iron manufacture. I will not anticipate these contributions to our proceedings further than to remark that both the ammonia-soda process and the basic iron-smelting process are having a more rapid growth on the Continent than in England. They are, however, also growing in England. Messrs. Brunner and Mond, of Northwich, have during the last two years, by successive improvements of their plant, but without any extensions, raised their produce from 940 tons a month to 1460 tons of 58 per cent alkali. The manufacture of steel from phosphoretted pig-iron has been for some time in abeyance in Cleveland, consequent on Messrs. Bolckow, Vaughan, and Co. constructing an entirely new plant, designed to overcome some of the mechanical difficulties of the process. This new plant has during the past week been put into operation with perfectly successful results so far, and we may hope soon to have the question solved, whether the manufacture of steel from phosphoretted pig-iron by this process can successfully compete commercially with steel made by the ordinary Bessemer process from non-phosphoretted pig-iron.

These are signs of the times, and point to change in these great branches of industry; perhaps not revolutionary change, but they certainly admonish capitalists to keep a good look out, and will, I trust, stimulate the younger members of the Society to extend their knowledge beyond the pale of their routine processes, so as to adapt themselves to possible innovations and to aid in the general progress.

And now, gentlemen, I must transport you thus soon and suddenly from the region of chemistry proper to its borderland of chemical physics—too far removed, I fear some of you may think, from the alkali manufacture of the Tyne—and proceed to the demonstrations of which I spoke.

In the first place I will exhibit to you the Electrical Blowpipe of M. Jamin. Here is the apparatus. It consists, you see, of a pair of carbon pencils—an electric candle, in fat—surrounded by a coil of insulated copper wire wound a few inches distant from the pencils in the plane of their axes. The current is so led that, in circulating round the coil, it will attract the electric arc formed at the lower end of the carbon pencils, and cause it to flash out almost in the form of a fish-tail gas flame. This spreading out of the arc, is the special feature of the action of the apparatus. It facilitates the application of the heat of the electric arc to the fusion of refractory substances, and enables us better to take advantage of this little-used means of producing a very high temperature.

I have here some old scrap platinum. I will place it on the crucible, and direct the arc upon it. This blowpipe is meant to be used with an alternating current, so as to equalise the rate of burning away of the carbons. I have not an alternating current at my command, and therefore use the arrangement of commutators which you see to reverse the current by hand from time to time. You see our platinum by this time is not only melted, but boiling.

While on the subject of alternating currents, I take occasion to remark on a letter of Mr. Preece in the *Times*, referring to the death of two persons, said to have occurred through their taking hold of the wires in connection with an apparatus supplying current to Jablochkoff's candle. One of these cases occurred some time ago; the other was more recent. Now, admitting for the moment that these deaths occurred directly from the shock (which I consider by no means proved), I do not think that the extreme views put forth by Mr. Preece as to the dangers consequent on electric lighting in general can be supported, and for this reason:—The machine which supplies a Jablochkoff's candle gives alternating currents; the machines which supply the ordinary electric arc, which supply my lamps, and which are more *generally* used for lighting, give a current constant in one direction. Now, although the physiological effect of the alternating currents is undoubtedly severe, yet the effect of touching the wires from a direct-current machine is merely that you feel at the moments of making and breaking contact a slight shock, but while you have hold you feel almost nothing. [Mr. Swan afterwards demonstrated practically the harmlessness of the current by taking hold of the wires from the dynamo-electric machine for some minutes.] I think Mr. Preece, knowing how many real difficulties are connected with electric lighting, should hardly have added to these by magnifying to so great an extent the dangers which in some cases *may* accompany it.

I will now proceed to speak of Dr. Siemens's Electrical Furnace :—

I am almost afraid to try to exhibit to you in action this furnace of Dr. Siemens's, for it only reached me this morning. It came broken, and the whole day has been spent in repairing the damage, so that we have had no opportunity of trying it before hand. In connection with the exhibition of this furnace, I cannot do better than read to you his own account of it, given at a meeting of the Society of Telegraph Engineers, and reported in the Society's Journal. Dr. Siemens says :—

"Amongst the means at our disposal for effecting the fusion of highly refractory metals, and other substances, none has been more fully recognised than the oxy-hydrogen blast. The ingenious modification of the same by M. H. Ste.-Claire Deville, known as the Deville furnace, has been developed and applied for the fusion of platinum in considerable quantities by Mr. George Matthey, F.R.S.

"The Regenerative Gas Furnace furnishes, however, another means of attaining extremely high degrees of heat, and this furnace is now largely used in the arts—among other purposes, for the production of mild steel. By the application of the open hearth process, 10 to 15 tons of malleable iron, containing only traces of carbon or other substances alloyed with it, may be seen in a perfectly fluid condition upon the open hearth of the furnace, at a temperature probably not inferior to the melting-point of platinum. It may be here remarked that the only building material capable of resisting such heats is a brick composed of 98·5 per cent of silica, and only 1·5 per cent of alumina, iron, and lime, to bind the silica together.

"In the Deville furnace an extreme degree of heat is attained by the union of pure oxygen with a rich gaseous fuel under the influence of a blast, whereas in the Siemens furnace it is due to slow combustion of a poor gas, potentiated, so to speak, by a process of accumulation through heat stores or regenerators.

"The temperature attainable in both furnaces is limited by the point of complete dissociation of carbonic acid and aqueous vapour, which, according to Ste.-Claire Deville and Bunsen, may be estimated at from 2500° to 2800° C. But long before this extreme point has been reached, combustion becomes so sluggish that the losses of heat by radiation balance the production by combustion, and thus prevent further increase of temperature.

"It is to the electric arc, therefore, that we must look for the attainment of a temperature exceeding the point of dissociation of products of combustion, and indeed evidence is not wanting to prove the early application of the electric arc to produce effects due to extreme elevation of temperature. As early as the year 1807, Sir Humphry Davy succeeded in decomposing potash by means of an electric current from a Wollaston battery of 400 elements; and in 1810 the same philosopher surprised the members of the Royal Institution by the brilliancy of the electric arc produced between carbon points through the same agency.

"Magneto-electric and dynamo-electric currents enable us to produce the electric arc more readily and economically than was the case at the time of Sir Humphry Davy, and this comparatively new method has been taken advantage of by Messrs. Huggins, Lockyer, and other physicists, to advance astronomical and chemical research with the aid of spectrum analysis. Professor Dewar quite recently, in experimenting with the dynamo-electric current, has shown that in his lime tube or crucible several of the metals assume the gaseous condition, as demonstrated by the reversal of the lines in his spectrum, thus proving that the temperature attained was not much inferior to that of the sun.

"My present object is to show that the electric are is not only capable of producing a very high temperature within a focus or extremely contracted space, but also such larger effects, with comparatively moderate expenditure of energy, as will render it useful in the arts for fusing platinum, iridium, steel, or iron, or for effecting such reactions or decompositions as require for their accomplishment an intense degree of heat, coupled with freedom from such disturbing influences as are inseparable from a furnace worked by the combustion of carbonaceous material.

"The apparatus which I employ consists of an ordinary crucible of plumbago or other highly refractory material, placed in a metallic jacket or outer casing, the intervening space being filled up with pounded charcoal or other bad conductor of heat. A hole is pierced through the bottom of the crucible for the admission of a rod of iron, platinum, or dense carbon, such as is used in electric illumination. The cover of the crucible is also pierced for the reception of the negative electrode, by preference a cylinder of compressed carbon of comparatively large dimensions. At one end of a beam supported at its centre is suspended the negative electrode by means of a strip of copper, or other good conductor of electricity, the other end of the beam being attached to a hollow cylinder of soft iron free to move vertically within a solenoid coil of wire, presenting a total resistance of about 50 units or ohms. By means of a sliding weight the preponderance of the beam in the direction of the solenoid can be varied so as to balance the magnectic force with which the hollow iron cylinder is drawn into the coil. One end of the solenoid coil is connected with the positive, and the other with the negative pole of the electric arc, and, being a coil of high resistance, its attractive force on the iron cylinder is proportional to the electromotive force between the two electrodes, or, in other words, to the electrical resistance of the arc itself.

"The resistance of the arc was determined and fixed at will within the limits of the source of power, by sliding the weight upon the beam. If the resistance of the arc should increase from any cause, the current passing through the solenoid would gain in strength, and the magnetic force, overcoming the counteracting weight, would cause the negative electrode to descend deeper into the crucible; whereas, if the resistance of the arc should fall below the desired limit, the weight would drive back the iron cylinder within the coils, and the length of the arc would increase, until the balance between the forces engaged had been re-established.

"The automatic adjustment of the arc is of great importance to the attainment of advantageous results in the process of electric fusion; without it the resistance of the arc would rapidly diminish with increase of temperature of the heated atmosphere within the crucible, and heat would be developed in the dynamo-electric machine to the prejudice of the electric furnace. The sudden sinking or change in electrical resistance of the material undergoing fusion would, on the other hand, cause sudden increase in the resistance of the arc, with a likelihood of its extinction, if such self-adjusting action did not take place.

"Another important element of success in electric fusion consists in constituting the material to be fused the positive pole of the electric arc. It is well known that it is at the positive pole that the heat is principally developed, and fusion of the material constituting the positive pole takes place even before the crucible itself is heated up to the same degree. This principle of action is of course applicable only to the melting of metals and other electrical conductors, such as metallic oxides, which constitute the materials generally operated upon in metallurgical processes. In operating upon non-conductive earth or upon gases, it becomes necessary to provide a non-destructible positive pole, such as platinum or iridium, which may, however, undergo fusion, and form a little pool at the bottom of the crucible.

"In this electrical furnace some time, of course, is occupied to bring the temperature of the crucible itself up to a considerable degree, but it is surprising how rapidly an accumulation of heat takes place. In working with the modified medium-sized dynamo-machine, capable of producing 36 webers of current with an expenditure of 4 horse-power, and which, if used for illuminating purposes, produces a light equal to 6000 candles, I find that a crucible of about 20 centimetres in depth, immersed in a non-conductive material, is raised up to a white heat in less than a quarter of an hour, and the fusion of 1 kilo. of steel is effected within, say, another quarter of an hour, successive fusions being made in somewhat diminishing intervals of time. It is quite feasible to carry on this process upon a still larger scale by increasing the power of the dynamo-electric machine and the size of the crucibles.

"By the use of a pole of dense carbon, the otherwise purely chemical reaction intended to be carried into effect may be interfered with through the detachment of particles of carbon from the same; and although the consumption of the negative pole in a neutral atmosphere is exceedingly slow, it may become necessary to substitute for the same a negative pole so constituted as not to yield any substance to the arc. I have used for this purpose (as also in the construction of electric lamps) a water pole, or tube of copper, through which a cooling current of water is made to circulate. It consists simply of a stout copper cylinder closed at the lower end, having an inner tube penetrating to near the bottom for the passage of a current of water into the cylinder, which water enters and is discharged by means of flexible india-rubber tubing. This tubing being of non-conductive material, and of small sectional area, the escape of current from the pole to the reservoir is so slight that it may be entirely neglected. On the other hand, some loss of heat is incurred through conduction in the use of the water pole, but this loss diminishes with the increasing heat of the furnace, inasmuch as the arc becomes longer, and the pole is retired more and more into the crucible cover.

"To melt a gram of steel in the electric furnace takes, it is calculated, 8100 heat units, which is within a fraction the heat actually contained in a gram of pure carbon. It results from this calculation that, through the use of the dynamo-electric machine, worked by a steam engine, when considered theoretically, 1 lb. of coal is capable of melting nearly 1 lb. of mild steel. To melt a ton of steel in crucibles in the ordinary air furnace used at Sheffield, from 2½ to 3 tons of best Durham coke are consumed; the same effect is produced with 1 ton of coal when the crucibles are heated in the Regenerative Gas Furnace, whilst to produce mild steel in large masses on the open hearth of this furnace, 12 cwts. of coal suffice to produce 1 ton of steel. The electric furnace may be therefore considered as being more economical than the ordinary air furnace, and would, barring some incidental losses not included in the calculation, be as regards economy of fuel nearly equal to the Regenerative Gas Furnace.

"It has, however, the following advantages in its favour:—1st. That the degree of temperature attainable is theoretically unlimited. 2nd. That fusion is effected in a perfectly neutral atmosphere. 3rd. That the operation can be carried on in a laboratory without much preparation, and under the eye of the operator. 4th. That the limit of heat practically attainable with the use of ordinary refractory materials is very high, because in the electric furnace the fusing material is at a higher temperature than the crucible, whereas in ordinary fusion the temperature of the crucible exceeds that of the material fused within it.

"Without wishing to pretend that the electric furnace here represented is in a condition to supersede other furnaces for ordinary purposes, the advantages above indicated will make it a useful agent, I believe, for carrying on chemical reactions of various kinds at temperatures and under conditions which it has hitherto been impossible to secure."

I must explain that I am not able to bring such a current to bear as will show fully the efficiency of the instrument. In a letter received from Dr. Siemens, he says:—

"When I used the furnace at the Society of Telegraph Engineers there were two dynamo-machines giving a current of 70 webers employed, and the fusion took place in thirteen minutes in a cold crucible; the second fusion on that occasion took only eight minutes.

"Yesterday I employed one machine having a current of 35 webers, the machine being wound by the method lately brought by me before the Royal Society, and I obtained complete fusion in twenty minutes, which I considered an excellent result.

"I do not know what current you have available to experiment with, but you will not succeed in obtaining any telling result unless you employ a machine giving at least 50 webers per second."

I have not anything like 50 webers of current per second, and therefore shall not obtain the telling result which Dr. Siemens speaks of. I will turn out the crucible now, and you will see that even with our comparatively small current, and in spite of the very short time during which it has acted, the pieces of hard steel which we put in are semi-fused and agglutinated together.

In connection with electric lighting the question of the storage of electricity is becoming of importance. M. Planté's secondary battery was the first apparatus in which this storage was attempted. It consists of two lead plates placed face to face, rolled up with a small space separating them, and immersed in dilute sulphuric acid On connecting them with the terminals of a galvanic battery or a dynamo-electric machine, the plate in connection with the positive pole becomes superficially peroxidised, while hydrogen is given off from the other plate. After being thus "charged," the cell retains the charge for a long while, and we can at any time obtain a current from it. By increasing the surface of the active lead plate, and substituting zinc for the other one, as has been done in the cells on the table, we obtain a current more intense and longer in duration than in the ordinary Planté cell. As you see, the battery keeps this platinum wire red-hot for a very considerable time. These cells were charged this morning, but not to the full extent of which they are capable, and they have doubtless lost much of their original charge by accidental contacts between the plates during transport; still the effect is very considerable. The secondary battery, however, is yet far from being all that it might be, and I would recommend it to the younger chemists of the Society as a promising subject of study.

I will now turn the current on to eighteen of my lamps which we have suspended overhead, and to this one on the table, and we will turn down the gas. You see what a very much whiter and pleasanter light it is than gas-light. In Mr. Preece's letter in to day's *Times*, before spoken of, he refers to the proposed use of electricity for the illumination of coal mines, and remarks that the popular idea that this would be a mode of illumination absolutely without danger is an error. He points out that there is a serious risk of accident by fire in case of disarrangement of the wires. Of course I need not tell chemists that, in the case either of a lamp breaking or of the wires accidentally coming in contact, there would, if the accident happened in a gas-laden atmosphere, be danger of explosion. But here again I think Mr. Preece rather exaggerates the danger. It is well not to ignore the difficulties which stand in the way of the application of the light to coal-mine ilumination; but, on the other hand, it is well not to be too easily deterred by the aspect of these difficulties from examining them with a view of seeing whether they cannot be overcome.

Before I close I should just like to direct your attention to this little instrument. It is Siemens's Electro-Dynamometer, and is at present measuring the current going through our lamps. It tells me that 19 webers per second are going through it, and, as we have altogether nineteen lamps, you see that it is a weber per second for each lamp.

I have now said everything that I purposed saying to you to-night. I must apologise for the somewhat informal way in which it has been delivered; but the fact is that my time has been so much occupied of late that I really had not leisure to prepare a more elaborate address, and judged it best to give you as good a substitute as I could. I must ask your indulgence if it has been a little incoherent.

Mr. Glover—I rise to propose a vote of thanks to Mr. Swan for his very able exposition of the present condition of electrical science as applied to useful purposes. The other night we heard—most of us—his lecture on electric lighting: to night we have gained an insight into what has been done in other directions with electricity. It has always been the dream of the chemist that a time would come when the so-called imponderable forces would become the active agents in our every-day life; it now seems as though that dream would be realised. It has been reserved for Mr. Swan to bring electric lighting into a position in which it promises to be a complete success. It may be said to be now with electric lighting as it was with gas lighting when Murdoch, in 1798, lit up the workshop of Boulton and Watt, and Winsor, in 1803, illuminated the Lyceum Theatre with gas. Since then we have seen the wonderful development of gas lighting. At that time all kinds of evils were predicted if gas should be brought into our houses. We were to be suffocated, blown up, poisoned: in fact, so dense was the ignorance, as we should now call it, of people in those days with regard to gas, that Acts of Parliament compelled Gas Companies to surround their gasometers with enormously strong walls, as such a mass of explosive material would be extremely dangerous. Even Sir Humphry Davy, when shown a large gasometer in the neighbourhood of London, said it was extremely dangerous, and could hardly be persuaded to stand by while the engineer struck a nail into the gasholder and lighted the issuing gas. I mention these facts to show that the objections to electric lighting stated by Mr. Preece and others may be safely left to be dealt with by scientific men. The electric light must now go on, and that state of things is largely due to Mr. Swan. I should like to see a portion of our town lighted with the beautiful light which is so soft and yet so brilliant, and does not pollute the air with any noxious vapours.

Mr. Clapham—I have much pleasure in seconding the vote of thanks to Mr. Swan. Mr. Glover has spoken of lighting with gas. It may perhaps interest some members to know that my father was the first in this town to light up his premises with gas, made specially by a small apparatus on the premises, and it is a curious circumstance that the very shop in Pilgrim Street which he then occupied is now in the possession of Mr. Swan, who will no doubt soon light it up with electricity.

PHILOSOPHICAL SOCIETY OF GLASGOW.

Chemical Section.

Mr. J. Mactear, F.C.S., F.I.C., President, in the Chair.

The opening meeting of this Section for the session 1880-81 was held in the Society's rooms on Monday, November 8. Fifteen gentlemen were proposed as Associates of the Section.

The President delivered a short opening address. He apologised for being unable, through pressure of business, to read the paper which he had promised on "The History and Technology of Sulphuric Acid," but expressed the hope that he would be able to read it before the end of the session. He congratulated the Society upon having obtained, this session, rooms of their own in which to meet. He pointed out the advantages which the Section offered to young chemists, and urged the Associates to do all in their power to promote the prosperity of the Section by taking an active part in its work, and by endeavouring to induce their friends to join it. He then sketched the past history of the Section. As early as 1840 a Chemical Section of the Philosophical Society existed, whose chief function it was to provide papers on chemical, geological, and mineralogical subjects to be read before the Society. Dr. Stenhouse, Prof. Thomson, Dr. And. Buchanan, Mr. John Tennant, Mr. Walter Crum, and Dr. J. H. Robertson were amongst the gentlemen upon the committee of the Section at that time. Since the constitution of the Section in its present form in 1868 a large number of valuable papers, many of them upon technical subjects connected with the chemical industries of the city, had been read before it. Mr. Mactear concluded by reading a list of the papers, and made remarks upon those of special interest.

Mr. J. E. Stoddart then read a paper on the "*Desilverisation of Lead by the Zinc Process*," as carried on at the Clyde Lead Works, Glasgow. He said:—

The treatment of argentiferous leads with zinc, for the purpose of extracting the silver and refining the lead, is by no means a novel process. About twenty years ago a metallurgist named Parks took out patents for desilverising rich leads by means of zinc, and a manufacturing firm adopted his process. They were, however, subsequently obliged to abandon it, in consequence of the difficulty experienced in the separation of the zinc from the concentrated silver, to admit of the cupellation of the latter metal. A German chemist named Flach afterwards took up the subject, and by running the alloy of zinc, silver, and lead, along with iron slag, through a peculiarly constructed blast-furnace, was enabled to free the concentrated silver-lead from zinc. He also proposed the use of this furnace for removing of traces of zinc from the desilverised lead, but this was abandoned in favour of the ordinary improving or calcining pan. The operation with the blast-furnace was found to be very troublesome, and as the greater portion of the zinc was entirely lost, was by no means economical. M. Manes, of Messrs. Guillem & Co., Marseilles, who were the first to work Flach's process, found out and patented a simple means of treating the alloy, and recovering the zinc by distillation. This is the process now in use and known as the Flach-Guillem process, and which is carried on in the following manner:—About 18 tons of "rich lead," containing generally from 60 to 70 ounces of silver per ton, are melted in a large cast-iron pot, to which 1 per cent by weight of zinc is added, and the whole well stirred for twenty minutes. The fires are drawn, and the contents allowed to settle and cool until the zinc rises to the surface, and forms a solid ring or crust containing the silver and other foreign metals. This alloy is removed to a small pot at hand, where part of the lead is sweated out, and the alloy thoroughly dried. The large pot with the lead now partially desilverised is again heated up, and treated in the same way as before, but with the addition of only a half per cent of zinc, which when it has risen to the top is removed as before, and dried. A third addition of a quarter of per cent of zinc is found necessary to take out the remainder of the silver, care being taken, on the cooling of this zincing, that all the crystals are cleanly skimmed off. The lead in the large pot is assayed, and found almost always to contain less than 5 dwts. of silver to the ton of lead: if it should happen to contain more, it is due to carelessness on the part of the workmen. The pot is now tapped, and the lead run down into an improving pan, where it is kept at a high heat for nearly eight hours, for the purpose of oxidising or burning off the small percentage of zinc which is left in it from the zincing process: after seven or eight hours' firing in this pan it should contain no trace of zinc. It is then tapped and run into moulds for market lead, or for the manufacture of lead products. The old improving pans were made of cast-iron, placed on a bed of sand, with a groove in the upper sides, which groove was filled with bone-ash to prevent the action of oxide of lead on the iron. These pans, from the giving way of the bone-ash, and the great wear and tear on the iron from the high heats necessary, were found to be both troublesome and expensive; they were very often under repair, and seldom lasted more than six or eight months. They have been superseded by an improving pan of cast-iron lined with brick inside. This pan, instead of being placed on a bed of sand, as was the case with the old improving pan, is hung on brick walls, and is quite open both below and round the outside. This new pan has been working in the patentee's works, Marseilles, for some years without any break down. It burns no more coal, and can be as economically worked in every way as the old pans. The zinc and silver alloy, after being dried, is melted in a plumbago crucible, covered on the top, well luted with fire-clay, connected with a small cast-iron receiver by means of a plumbago pipe, and fired up with coke. The zinc distils over, and is condensed in the iron receiver. After all the zinc has been distilled, the pipe is disconnected, the cover removed, and the lead and silver left in the crucible is ladled out into moulds. Thence it is taken to the refinery, where it is cupelled in the usual way. The block of metallic zinc recovered in the condenser is removed, and used over again in the first part of the process. All the oxide of lead and dross formed in the different processes are taken to the reducing-furnace, mixed with coal-dross, and reduced back to the metallic state. The refuse from this furnace still contains some lead, and is put through the slag hearth, a blast-furnace fired with coke, the fumes of lead oxide from which are condensed in what is known as Johnson's patent condenser, and are all recovered. The lead from the slag hearth, containing a number of impurities, as copper, antimony, iron, or sulphur, is taken to the improving furnace—a furnace built in exactly the same way as the dezincifying pan. About 20 tons of this lead are heated for a period generally from four to five days, but the time varies according to the amount of impurities present. The oxidised impurities, as they are formed, float to the surface, and are skimmed off by the workman, who is made to keep the lead perfectly clean, so as to have a fresh surface always exposed to the action of the flame. The dross skimmed off is at first of a black colour, but gradually becomes lighter as the operation goes on, until it shows nothing but yellow oxide of lead. When this appearance is noted the pan is tapped into moulds, or into the desilverising pot, where it is treated with zinc, and the silver extracted as in the manner before described. By this process the lead can be desilverised and turned out in the shape of market lead in thirty hours from the time it is put in process; the loss in working being not more than 1¼ per cent, and the amount of oxide of lead formed is very much less than that formed in any of the other processes, thereby effecting a very considerable saving in the working expenses. It makes an excellent quality of sheets, pipes, red-lead, and litharge, and has even been used for the manufacture of white-lead. There is, however, one product it cannot be used for by itself, and that is the manufacture of chemical lead. Your President gave us a very interesting paper on this subject last session, showing that the reason of its not being suitable for this was on account of its extreme purity. I understand that Mr. James Napier, jun., of this Society, has done a number of experiments in the same direction, and found that by adding to it a small percentage of copper or antimony, or both, a good chemical lead can be obtained. That all the silver is thoroughly taken out may be seen from the fact that there is an excess of silver obtained on the large scale to the extent of nearly 2 per cent over the assays. An analysis of the market lead gave—Antimony, 0·0015, and silver 0·0004 per cent, a trace of copper, but no iron or zinc; from which it will be seen that the lead refined by the zinc process is almost chemically pure, and to this is due the finer quality of the products manufactured from it.

A short discussion, chiefly on the durability of various qualities of lead, followed the reading of Mr. Stoddart's paper, and it was arranged to have a special discussion of the subject at the next meeting of the Section.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Note.—All degrees of temperature are Centigrade, unless otherwise expressed.

Bulletin de la Société Chimique de Paris.
No. 12, 1880.

Chemical Stability of Matter in Sonorous Vibration.—M. Berthelot.—Already noticed.

The Density of the Vapour of Iodine.—MM. J. M. Crafts and F. Meir.—This paper cannot be reproduced without the accompanying woodcut.

Preparation of Ashes Destined for the Extraction of Iodine from Sea-weeds.—Dr. Thiercelin.—The most advantageous weeds for this purpose are the two varieties of *Fucus digitatus*. The author states that he has succeeded in extracting from the plant 3 per cent of iodine.

Manufacture of Phosphoric Acid.—M. A. Colson.—Natural phosphates, unground, are dissolved in dilute hydrochloric acid. When the acid has ceased to act the clear solution is run off from the insoluble matters and mixed with sulphuric acid enough to saturate all the dissolved lime, leaving a mixture of hydrochloric acid, dilute phosphoric acid, and calcium sulphate. This mixture is submitted to pressure to separate the sulphate from the free acids, which are then concentrated, and the hydrochloric acid is condensed and collected for use by means of ordinary columns.

Complex Acids Containing Boric Acid.—Dr. F. Mauri.—The author has formed boro-tungstic acid by dissolving tungstic anhydride in a solution of borax, and continuing to add the former until the liquid is no longer rendered turbid by hydrochloric acid. He is engaged with the formation and the study of the boro-molybdic acid and its salts.

Compound of Titanium Tetra-chloride and of Phosphorus Proto-chloride.—M. Armand Bertrand.—The composition of this compound is represented by the formula $TiCl_4PCl_3$.

Compound of Titanium Tetra-chloride and Ethyl Oxide.—M. Armand Bertrand.—If the vapours of these two bodies are brought in contact, fine crystals of a greenish yellow colour are produced.

Reduction of Ethyl Nitrate by Alcohol.—M. Armand Bertrand.—Nascent ethyl nitrate is reduced in presence of alcohol, yielding ethyl nitrite and aldehyde.

Products Contained in the Coke of Petroleum.—MM. L. Prunier and Eug. Vareune.—In the experiments of the authors, the accumulation of the carbon is effected with an increasing rapidity, and the weight of the molecule rises to a limit still little known, but which can be no other than the formation of insoluble bodies whose richness in carbon is equal or even inferior to that of the bodies which have remained soluble. We reach thus, by a progression easy to conceive, the term of the series which must equally include crystalline bodies such as graphite and diamond. It is known, on the other hand, that the higher polymers, when submitted to very high temperatures, seem to depolymerise themselves (as happens with meta-styrolene), yielding gaseous carbon compounds.

Preparation of Malonic Acid.—M. E. Bourgoin.—Already noticed.

On Tetric and Oxytetric Acids and on their Homologues (Continued).—M. Eug. Demarçay.—Already noticed.

Formation Heat of Gun-Cotton.—MM. Sarrau and Vieille.—The gun-cotton in question was a commercial sample—a mixture of bi- and trinitro-cellulose. The authors give as the formation heat 542 calories per kilo., or 620 per equivalent.

Isomers of Phloro-glucin.—M. Arm. Gautier.—The bodies examined are œno-glucin, querci-glucin, and pheno-glucin.

Action of Mono-bromated Diphenyl-methan upon Ammonia.—MM. C. Friedel and M. Balsohn.—If the ammonia is in alcoholic solution, ammonium hydrobromate is deposited; and the alcoholic liquid, if precipitated with water, yields, as a principal product, a mixed ethyl-benzhydrolic ether. Concentrated aqueous ammonia acts differently; the crystalline bromine is gradually transformed, and in twenty-four hours the mass becomes liquid. In forty-eight hours more it becomes solid, and then it no longer contains bromated diphenyl-methan.

New Synthesis of Diphenyl-Acetic Acid.—MM. C. Friedel and M. Balsohn.—Already noticed.

Correspondenz-Blatt des Vereines Analytischer Chemiker.
No. 15.

Colorimetric Determination of Chlorine in Potassium Bromide.—C. Roth.—One grm. potassium bromide is ground to a powder with an approximately equal quantity of potassium bichromate, placed in a flask holding 100 c.c. and covered with 5 c.c. concentrated sulphuric acid. The flask is then connected air-tight, by means of an adaptor ground to fit its mouth, with a receiver containing 100 c.c. very dilute ammonia (5 or 6 drops of caustic ammonia to 100 c.c. of water). A gentle heat is applied and raised to about 128°. There should be two large bulbs blown on the connection tube to prevent the reflux of the liquid. When all the chlorine has thus been expelled, the distillate is compared with solutions of ammonium chromate of known strength prepared for the purpose.

Les Mondes, Revue Hebdomadaire des Sciences.
No. 8, July 15, 1880.

New Dielectric Machine.—M. Le Dantec.—An illustrated paper, which does not admit of reproduction.

MISCELLANEOUS.

The Royal Society.—The following is the list of Officers and Council for the ensuing year:—

President—William Spottiswoode, M.A., D.C.L., LL.D.

Treasurer—John Evans, D.C.L., LL.D.

Secretaries—Prof. George Gabriel Stokes, M.A., D.C.L., LL.D.; Prof. Thomas Henry Huxley, LL.D.

Foreign Secretary—Prof. Alexander William Williamson, Ph.D.

Other Members of the Council—William Henry Barlow, Pres. Inst. C.E.; Rev. Prof. Thomas George Bonney, M.A., Sec. G.S.; George Busk, F.L.S.; Right Hon. Sir Richard Assheton Cross, G.C.B.; Edwin Dunkin, V.-P. R.A.S.; Alexander John Ellis, B.A.; Thomas Archer Hirst, Ph.D.; William Huggins, D.C.L., LL.D.; Prof. John Marshall, F.R.C.S.; Prof. Daniel Oliver, F.L.S.; Prof. Alfred Newton, M.A., Pres. C.P.S.; Prof. William Odling, M.B., V.-P.C.S.; Henry Tibbats Stainton, F.G.S.; Sir James Paget, Bart., D.C.L.; William Henry Perkin, Sec. C.S.; Lieut.-Gen. Richard Strachey, R.E., C.S.I.

TO CORRESPONDENTS.

A. E. S.—The iridescence on ancient glass is supposed to be due to the removal of alkali from the surface by long-continued weathering. A film of silica is left on the surface, which shows the colours of thin plates.

Errata.—Page 261, col. 1, line 9 from top, *for* 13·2 *read* 13·8. Page 263, col. 1, line 24 from bottom, *for* ekaluminium-gallium *read* ekaluminium or gallium, *and for* equabor-scandium *read* ekabor or scandium.

NOTES AND QUERIES.

"The Nature of Nitre."—I picked up the other day, from a pile of old books, a curious little book, evidently very old, the title-page of which is unfortunately missing, but which is otherwise complete. It treats—"Of the Name and Nature of Nitre," under the following headings:—"The Peripatetical Definition of Nitre," "Of the Generation and Place of Nitre," "That Nitre is a general cause of Meteors," "That Nitre is a material cause of Lightning and Thunder," "That the Fiery Rain of Brimstone and Fire on Sodom and Gomorrah was Lightning, and that Nitre is expressed by the word Fire," "The Use of Nitre in the Great and Noble Art of Artillery," "The Use of Nitre in Chymistry," &c. Could you kindly inform me, in your "Notes and Queries," as to the age and author of this little book?—W. W. S.

MEETINGS FOR THE WEEK.

Monday, 6th.—Medical, 8.30.
—— London Institution, 5.
—— Society of Arts, 8. "Some Points of Contact between the Scientific and Artistic Aspects of Pottery and Porcelain," by A. H. Church, F.C.S.
—— Royal Institution, 5. General Monthly Meeting.
Tuesday, 7th.—Civil Engineers, 8.
Wednesday, 8th.—Society of Arts, 8. "London Fogs," by Dr. Alfred Carpenter.
—— Microscopical, 8.
Thursday, 9th.—Royal, 4.30.
—— Royal Society Club, 6.30.
—— Mathematical, 8.
—— London Institution, 7.
Friday, 10th.—Astronomical, 8.
—— Quekett, 8.
—— Clinical, 8.30.
Saturday, 11th.—Physical, 3. "On the rate of Loss of Light from Phosphorescent Surfaces," by Lieut. L. Darwin, R.E. "On the Determination of Chemical Affinity in Terms of Electromotive Force," by Dr. Alder Wright.

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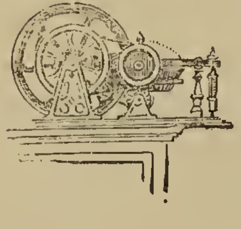

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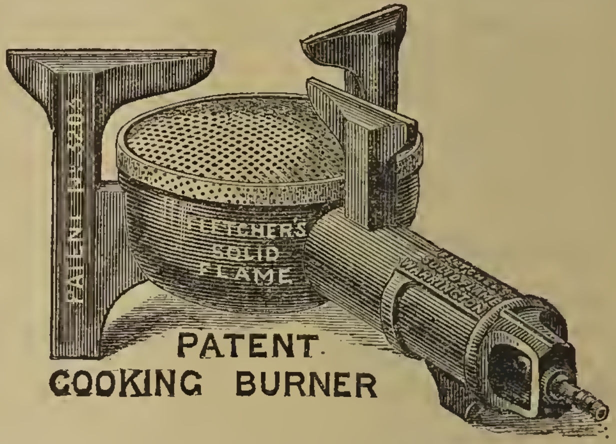

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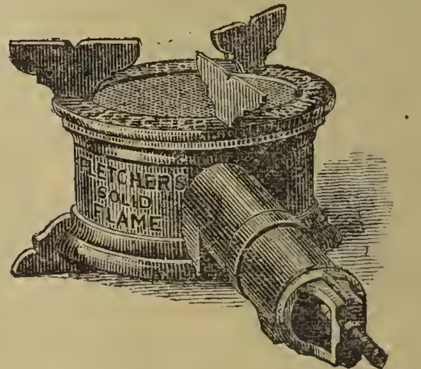

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THE CHEMICAL NEWS.

Vol. XLII. No. 1098.

THE ESTIMATION OF CHROMIUM IN IRON AND STEEL.

By J. OLIVER ARNOLD, F.C.S.

This subject has received comparatively little attention from iron and steel chemists, and, as far as I know, no satisfactory method has yet been published. The method in general use is volumetric and indirect. The steel is dissolved in dilute H_2SO_4, the $Cr_2(SO_4)_3$ thus formed is oxidised to H_2CrO_4 by means of $KMnO_4$. The solution is filtered, acidified, and a known weight (in excess) of $FeSO_4$ is added. The excess is determined by a standard solution of $K_2Cr_2O_7$. The difference between the original amount and the excess is the amount of Fe converted from the dyad to the tetrad condition by the chromic acid. From this the Cr is estimated; every 3 equivs. Fe represent 1 equiv. Cr.

This method I find to be most unsatisfactory. In the first place, the results depend upon a ferrous solution, liable at any moment to accidental oxidation, and from this cause alone, when experimenting with steels into which a known amount of Cr had been introduced, I have observed errors ranging from 0·05 to 0·2 per cent. But in the actual alloy a constant cause of error arises from the fact that chrome-steel is not completely soluble in dilute H_2SO_4, even after long-continued boiling. A black residue remains, consisting of Fe, Mn, and Cr. If this residue is thoroughly washed with hot water, dried, and fused with Na_2CO_3, the mass dissolved out with hot water, a drop of alcohol added, and the solution filtered, the yellow colour of the chromate will be seen; it may be reduced with HCl, ammonia added, and a distinct precipitate of $Cr_2(HO)_6$ obtained. In the case of a soft steel that cannot be crushed to a fine powder, the error becomes most serious. I would therefore propose the following direct and gravimetric method:—

Weigh out from 1 to 5 grms. of the steel (in drillings) according to the amount of Cr present (this may be ascertained by a rough colorimetric test). Place the metal in a wide, covered beaker, and add 20 c.c. of strong HCl; heat till all action is at an end; rinse the cover and sides of the beaker from splashings, and evaporate the solution *gently* to complete dryness. If the evaporation has not been too rapid, the chlorides may be almost entirely detached from the bottom of the beaker in a brittle cake. This is broken up into small pieces by means of a platinum spatula, and as much as possible is brushed out into a clean dry porcelain dish. A small quantity of the chlorides will, however, remain adhering to the beaker; this may be removed with 2 or 3 c.c. of dilute HCl. The solution is poured into a deep platinum crucible, the beaker rinsed with 1 or 2 c.c. of water, the washings added to the crucible, the contents of which are now evaporated to dryness on a sand-bath. When dry the main quantity of the chlorides is carefully brushed out of the porcelain into the platinum dish, and is reduced to a fine powder by means of a little pestle made from glass rod. The spatula and pestle are cleaned into the crucible. The finely-divided chlorides are now thoroughly mixed with an excess of fusion mixture (1 part dry Na_2CO_3 to 1 part powdered KNO_3), a cover is placed over the crucible, and its contents are fused over a gas blowpipe till quite liquid. By this treatment the Fe is converted into insoluble oxide, the Mn, Si, and Cr respectively into alkaline manganate, silicate, and chromate. When cool the crucible is placed in a beaker containing 80 c.c. of boiling water, and is gently boiled till the fused mass is detached and dissolved out. This may be assisted by occasional stirring with a glass rod. When clear from oxide, remove the crucible and cover, washing them well with hot water. Add 3 or 4 drops of alcohol to decompose the manganate, and allow the oxides of Fe and Mn to settle thoroughly. When the supernatant liquid is quite clear it is decanted on a close double filter, the filtrate being received into a clean beaker. The precipitates are disturbed as little as possible. When all the clear liquid has passed through, the filter is well washed with hot water. The precipitates are now washed twice by decantation with 30 c.c. of hot water; at the second washing the contents of the beaker are allowed to drain *gently* into the filter, which is now allowed to drain thoroughly, and is removed without further washing. These precautions in washing must be strictly carried out, as the Fe_2O_3 is in such an exceedingly fine state of division that any attempt to wash or disturb it on the filter will inevitably cause some of it to pass through into the chromium solution. The *clear* yellow filtrate contains the Cr and Si; to it is added 20 c.c. of HCl, the cover being kept on the beaker, to prevent projection of the solution by the evolved CO_2. The solution is now well boiled until all the CO_2 and nitrous fumes are driven off. Its colour will now be green owing to reduction to chloride. Dilute AmHO is added to alkaline reaction and the solution heated nearly to boiling. The resulting precipitate consists of $Cr_2(HO)_6,Si(HO)_4$, mixed with alkaline salts. It is collected on a filter (previously well washed with hot dilute HCl to free it from iron), and is slightly washed. When the washings have drained through, the precipitate is dissolved off the filter with hot dilute HCl, the filtrate being received into the beaker in which the precipitation took place. The solution is now evaporated to dryness to convert the $Si(HO)_4$ into SiO_2. The soluble Cr_2Cl_6 is taken up with 10 c.c. HCl and 90 c.c. of water, and is filtered through a washed filter into a clean beaker, the filter being well washed. The precipitation is now repeated as above, and the $Cr_2(HO)_6$ comes down free from silica and alkaline salts. It is collected, washed, dried, ignited, and weighed as Cr_2O_3.

Precautions.—Only 3 or 4 drops of alcohol should be added, as this quantity is sufficient, not only to precipitate the manganese, but also to effect the reduction to chloride. If too much is added organic compounds are formed, which tend to prevent the complete precipitation of the hydrate. The ammonia in the last precipitation should be added in the least possible excess, and the solution should be heated gently *nearly* to boiling. If any great excess of ammonia be present, and if the solution is boiled, the glass of the beaker is attacked and the result is high. The method if carefully carried out is accurate, as the following analyses will show:—

Three quantities of a steel free from chromium, each weighing 1·98 grms., were taken; to each was added 0·0563 grm. $K_2Cr_2O_7$. This mixture corresponds to a steel containing 1 per cent Cr.

A gave 0·973 per cent chromium.
B ,, 1·022 ,, ,,
C ,, 0·978 ,, ,,
Mean, 0·991 per cent.

Three quantities weighing 1·99 grms. were now taken, and 0·02815 grm. pure $K_2Cr_2O_7$ was added to each. This mixture corresponds to a steel containing 0·5 per cent Cr.

A′ gave 0·511 per cent Cr.
B′ ,, 0·470 ,, ,,
C′ ,, 0·508 ,, ,,
Mean, 0·496 per cent.

The results B, A′, C′, are high, owing to boiling off excess of ammonia. I believe the error to average 0·03 per cent low, as a third series corresponding to 1 per cent Cr gave, when excess of ammonia and boiling were avoided:—

1 gave 0·966 per cent chromium.
2 „ 0·972 „ „
3 „ 0·976 „ „
Mean error, 0·029 low.

With average care the error will never exceed 0·05 per cent. A duplicate estimation can be carried out in a day and a half.

I take this opportunity of thanking Mr. J. W. King for his valuable assistance and suggestions through a long course of experiment.

Sheffield, November 23, 1880.

NOTE ON THE RUSTING OF IRON.

By Dr. T. L. PHIPSON, F.C.S.

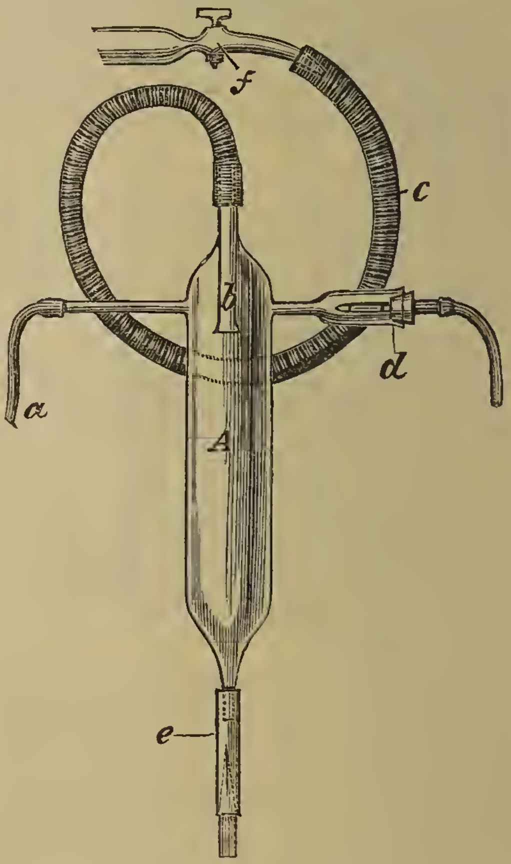
In the Chemical News, vol. xlii., p. 271, a correspondent asserts that in making some photographic experiments, he found that iron and steel do not rust when immersed in solutions of caustic soda and caustic potash, but he appears to have been unable to discover any explanation of the fact in the books and journals to which he has access. "I cannot understand," he says, "why the alkali in the solution prevents the oxygen in the water acting on the iron or steel." The fact has been known for a great many years, and the true explanation was pointed out, I believe, for the first time, by the late much-regretted Dr. Crace-Calvert, to whom modern chemistry is indebted for much useful work. Dr. Calvert's explanation is in strict accordance with the theory propounded some years previously (1858) in my paper on "Catalytic Force," often alluded to in this journal. His experiments showed that the phenomenon of the rusting of iron was due to the presence of carbonic acid, this body forming the *third substance* requisite, according to my theory of catalysis, to complete the galvanic chain. Without the presence of this carbonic acid, or some other *third substance* capable of taking its place, the oxygen cannot combine with the iron at ordinary temperatures. A high temperature acts, as we know, like electricity in promoting the combination. As long as there is free caustic alkali present, of course there can be no carbonic acid, and no rust is formed.

A NEW FORM OF FILTER PUMP.

By THOMAS BAYLEY.

There are two forms of filter pump in use in laboratories at present, the Bunsen pump and the pulse pump. Both are worked by water; the Bunsen pump requires a fall of more than 30 feet to give a fairly good vacuum, and the pulse pump can only be used with water delivered under high pressure. It must very often happen that neither a fall of 30 feet, nor a supply of water at high pressure, can be procured, and yet a filter pump may be desired. Under these circumstances the instrument figured below may be used with good effect. It consists of a glass tube A, with which four other tubes communicate, as shown. The tube *a* is connected to a flask from which steam issues at atmospheric pressure, and the tube *b* is connected to the water supply by means of a piece of caoutchouc tubing *c*, about 2 feet long. At *d* there is another tube, and between this and the central tube A is a Bunsen's valve, opening into A. At *e* there is another Bunsen's valve, opening into the external air. The action of the pump is explained thus:—The tube A being full of air a current of steam enters through *a* and expels the air through the valve *e*. The tap *f* is all the time open to such an extent that a small stream of water would flow through it; a fall in frequent drops of the steam current were absent. The steam current entering A meets the water in *b* and forces it backwards, and at the same time the caoutchouc tubing swells slightly under the influence of the pressure. During about one-third of a second the current of steam continues and the water current is arrested; then suddenly the water, which has been kept back and accumulated, enters into the space containing the steam; the latter is instantly condensed, a sharp convulsion takes place, and air is sucked through the valve D. The cycle of actions then recommences, the steam drives at the air, the inrush of water is repeated, and air again passes into A through the valve *d*.

While a pump of this kind was working satisfactorily, and producing a vacuum of 22 inches of mercury, the following observations were made:—The pulsations took place every second; through the valve *e* steam mixed with air passed for half a second, followed by hot water for the same period: the latter was delivered at a temperature of 55° or 60° C. to the extent of about 150 c.c. per minute. The pump working in this manner cannot of course yield a full vacuum, because the pressure of aqueous vapour at a temperature of 60° C. is about 6 inches of mercury, and it is manifest that the valve *d* will not open unless the pressure in the receiver is greater than this. However, it is quite possible that in the early stage of condensation the temperature in the interior is less than the temperature at which the water is expelled, and of course a better effect may be obtained by using more water. The pump acting as described produced a considerable vacuum in a 40 oz. bottle in a few minutes.



I have used a slight modification of the Bunsen valve for this and other purposes. The arrangement is shown at *e*, the rod which closes the end of the caoutchouc tube forming the valve is drawn out and lies under the slit. This prevents the collapse and failure of the valve.

This pump could no doubt be fitted up for use either as a Bunsen pump or as a steam pump, or as the two combined. By a simple arrangement it can be made to exhaust, to blow air, and to deliver boiling water at the same time. The boiling water obtained is the water flowing from the pump *subsequently* mixed with the excess of steam. The water in the flask boils at a small fraction above the ordinary pressure, the excess being required to open the exit valve.

NOTE ON SIEMENS-MARTIN STEEL.

By SERGIUS KERN, M.E., St. Petersburg.

In most cases this steel is used for shipbuilding and boiler making. In the manufacture of plates the author has noticed that the Siemens-Martin ingots stand a better heat and roll more softly than Bessemer ingots containing the same quantity of carbon. Moreover, Siemens-Martin ingots contain, as a rule, less manganese than Bessemer ingots, and as plates containing much manganese are more liable to oxidise, the Siemens-Martin plates are preferable for the above-mentioned purposes.

The following table shows what beautiful results are obtained in testing Siemens-Martin steel. Giving a high elongation, it shows a good breaking strain, taking into consideration the percentage of carbon (0·45 to 0·60 per cent). Bessemer steel containing the same amount of carbon never gives such results. The specimens tested were intended for other purposes than plate-rolling, but by annealing, most of this steel will be quite good enough to stand Lloyd's tests.

Siemens-Martin Steel.

Length 8″; thickness $\frac{3}{8}''$; percentage of carbon 0·45 to 0·60.

Samples.	Breaking Strain. Tons per Sq. Inch.	Elongation. Per cent.
1.	34·49	18·40
2.	33·15	21·10
3.	35·81	20·00
4.	42·53	20·00
5.	34·49	21·90
6.	38·03	16·60
7.	43·65	19·10
8.	36·93	20·00
9.	44·29	16·10
10.	40·29	17·40
11.	40·50	23·70
12.	31·80	20·30

ANALYSIS OF FORAGE BISCUITS.

By C. J. H. WARDEN, Bengal Medical Staff, Chemical Examiner to Government.

The forage biscuits which form the subject of the following report were forwarded to the Chemical Examiners' Department by the Assistant Commissary General, Calcutta.

The biscuits were circular in shape, with a central hole, and weighed on an average 100 grms. each. A physical examination resulted in the detection of linseed, peas, and husks, not unlike those of barley. The marked indications of phosphoric acid and iron, subsequently found in the ash, also favoured the view of the presence of this cereal.

In order to obtain a sample for analysis portions of several biscuits were pulverised; 100 parts contained:—

	A.	B.	Mean.
Water	13·611	13·721	13·666
Albuminoids.. ..	15·514	15·673	15·594
Carbohydrates ..	57·525	56·380	56·953
Fibre	1·022	1·453	1·237
Fat	7·326	7·920	7·623
Ash	5·002	4·853	4·927
	100·000	100·000	100·000

Albuminoids = N × 6·33. Nitrogen was estimated by Varrentrap and Will's method, a long combustion-tube being employed, and the ammonia absorbed by dilute hydrochloric acid, the chloride being weighed as the ammonic-platinic chloride. Fibre was determined by the acid and alkali process.

The ash was of a reddish colour, and contained iron, phosphoric, sulphuric, and traces of nitric and carbonic acids, together with lime, &c. Chloride of sodium was present to the extent of 0·806 per cent calculated on the biscuits, and was estimated in a portion which had been charred at a temperature below redness. The albuminoids, or nitrogenous constituents, are present to the extent of nearly 16 per cent. Grain *Cicer arietinum*, on which horses are largely fed in India, contains 22·7 per cent, peas 23 to 28 per cent, and barley and oats 12·9 and 14·3 per cent respectively. The nitrogenous principles are, therefore, fairly represented.

Fat is present in rather larger amount than is usual in the diet of horses. Grain, barley, and oats contain 3·7, 2·7, and 5·5 per cent respectively. It is possible that an excess of fat in the food might disagree after a time; but this is a point which can only be decided after actual experiment.

The linseed was probably in the form of meal, after removal of most of the oil, and was most likely added as a material with which to agglutinate the other ingredients, and so admit of a finer biscuit being manufactured. At the same time linseed is nutritious and assists in the formation of fat and muscle.

I have recently, through the courtesy of Messrs. Lord and Co., Calcutta, been furnished with a copy of the extract of the proceedings of a Committee assembled at Meerat in June last, to report on these biscuits. Nine horses were selected for experiment, all country-breds, of nearly similar shape, size, and condition, and all subjected to the same treatment as regards work. They were watered three times a day before each feed; none of them had any bedding.

Lot 1. Consisting of three horses, were fed on 4 seers grain and about 15 seers grass daily.

Lot 2. Three horses were fed on 3 lbs. biscuits and about 15 seers of grass.

Lot 3. Three horses were fed on 6 lbs. of biscuits only, no grass being allowed.

The experiments commenced on the 25th March and terminated on the 18th June. The Committee reported that the nourishing properties of the biscuits were very good, and that with one exception all the horses had kept their condition and carried good coats, so that they differed in no way from the other horses in the regiment.

Calcutta.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, December 3, 1880.

Dr. Gilbert, Vice-President, in the Chair.

The following certificates were read for the first time:— W. E. Benton, A. H. Coffin, M. O. Gossett, A. J. King, E. Luck, W. J. Stavert, R. W. Walker, G. J. Wishart.

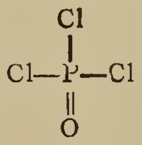
During the evening the following gentlemen were balloted for and declared duly elected Fellows of the Society:—J. O. Arnold, A. E. Barclay, J. K. Crow, A. G. Chamberlain, A. C. Cockburn, P. H. Cathcart, P. F. Frankland, T. Farrington, A. E. Garrod, A. E. Hawkes, R. Holliday, A. Jesseman, W. B. Mason, J. A. Macfarlane, H. A. Mott, R. T. Plimpton, C. H. Sharples, F. M. Sexton, B. H. Thwaite, J. H. Thompson, J. M. Wyborn. Prof. Dewar and Dr. Sprengel were elected Scrutineers of the Ballot.

The Chairman called upon Dr. Ramsay to read a paper "*On the Volumes of Sodium and Bromine at their Boiling-Points*," also a joint paper "*On the Volume of Phosphorus at its Boiling-point*," by D. O. Masson and W. Ramsay. The method already described (*Chem. Soc. Journ.*, 1879,

471) was used in the experiments on sodium with but slight modifications, an iron vessel being substituted for one of glass. The specific gravity of sodium at its boiling-point was found to be 0·7414: its specific volume (the reciprocal of the sp. gr.) = 1·349; the atomic volume (sp. vol. × at. wt.) = 31·0. In a similar way, the mean sp. gr. of bromine at its boiling-point was found to be 2·9483; sp. vol. 0·3392; at. vol. 27·135. With phosphorus the following numbers were obtained:—Sp. gr. 1·485; sp. vol., 0·6734; at. vol., 20·91; with a probable error of ± 0·3987. The authors then compare the atomic volumes of the above elements in the free and combined conditions:—

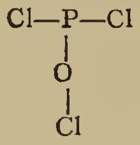
	Free.	Combined.
Br	27·135	28·1
S	21·60	22·6 and 28·6
P	20·91	25·3
Na	31·00	not determined

The atomic volumes of bromine are identical. The at. vol. of sulphur in the free state is nearly identical with its lowest value when combined. As nitrogen is known to have at least three atomic volumes, we should expect phosphorus to have more than one: in the following compounds its atomic volume is doubtless about 25:—PCl_3, PBr_3, $PCl_2(C_2H_5O)$; but in $POCl_3$, $PSCl_3$, and $POBrCl_2$ the authors question the correctness of the calculations. The oxygen and sulphur may have the values of 12·6 and 28·6 instead of 7·8 and 22·6; if so, the atomic volume of phosphorus will become in $POCl_3$ 21·1 (mol. vol. of $POCl_3$ = 101·4 (O 12·2 + Cl_3 68·2); in $PSCl_3$, 19·4; in $POBrCl_2$, 21·7; the mean being 20·7, which fairly coincides with its value in the free state (20·91). Now P in PCl_3 is undoubtedly a triad—



$$\begin{matrix} & Cl & \\ & | & \\ Cl- & P & -Cl \\ & \| & \\ & O & \end{matrix}$$

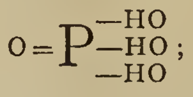
or



$$\begin{matrix} Cl- & P & -Cl \\ & | & \\ & O & \\ & | & \\ & Cl & \end{matrix}$$

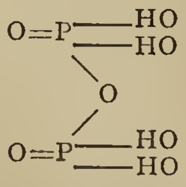
In $POCl_3$ and $PSCl_3$ it may be either a triad or pentad. As the pentachloride of phosphorus furnishes $POCl_3$ by a simple and direct reaction, it is probable that the correct formula is $O{=}PCl_3$, and therefore we might expect the pentad phosphorus to have in this compound a different atomic volume to the triad phosphorus in PCl_3. Moreover, bromine in the free state has the same value as in combination: and sulphur has in the free state the lowest of the two values which it has in the combined state. It is therefore necessary to ascribe two atomic volumes to phosphorus, viz., 20·8 in compounds in which it acts as a pentad and when free, and 25·3 in its triad capacity. As ortho-phosphoric acid is obtainable directly from the oxychloride by the action of water, the formulæ of the phosphoric acids must be—

Ortho,

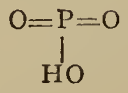


$$O{=}P\begin{matrix}-HO\\-HO\\-HO\end{matrix};$$

Pyro,



and Meta,



$$\begin{matrix} O{=} & P & {=}O \\ & | & \\ & HO & \end{matrix}$$

In conclusion, the author states that from the facts mentioned in the preceding paper the theory of "steres," as applied by Schröder to boiling liquid compounds, is untenable.

The Secretary then read a paper "*On the Specific Volume of Chloral*," by Laura Maude Passavant. Great care was taken to dehydrate the chloral used and to avoid the formation of free hydrochloric acid, which appears to have a marked influence in polymerising the substance. The chloral used in these experiments has maintained its turbidity for a considerable time, and has apparently not formed any meta-chloral. It was obtained by treating commercial re-crystallised chloral with strong oil of vitriol, and afterwards distilling it twice over lime. Its vapour-density was found to be 72·21; its boiling-point 97·73°. Its specific gravity at 0° compared with water at 4° was 1·54179 and 1·54170 in two observations. Its rate of expansion was also determined. The numbers obtained agree well with those of Thorpe, but differ from those of Kopp. As calculated from the observations of the authoress of the present paper, the relative volume of chloral at its boiling-point (97·73°) is 1·12602. Its sp. gr. at that temperature,—

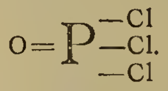
$$\frac{1{\cdot}54179}{1{\cdot}12602} = 1{\cdot}3692,$$

compared with water at 4° and its specific volume is therefore—

$$\frac{147{\cdot}01}{1{\cdot}3692} = 107{\cdot}37.$$

This number agrees well with that calculated by Thorpe (107·8), and shows that the oxygen atom in chloral has precisely the same value as in aldehyd.

Dr. Armstrong said that from the observations of Dr. Ramsay there was no doubt that the formula of oxy-trichloride of phosphorus was—



$$O{=}P\begin{matrix}-Cl\\-Cl.\\-Cl\end{matrix}$$

He should like to raise the question as to whether the atomic volume was a constant. To take Thorpe's paper, the atomic volume of chlorine varied between 21·6 and 23·2; the mean being 22·7. These differences seemed to him very large and outside the limits of experimental error. Although it had been shown that there was undoubtedly a great difference in this respect between an element in the triad and pentad condition, he did not think we were justified in assuming that we had determined the atomic volume of an element.

Prof. Dewar said that from the method of calculating the atomic volumes very small differences in experimental results would lead to large differences in the numbers obtained. He thought that the coincidences already obtained between the atomic volumes of an element in the free and combined state were, at all events, very striking facts.

Mr. Neison pointed out that there might be a systematic as well as an experimental error: the former could be got rid of by increasing the number of experiments.

Dr. Ramsay, in reply, said there were two reasons which might account for the variations in the experimental numbers:—The difficulty of obtaining the various substances in a pure state: and, secondly, that the atomic volumes were all determined at the particular pressure of the atmosphere: perhaps at some other pressure the atomic volumes might agree more closely. He hoped to present a paper which would answer the question whether the atomic volumes do or do not have the same ratios at varying pressures.

"*Note on the Formation of Carbon Tetrabromide in the Manufacture of Bromine*," by J. C. Hamilton. A quantity of a white crystalline substance was obtained as a residue after distilling a quantity of commercial bromine. It melted at 90·1,° and was decomposed by boiling with an alcoholic solution of soda, yielding a white precipitate of NaBr. This, on precipitation with silver nitrate, indicated 97·1 per cent of bromine in the original deposit. Carbon tetrabromide has 96·4 per cent. This substance is probably derived from the action of bromine on the organic matter in the seaweed.

Mr. Howard said that he had seen the same deposit from Scotch and American bromine: German bromine had never yielded a similar substance.

"*Researches on the Relation between the Molecular Structure of Carbon Compounds and their Absorption Spectra*," by W. N. Hartley. A preliminary notice of the work in this paper was read at a meeting of the Society in June last. Part I. contains general conclusions as to the nature of the actinic absorption exerted by various carbon compounds. Part II. contains an account of experiments which prove the generally diactinic character of substances constructed on an open chain of carbon compounds. Part III. discusses the nature of the actinic absorption exerted by various closed chains of carbon atoms. Facts are adduced to show that the formulæ which have been proposed for camphor and camphoric acid, and for the terpenes, are correct as far as the carbon nucleus is concerned only when, in the case of the first two bodies, the nucleus is represented by a closed chain of singly-linked atoms, and, in the case of the terpenes, when the closed chain contains two pairs of doubly-linked carbon atoms. Benzene hexachloride shows no absorption-band. The following conclusion has been established:—No molecular arrangement of carbon atoms causes selective absorption unless each carbon is itself united to other than carbon atoms, as in the case of benzene. Part IV. "On the Absorption-spectra of Condensed Benzene Nuclei." The extraordinary degrees of dilution which substances like naphthalene and anthracene are capable of withstanding before all actinic absorption disappears (which were previously mentioned) have been verified. Attention is drawn to the occurrence of a series of absorption-bands in naphthalene and anthracene spectra, similar to those seen in the spectra transmitted by benzene. V. "On the Cause of Absorption-bands in the Spectra Transmitted by Benzene and its Derivatives." Theoretical considerations lead to the conclusion that general actinic absorption is due to the vibrations of molecules as a whole, but that selective absorption is caused by a series of secondary vibrations.

The Society then adjourned to December 16, when Dr. Carnelly will give a demonstration on his recent researches as to the effect of pressure on the melting-point of ice, and following papers will be read:—"The Estimation of Nitrogen by Combustion, including Nitro-compounds," by J. Ruffle; "On some Naphthalene Derivatives," by Dr. Armstrong and Mr. Graham; "On some Hydrocarbons Present in Resin Spirit," by Dr. Armstrong.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, November 2, 1880.

EDWARD SCHUNCK, Ph.D., F.R.S., V.P., in the Chair.

"*On the Conditions of the Motion of a Portion of Fluid in the Manner of a Rigid Body*," by R. F. GWYTHER, M.A.

"*Did Pascal Invent the Wheelbarrow?*" by WILLIAM E. A. AXON, M.R.S.L.

"*On the History of the Artificial Preparation of Indigo*," by CARL SCHORLEMMER, F.R.S.

One of the most brilliant discoveries which lately has been made is that of the synthesis of indigo, the Indian colour which is mentioned by Dioskorides and Pliny, as well as by the Arabians. It was, however, only after the discovery of the sea passage to India that it became generally known in Europe; but its use as a dye was greatly retarded by the opposition it met with from the large vested interests of the cultivators of woad, *Isatis tinctoria*, the European indigo plant. The English, French, and several German governments were induced by the growers of woad to promulgate severe enactments against it. Thus Henry IV. of France issued an edict condemning to death anyone who used that pernicious drug called "Devil's Food." The employment of woad was, however, gradually superseded by that of indigo; and as soon as organic chemistry had advanced far enough, chemists began to examine this important colouring matter, which was first obtained in the pure state by O'Brien, who states in his treatise "On Calico Printing," 1789, that on heating indigo the colouring matter volatilises, forming a purple vapour, which condenses as a blue powder, whilst the impurities of the commercial product are left behind. Indigo-blue, or indigotin, as the pure compound is called, was afterwards analysed by several chemists, who found that its most simple formula is C_8H_5NO, which was subsequently doubled for several reasons.

The literature of the chemistry of indigo is very large. Of the numerous researches I can here mention only those bearing directly on my subject.

In 1840, Fritzsche found that on distilling indigo with potash a basic oil is produced, which he called *Aniline*, $C_6H_5.NH_2$, from anil, "by which name the Portuguese introduced indigo first into Europe. The word is Arabic, and means simply the blue." In the following year he obtained, by boiling indigo with caustic soda solution and manganese peroxide, a compound which he called anthranilic acid, and which is now known as *orthamidobenzoic acid*, $C_6H_4(NH_2)CO_2H$. He also observed that by heat it is resolved into aniline and carbon dioxide.

At about the same time Erdmann and Laurent independently studied the action of oxidising agents on indigo, and obtained *isatin*, $C_8H_5NO_2$, which is not a colouring-matter. The further examination of this body led to most interesting results, but as those are not directly connected with the subject of this paper I cannot discuss them here. We must therefore proceed at once to 1865, when Baeyer and Knop found that by acting on isatin in an alkaline solution with hydrogen in the nascent state it is converted into a yellow crystalline body, which they called *dioxindol*, $C_8H_7NO_2$. This is easily further reduced in an acid solution to *oxindol*, C_8H_7NO, which forms colourless needles, and on its vapour being passed over red-hot zinc-dust it loses its oxygen, *indol*, C_8H_7N, being formed, which is also a colourless crystalline compound, and a most interesting body, inasmuch as it is also formed, as Nencki and Kühne have shown, in pancreatic digestion, and is contained in the fæces.

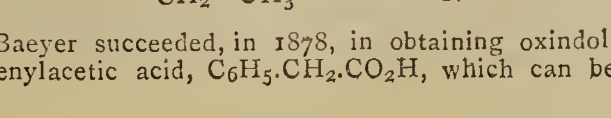
In 1869 Baeyer and Emmerling obtained indol from cinnamic acid, which occurs in several plants, and can be obtained artificially from coal-tar, as I shall show further on. By the action of nitric acid it yields two isomeric nitro-compounds. One of them, called ortho-nitro-cinnamic acid, loses, when heated with caustic potash and iron-filings, carbon dioxide and oxygen, and indol is formed, $C_8H_6(NO_2)CO_2H = C_8H_7N + O_2 + CO_2$.

The same chemists discovered (1870) a method by which isatin can again be reduced to indigo-blue. By heating it with a mixture of phosphorus trichloride, acetyl chloride, and phosphorus to 70° to 80°, they obtained a green liquid which, when poured into water, deposited, on standing exposed to the air, a blue powder containing indigotin. At the same time a purple colouring matter was formed, which they called indigo-purpurin.

It has been known for some time that urine, on standing, sometimes deposits indigo-blue. Jaffé, in 1870, found that he could produce it by the subcutaneous injection of indol; and Nencki, who confirmed this observation in 1875, was able to convert indol into indigotin by the action of ozonised air. But the yield is only very small, as the colouring matter readily undergoes further oxidation.

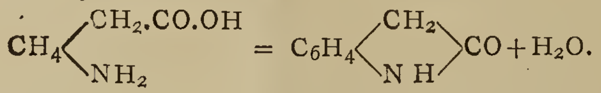
However, the synthesis of indigo was thus completed, because indol can be built up from its elements; but chemists were not satisfied with it, the method being neither a practical one nor giving any clue as to the chemical constitution of indigo.

In the next year Baeyer and Caro found a very simple and elegant method for preparing indol; they obtained it by passing the vapour of ethyl-aniline through a red-hot tube:—



$$C_6H_5-N\Big\langle\begin{matrix}H\\ CH_2-CH_3\end{matrix} = C_6H_4\Big\langle\begin{matrix}CH_2-\\ N=\end{matrix}CH+2H_2.$$

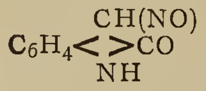
Baeyer succeeded, in 1878, in obtaining oxindol from phenylacetic acid, $C_6H_5.CH_2.CO_2H$, which can be prepared synthetically by different methods, and may be easily obtained from toluol. By treating the acid with nitric acid it is converted into the ortho-nitro compound, which is easily reduced to the corresponding amido-acid. But this, like several other ortho compounds, readily loses water and yields oxindol :—



$$\mathrm{CH_4}\left\langle\begin{matrix}\mathrm{CH_2.CO.OH}\\ \mathrm{NH_2}\end{matrix}\right. = \mathrm{C_6H_4}\left\langle\begin{matrix}\mathrm{CH_2}\\ \mathrm{N\,H}\end{matrix}\right\rangle\mathrm{CO}+\mathrm{H_2O}.$$

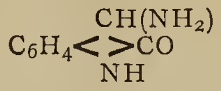
This compound, as Baeyer and Knop had already found, is converted by the action of nitrous acid into nitrosoxindol. On treating this with nascent hydrogen it is transformed into amidoxindol, and this yields on oxidation isatin, the constitution of these bodies being expressed by the following formulæ :—

Nitrosoxindol.



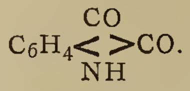
$$\mathrm{C_6H_4}\begin{matrix}\mathrm{CH(NO)}\\ <\;>\\ \mathrm{NH}\end{matrix}\mathrm{CO}$$

Amidoxindol.



$$\mathrm{C_6H_4}\begin{matrix}\mathrm{CH(NH_2)}\\ <\;>\\ \mathrm{NH}\end{matrix}\mathrm{CO}$$

Isatin.



$$\mathrm{C_6H_4}\begin{matrix}\mathrm{CO}\\ <\;>\\ \mathrm{NH}\end{matrix}\mathrm{CO}.$$

I have already stated that isatin can be reduced to indigo-blue : Baeyer endeavoured now to find a more simple method for effecting this. By acting with phosphorus pentachloride on isatin he obtained a compound which he called isatin chloride, which nascent hydrogen converts into indigotin, $2C_8H_4ClNO+2H_2=C_{16}H_{10}N_2O_2+2HCl$.

As far back as 1869 Kekulé predicted isatin to possess the constitution which it has been proved to have by Baeyer's researches, and two of Kekulé's pupils, Claisen and Shadwell, discovered in 1879 a very simple synthetical method for preparing it. By acting with phosphorus chloride on ortho-nitro-benzoic acid, $C_6H_4(NO_2)CO_2H$, the chloride, $C_6H_4(NO_2)COCl$, is formed, which when heated with silver cyanide yields the nitrile, $C_6H_4(NO_2)COCN$. On heating the latter with a solution of caustic potash it is converted into ortho-nitro-phenyl-glyoxylic acid, $C_6H_4(NO_2)CO.CO_2H$, and this is converted by nascent hydrogen into the amido compound, which, like other otho compounds, loses water and yields isatin.

I have now given you a sketch of the history of artificial indigo up to 1879, when I wrote :—" The artificial production of indigo has so far merely a theoretical interest; whether the time will come when simplified methods will admit of its manufacture on a large scale remains to be seen. But even if not, the indigo-purpurin, which is always formed together with the blue, may become of importance as a colouring matter. This body, as Dr. Schunck has shown, is identical with his *indigorubin*, which always occurs, but in small quantity only, in indigo. Dr. Schunck has traced the formation of this beautiful purple colour in *Polygonum tinctorium*, a plant used in China and Japan for the preparation of indigo. He has cultivated it for several years, and found that the young plants do not contain a trace of it. It can be only obtained from plants having attained an advanced stage of development. It dyes under the same conditions as indigotin does; but while the latter dyes a dull dark blue, indigorubin dyes a fine purple shade. Dr. Schunck, who is an authority on these matters, is of opinion that if it could be obtained in quantity it would be a most valuable addition to the colours now in use."*

Since this has been written Baeyer has succeeded in finding a method which to all probability will soon be employed for the manufacture of indigo-blue on a large scale. The starting-point is from cinnamic acid, which occurs in nature, being found in gum-benzoin, styrax, balsam of Peru, and a few other aromatic bodies. These sources would be, however, far too expensive, and the quantity obtained therefrom much too small, to make use of them. Now, Bertagnini found, as early as 1856, that this acid may be obtained artificially by heating benzaldehyd, or oil of bitter almonds, with acetyl chloride,—

$$C_6H_5.CHO+CH_3.COCl=C_6H_5.C_2H_2.CO_2H+HCl.$$

Since that time several processes have been found for obtaining oil of bitter almonds from toluol and from benzoic acid. The first point to be settled was therefore to ascertain which is the cheapest and best method for preparing this compound, as well as acetyl chloride, which is produced by the action of phosphorus chloride on acetic acid.

W. H. Perkin, F.R.S., has discovered another synthesis of cinnamic acid, which probably may also be of practical value. He obtained it by boiling benzaldehyd with acetic anhydride and sodium acetate.

By the action of nitric acid on cinnamic acid we obtain ortho-nitro-cinnamic acid, $C_6H_4(NO_2)C_2H_2CO_2H$, which readily combines with two atoms of bromine to form dibrom-nitro-phenyl-propionic acid. This compound, by the action of alkali, is transformed into ortho-nitro-phenyl-propiolic acid,—

$$C_6H_4(NO_2)C_2H_2Br_2.CO_2H+2NaOH= \\ C_6H_4(NO_2)C_2.CO_2H+2NaBr+2H_2O.$$

The latter acid yields pure indigo when its alkaline solution is heated with a reducing agent, such as grape-sugar, indigotin being deposited in the crystalline state,—

$$2C_9H_5NO_4+2H_2=C_{16}H_{10}N_2O_2+2CO_2+2H_2O.$$

Besides this method Baeyer has patented some others in which also cinnamic acid is used. These processes are now worked out by two of the greatest colour works on the Continent.

How far the artificial production of indigo will be a commercial success remains to be seen. As far as I understand, it is at present only intended to manufacture nitro-phenyl-propiolic acid, which, when mixed with an alkali and grape-sugar is printed on the cloth. By the action of steam a pure indigo-blue is produced, which would form a most valuable addition to the host of steam-colours which are now so largely in use.

In conclusion I must mention another of Baeyer's discoveries which promises to be of practical value. We can easily replace in isatin one atom of hydrogen by bromine, the nitro group, amido group, &c. By subjecting these substituted isatins to the action of phosphorus chloride they are converted into chlorides, and these yield, by treatment with reducing agents, substituted indigos. These bodies are all coloured, and their properties are very similar to those of indigo. It appears not improbable that some of them might find application in dyeing or printing, and be prepared, not from isatin, but from substituted cinnamic acids.

When, twelve years ago, the artificial madder colours were discovered, it was not believed that they could be produced in sufficient quantity nor cheap enough to compete successfully with the natural colours. To-day the cultivation of madder has almost ceased : whether this will happen in the case of indigo is a question which I think will soon be solved.

THE AMERICAN CHEMICAL SOCIETY.*

The October meeting of the Society was held on Thursday, October 7, 1880, at University Building, New York. Dr. E. R. Squibb presided.

The following gentlemen were elected :—*As Members*—C. H. Torry, School of Mines, New York; G. T. Bihn, Philadelphia, Penn.; Dr. Karl Stahl, Chicago, Ill.; Hermann Poole, Buffalo; N.Y. *As Associate Members*—J. R. Savage, Philadelphia, Penn.; Thomas Harrison, Philadelphia, Penn.

The following gentlemen were nominated :—*As Members*—Albert C. Hale, Golden, Colorado; Narcisse Pigeon, New York City; Theodore Lungwitz, Jersey City; Byron N. Wittmann, Reading, Pa.; Henry J. Seeman, Catasauga, Penn.; Chas. W. Schmich, Arbisonia, Penn.; Dr. Henry Friedburg, N.Y. *As Associate Members*—Hermann T.

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* " The Rise and Development of Organic Chemistry." Manchester: Cornish.

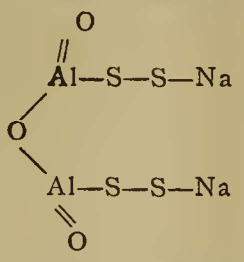
* Communicated by Arthur H. Elliott, F.C.S., Recording Secretary.

Vielté, School of Mines, N.Y.; T. G. Wiechmann, School of Mines, N.Y.; T. D. O'Connor, School of Mines, N.Y.; Charles Kyte, jun., London, England.

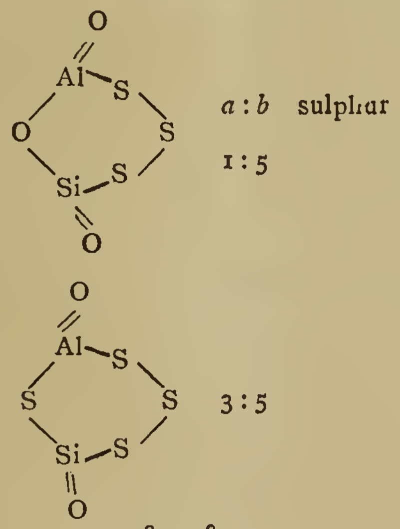
The following paper was read:—"*On the Constitution of Ultramarine*," by H. Endemann, Ph.D. (This paper has appeared in full in the Chemical News.)

In the discussion which followed the reading of this paper, the author made the following statements:—

Most of the ordinary ultramarines of commerce appear to contain a colour nucleus of the following composition:—



Which must be considered as being derived from a white or green ultramarine containing already interpolated sulphur. The action of aqueous acids will produce from such a compound H_2S+3S. The action of chlorine upon green ultramarine, when blues are obtained containing interpolated sulphur while but little chloride of sulphur is formed, is equivalent to the action of chlorine upon sodium sulphide where also polysulphides are obtained. The action of hydrogen chloride upon white and green ultramarines is insufficiently studied. It is stated that no sulphuretted hydrogen is produced; this, however, is true only to a certain extent. The blue obtained by this process contains the same, or nearly the same, quantity of sulphur as the green ultramarine. If we should accept for the blue thus produced the same formula as for the one ordinarily produced in the manufacture an evolution of hydrogen would have to be accepted. No investigations to prove its presence in the products of the reaction have as yet been made. Further research upon this subject is therefore desirable. The silicate rest must not be considered as acting in the manner of a flux. The large excess of silica in it would, if the mass should become melted, reduce the aluminium to its original condition. "Alumina" and the colour would then disappear. If we accept silicon as replacing aluminium in the colour nucleus to the extent of one-half, we would probably have as colour nuclei compounds of the following structural formulæ:—



&c., &c.

That is, we might then consider the colour nucleus to be free from sodium. Whether this is not really the formula can only be determined by experiment. If by the conversion into red and yellow a further loss of sodium can be demonstrated we should adhere to the formula containing aluminium mainly, with perhaps partial substitution by silicon. If no such loss takes place this latter view of the composition of ultramarine becomes more probable. Considering, however, that in silver ultramarine which is obtained from ordinary ultramarine by replacing the sodium by silver, not all the silver can be accepted as being in the same relative position to the other constituents, some of it being easily converted into chloride, while the rest cannot be thus attacked. The author, therefore, is inclined to consider the blue ultramarine as being derived mainly from aluminium. The silicate rest is then only by its properties, and perhaps an analogous constitution, favourable to the existence of the colour.

The following papers by Prof. Wiley, of Lafayette, Indiana, were read by title and referred to the Committee on Papers and Publications:—"The Rotary Power of Commercial Glucose and Grape-Sugar: A Method of Determining the Amount of Reducing Substances Present by the Polariscope." "The Effect of Heating with Dilute Acids and treating with Animal Charcoal on the Rotary Power of Glucose."

NATIONAL ACADEMY OF SCIENCE, NEW YORK.

November 16, 1880.

At the opening of the afternoon session Professor **Gibbs** explained a new general method in analysis discovered by himself and brought to a certain degree of perfection by experiments. The former process of determining metals by electrolysis, he said, had been found operative with only one or two metals, such as cadmium and mercury. The new method was simply to place the metal in solution in a beaker, add pure mercury, and connect the mercury with an electric battery. By the electric action the metal was thrown down upon the mercury. It was necessary only to weigh the mercury and the beaker beforehand, and then after the process to determine the metal by again weighing the vessel and the mercury. This method, he said, was applicable to mercury, tin, cobalt, and other metals. It did not apply in arsenic and antimony. He did not despair of separating potassium and sodium by the process, although his experiments with these metals had not been completely successful.

Professor **Hunt** said this process came with the beauty and force of a revelation; its simplicity recommended it. Every chemist would await further developments with great interest. He also asked what battery power was used.

Professor **Gibbs** said the power of the battery was immaterial except in point of time. The stronger the power the shorter the time required for the process.

Electric Lights.

Professor **Morton**'s first paper treated of some recent experiments made by him in determining the electro-motive force of the Brush dynamo-electric machine. Various instruments for the accurate measurement of electric currents of great strength were first described, and then the measurements of potential in the above-mentioned machine. These showed that each pair of coils on the armature of the machine developed a fluctuating electro-motive force, the projection of which gave a kind of oval curve around the centre of the diagram.

When these curves for each pair of coils are combined, it is found, Professor Morton said, that they show a kind of eight-lobed figure with intersecting lines in certain places. These intersections, if compared with the positions of the commutator, are found to coincide exactly with the points at which rupture of circuit occurs, and thus show that each pair of coils is thrown out, not at the point where its force is least, but at that at which its electro-motive force is equal to that from which it breaks; thus suppressing a spark, but only at a certain sacrifice of theoretical efficiency.

Professor **Morton** also read a paper on the measurement of new forms of electric lamps operating by incandescence. After explaining his method of measurement, he gave the results of a number of experiments which he had recently made with the new Maxim lamp. In this lamp a filament of carbon made from any carbonisable substance, such as paper or card, is placed in a globe. The globe is exhausted of air to a certain point and filled with the vapour of gasoline. The electric current is then passed through the globe. By a singular action the decomposed gasoline deposits carbon upon the filament at the point where it is weakest, and therefore hottest. This portion thus finally becomes of the same conductivity with the rest of the filament, when the action of the intense heat seeks out the next weakest point of the filament, and this process is repeated indefinitely. This property of self-correction, Professor Morton said, was of great importance. The capacity of the lamp for developing light is remarkable. One of these lamps, showing a light of 40 candles, was run to 240 candles per horse-power; another of 52 candles, in reference to power, was run to 336 candles per horse-power; another of 12 candles was run to 136 candles per horse-power. This latter lamp, with the light increased to 49 candles, was run to 426 candles per horse-power, and with the light of 98 candles was run to 607 candles per horse-power. This was far within what the lamp would stand. It could be run to 250 candle-power, and it is said, it has been run to 1500 candles. The law of the increase of efficiency with the increase of the intensity of action was thus verified in this as in other cases. It has also been found that the resistance decreased with the increase of temperature.

Professor **Gibbs** expressed much astonishment at the results obtained by Professor Morton, and said that nobody had dreamed that the light of incandescence could compete with the light of the arc.

Professor **Morton** explained that his references to horse-power had been to the power in the electric current, measured in the usual way, and not to the power in the machine. He added that he would greatly like to compare the Maxim lamp with Edison's latest lamps provided with filaments of bamboo cane. By an accident to his apparatus, Mr. Edison had not been able to redeem a promise to send Professor Morton one of his lamps, but Mr. Edison had written him to the effect that his latest lamps with 16 candle-power, had been run to 55 candles per horse-power. This, Professor Morton remarked, was a decided improvement upon the efficiency of Mr. Edison's horse-shoe lamp. This result compared with the result obtained with the Maxim lamp as 155 to 240.

Professor **Barker** said that he was much interested in the Edison and the incandescent lamps, and that the statements made by Professor Morton certainly marked a year of great progress in electric lighting. One of the greatest difficulties had been in making a fibre that would not disintegrate. A bamboo fibre at fifty candle-power lasted about an hour. The lamp of Mr. Maxim was a great step in advance, it practically caused the fibre to last indefinitely. He understood that Mr. Edison had recently been working in the same direction.

Professor E. D. **Cope** followed Professor Morton with two papers on Nimravidæ and Miocene Canidæ. He described certain types of Felidæ now extinct, and ascribed their destruction to lack of food supply. Their functions were the same as those of the present Felidæ. Like these, only in a greater degree, they destroyed a larger portion of life than they were able to consume.

GERMAN ASSOCIATION OF NATURALISTS,

The Fifty-third Meeting, at Danzig,

September 18 *to* 24, 1880.

On Monday, the 20th September, the scientific proceedings of the only Section opened began by the reading of a paper by Dr. Emmerling, from Kiel, on "*Acetol and Grape Sugar*," after having been put off, owing to the absence of the author.

Some time ago the author, in company with Dr. Richard Wagner (as has been published in *Liebig's Annalen*), was engaged with the recovery of the hydroxyl compounds of the acetons from the so-called acetols, and, indeed, he performed it by the action of argentic oxide and water on mono-brom-aceton, but more completely by treating the latter with potassic carbonate, because by this method a great number of disagreeable by-products are avoided. The acetol itself, as only few bodies do, reduces alkaline oxide of copper in the cold, in fact one part of acetol will reduce two parts of oxide of copper; even a very dilute solution of this body possesses this property. Up to the present this has not been accomplished when free from water. All the methods which have been attempted for this purpose, such as the use of chloride of lime, anhydrous sulphate of copper, or potash, destroy it; fractional distillation succeeds no better than freezing; both methods would from combination at last form a solution, which would contain at the most only 11 per cent of acetol. This solution has a sweetish taste, resembling walnuts; it happens to have the same boiling-point as water, and reduces besides copper also silver and bismuth. By its oxidation with potassic chromate, acetic and carbonic acids are produced, thus—

$$CH_3COCH_2OH+O_3=CH_3COOH+CO_2+H_2O.$$

By the action of potash-lye on grape-sugar, a similar distillate to the above was obtained, which showed the same property of reducing, although in a much less striking manner. From this time the author, in company with Dr. Loges, so changed the experiment that by putting into anhydrous melted grape-sugar small pieces of potash, he obtained as distillate a body which exists probably as a mixture of two different bodies. One with a boiling-point from 90° to 95° has not yet been examined; the other watery portion, however, behaves in a remarkably similar manner to the already-described acetol, both in its power of reduction and in its products of decomposition; only in the latter case the formation has been found to be 2 molecules of acetic acid to 1 molecule of CO_2, from which it is probable that the new compound contains 5 atoms of carbon in a molecule. Hence it follows that we probably have to do with a keton-alcohol in this case, which, like benzoyl-carbinol and acetol, shows the property of great reducing power. At the conclusion several proposals were made for preparing acetols free from water, by means of diffusion, meta-phosphoric acid, &c.

Dr. **Schulz**, of Strasberg, Alsace, then proceeded to read a paper "*On the Constitution of Phenanthrens*," in which he chiefly tried to prove what was earlier supposed by Fittig and Ostermayer in *Liebig's Annalen*, namely, that it is a derivative of diphenyl-carbinol.

To this followed a discourse by Dr. **Scheibler**, from Berlin, "*On Saccharin*." The constituents of sugar divide in such a manner that one part remains unaltered by acids and the other unaltered by alkalies. Cane-sugar under the influence of acid turns to glycose, which again separates into two compounds, distinct one from the other, and which are known as dextrose and lævulose, while this last, by the action of alkalies, separates into the utmost products of decomposition. A mixture of dextrose and lævulose, treated with milk of lime, is coloured a dark brown-red; by separation of the mixture, a chamois yellow lime-salt with an acid, as yet little known, of which one is soluble in ether. After filtration the dissolved lime is precipitated by means of oxalic acid, the filtrate is evaporated until it is of a syrupy consistency, and then left to crystallise. In this manner, bodies in rhombic crystals, having the formula of cane-sugar, are obtained; these have the melting-point of cane-sugar, about 160° to 161°, and are characterised by their property of being entirely volatilised on the application of heat. This is a property which has not yet been noticed to be possessed by saccharine bodies. The rotatory power of saccharine is, although arising from the so-called sugar mixture of dextrose and lævulose, only right-handed, and, indeed, it is as strongly marked as with cane-sugar; the dextro-rotatory power of the latter is $+67{\cdot}2^\circ$, and that of saccharine is $+93{\cdot}5^\circ$. Reduer has also examined the acetyl-compound, and it is found to be a thick cohesive fluid, which is undecomposed on distillation. Saccharine, similar in relation to its crystalline form, is a body discovered previously by Berthelot, which he named trehalose; it has a rotatory power of $+199^\circ$, and its formula is $C_{12}H_{26}O_{13}$; and arabinose, from gum-arabic; or, also, as the preceding shows, what may be represented a beet-sugar, having a rotatory power of $+121^\circ$, and the formula $C_{12}H_{24}O_{12}$. It is worthy of remark that the angle of the prism of saccharine, arabinose, and of trehalose amounted to 111° and some minutes.

Then followed a discourse by G. Westphal, from Celle, Hanover, on a new contrivance patented by him for adjusting the beam of a chemical balance. By means of this arrangement it is possible to entirely adjust the beam in every position. First of all the level of the balance is established, then the parallelism of the beams, and after that the angle of oscillation, and lastly the length of the arms. Each separate adjustment can be done independently of the others, and can be proved by weighing, so that it is by this means easily possible for the chemist or physicist to adjust his balance for most delicate weighing in the shortest time. The details were shown by a model, and the whole was put together; so as to prevent errors in weighing, the whole arrangement, even the stand, was platinised, which offers a surer protection against mercury than gilding does. Further, a revolving weight table with glass surface was shown, which renders the handling of the weights much easier, and also contributes much to the appearance of the whole.

In regard to platinising, Professor Böttger, from Frankfort, gave a short notice on a process, the design of which is to get good results by the wet way. It depends entirely on the possibility of getting a large quantity of ammonio-chloride of platinum in solution, and it appears that a moderately concentrated solution of citrate of soda best fulfils this purpose. Two small Bunsen elements are quite enough to effect the electrolytic decomposition, and to deposit a regular durable coating of platinum.

In another discourse by Dr. Blockmann, from Königsberg, the question was raised—Why does the gas mixture of a Bunsen burner burn with a luminous flame after being heated? If, in the top of a Bunsen burner, a platinum tube, which can be very easily made from thin platinum foil, is placed, and this tube is heated to dull redness by means of another burner, the previously non-luminous flame will become luminous, and will even give the same amount of light as pure coal-gas. This phenomenon has been explained in the following manner:—Luminous flames can by the withdrawal of heat (such as mixing N, CO_2, or H_2O) be made to burn non-luminously, and, on the other hand, non-luminous flames are caused to burn brightly by the addition of heat. In a like manner, it is said, the luminosity of the flame in the above-described experiment depends entirely on the temperature which the gas experiences when passing through the heated platinum tube. This manner of accounting for it did not satisfy the lecturer. First of all he believed that it must be found whether or not the mixture of gas and air undergoes a change in its chemical composition upon contact with the glowing platinum. Analysis alone can clear up this point. Therefore the mixture of gas and air passing through the platinum tube was collected before and during the heating, and analysed. The mixture before heating contained in round numbers 60 per cent air and 40 per cent coal-gas. After passing the heated portion, the greatest part of the hydrogen and all the oxygen had disappeared, the marsh-gas and the heavy hydrocarbons of the coal-gas showed only a slight decrease, and the carbonic oxide a slight increase; the products of combustion were 22·2 volumes of steam, and 1·4 volumes of carbonic acid. The platinum foil (10 c.m. × 8 c.m.), which had served for all the other experiments, was then rolled up into a spiral form, and pushed into a difficultly fusible glass tube of the same width as the Bunsen burner. The gas mixture was then passed through and heated, and even by daylight the platinum spiral was distinctly seen to remain red-hot after the withdrawal of the flame, and water collected in perceptible drops in the cooler part of the tube. The gas mixture which had passed over the heated platinum contained only 20 per cent of inflammable constituents, 55 per cent of nitrogen, and 25 per cent of products of combustion, and was generally on the verge of inflaming. The flame was not luminous, and appeared already partly destroyed, and belongs, as we see, to the categoric non-luminous flames from inert gases. By warming from the outside the flame was not made to be luminous, for the attenuation was too great. Also by making a mixture of gas and air in a gasometer, and then allowing it to pass through the same Bunsen burner and igniting it, it was not possible to make the flame luminous, however much the platinum tube was heated. This experiment proves that the reason of the luminosity of flames under the conditions in question, namely, the raising of temperature which the gas experiences by passing through the glowing platinum tube, is not to be found. The speaker then proceeded to consider the manner in which the air enters the Bunsen burner, and the action which the heating of the tube of the burner has on the motion of the gas inside, and it was found that according to the amount of heating that the air was partly or entirely hindered from entering the air-holes at the bottom of the burner. By this the original conditions of the air and coal-gas were changed, as though the air-openings were entirely shut, and only for this reason was the flame luminous.—*Chemiker Zeitung*.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Note.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 19, November 8, 1880.

Heat of Formation of Dimethyl and its Relations with the Methylic and Ethylic Series.—M. Berthelot.—The author maintains that dimethyl or ethylene hydride constitutes a link, at once theoretic and experimental, between the methylic series—toward which it plays the part of a radicle both by its formula and its origin—and the ethylic series which it develops by its methodical transformations. Between formene and dimethyl pyrogenous reactions even establish the existence of certain mobile equilibria where these two bodies produce each other reciprocally. The transformation of formene into dimethyl takes place by the separation of hydrogen according to the same volume proportions as the decomposition of the hydracids. The thermic relations between dimethyl and the methylic compounds do not differ greatly from those which exist between hydrogen or the easily reducible metals and their binary compounds. But there is this essential difference that hydrogen and the metals play the part of radicles, both analytically and synthetically, whilst dimethyl, formed analytically at the expense of the methylic compounds, does not in general reproduce them by direct synthesis.

Preparation of a New Alimentary Substance, "Nutricine."—E. Moride.—This mixture is made up of muscular flesh, freed from bones, tendons, &c., along with farinaceous substances slightly stove-dried.

Transformation of Oxygen into Ozone by the Electric Effluve in Presence of a Foreign Gas.—P. Hautefeuille and J. Chappuis.—A very small quantity of chlorine serves to prevent the transformation of oxygen into ozone. If a very small volume of chlorine is introduced into ozonised oxygen, the ozone is completely destroyed during the act of electrisation, a very unstable chlorine compound being probably formed. Nitrogen mixed with oxygen enables the latter to be transformed into ozone in a larger proportion than if the latter gas were present alone. Hydrogen and silicon fluoride do not hinder ozonisation.

Action of Chlorine and of Hydrochloric Acid upon Lead Chloride.—A. Ditte.—When at a given temperature chlorine is passed into a liquid containing an excess of lead chloride and increasing quantities of hydrochloric acid, this gas, acting at first upon the chloride dissociated by the water, forms lead peroxide and hydrochloric acid, between which a particular state of equilibrium is established. As the quantity of acid augments there are produced larger and larger quantities of perchlorinised lead hydrochlorate, always dissociated in such a manner that the liquid contains constantly hydrochloric acid and free chlorine.

Combinations of Ammoniacal Gas with Palladium Chloride and Iodide.—M. Isambert.—There is no essential difference between the combinations capable of giving rise to ammonio-metallic chlorides and those merely known as ammoniacal chlorides. The heat of combination is merely greater in the first case than in the second.

The Formation of Chloroform by Alcohol and Chloride of Lime.—A. Béchamp.—Experiment confirms the theory that the alcohol attacks the hypochlorous acid itself in the hypochlorite, the chlorine and hydrogen of which simultaneously remove hydrogen from the alcohol; afterwards the chlorine goes on removing hydrogen, for which the oxygen is substituted, producing chloral, which is split up by the hydrate of lime.

No. 20, November 15, 1880.

Researches on Isomerism: Benzine and Dipropargyl.—MM. Berthelot and Ogier.—The authors have not succeeded in the direct conversion of dipropargyl into benzol, but their experiments have shown the great aptitude of the former body to become polymerised, as theory requires. There is liberation of heat and consequently loss of energy, either when several distinct bodies combine to form a new substance (combination property so-called); or when several identical molecules unite to form a more condensed body (polymerism); or, lastly, when a body endowed with a certain capacity of saturation is transformed into an isomeric body of the same degree of condensation, but whose aptitude to combine with other bodies by addition is smaller (kenomerism). There is then a sort of internal saturation effected in the substance, in accordance with the hypothesis of an interchange of atomicities among the elements. Their studies on terpilene as compared with camphene, and on dipropargyle compared with benzol, give these notions a surer basis and a mechanical significance. Phenomena of this order may be produced in the formation of a great number of other compounds, such as the metallic oxides and the bodies known only in a single state, whose ultimate capacity of saturation is diminished by these phenomena.

On Papaine: A New Contribution to the History of the Soluble Ferments.—A. Wurtz.—Papaine was found to dissolve a thousand times its weight of most fibrin, the greater part of which was transformed into a pepton not precipitable by nitric acid. In consequence of the complete hydration of the fibrin there is even formed a small quantity of a crystalline amidic body.

Enrichment of Plumbiferous Earth by Means of a Stream of Air.—M. Delesse.—A blast apparatus has been employed to remove the lighter particles from certain pure plumbiferous earths, containing originally not more than 7 per cent of metal. The process was not found applicable when the grains of earth were very minute. The proportion of silver was found to decrease as that of lead increased. The process did not work satisfactorily with the galena of Génolhac with a quartzose and dolomitic gangue.

Compressibility of Oxygen, and the Action of this Gas on Mercury in Experiments where these Bodies come in Contact.—E. H. Amagat.—The author affirms that under the conditions of pressure and temperature at which his experiments have been conducted, mercury and oxygen, both perfectly pure and dry, may remain in contact for a time longer than is required for experimental purposes without the least absorption being perceptible.

The Liquefaction of Ozone in the Presence of Carbonic Acid, and on its Colour in the Liquid State.—P. Hautefeuille and J. Chappuis.—We have ascertained that the sudden liberation of ozonised oxygen from pressure occasions the formation of a dense mist, the certain sign of a change of the condition of the ozone; but is it possible to obtain this body in the state of permanent drops, and is the liquid ozone coloured? This is what we have endeavoured to find out by compressing, with the precautions indicated in a former memoir (*Comptes Rendus*, xci., p. 522), ozone prepared at the low temperature produced by passing a current of dry air into methyl chloride. This gas, compressed at 200 atmospheres in the capillary tube of Cailletet's apparatus cooled down to −23°, assumes a blue colour, which becomes deeper and deeper as the pressure increases, but which does not yield a visible liquid distinguishable from the gas by a meniscus. If we place the upper portion of the capillary tube in liquid nitrogen protoxide, the intensity of the colouration augments considerably in this portion, which is cooled to −88°. The lower part being kept at −23° we may judge of the difference of the shade, and estimate that the ozone at −88° is three or four times more deeply coloured than at −23°. The intensity of the colouration increases, therefore, as the temperature falls. After a few minutes the temperatures of the two portions of the tube differ but little, and the gas takes a uniform deep blue colour. The ozone is then imprisoned in a vessel closed by solidified mercury, the meniscus of which remains brilliant and absolutely unattacked by ozone at this low temperature. Under these conditions it may be ascertained that the capillary tube does not contain a drop of liquid. Do these experiments prove that ozone is blue in the liquid state? Such a conclusion would be strained, for if a gas becomes more deeply coloured on cooling it does not follow that it would retain its colour on a change of physical condition, although in hyponitric acid we find that the colour of the liquid acid and that of its vapour differ so much the less as the temperature falls. But we may endeavour to determine the liquefaction of ozone by adding to the mixture of ozone and oxygen a large proportion of carbonic acid; an artifice which has enabled us to recognise new facts. The comparative study of the mixtures of oxygen with ozone and with carbonic acid has shown us that the liquefaction point of ozone differs little from that of carbonic acid. Not being able sufficiently to increase the proportion of ozone in the mixture, so as to diminish the retardation which a large proportion of a permanent gas occasions in the process of liquefaction, we added carbonic acid to the ozonised oxygen. The compression in a capillary tube kept at −23° by means of chloride of methyl, of a mixture of carbonic acid, and of oxygen ozonised at a very low temperature, gives results analogous to those obtained with mixtures of several liquefiable gases, but which are in this case rendered quite distinct. Slow compression enables us to obtain a liquid separating itself from the gas by a mixture; this liquid is not colourless, as is the case with liquid carbonic acid; it is decidedly blue, and its shade does not seem to differ from that of the gas above. This is a stable condition, which is permanent as long as the gases remain under pressure. If we slightly release the gases and immediately compress again, we see above the mercury a sky-blue liquid column, much more deeply coloured than the gas. The cold of the release occasions an abundant vapour formed of carbonic acid and of ozone in the liquid or solid state, for the latter body is then cooled below its critical point, and the plentiful liquefaction of the carbonic acid produced by compression collects a portion of this ozone. What proves that the matter takes place thus is that the colouration of the liquid diminishes, and that in a few minutes the liquid and the gas take the same shade. The ozone collected at first by the liquid carbonic acid becomes diffused, the atmosphere of the tube not containing the vapour of ozone in a state of saturation. In the same manner as the compression of a mixture of oxygen, carbonic acid, and nitrogen monoxide gives a mixed liquid formed of the two liquefied gases, a mixture of oxygen, carbonic acid, and ozone gives a mixed liquid containing liquefied ozone; this ozone gives a blue colour to the liquid obtained in our experiments. These facts enable us to foresee that we should obtain ozone in liquid drops by compressing at a very low temperature the mixture of ozone and oxygen prepared at −88°, the proportion of ozone rising according to our experiments to more than 50 per cent, and that under these conditions we should have a very deep blue liquid. Colourations have already been employed in chemistry to solve controversy questions. It is sufficient to refer to the experiments of M. H. Sainte-Claire Deville on the dissociation of phosphorus perchloride and mercury iodide. The colouration of ozone in the liquid and the gaseous state enables us to show that the products of the decomposition of carbonic acid by the effluve contain a large proportion of ozone. For this purpose it is sufficient to compress them, which is easily effected by transforming the reservoir of Cailletet's tube into an effluve apparatus in which the carbonic acid is submitted to electric discharges for several hours before being compressed. The compression of the gas cooled down to −23° gives a gas as coloured as its proportion of ozone indicated by M. Berthelot admits of, and at a certain pressure the carbonic acid which has not been decomposed liquefies and turns blue. We establish thus, without any reagent, the large proportion of ozone found in the oxygen derived from the decomposition of the carbonic acid. This conclusion is what has been already indicated in a former paper (*Comptes Rendus*, xci., p. 762), and it is conformable to one of the hypotheses laid down by M. Berthelot on the nature of the oxidising product formed from carbonic acid by electric discharges.

On Malleable Cast-iron.—M. L. Forguignon.—Malleable cast-iron appears as an intermediate body between steel and grey pig-iron, from which it differs by the special nature of its amorphous graphite, and by its greater tenacity; it is distinguishable from steel by its slight extensibility and its large proportion of graphite.

Inflammation-Temperatures of Gaseous Mixtures.—MM. Mallard and Le Chatellier.—The detonating mixture of hydrogen and oxygen explodes between 552°—569°, and this temperature is only lowered by 30° if the proportion of oxygen is increased by one half. The addition of nitrogen to the mixture scarcely causes any difference in the temperature of ignition, which, however, is increased a little by the addition of carbonic acid. The detonating mixture of carbonic oxide and oxygen explodes at 647·5°, and large variations in the proportions of the gases scarcely affect the temperature. An addition of nitrogen has little influence, but the presence of carbonic acid raises the temperature notably. Detonating mixtures prepared with hydrogen monocarbide have not, properly speaking, any precise and definite temperature of ignition, but it is not higher than 790°, and ignition may take place much lower.

MISCELLANEOUS.

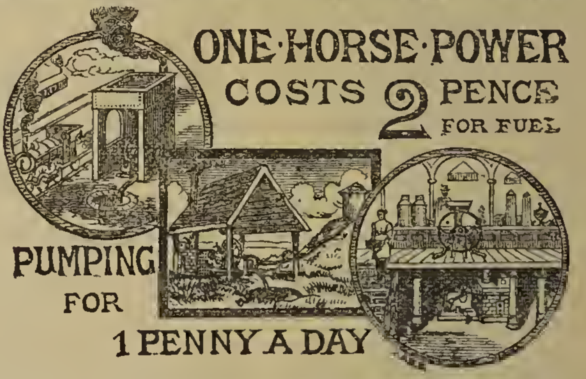
Alkali Waste as a Manure.—At a recent meeting of the Farnworth Agricultural Society, Mr. Gossage called the attention of the farmers present to the value of alkali-waste as a manure. This substance is well known to have accummulated in the Widnes district to a serious extent, and any method by which it might be utilised or even got rid of would prove a great boon to the neighbourhood. Mr. Gossage informed his hearers that alkali-waste consists of calcium sulphide and sulphate, the former ingredient rendering it a valuable application for land foul with weeds, noxious insects, &c., whilst the sulphate, otherwise known as gypsum, fixes ammonia and decomposes the alkaline silicates of clay soils, thus rendering their potash available for the demands of vegetation. He recommends it to be applied to the extent of about 25 to 50 tons per acre, being spread evenly over the fields early in the autumn, and left to lie for a month or six weeks before being ploughed in. He adds the caution that a sufficient time must be allowed to pass over before any seed is sown in land which has been thus treated. As, however, the production of alkali waste at Widnes amounts to 1000 tons daily, it appears doubtful if a sufficient quantity of land requiring such dressing is to be found within prohibitive distances. Nor must it be forgotten that the constant application of a manure which supplies neither nitrogen, phosphoric acid, nor potash, cannot be long continued. From the fact that fresh applications have still to be sought for alkali-waste it may be inferred that the success of the processes for the regeneration of the sulphur present is less decisive than might be wished.

MEETINGS FOR THE WEEK.

Monday, 13th.—Medical, 8.30.
— Royal Geographical, 8.30.
— London Institution, 5.
Tuesday, 14th.—Civil Engineers, 8.
— Medical and Chirurgical, 8.30.
— Photographic, 8.
— Anthropological Institute, 8.
Wednesday, 15th.—Society of Arts, 8.
— Meterological, 7.
— Geological, 8.
Thursday, 16th.—Royal, 4.30.
— Chemical, 8. "The Estimation of Nitrogen by Combustion, including Nitro-compounds," by J. Ruffle. "On some Naphthalene Derivatives," by H. E. Armstrong and N. C. Graham. "On some Hydrocarbons Present in Resin Spirit," by H. E. Armstrong.
— Philosophical Club, 6.30.
— London Institution, 7.

TO CORRESPONDENTS.

Geo. Pennington.—"Destructive Distillation," by Dr. E. J. Mills London: Van Voorst.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THE CHEMICAL NEWS.

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ON THE ESSENTIAL PROPERTIES AND CHEMICAL CHARACTER OF BERYLLIUM (GLUCINUM.)[*]

By L. F. NILSON and OTTO PETTERSSON.

In this paper we wish to call attention to some experimental facts which may give a clue to the real nature of beryllium, an element which since the beginning of this century has been the enigma of inorganic chemistry.

The oxide of beryllium was discovered in 1795 by Vauquelin. It was considered a monoxide, BeO, until 1815, when Berzelius[†] ranged it, principally on account of its basic sulphates, among the sesquioxides. The weighty reasons for this arrangement, never since refuted, which Berzelius added later in the fifth edition of his "Lehrbuch d. Chemie," p. 1225, are too well known to be recorded here.

In 1842 Awdéeff[‡] analysed the double sulphates and fluorides of beryllium, which showed qualities not agreeing entirely with the analogous compounds of aluminium, iron, chromium, &c. As the formula of these compounds could be written

$$KO,SO_3+BeO,SO_3,$$
$$KFl+BeFl,$$

this was considered by the chemists of that time to support strongly the old theory. Still the classification of Berzelius prevailed and was confirmed by H. Rose,[§] who showed the correspondence of the molecular volumes of Be_2O_3 with Al_2O_3, and also by Ebelmen,[‖] who obtained the oxide of beryllium in crystals isomorphous with Al_2O_3. Subsequently an elaborate work, "De Glucium et de ses Composées," 1855, by Debray,[¶] once more caused a change in the prevalent opinion.

Debray regarded beryllia as an isolated member of the series occupying a position intermediate between the monoxides and the sesquioxides, and showing marked analogies with both groups, but not intimately connected with either of them by isomorphism. As the analyses of its compounds in most cases agreed better with the simple formula BeO, this was preferable to Be_2O_3.

Klatzo,[**] in 1868, endeavoured to decide this matter finally by the assumption of a complete isomorphism between the sulphates of Be and Mg, Co, Fe, Ni, and though Marignac,[††] in 1873, proved that this pretended isomorphism did not exit, and was founded on a grave mistake, the opinion that beryllia was a monoxide was at this time universally accepted by chemists. Theoretically this opinion was founded on the "periodic law" of Mendeleeff.[‡‡] The classification of beryllium at the head of the second group among the diatomic elements is a leading point in the theory of Mendeleeff.

If the composition of beryllia was Be_2O_3, and the atomic weight of beryllium = 13·8, instead of 9·2, the place of Be = 9·2 in the system would be vacant, and the order of the series partially reversed, the "atomic analogies" would be overthrown, and, still worse, beryllium = 13·8 would find no place at all in the system, except in the fifth group among the five-atomic elements, to which it certainly does not belong.

The final decisive proof still wanting to confirm the ideas of Mendeleeff's was furnished, 1877, by Reynolds,[*] who found the specific heat of metallic beryllium = 0·642, which (Be = 9·2) makes the atomic heat = 5·9, in accordance with the law of Dulong and Petit.

About a year before the publication of Mr. Reynolds, we had succeeded by means of a new method in isolating metallic beryllium from its chloride. We employed[†] a massive crucible of wrought-iron, hermetically closed by a screw-plug, wherein equivalent quantities of beryllium chloride and metallic sodium were heated to redness. Metallic beryllium was thus obtained, partly fused into globules, partly forming aggregations of little prismatic crystals, which in brightness and colour resembled needles of polished steel. The metal was, as might be expected, not absolutely pure. The analysis was somewhat difficult, the mean result of a number of accordant determinations being :—

Be (metallic) ..	86·94	per cent.
Be_2O_3	9·99	"
Fe	2·08	"
SiO_2	0·99	"
	100·00	

We next determined its specific heat by the method of Bunsen (ice-colorimeter). We here met with quite unexpected difficulties; but, having given up the original arrangement of the experiment described by Bunsen[‡] as impracticable, we, by means of an arrangement similar to to that recommended by Schuller and Wartha,[§] obtained the following results :[‖]—

Specific heat of Be = 0·4084 between 0° and 100°.
Atomic heat of Be = 5·64 [Be = 13·8].

These results, not in accordance either with those recently obtained by Mr. Reynolds or the periodic law of Mendeleeff, were not accepted without hesitation by chemists. Notwithstanding that, the editor of the new edition of Gmelin's *Handbuch d. Chemie*, Professor Kraut, altered the formulæ of beryllia and its compounds in the part of the great encyclopædia of chemistry then passing through the press, several other chemists publicly or privately commented on our work, and urged us to pursue our inquiries further. Among the objections thus made we deem the following most worthy of discussion.

Mr. Lothar Meyer[¶] hints that the equivalent of beryllium may be wrong, and suggests a revision of that number. If it should be found lower than 4·0 instead of higher, beryllium may still be considered a three-atomic element, without upsetting the periodic law. In that case it would only be necessary to interpose a new group of metals between the trivalent and quadrivalent elements. We had no reason to doubt the accuracy of the old number (= 4·6), which we had found and verified by many analyses, but we redeemed our promise to Mr. Lothar Meyer,[**] and undertook the following determinations of the atomic weight of beryllium.

In order to determine this value with the utmost accuracy, we thought it safest to choose the simplest possible method viz., the analysis of its sulphate. Originally we also thought of analysing the chloride by titration with silver nitrate, but having found that sublimed beryllium chloride could not be obtained entirely pure, on account of its corrosive action on glass, we gave up the idea.

Sulphate of beryllium is undoubtedly a neutral salt, and is easily obtained in beautiful crystals, which do not

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[*] A Paper read before the Royal Society, Nov. 18th, 1880.
[†] *Schweigg. Journ. f. Ch. u. Ph.*, xv., p. 296.
[‡] *Pogg. Ann.*, lvi., p. 101.
[§] *Pogg. Ann.*, lxxiv., p. 429.
[‖] *Ann. d. Ch. u. Pharm.*, lxxx., p. 211.
[¶] *Ann. de Chimie et de Phys.*, [3], xliv., p. 5.
[**] "Ueber die Constitution der Beryllerde." Dorpat, 1868. *Journ. f. Prakt. Ch.*, cvi , p. 227.
[††] *Ann. de Chim. et de. Phys.*, [4], xxx., p. 45.
[‡‡] *Ann. d. Ch. u. Pharm.*, *Suppl.*, viii., p. 151 (1871).

[*] *Phil. Mag.*, [5], iii., p. 38.
[†] For the details of the experiment see "Darstellung und Valenz des Berylliums," *Pogg. Ann.*, [2], p. 554 (1878).
[‡] *Pogg. Ann.*, cxli., p. 1.
[§] *Pogg. Ann.*, [2], ii., p. 359.
[‖] *Obs.*—Allowance is made for the impurities of the metal. The specific heat of Be_2O_3 was found = 0·2471 between 0 – 100° C.
[¶] *Ber. d. Deutsch. Chem. Gesellsch.*, xi., p. 576.
[**] *Ber. d. Deutsch. Chem. Gesellsch.*, xi., p. 906.

Experiment.	Weighed Sulphate.	Loss of Water at 100° C.		Loss of Water and SO_3.		Beryllia.		Equivalent of Beryllium.	
	Grs.	Gr.	Per cent.	Grs.	Per cent.	Gr.	Per cent.	O=8.	O=7·98.
I.	3·8014	0·7696	20·245	3·2627	85·829	0·5387	14·171	4·556	4·544
II.	2·6092	0·5282	20·244	2·2395	85·831	0·3697	14·169	4·552	4·542
III.	4·3072	—	—	3·6973	85·840	0·6099	14·160	4·545	4·533
IV.	3·0091	—	—	2·5825	85·824	0·4266	14·176	4·557	4·550
Mean ..	—	—	—	—	85·831	—	14·169	4·552	4·552

change in the air; but at 100—110° C. it loses half of its water, at 250° it becomes anhydrous, and after heating to light redness pure beryllia remains. Still there is a difficulty in the analysis; for anhydrous sulphate and pure beryllia, as obtained by calcination, are both very hygroscopic substances. We therefore chose the hydrated sulphate, which could be weighed with the greatest accuracy, as the most fitting substance to start with; this salt allowing pulverisation and pressure[*] without losing a trace of its constituent water. The sulphate was prepared by heating to dryness an aqueous solution of sublimed chloride with an excess of pure sulphuric acid. By repeated crystallisation the sulphate could easily be purified from a slight trace of calcium sulphate, originating from the action of the gaseous chloride on the glass tubes.

The analyses I., II., refer to beryllium sulphate obtained in this manner; for III., IV., the chloride was precipitated with ammonia and the hydrate treated with sulphuric acid. The sulphate was repeatedly crystallised. The difficulty in the analysis is the weighing of the calcined beryllia. For this purpose the crucible, still red-hot, was placed in an exsiccator filled with anhydrous phosphoric acid, and after cooling placed immediately on a scale pan of a Bunge's balance, the equilibrium being beforehand approximately established. In this manner the whole operation required only a few seconds.

By spectroscopical test Professor Thalén has found the beryllia used for these determinations to be absolutely pure.

The hydrated beryllium sulphate has given in the determinations, thus executed, the values shown in Table above.

The equivalent of beryllium has hitherto been determined by—

Berzelius	=	4·9	(analysis of the sulphate).
Awdéeff	= {	4·71	" " "
		4·93	(analysis of the chloride).
Weeren	=	4·63	(analysis of the sulphate).
Klatzo	=	4·59	" " "
Debray	=	4·61 — 4·71	(analysis of the oxalate).
Nilson and Pettersson..	=	4·552	(analysis of the sulphate).

All these numbers are higher than 4·0, and consequently the atomic weight of beryllium, if trivalent, must be 13·65, consequently higher than that of carbon. The before-mentioned supposition is consequently proved to be unfounded.

Mr. Lothar Meyer further observes that the atomic heat of the oxygen in beryllia, if a sesquioxide, would be less than in any other oxide known. In the next paper we give the whole series of our determinations of the molecular heats of the rare earths and their sulphates. From this survey, which shows that beryllia, with regard to heat and volume, occupies its proper place at the head of the sesquioxides, we here only extract a few determinations.[†]

If c signifies the specific heat,—

		Atom. heat of oxygen.
In Be_2O_3	$c = 75·32 \;.\; 0·2471 = 18·61$	
Be_2	$c = 27·32 \;.\; 0·4246 = 11·60$	
	$7·01 = 3.$	2·34
,, Al_2O_3	$c = 102·8 \;.\; 0·1825 = 18·78$	
Al_2	$c = 54·8 \;.\; 0·2143 = 11·74$	
	$7·04 = 3.$	2·35
,, Sc_2O_3	$c = 136·0 \;.\; 0·1530 = 20·81$	
Sc_2	$c = 88·0 \;.\; 0·1454 = 12·80$[*]	
	$8·01 = 3.$	2·67
,, Ga_2O_3	$c = 184 \;.\; 0·1062 = 19·54$	
Ga_2	$c = 136 \;.\; 0·0802 = 10·91$	
	$8·63 = 3.$	2·88
,, In_2O_3	$c = 274·8 \;.\; 0·0807 = 22·17$	
In_2	$c = 226·8 \;.\; 0·0570 = 12·92$	
	$9·25 = 3.$	3·08

According to the determinations made by us under identical conditions, and therefore strictly comparable, the atomic heat of oxygen in beryllia = Be_2O_3 is the same as that in alumina; this, however, can by no means be considered as exceptional. Alumina and beryllia are the leading members of a group of sesquioxides, wherein the atomic heat and (as will be seen from the following paper) the atomic volume of oxygen increases with increasing values of the atomic weight of metals.

We will now refer to another objection raised to our former researches. Mr. Brauner[†] admits the specific heat of beryllium to be 0·4084 between 0—100°, but supposes that it may rapidly increase with the temperature in the same way as does the specific heat of C and Bo. If this were the case, he thinks beryllia could be BeO, the atomic heat of beryllium = 3·76 between 0—100°, and normal = 6·4 first at a much higher temperature. In our detailed paper we have tried to meet such an objection, by pointing out that no *metallic* element is as yet known, the atomic heat of which does not agree with the law of Dulong and Petit. However, in order to remove any doubt in this respect, we have determined the specific heat of beryllium at different temperatures lying between 0—300° C. We sifted the metal used in our former determinations through a gauze of platinum, the holes of which were 0·25 sq. millim. For the following experiments we used only that part which did not pass through the gauze, on the supposition that this, consisting of globules and larger crystals, was the purer metal. The analysis confirmed this opinion, for the composition was found to be:—

Beryllium	94·41
Beryllia..	4·89
Iron	0·70
	100·00

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[*] It was pressed between sheets of fine, porous, bibulous paper, the surface of which had been previously smoothed by heavy pressure.

[†] Our determinations refer to pure oxides obtained by chemical operations. They are also strictly corresponding and comparable. Other determinations with native alumina (sapphire), made by different methods (Regnault, Neumann), gave a higher number. Determinations by means of the ice-calorimeter always give smaller results, because the standard measure for the heat developed is greater (1 calory = mean of the specific heat of water between 0—100° C.).

[*] The atomic heat of scandium is supposed to be = 6·4, according to the law of Dulong and Petit.

[†] *Ber. d. Deutsch. Chem. Gesellsch.*, xi., 1872.

The following tables gives only the results, not the details of our determinations;[*] the values are referred to pure beryllium, making allowance for the impurities of beryllia and iron:—

Temperature.	Specific heat.	Atomic heat.	
0— 46·30° C.	0·3959	5·46	Heating in the vapour of CS_2.
0— 46·30	0·3950		
0— 46·30	0·3980		
0— 46·50	0·4005		
0—100·18	0·4250	5·79	Heating in the vapour of water.
0— 99·97	0·4242		
0—214·0	0·4749	6·48	Heating in the vapour of nitrobenzol.
0—214·0	0·4751		
0—299·5	0·5054	6·90	Heating in the vapour of diphenylamin.
0—299·5	0·5066		

Thus the specific and atomic heat of beryllium increase with the temperature, but a comparison with the same numbers for iron between 0—300° C. shows that such an increase of these values is not unusual.

	0—100°.		0—300°.		
	Specific heat.	Atomic heat.	Specific heat.	Atomic heat.	Authority.
Iron	0·1124	6·29	0·1266	7·09	Bède.
Beryllium ..	0·4246	5·79	0·5060	6·90	Nilson and Pettersson.

Between 0—100° C., the atomic heat of beryllium is equal to that of aluminium = 5·87, and gallium = 5·59 at 214° C. it is normal = 6·48, and at 300° C. it has attained the same value as iron at the same temperature. Beryllium can thus certainly not be compared with the diamond in this respect, the specific heat of which, according to the researches of Weber, being many times higher at 0—300° C. than at 0—100° C.

Every doubt as to the real atomic weight of beryllium must be removed by the results above mentioned. Fixed at 13·65, according to our determinations, the value of its atomic heat becomes perfectly harmonious with the law of Dulong and Petit.

We will, in conclusion, say a few words upon a paper wherein the results of our former researches have been criticised. In the *Proceedings of the Royal Society*, 1879, Mr. Carnelley applies a new method of calculating the fusion-points of halogen compounds, and applying it to those of beryllium, argues in the following manner:—

Beryllium must be either a dyad or a triad, and must belong either to the second or third group of Mendeleeff's series; if a dyad or Be = 9·2, its chloride, $BeCl_2$, can be calculated to fuse at +547—+600° C., which is confirmed by experiment; if a triad or Be = 13·8, the chloride, $BeCl_3$ or Be_2Cl_6, ought to fuse about 500 degrees lower, *i.e.*, at +50°—+100° C., which it obviously does not; *ergo*, beryllium is a dyad, and Be = 9·2.

We presume that Mr. Carnelley's knowledge of the physical properties of the triads is, like our own, very limited. With the exception of aluminium, we really know little or nothing of the melting- or boiling-points of chlorides, bromides, and iodides belonging to this group, and we think analogies taken only from one member, aluminium, to be too narrow a base for a calculation which is meant to apply to the whole group. There may be chlorides, bromides, and iodides which do not behave like Al_2Cl_6, in regard to boiling- and fusion-points. We will, in the following paper, give reasons for our opinion that beryllium and aluminium are each leading members of two different groups of trivalent metals. The nearest relatives of beryllium among these are neither calcium and magnesium, with which it has, in fact, little or nothing in common, nor aluminium, with which it has very much more in common, but rather the rare elements, scandium, yttrium, erbium, and ytterbium. We believe that Mr. Carnelley ought to try his calculation on the halogen compounds of the rare elements before asserting "that Nilson's and Pettersson's determination of the specific heat of beryllium must be incorrect." If the properties of the halogen compounds of these elements should be found to agree with the calculation, then we confess that the matter becomes somewhat uncertain, for then one will have to choose between the law of Dulong and Petit and that of Carnelley.[*]

Our above-mentioned experimental researches, confirmed still more by the experiments, which will be quoted in a second paper, lead us to the conclusion that the real atomic weight of beryllium is = 13·65. But with this value the periodic law cannot admit this element among the metals nearest related, and this fact obviously militates against its general applicability. Before concluding this memoir, we will just point out that this is not an isolated case of its kind. For the element which should take its place beween Sb = 122 and I = 127, the periodic law requires an atomic weight = 125; with regard to its general properties tellurium is quite admissable in this place, but its atomic weight = 128 is too high. Although this number was the result of the determinations of Berzelius and V. Hauer, this want of accordance with the periodic law induced Willis[†] to make a new determination, but he only confirmed the former results. Thus neither tellurium nor beryllium can be fitted into Mendeleeff's system. And further, after Councler's[‡] discovery of the boroxychloride, $BoOCl_3$, boron may be considered as five-atomic, but it certainly cannot be placed among elements of that valence; and when once the chemistry of the rare earth-metals shall be made clear, where can be placed all these elements, the number of which has already become very great and doubtless will be still augmented? Already erbium and ytterbium, with the now fixed atomic weights of 166[§] and 173,[||] for the pure metals (the earths = Er_2O_3 and Yb_2O_3), can scarcely be ranged in Mendeleeff's system in places indicated by their relation to the other earth-metals or by their "atomic analogies."

In consequence of what has been indicated here, the periodic law in its present condition cannot be said to be quite an adequate expression for our knowledge of the elements; this theory, however, having given the most striking proofs that the truth in many respects has been found (as for example: the new formulæ for the rare earths = R^2O^3 instead of RO, and the discovery of gallium and scandium, the existence of which the law has foreseen in the elements eka-aluminium and eka-boron), we may expect that the periodic law may be so modified and developed that it can embrace and explain every fact stated by experiment.

THE DETERMINATION OF SILICON AND TITANIUM IN PIG-IRON AND STEEL.[¶]

By THOMAS N. DROWN, M.D., and
PORTER W. SHIMER, M.E., Layfayette College, Easton, Pa.

In a communication to this Institute at the Baltimore meeting, February, 1879, on the "Determination of Silicon in Pig-iron and Steel,"[**] the method recommended was the

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[*] We have also been obliged to determine the increase of the specific heat of beryllia and platinum by higher temperatures in the same way as that of beryllium itself, the metal for the experiments being enclosed in little capsules of platinum foil, hermetically soldered with chemically pure gold. Glass tubes cannot be employed at higher temperatures than 100° for two reasons: 1st, the glass would crack when suddenly cooled to 0°; and 2nd, its specific heat increases very rapidly.

[*] *Ber. d. Deutsch. Chem. Gesellsch.*, v. 303.

[†] *Liebig's Ann. d. Ch.*, ccii., p. 242.

[‡] *Ber. d. Deutsch. Chem. Gesellsch.*, xi., p. 1108.

[§] According to Cleve.

[||] According to Nilson.

[¶] *Transactions of the American Institute of Mining Engineers* vol. viii.

[**] *Transactions*, vol. vii., p. 346.

treatment of the metal with nitric acid until action had ceased, and then evaporating with sulphuric acid until the nitric acid was nearly or quite driven off. After filtration of the siliceous and carbonaceous residue, and washing with hot water and hydrochloric acid, a silica was obtained, on ignition, which was quite pure. Since this paper was read before the Institute, we have had large experience with the method, and find it uniformly reliable. Results are obtained in a few hours with the greatest accuracy.

While testing the method and comparing it with others in general use, and also with some new methods, there have been developed some facts which may be of sufficient value to lay before the members of the Institute.

Interesting results were obtained by the treatment of iron borings in a platinum crucible with acid potassium sulphate at a red heat. The operation must be conducted with care, to prevent too violent action; but a little practice will enable one to effect the complete oxidation of 1 grm. of iron (the amount usually taken) in from twenty to thirty minutes. On subsequent solution of the fused mass in water, a little hydrochloric acid is added to dissolve any ferric oxide which may adhere to the crucible. For 1 grm. of iron about 25 grms. of the acid potassium sulphate are used. This amount is ordinarily added at once to the iron in the crucible. The operation must, of course, be carefully watched that the mass does not flow over the top. It should not mount higher than three-fourths of the height of the crucible, which should have a capacity of not less than 70 c.c. If the operation has been successful, a nearly white mass will remain in the crucible, without a particle of graphite. The mass may be poured out while liquid; but a more convenient method is to insert into the fluid mass a piece of heavy platinum wire, bent at the end, and then allow the mass to solidify around it. The crucible is then slightly warmed to loosen the contents, which can be lifted out by the wire. The fused mass, with the crucible and lid, is put at once into boiling water with some hydrochloric acid. When solution is complete, the silica is filtered off, and washed with hot dilute hydrochloric acid and water. After drying, the filter, with its contents, is ignited and weighed. The resulting product should be pure white. While accurate results have been obtained by this method in forty-five minutes, yet a long experience with it shows that it is not to be relied on for all kinds of iron and steel. The following are some of the results obtained:—(Table A.)

In the above table will be noticed many results which vary greatly from the true percentage, and for which variation no sufficient explanation is at hand. In general, it may be said that irons high in silica give better results than those low in silicon. With silicon over 1 per cent the tendency is toward too high results; with silicon under 1 per cent the tendency is toward low results. When the silicon is about one-half of 1 per cent or lower, the results are, moreover. very uncertain, as will be seen from the figures for mottled and white iron, also for spiegeleisen and steel. In one experiment on a sample of Bessemer steel (No. 16) no silicon was found, while, in another experiment with the same steel, made by completely driving off the free sulphuric acid from the acid sulphate, and then adding a fresh portion, the percentage of silicon obtained agreed with that by nitric and sulphuric acids. For iron or steel very low in silicon, this last procedure is necessary to get even approximate results; but for ordinary pig-irons it gave no better results than were obtained by simply heating the borings with acid potassium sulphate until all traces of graphite had disappeared. Silver-grey iron is with difficulty oxidised by this method, although the results obtained from one sample were reasonably good.

For Bessemer works, where a rapid method for the determination of silicon is often desirable, this method will perhaps find a useful application. It should be mentioned that we have found great difficulty in buying acid potassium sulphate free from silica or other insoluble matter. In all cases we found it necessary to purify the sulphate by solution in water, filtration, evaporation, and fusion.

Some variations were tried on the method. The pig-iron was first oxidised in the crucible by nitric acid, and the resulting product treated with the acid sulphate. Again, nitre was used in connection with the acid sulphate. In another series of experiments, the iron was heated to redness for some time with sodium carbonate (which has the effect of oxidising energetically the carbon and silicon[*]), and subsequently treated with sulphuric acid and acid sulphate. These variations were not accompanied with any better results than when the acid sulphate was alone used.

The high results are mostly caused by oxide of iron, which attaches itself in small amount to the upper part of the crucible, and which is somewhat slow of solution in acid. It does not follow that silica which is quite white after ignition is free from iron.

The facility with which pig-iron and steel can be brought into complete solution by fusion with acid potassium sulphate will perhaps recommend this procedure when other ingredients besides silicon are to be determined.

In comparing the silicon results obtained by the nitric and sulphuric acid process with those obtained by the use of hydrochloric acid, we noticed that the results by the latter process were almost invariably higher when the residual silica obtained after burning off the carbon was not re-fused with alkaline carbonates. The same is true when sulphuric acid is used alone without nitric.

In many cases the silica was found to contain iron oxide or other bases, but the higher results were also obtained when the silica was found to be free from metallic oxides. Investigations showed the presence of titanic acid, and an extended series of experiments has shown that titanium is very generally present in pig-iron.

In determining the titanium Riley's method was generally used, which consists in treating the pig-iron with hydrochloric acid and filtering off the siliceous graphitic residue, which is, after ignition, fused with acid potassium sulphate. This method gives fair results; but a more accurate method we found to be the treatment of pig-iron in a porcelain boat in a glass tube with dry chlorine at a red-heat. Pig-iron thus treated is almost completely volatilised, a small carbonaceous residue—5 per cent or less—remaining in the boat. The ferric chloride, with some manganic chloride, condenses in the glass tube (which should be long enough to allow of this), and the non-metals are driven over as gaseous chlorides.

For the absorption of the silicon and titanium a series of three or four tubes or bottles of water is used. No precipitate is noticed in the water, but on boiling titanic acid contaminated with silica is precipitated. To determine the silica and titanic acid, the contents of the bottles are poured into an evaporating-dish and strongly acidified with hydrochloric acid. 15 c.c. of sulphuric acid (sp. gr. 1·23) are added, and the solution evaporated until all the hydrochloric acid is expelled. The silica is thus rendered insoluble, and the titanic acid retained in solution, from which it can be precipitated after dilution by boiling. The results by this method are always a little higher than those obtained by Riley's method. In the treatment of pig-iron by nitric acid and sulphuric acid, the silica obtained is free from titanic acid, which goes entirely into the filtrate. It is not possible, however, to get more than about one-third of the total amount by precipitation by boiling, owing doubtless to the presence of the relatively large amount of iron in solution.

The following table (B) shows the relation between the silicon and titanium in a few pig-irons containing notable quantities of titanium.

Other determinations of titanium in pig-irons by Riley's method are as follows:—

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[*] *Transactions*, vol. vii., p. 146.

TABLE A.

	1.	2.	3.	4.	5.	6.	7.	8.	9.
Silicon by nitric and sulphuric acids	0·737 0·739	0·772 0·777	1·24 1·25	1·214	2·35 2·37	2·40 2·42	2·40 2·40	0·755 0·757	0·622 0·629
Silicon by fusion with acid potassium sulphate ..	0·628 0·677	0·772 0·702 0·677	1·41	1·250	2·37	2·53	2·46 2·47 2·47 2·49 2·50 2·50 2·52 2·52 2·54 2·58 2·69	0·749 0·761	0·535 0·566 0·575 0·591

	10.	11.	12.	13.	14.	15.	16.	17.	18.
Silicon by nitric and sulphuric acids	1·92	0·207 0·208 0·210	4·40 4·39	0·165 0·166	0·929	3·75	0·027 0·027	0·682 0·663	0·205 0·027
Silicon by fusion with acid potassium sulphate ..	1·94 1·94	0·093 0·141 0·231 0·287	4·60	0·116 0·130	0·929	3·94 4·01 4·02 4·19	0·000 0·025	0·529 0·563 0·598 0·628 0·661	0·065 0·115 0·120 0·197 0·209

1. Richmond warm-blast charcoal iron, No. 3. 2. Greenwood cold-blast charcoal, No. 1. 3. Dutchess, anthracite, No. 1. 4. Hecla cold-blast charcoal, No. 2. 5. Bushong, anthracite, No. 1. 6. Leesport, anthracite, No. 1. 7. South Easton, anthracite, No. 1. 8. Glendon, grey forge. 9. Glendon, mottled. 10. Durham, anthracite. 11. White iron. 12. Silver-grey iron. 13. Spiegeleisen. 14. Source unknown. 15. Source unknown. 16. Bessemer steel. 17. Bessemer steel. 18. Sanderson tool-steel.

TABLE B.

	Glendon grey forge.	Silver-grey.	Source unknown.	Source unknown.	Leesport.	Bushong.
Titanium by Riley's method	0·099	0·114	—	0·318	0·115	0·225
Titanium by chlorine method	—	0·278	0·216	0·374	—	—
* Titanium calculated as silicon	0·077	0·217	0·170	0·291	0·081	0·173
Sum of last with true percentage of silicon	0·832	4·607	1·460	1·751	2·481	2·523
Silicon by HCl method, without re-fusing	0·811 0·815 }	4·650	1·640	1·840	{ 2·520 2·550	2·590 2·580
True percentage of silicon	0·755	4·390	1·290	1·460	2·400	2·350

* That is, the amount of silicon which would be calculated from the titanic acid mixed with the silica resulting from the hydrochloric acid treatment.

TABLE C.

Pig-iron treated.	Total percentage of residue in boat.	Siliceous residue after burning off carbon.	Percentage of carbon thus determined.	Silicon determined in this residue.	Silicon from solution in water.	Total silicon by Cl method.	Total silicon by HNO_2 and H_2SO_4 process.
South Easton	3·92	0·292	3·628	0·031	2·335	2·366	2·400
Glendon grey forge..	4·17	0·200	3·970	0·022	—	—	0·756
Glendon mottled ..	4·29	0·302	3·988	0·015	0·444	0·459	0·622
,, ,, ..	4·15	0·266	3·884	—	—	—	0·622
Source unknown (1)..	4·53	0·226	4·304	0·032	0·873	0·905	0·970
,, ,, ..	4·64	0·277	4·363	0·043	0·908	0·951	—
Source unknown (2)..	4·75	0·415	4·335	0·083	1·260	1·343	1·460
,, ,, ..	4·74	—	—	0·080	1·340	1·420	—
,, ,, ..	5·22	—	—	0·080	1·330	1·410	—
,, ,, ..	4·83	0·483	4·497	0·080	1·300	1·380	—
White iron..	4·04	0·224	3·816	0·080	0·152	0·232	0·209
Silver-grey	3·38	0·240	3·140	0·045	4·090	4·135	4·400

	Per cent of Titanium.
Richmond, warm-blast, charcoal, No. 3 ..	0·018
Greenwood, cold-blast, charcoal, No. 1 ..	0·052
Hecla, cold-blast, charcoal, No. 2	0·048
Dutchess, anthracite, No. 1	0·055
Leesport, anthracite, No. 1	0·115

A few more details of the treatment of pig-iron with dry chlorine may be worth giving in the accompanying tabular form:—(See Table C.)

It will be seen from the above that the silicon is fairly accounted for in nearly all instances. A more thorough absorption of the silicon chloride by water—or, perhaps still better, by an alkaline solution—may give the full amount of silicon. As far as experiments go, there is no silicon with the condensed ferric chloride in the tube. Phosphorus is present in the ferric chloride, and sulphur is present as sulphuric acid in the water used for absorption, but we have not yet followed up these elements.

When the carbonaceous and siliceous residue in the boat is treated with water a portion goes into solution, and in this solution may be detected, besides manganese, which we might expect, aluminium, magnesium, and calcium. Whence come these latter metals? Were they present in combination with the iron, or do they simply indicate the presence of cinder in the iron?

In the portion of the residue insoluble in water these elements are likewise found, and it may be that the soluble calcium, magnesium, &c., were present alloyed with the iron, and the insoluble compounds of these metals were in the cinder. More experiments are needed to clear up this doubt.

In an experiment bearing on this point, dry chlorine was passed at a red-heat over cinder which had not been more than twenty-four hours out of the furnace, and it was found to increase in weight about 3 per cent, showing that the absorption of chlorine was not very marked. . In a specimen of old cinder the gain in weight under the same conditions was from 16 to 19 per cent. The action seemed to be the conversion of carbonates of the alkaline earths into chlorides.

When dry chlorine is passed over a mixture of a titaniferous ore and charcoal at a low red-heat titanium chloride is volatilised; but when a mixture of a blast-furnace cinder and charcoal is similarly treated no silicon chloride is formed. It is possible, therefore, that the silicon remaining in the boat after the treatment of pig-iron by chlorine may result from the presence of cinder in the iron. More experiments are needed before any decided assertion can be made on this point.

It was expected that the treatment of pig-iron by chlorine at a low red-heat would give a separation of iron from manganese. We were unsuccessful in effecting this separation. In all cases some manganese was found with the ferric chloride. With spiegeleisen or ferro-manganese the fusion of the manganic chloride in the boat rendered it difficult to volatilise all the iron.

In the paper previously alluded to, on the determination of silicon, it was stated that in the treatment of pig-iron by hydrochloric acid about one-third of the silicon was found in solution and two-thirds in the residue. Further experiments have shown that the relative amounts of silicon in solution and in the residue depend on the strength of the hydrochloric acid. Thus, in an iron containing 0·738 per cent of silicon we found in the insoluble residue, after treating with hydrochloric acid, as follows:—

With acid of sp. gr. 1·20	0·616
,, ,, 1·12	0·440
,, ,, 1·015	0·006

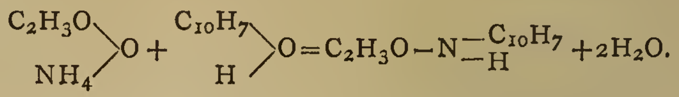
Again, in an iron with 2·36 per cent of silicon we found:—

With acid of sp. gr. 1·20	2·26
,, ,, 1·12	2·05
,, ,, 1·015	0·02

There is no loss of silicon by volatilisation in treating grey or white iron with hydrochloric acid.

MONO-NAPHTHYLAMINES FROM NAPHTHOLS.

Messrs. V. Merz and W. Weith announced some time ago (*vide* Chemical News, vol. xli., p. 294), that on heating β-naphthol with ammoniacal zinc chloride only little primary amine is formed, along with much dinaphthylamine. They now announce at the meeting of the Zürich Chemical Society of November 29th, that next to no dinaphthylamine is formed if α- or β-naphthol is heated to 270° or 280° C. with ammonium acetate and glacial acetic acid, or preferably with a mixture of ammonium chloride and sodium acetate. In this case (as proved by several experiments carried out by their assistant, Mr. Calm) sometimes more than 90 per cent of the theoretically possible quantity of α- or β-naphthyl-acetylamine is formed, thus—



$$\left.\begin{matrix}C_2H_3O\\NH_4\end{matrix}\right\rangle O + \left.\begin{matrix}C_{10}H_7\\H\end{matrix}\right\rangle O = C_2H_3O-N\begin{matrix}-C_{10}H_7\\-H\end{matrix} + 2H_2O.$$

By heating the acetyl compounds with milk of lime preferably under pressure, the primary amines are set free

The two mono-naphthylamines are also obtained in a very simple manner and in large quantities if the naphthols are heated to 260° C., not with ammoniacal zinc chloride, but with ammoniacal calcium chloride, made from porous calcium chloride. The yield amounted to 70 per cent and upwards; the experiments were carried out by Mr. Benz. Similarly to the compound of calcium chloride with ammonia, that of calcium chloride with aniline also excels by its facility of entering into chemical reactions (*reactions vermögen*).

PROCEEDINGS OF SOCIETIES.

PHYSICAL SOCIETY.

Saturday, December 11, 1880.

Prof. W. G. Adams in the Chair.

New members—Mr. W. R. Brown, Mr. T. Wrightson, C.E.

Lieut. L. Darwin, read a paper "*On the Rate of Loss of Light from Phosphorescent Substances.*" His experiments were made at Chatham on Balmain's luminous paint by comparing the intensity of the phosphorescent light with the light of a gas burner; the luminous surface being kept cool by placing ice and water near, as a slight increase of temperature in the surface considerably increases the quantity of light given off in a certain space of time. The supply of light was communicated to the paint from a mirror reflecting sunlight. A table and a curve exhibited to the meeting showed the rate of loss found by Lieut. Darwin. It is independent of the original intensity of the illumination. According to the curve the light diminishes very nearly in proportion to the square of the intensity of the light. In a report on the use of Balmain's paint it had been stated that the phosphorescence became brighter a few minutes after exposure in the dark; but the curve showed this to be an error, due probably to the fact that the eye becomes more sensitive to light after being a few minutes in the dark.

Mr. Pearsall emphasised the advantages of such a light in fiery mines.

Prof. Guthrie enquired if the phosphorescent power grew weaker by time, and Lieut. Darwin instanced a specimen eighty years old to the contrary; but

Mr. W. Crookes stated that in damp air sulphuretted hydrogen was given off, and these luminous substances deteriorated; if sealed in a vacuum they would not. Mr. Crookes remarked that in Balmain's patent it was stated that the phosphorescence died out sooner when exposed to a strong light for a short time than to a weak light for a longer time; but Lieut. Darwin thought this was explained by the slow decrease in the lower part of the curve when the phosphorescence became faint.

Mr. R. J. Lecky mentioned that Evelyn in his diary (1650) describes a phosphorescent powder as "bottling up" sunlight.

Dr. Coffin enquired if short exposure to strong light was equivalent to long exposure to feeble light. Lieut. Darwin thought not.

Dr. C. R. Alder Wright read a full paper on the "*Determination of Chemical Affinity in Terms of Electromotive Force.*" He considered first the value of the B.A. unit of resistance, which from different experimenters might be taken as really 1·005 earth quadrants per second, or not more than ½ per cent out. Clark's element when carefully prepared was practically correct at 1·457 volts, and it kept constant for three or four months after being made, but deteriorated thenceforth some 3 per cent in about two years. The deterioration was assisted by air, which could not be well excluded by the paraffin cork, as it cracked. If sealed in a Sprengel vacuum the element lasted better. Joule's mechanical equivalent of heat (J) he estimated at 42×10^6, or not over 1 per cent greater than Joule's water value. The chief result of Dr. Wright's researches was the conclusion that the action of a current in electrolysis is to decompose the electrolyte into "nascent" products, which evolve heat in changing into ordinary products of electrolysis. These nascent products may be the ultimate atoms composing the molecules of the ordinary products, and the heat is given out in these atoms coming together to produce molecules, say, of oxygen and hydrogen in the case of water. A number of deductions from this theorem are verified by experiment. One of these is that no gas battery can give a higher E.M.F. than 1·5 volts. A result not before published is that the E.M.F. of a Daniell cell is a function of the current, and is a maximum when the current is indefinitely small. The variation may amount to 10°. Therefore all methods of determining resistance by means of two currents of different strength are inaccurate. The experiments also verified Faraday's law that there is no conduction in an electrolyte without electrolysis.

Prof. **Adams** enquired if Dr. Wright had seen the letter of Prof. Rowland's assistant to the effect that Dr. Wright's former estimate of the ohm was on the wrong side of unity. He had been too busy to see it.

Prof. **Foster** thought that the variation of E.M.F. in a cell with the current was to be expected, and was probably due to the slowness of diffusion.

Dr. **Wright** thought diffusion would account for it.

Dr. **Lodge** said that there was no way of measuring the resistance of a cell except by employing two currents of different strength, and therefore it was necessary to know the law of variation of E.M.F. with current strength.

Dr. **Wright** stated that he had found two methods of proceeding with currents of the same strength.

With regard to the deduction of Dr. Wright that no current passes without producing electrolysis, Mr. **Walenn** enquired if the ordinary law of solution held when there was no evolution of hydrogen, and was answered in the affirmative.

Prof. **Guthrie** cited the experiments of Mr. C. V. Boys and himself on the conductivity of liquids as an instance of a current passing without electrolysis, or if there was decomposition it was followed by instant recomposition.

Dr. **Wright** thought there must be electrolysis in Dr. Guthrie's experiments (which were conducted by rotating a glass vessel filled with the liquid between the poles of a magnet, after Arago's experiment), because some two parts of the rotating vessel would be at different potentials, and a current would be set up in the liquid.

The Society then adjourned till after Christmas.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, November 16, 1880.

E. W. **Binney**, F.R.S., F.G.S., President of the Section, in the Chair.

"*Note on the Presence of Sulphur in Illuminating Gas*," by **Harry Grimshaw**, F.C.S.

That crude illuminating gas from coal contains a certain amount of compounds of sulphur is of course a well-known fact. That even the best quality of coal-gas, when purified and made ready for consumption, contains still a certain amount of sulphur compounds is also well, but perhaps not generally, known.

An accidental, somewhat peculiar, but very practical demonstration of this fact recently came under my observation, which I thought might be of some little interest to the Society. On the interior of the glass globe surrounding a gas jet in the hall of my house I had frequently noticed the presence of drops of condensed liquid. The jet being near the outer door, and the globe consequently exposed to a rather cold current of air, I merely considered it to be drops of water formed by the burning hydrogen of the coal-gas, and condensed on the cold surface of the glass. I noticed, however, that when the glass became heated through the turning on of a larger flame, the moisture did not, as it ought according to all reasonable expectation, evaporate. I was curious enough to take down the globe, wipe out a few of the drops on slips of paper, and rinse the rest off with water, which I preserved. Having my suspicions from the rather oily appearance of the drop, I warmed the slips of paper a little, and immediately obtained a very fine reaction for sulphuric acid, by the copious blackening and charring of the paper in those places where the liquid had touched it. I then applied the usual chloride of barium test, and obtained a plentiful precipitate of sulphate of barium from the washings of the globe; thus showing, of course, that the oily drops were literally nothing but tolerably strong oil of vitroil.

The genesis, so to speak, of these drops of sulphuric acid is of course easily explained and pretty well understood. The sulphur of course originally exists in the coal used for gas making, mostly combined with iron as sulphide. In the crude coal-gas it exists as sulphuretted hydrogen (H_2S), bisulphide of carbon (CS_2), and to a small extent as sulphur dioxide (SO_2). The greater portion of these compounds of sulphur are absorbed by the ammonia liquor which condenses from the gas itself in the scrubbers, and by the lime and oxide of iron in the purifiers, through which the gas passes on its way to the gasholder, but nevertheless a sufficient quantity remains in the gas to produce the effect above described, and to also produce a bright but very objectionable green deposit of sulphate of copper on the brass of the gaspipe above the jet I have alluded to.

The presence of the corrosive substances, sulphur dioxide and sulphuric acid, in the products of combustion of coal-gas, has always been known and always acknowledged, and yet but little notice of their effects has been taken. It has been said, "The quantlty is so small and is disseminated through such a bulk of other gaseous bodies." It is as well to see what this means. If the amount of sulphur in coal-gas is reduced to 10 grains per 100 cubic feet, it is considered a very pure gas indeed. Manchester gas is supposed to attain to this degree of purity, and is, I believe as a rule, a better gas in this respect than that of many other towns. For my own part I should feel inclined to think that only a small portion of the gas made in this country uniformly reaches this standard. However, say that the gas which I burn in my house contains no more than 10 grains of sulphur per 100 cubic feet. This means 100 grains, or about a quarter of an ounce (about) per 1000 cubic feet. I find that I burn on an average, through five or six jets per evening, 8900 cubic feet per quarter. This contains 890 grains of sulphur, which is equal to 2670 grains of sulphuric acid (H_2SO_4); so that I turn into the atmosphere of my house, mostly into one room, nearly 6 ounces of sulphuric acid in three months. This is 24 ounces, or 1½ pounds per annum. Now if I had been burning the Leeds gas, of the quality which has recently been subjected to a good deal of criticism, and which is stated to contain 40 grains per 100 cubic feet, then I should be subjecting the contents of my house to the action of 1½ lbs. of vitriol per quarter, or 6 lbs. per annum. In many cases, certainly in those cases where the contents of the room are most liable to damage, the above amount of gas, namely that from 5 or 6 lights, is given off into the atmosphere of one room.

Almost all the objects in the upper parts of a room are susceptible to damage by the vapour of sulphuric acid. I do not take into consideration the presence of sulphur dioxide, for it is almost impossible that this body could escape conversion into sulphuric acid in a very short space of time, in the presence of oxygen, the vapour of water, and heat. Ceilings, cornices, wall-papers, pictures, with their cords or chains, and frames, books, and so on, are all objects which are susceptible to the corrosive action of the vapour of sulphuric acid, and there cannot be much doubt that in a longer or shorter time they will all suffer from the presence of the sulphur in gas where the latter is burned in any quantity. The great danger in the action of sulphuric acid, in these cases, is that it is a cumulative one. Its action will be accumulative in a longer or shorter time under the following conditions respectively, burning the same amount of gas in each case. In a longer time if the ventilation is *very good*, and the walls of the apartment perfectly free from any *dampness*. In a shorter time when these two conditions are unfavourable, especially when the walls or portions of them are not perfectly dry, the presence of moisture naturally favouring the formation of sulphuric acid, and also absorbing it when formed and assisting its action. In such cases I have known a most rapid action upon wall-papers of certain kinds, and on organic materials where in contact with metals, notably brass, both the metal and the organic fibre being rapidly corroded.

Many of the objects in the upper portions of rooms to which I have alluded are replaceable, at some little expense, of course, such as the colouring of ceilings and wall-papers, and we may make up our minds to putting up with their deterioration for the sake of the excellent light and convenience of gas-lighting; but to expose such objects as valuable pictures and books to the extensive action of the products of combustion of coal-gas as we at present consume is most injudicious, and is, there can be little doubt, the cause of a great deal of irreparable injury in many cases. My own opinion is, that now that mineral and other oils for illuminating purposes are so cheap, and lamps for their consumption are so admirably constructed and elegant in design, there is not the slightest reason why valuable collections of pictures and books should be exposed to the sulphurous emanations of coal-gas.

Since writing the above I find by an abstract in the *Journal of the Chemical Society* (1880, vol. ii., p. 836), and also in the Chemical News (vol. xli., p. 64), that a Mr. W. R. Nichols confirms the generally accepted view that the deterioration of library bindings is mainly due to the action of the sulphuric acid from coal-gas, and he finds that Morocco is not so much affected as Russia leather and calf skin, and that ordinary sheep skin is attacked by this body.

Addenda.—For methods of estimating the sulphuric acid formed during the combustion of coal-gas, and proof that sulphuric acid is formed, see Young and others. *Analyst*, vol i., p. 143; vol. ii., pp. 66, 67, 118, 133, 135, 139; and vol. iii., p. 201. For the effect of gas on the books of the libraries of the Athenæum, London, Royal College of Surgeons, Portico Library, Manchester, and Literary Society, Newcastle-on-Tyne, see Dr. Letheby's earlier reports to the London Corporation.

THE AMERICAN CHEMICAL SOCIETY.[*]

The November meeting was held on Thursday evening, November 4th, at University Building, New York; Dr. A. R. Leeds in the chair. The following papers were read:—

"*Isomorphous Salts and Fractional Crystallisation*," by Lucius Pitkin, School of Mines, Columbia College. This paper was a reply to an objection concerning the chemical nature of the platinum salts described by the author at the meetings of November, 1879, and May, 1880. Mr. Pitkin based his argument upon the invariability of composition which the salts showed under varying conditions, and also on the difference in solubility of the double chloride and the double bromide of platinum and potassium. The solubility of the double chloride has been often determined; the solubility of the double bromide, as found by three experiments, is 1 part in 10 of water at 100° C. In discussing this paper, Dr. Endemann expressed himself as satisfied that the salts were really definite chemical compounds, and withdrew his previous objection. He further stated that he was glad the incidental remarks made by him on a previous occasion had led Mr. Pitkin to prove beyond doubt the existence of real chemical compounds.

"*Upon the Production of Ozone, by Heating Substances containing Oxygen*," by Dr. A. R. Leeds, Steven's Institute. From certain theoretical ideas entertained by Schönbein concerning the state in which the oxygen existed in the bodies experimented upon, the author was led to anticipate that on heating ozone would be evolved, and he included in the number of the substances which contained combined oxygen either partly or wholly in an ozonised condition, the oxides and superoxides of all the noble metals, the superoxides and some acids of the common metals, and a number of the states of oxidation of non-metallic elements. Although these ideas were presumably erroneous, in view of the ready conversion of ozone into ordinary oxygen on the application of heat, yet Scönbein not only thought he had experimentally verified them, but his results have been apparently confirmed by later observers, and have been incorporated as established truths in the excellent "Historisch-Critische Studien über das Ozon," just published by Professor Engler. Dr. Leeds showed that in the case of the peroxides of lead and manganese, the latter both in native and artificial condition, of mercuric and of argentic oxides, the apparent ozone reaction was due both to impurities in the substances employed—and which could not be eliminated by washings, however frequently repeated in the course of preparation,—and to the action of oxygen itself on the tests. The same was found to be true of periodic acid, which Bellucci stated yielded ozone on heating to 130°—135°. The statement of Bellucci, previously made by Crofts, that iodic acid gives off ozone in the act of crystallisation, had already been shown by the author to be incorrect, the apparent ozone reaction disappearing after purification of the acid by repeated crystallisation.

"*On the Action of Hyponitric Anhydride upon Organic Bodies*," by Dr. A. R. Leeds (second paper). The crystalline products formed in toluene after being so treated, and after standing for a number of months, were oxalic acid and a body having the ultimate composition of dioxybenzoic acid, but differing in its characters from the four modifications of dioxybenzoic acid hitherto described. The supernatant oil was orthonitrotoluene, yielding, on distillation with water, benzoic acid in the last portions of the water used, and the residue consisting almost entirely of dinitro-para-cresole. From 250 grms. of toluene so treated, 10 grms. of pure dinitrocresole (Victoria yellow) were obtained, and this easy process of preparing the dyestuff may, perhaps, find its application in the arts. Xylene, similarly treated, gave a crystalline product consisting of oxalic and phthalic acids. The oily portion was paraxnitroxylene, yielding in the last waters used for washing it by distillation an abundant crop of paratoluic acid crystals. From phenol the only product obtained had been picric acid, and from anthracen, anthraquinone, the conversion into these bodies by the action of hyponitric anhydride being readily effected.

"*New Colouring Matters Produced by the Action of Nitroso-dimethylaniline-hydrochlorate on Sulphuretted Phenols*," by Jas. H. Stebbins, jun. If one molecule of the soda salt of beta-naptholsulpho-acid be dissolved in its own weight of glacial acetic acid at a temperature of 110° C. and then treated with a molecule of nitroso-dimethyl-aniline hydrochlorate, the result is, first, a brown

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[*] Communicated by Arthur H. Elliott, F.C.S., Recording Secretary.

syrupy mass, then it turns to a dark brown, and finally there is a large disengagement of gas. This completes the reaction. The dark beetle-green pasty mixture thus formed is heated a short time longer to drive off all excess of acetic acid. It is then dissolved in boiling water and saturated with caustic potash. This throws down a light brown precipitate, which is collected on a filter and dissolved in dilute sulphuric acid. In this acid it dissolves with a fine blue colour, and on evaporation a bronze coloured mass is obtained, which is readily soluble in water, and dyes silk and wool of a blue colour in an acid bath.

"*A Process for Detecting Starch Sugar mixed with Commercial Cane Sugar*," by P. Casamajor. Methylic alcohol of about 1·14 specific gravity (Gay-Lussac's alcoholometer 50°) will dissolve its own weight of cane sugar (including impurities usually present in a straight sugar). It will dissolve a comparatively smaller weight of commercial starch sugar. The quantity is known to be less because the saturated solution of cane sugar is syrupy, while that of the starch sugar is very fluid. The author uses a saturated solution of starch sugar to dissolve a cane sugar suspected of containing starch sugar. After a few minutes the cane sugar, inverted sugar, &c., are dissolved, and the starch sugar, if present, will be seen undissolved in chalky specks. If a weighed sample of sugar is taken, the starch sugar left undissolved may be thrown on a weighted filter, washed rapidly with methylic alcohol of 98·5 per cent (92·5° Gay Lussac's Alcoholometer) dried and weighed.

CORRESPONDENCE.

THE GRAHAM MEDAL.

To the Editor of the Chemical News.

Sir,—Prof. Williamson, Graham's successor in the Chair of Chemistry at University College, London, having kindly complied with the request of the Committee of the Chemical Section of the Philosophical Society of Glasgow, that he should act as adjudicator in the competition for the Graham Medal, the Committee will esteem it a favour if you can find room for an intimation to this effect in the columns of the Chemical News.—I am, &c.,

James G. Dobbie, Secretary.

Chemical Laboratory, University of Glasgow,
December 8, 1880.

BLUE COLOUR OF THE ATMOSPHERE.

To the Editor of the Chemical News.

Sir,—In a note recently presented to the Chemical Society "On the Probable Absorption of Solar Rays by Atmospheric Ozone," I am afraid my views were stated in too concise a manner to ensure their being free from misinterpretation. I am credited, I believe, with having ascribed the colour of blue sky to atmospheric ozone. The sentence which seems to have given rise to this idea stands thus in my manuscript :—"If these determinations be correct, there is as much as 40 times the quantity of ozone in the air as is necessary to produce a blue colour, in which case it follows that the blue tint of the atmosphere must certainly be due to some extent to ozone, at times if not always." What I had in mind was, that the blue tint of distant objects when the proportion of ozone was at its maximum, would *in part* be caused by being viewed through a blue medium, which at other times would vary in depth of tint or be entirely absent, according to the meteorological conditions which cause variations in the quantities of ozone. I was led to this remark by having frequently observed that the colour of blue distance changes greatly, being not only sometimes deeper in tint but also occasionally more transparent than usual. The chief portion of my paper dealt with the ultra-violet rays and the improbability of the ammonia carbon dioxide, oxides of nitrogen, or water vapour causing the absorption seen in photographs of the ultra-violet spectrum.—I am, &c.,

W. N. Hartley.

Royal College of Science for Ireland,
Stephen's-green, Dublin, Dec. 14, 1880.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Note.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 20, November 15, 1880.

Presence of Phosphorus in the Rocks of Bretagne —M. G. Lechartier.—Phosphoric acid is very generally present in the granites and schists of Bretagne.

Composition of the Petroleum of the Caucasus.—MM. P. Schützenberger and N. Tonine.—All portions of this petroleum contain saturated carbides of the formula $Ꞓ_n H_{2n}$, which the authors name paraffenes. At a bright red heat they yield benzinic carbides, $Ꞓ_nH_{2n-6}$, naphthalin and a little anthracen. At dull redness the products are along with unaltered paraffenes, products which unite energetically with bromine, and which are converted into resinous polymers of ordinary sulphuric acid. It is difficult to isolate by means of fractional distillation definite products with constant boiling-points.

Zeitschrift für Analytische Chemie.
Part 2, 1880.

Removal of Large Quantities of Sodium and Potassium Chlorides in Mineral Analyses.—Dr. Fr. Muck.—The author remarks that these chlorides often accumulate to a serious extent and prove very inconvenient. He evaporates the filtrates containing magnesia (whether before or after the precipitation of the lime) almost to dryness, covers the residue with cold, fuming hydrochloric acid, stirs well, waits a few minutes, decants repeatedly through a filter formed of glass beads and cotton, and finally washes the saline mass upon the filter completely with strong hydrochloric acid. The residue obtained after evaporating the filtrate contains, in addition to magnesium chloride, very little fixed chlorides, and requires for its solution but a very small quantity of water. The use of glass beads containing lead must be carefully avoided, and also "glass wool," which apparently cannot be procured uncontaminated with lead.

Determination of Assimilable Phosphoric Acid by Dr. Petermann's Process.—Dr. Brunner.—A summary of the principal points in Dr. Petermann's method, the so-called Belgian commercial process. The ammonium citrate is prepared by dissolving citric acid in ammonia, so that the liquid may have a decided but not too powerful ammoniacal reaction. It is diluted to sp. gr. 1·09, filtered, and preserved in well-stoppered bottles. The quantity of the sample operated upon may be—of mixed manures, ammoniacal superphosphates, &c., 10 grms., superphosphates 5, and precipitated phosphates 2. 100 c.c. of the citrate are put in a small washing-bottle, and the weighed quantity of manure is washed with a slight stream into a small porcelain mortar; it is lightly rubbed up with the pestle, and gradually elutriated into a 500-c.c. flask, rinsing with the washing-bottle till the 100 c.c. are consumed. As little water as possible should be used in addition. The flask and its contents are next heated to 35° in the water-bath for exactly an hour, with occasional shaking; then filled up to the mark, mixed by repeated inversion, and filtered. The liquid which first passes is always turbid, and only the subsequent portion, when perfectly bright, should be taken for analysis. 50 or 100 c.c. of the clear filtrate are then precipitated with a sufficient quantity of magnesium chloride solution, well stirred, made strongly ammoniacal, and filtered after standing for six hours. After washing with ammonia, the precipitate is ignited in the usual manner, and weighed as magnesium pyrophosphate. If the quantity of phosphoric acid soluble in water is required it must be determined in a fresh portion. It is remarkable that in the analysis of certain very rich superphosphates the total phosphoric acid as determined gravimetrically by the molybdenum process was repeatedly found lower than the assimilable phosphoric acid.

Reverted Phosphoric Acid.—Dr. C. F. Meyer.—Already published.

Volumetric Determination of Phosphoric Acid in Crude Phosphates and Superphosphates by Means of Uranium in Presence of Ferric Oxide.—Carl Mohr.—The author proposes the following process:—2 or 5 grms. of the finely-powdered mineral are repeatedly boiled with small quantities of dilute nitric acid; the liquids are mixed in a measuring flask containing 100 or 250 c.c., and when cold filled up to the mark. In case of superphosphate similar proportions are observed, but distilled water is used instead of nitric acid. 10 or 25 c.c. of the filtrate are mixed with a solution of sodium acetate till a permanent turbidity is produced. The solution of uranium acetate is then allowed to flow in, heating gently at first, and afterwards to a boil, and before the precipitation is at an end a few granules of potassium ferrocyanide are added. The ferric phosphate is decomposed, the phosphoric acid enters into solution, the ferric oxide becomes Prussian blue and mixes with the uranium phosphate. The complete transformation of the ferric oxide into Prussian blue is ascertained when a drop of the clear liquid upon a porcelain plate shows no further colouration with ferrocyanide. The hot liquid very rapidly deposits the suspended precipitate. The author presses the rounded end of a moist thin glass rod upon ferrocyanide in a dry powder, when so much clings to the glass as to be sufficient for 10 c.c. of a mineral containing a slight amount of iron. It is important to defer the further addition of the uranium solution till all the ferric oxide is transformed. The addition of the uranium solution is then continued till the known colouration with potassium ferrocyanide indicates the end of the process. The first drop of uranium solution should not occasion a red colouration where it falls. If this happens, a new portion must be taken, and the operation repeated. As in the ordinary process of titrating phosphoric acid with uranium, the solution is rarely absolutely free from iron, the final reaction disappears after it has been already produced—a circumstance which often leaves the analyst in doubt to the extent of entire c.c. This disappearance of the final reaction may be avoided by the careful application of the method described above.

Volumetric Determination of Red-Lead.—Friedrich Lux.—The author places 2·07 grms. of the sample in a porcelain dish holding about 300 c.c., and adds 20 to 30 c.c. dilute nitric acid, heating gently, and stirring. In a few minutes the red-lead is resolved into lead oxide, which dissolves, and peroxide, which is insoluble. He then adds 50 c.c. of a one-fifth normal solution of oxalic acid, and heats to a boil. The peroxide is decomposed and dissolved, and the nature of the liquid shows to a certain extent the quality of the sample. Heavy spar, lead sulphate, clay, iron oxide, and gypsum in large quantities appear as a turbidity or a sediment, whilst if the red-lead is pure a perfectly clear and colourless solution is obtained. The liquid is kept at a boil and the excess of oxalic acid is determined with one-fifth normal permanganate, accurately standardised with solution of oxalic acid. The number of c.c. of permanganate consumed is deducted from 50, and the difference shows the percentage of lead present as peroxide. As not more than 30·21 per cent of lead can be present as peroxide, and consequently 19·79 c.c. of solution of oxalic acid must remain undecomposed, 5 to 10 c.c. of permanganate are added at once, and are immediately decolourised. Towards the end the decolourisation is slower, and the operation may be regarded as complete when the rose colour produced by 2 drops of permanganate does not disappear in half a minute. After the liquid has been rendered colourless by boiling for a few minutes, it is nearly neutralised with ammonia, mixed with a sufficient quantity of ammonium acetate, and titrated in the usual manner with solution of bichromate, 14·761 grms. per litre. The number of c.c. consumed gives the total percentage of lead. If the quantity of lead present as peroxide is deducted from the total, the lead existing as oxide is found, and the composition of the sample is known.

Direct Determination of Soda in Potash.—Dr. A. van Hasselt.

Estimation of Butter.—L. Medicus and S. Scherer.—The authors pronounce the method proposed by Reichert for the analysis of butter to be the most correct in principle, and they apply it in determining whether the composition of butter is unaffected by melting and subsequent congelation. They find that those portions of the mass which are the last to cool retain the largest proportion of the volatile fats. Thus an average sample of butter, 2·5 grms., required for saponification 14 c.c. decinormal soda. The bulk was then melted, allowed to cool spontaneously, and different parts of the mass were analysed. The upper layer required 13·3 c.c.; the bottom, 14·2; the sides, 14·4, and the middle, 17·3. The authors point out the necessity of great care in sampling butters and fats for analysis. In conclusion they give a table showing the quantity of decinormal soda consumed by 2·5 grms. butter as compared with other fats and oils:—Butter, 14 c.c.; lard, 0·2; turnip-seed oil, 0·3; rape oil, 0·4; oil of sesame, 0·35; olive oil, 0·3, and palm oil, 0·5.

Simple Apparatus for Extraction.—L. Medicus.—This paper cannot be intelligibly extracted without the accompanying illustrations.

How Long can Phosphorus be Detected in Cases of Poisoning?—L. Medicus.—In the case of a hen poisoned with phosphorus the digestive organs were found luminous on the twenty-third day after death, and phosphorus was readily detected.

Rapid Detection of Aloes in Elixirs, Liqueurs, and Beer.—Hugo Bornträger.—The liquid in question is well shaken with twice its volume of benzine, and the behaviour of the latter with ammonia is observed. If no red colouration is perceived aloes is either entirely absent or present in quite insignificant quantities.

Analysis of Certain Beers.—E. Schrader.—The beers in question are produced in various places in north-eastern Prussia. Their highest percentage of alcohol is 4·480.

New Gas Blast-Lamp.—Julius Schober.—This paper requires the accompanying woodcut.

Reports on the Progress of Analytical Chemistry. I. General Analytical Methods, Operations, Apparatus, and Reagents.—W. Fresenius.

Application of Sulphuretted Hydrogen in the Dry Way.—From the *Comptes Rendus*, 89, 167.

Determination of High Melting-points.—T. Carnelley.—From the *Journal of the Chemical Society*, 16, p. 365.

Determination of High Boiling-points.—T. Carnelley and W. Carleton Williams.—From the *Journal of the Chem. Soc.*, 1878, 281.

Gas Analysis.—Dr. Clemens Winkler.—Notice of an independent work, apparently of a very valuable character.

Stathmetometric Method of Chemical Analysis.—H. Hager.—Already noticed.

Tables on the Expansion of Water by Heat.—P. Casamajor.—See Chemical News, xxxv., 160, and xxxviii., 137.

The remaining papers in this section are taken from the *Berichte der Deutsch. Chem. Gesell.*, the Chemical News, &c.

II. Chemical Analysis of Inorganic Bodies.—H. Fresenius.

Volumetric Determination of Cerium.—E. Stolba.—Already noticed.

Separation of Cobalt and Nickel.—Ph. Dirvell.—From the *Comptes Rendus*, 89, 903.

Detection of Small Quantities of Mercury.—E. Teubner.—A modification of Eschka's process. The mercury is condensed upon a gold capsule. The peculiar arrangement of the apparatus is shown in an accompanying figure.

The remaining notes under this section are abstracts from the *Annalen der Chemie*, *Journ. f. Prakt. Chemie*, &c.

The same remark applies to the section on the analysis of organic matter.

Determination of Water in Milk.—H. Geissler.—The method depends on the use of an instrument patented by the author and here figured.

Analysis of Wine.—The methods here summarised have been noticed.

An Improved Microscope for the Examination of Meat.—E. Schmidt and Haensch.—The instrument is described and figured.

Testing Butter.—Compiled from the *Comptes Rendus*, the *Analyst*, the *Pharm. Central-Halle*, &c. In order to distinguish genuine butter from so-called "oleo-margarine," Hager saturates a cotton-wick with the melted sample, lights, and allows it to burn for two minutes, and judges its quality by the smell. Artificial butter gives the well-known offensive odour of an extinguished tallow candle.

Determination of the Specific Gravity of Solid Fats, Paraffin, &c.—H. Hager.—Globular drops of the fats are caused to float in a liquid which consists according to circumstances of alcohol, water, or glycerin.

Adulteration of Bees'-Wax.—See *Pharm. Central-Halle*, *Corr.-Blatt des Vereins Analyt. Chemiker*, 2, 57.

Analysis of Superphosphates.—See *Berichte der Deutsch. Chem. Gesellsch.*, 9, 1809, *Annalen der Chemie*, 198, 290.

Examination of Quinine Sulphate and Muriate.—O. Hesse.—See *Arch. d. Pharm.*, 213, 490.

For analytical methods referring to physiological and pathological chemistry we must refer to the original.

Forensic Chemistry.

Poisonous Alkaloids from Putrescent Albumen.—F. Selmi.—Already noticed.

Preparation of Sulphuretted Hydrogen for Chemico-Legal Purposes.—The author refers with approval to Otto's method of obtaining sulphuretted hydrogen from calcium sulphide.

Recognition of Human and Animal Blood.—H. Schmidt.—The author steeps the blood for twenty-four hours in potassa-ley at 30 per cent and measures the corpuscles. Those from the blood of the dog are always smaller than from human blood.

MISCELLANEOUS.

The Midland Railway Company.—We are informed that there were considerably more than 100 applicants for the appointment of Chemist to the Midland Railway Company, recently vacant through the decease of the late Mr. S. Bacon. Mr. Leonard Archbutt, formerly a pupil, and till lately chief assistant of Mr. A. H. Allen, is the successful candidate.

South London School of Pharmacy.—The Medals and Certificates for the First B. Course were presented to the following successful competitors on Saturday, December 11, 1880:—Senior Chemistry—Medal, Mr. Preston. Junior Chemistry—Medal, Mr. Cracknell; Certificate, Messrs. Llewellyn and Haines (equal). Botany—Medal, Mr. Fisher; Certificate, Mr. Fry. Materia Medica—Medal, Mr. Llewellyn; Certificate, Mr. Cracknell. Pharmacy and Practical Dispensing—Medal, Mr. Haines; Certificate, Mr. Fisher.

On Technical Museums and Libraries.—At a meeting of the Manchester Scientific and Mechanical Society, held on November 19th, Mr. Alderman W. H. Bailey read a paper on the value of free museums and libraries to the manufacturing interest of the country. He reminded his hearers that prior to the invention of the steam-engine it would be difficult to name any great industry in which we were superior to other nations, and that now this source of power was no longer confined to any one people, only those who can use the forces of nature with the highest wisdom will be first in the race. He remarked that our great inventors at the commencement of this century were nearly all men of ancestral, *i.e.*, hereditary, power, and not having much to do—or as we should express it, not being over-worked and worried—they made great discoveries. Speaking of Richard Roberts, who invented a marvellous number of tools and machinery, he said that this man never tried to do anything at all without first informing himself of all that had been done before. In order to give inventors, especially among the working-class, the means of gaining such knowledge, libraries and museums of models, &c., were needed. He contended that both the museum and the library in every town should be specially adapted to the trade of the district. He referred in terms of warm approval to a catalogue drawn up by Mr. Folkard, of the Wigan Free Library, showing all the works relating to mining and manufacturing, and indexing the transactions of learned societies contained in the library as far as the industries of the district are concerned. It is scarcely needful to add that, *mutatis mutandis*, all these views hold good for the chemical as well as for the mechanical arts.

MEETINGS FOR THE WEEK.

Monday, 20th.—Medical, 8.30.
—— London Institution, 5.
Tuesday, 21st.—Civil Engineers, 8. (Anniversary.)
Thursday, 23rd.—London Institution, 7.

TO CORRESPONDENTS.

W. M. Paterson.—The publisber is David Louis Wolff, Magdeburg.

F. N. Harvey.—1. Prof. Wiley's papers will probably be published in some American journal of chemistry. 2. It is only published in German.

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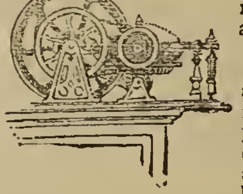

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THE CHEMICAL NEWS.

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ON HEAT CONDUCTION IN HIGHLY RAREFIED AIR.*

By WILLIAM CROOKES, F.R.S.

The transfer of heat across air of different densities has been examined by various experimentalists, the general result being that heat conduction is almost independent of pressure. Winkelmann (*Pogg. Ann*,, 1875, 76) measured the velocity of cooling of a thermometer in a vessel filled with the gas to be examined. The difficulty of these experiments lies in the circumstance that the cooling is caused not only by the conduction of the gas which surrounds the cooling body, but that also the currents of the gas and, above all, radiation play an important part. Winkelmann eliminated the action of currents by altering the pressure of the gas between 760 and 1 millim. (with decreasing pressure the action of gas currents becomes less); and he obtained data for eliminating the action of radiation by varying the dimensions of the outer vessel. He found that, whereas a lowering of the pressure from 760 to 91·4 millims. there was a change of only 1·4 per cent in the value for the velocity of cooling, on further diminution of the pressure to 4·7 millims. there was a further decrease of 11 per cent, and this decrease continued when the pressure was further lowered to 1·92 millim.

About the same time Kundt and Warburg (*Pogg. Ann.*, 1874, 5) carried out similar experiments, increasing the exhaustion to much higher points, but without giving measurements of the pressure below 1 millim. They enclosed a thermometer in a glass bulb connected with a mercury pump, and heated it to a higher temperature than the highest point at which observations were to be taken; then left it to itself, and noted the time it took to fall through a certain number of degrees. They found that between 10 millims. and 1 millim. the time of cooling from 60° to 20° was independent of the pressure; on the contrary, at 150 millims. pressure the rate was one-and-a-half times as great as at 750 millims. Many precautions were taken to secure accuracy, but no measurements of higher exhaustions being given the results lack quantitative value.

It appears, therefore, that a thermometer cools slower in a so-called vacuum than in air of atmospheric pressure. In dense air convection currents have a considerable share in the action, but the law of cooling in vacua so high that we may neglect convection has not to my knowledge been determined. Some years ago Professor Stokes suggested to me to examine this point, but finding that Kundt and Warburg were working in the same direction it was not thought worth going over the same ground, and the experiments were only tried up to a certain point, and then set aside. The data which these experiments would have given are now required for the discussion of some results on the viscosity of gases, which I hope to lay before the Society in the course of a few weeks; I have therefore completed them so as to embody the results in the form of a short paper.

An accurate thermometer with pretty open scale was enclosed in a 1½ inch glass globe, the bulb of the thermometer being in the centre, and the stem being enclosed in the tube leading from the glass globe to the pump.

Experiments were tried in two ways:—

I. The glass globe (at the various exhaustions) was immersed in nearly boiling water, and when the temperature was stationary it was taken out, wiped dry, and allowed to cool in the air the number of seconds occupied for each sink of 5° being noted.

II. The globe was first brought to a uniform temperature in a vessel of water at 25°, and was then suddenly plunged into a large vessel of water at 65°. The bulk of hot water was such that the temperature remained sensibly the same during the continuance of each experiment. The number of seconds required for the thermometer to rise from 25° to 50° was registered as in the first case.

It was found that the second form of experiment gave the most uniform results; the method by cooling being less accurate, owing to currents of air in the room, &c.

The results are embodied in the following Table:—

(Rate of Heating from 25° to 50°.)

Table I.

Pressure.	Temperature.	Seconds occupied in rising each 5°.	Total number of seconds occupied.
760 millims.	25°	0	0
	25 to 30	15	15
	30 to 35	18	33
	35 to 40	22	55
	40 to 45	27	82
	45 to 50	39	121
1 millim.	25°	0	0
	25 to 30	20	20
	30 to 35	23	43
	35 to 40	25	68
	40 to 45	34	102
	45 to 50	48	150
620 M.*	25°	0	0
	25 to 30	20	20
	30 to 35	23	43
	35 to 40	29	72
	40 to 45	37	109
	45 to 50	53	162
117 M.	25°	0	0
	25 to 30	23	23
	30 to 35	23	46
	35 to 40	32	78
	40 to 45	44	122
	45 to 50	61	183
59 M.	25°	0	0
	25 to 30	25	25
	30 to 35	30	55
	35 to 40	36	91
	40 to 45	45	136
	45 to 50	67	203
23 M.	25°	0	0
	25 to 30	28	28
	30 to 35	33	61
	35 to 40	41	102
	40 to 45	55	157
	45 to 50	70	227
12 M.	25°	0	0
	25 to 30	30	30
	30 to 35	37	67
	35 to 40	41	108
	40 to 45	58	166
	45 to 50	86	252
5 M.	25°	0	0
	25 to 30	38	38
	30 to 35	43	81
	35 to 40	54	135
	40 to 45	71	206
	45 to 50	116	322
2 M.	25°	0	0
	25 to 30	41	41
	30 to 35	51	92
	35 to 40	65	157
	40 to 45	90	247
	45 to 50	165	412

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* Abstract of a Paper read before the Royal Society, Dec. 16, 1880.

* M = millionth of an atmosphere.

There are two ways in which heat can get from the glass globe to the thermometer—(1) By radiation across the intervening space; (2) by communicating an increase of motion to the molecules of the gas, which carry it to the thermometer. It is quite conceivable that a considerable part, especially in the case of heat of low refrangibility, may be transferred by "carriage," as I will call it to distinguish it from convection which is different, and yet that we should not perceive much diminution of transference, and consequently much diminution of rate of rise with increased exhaustion, so long as we work with ordinary exhaustions up to 1 millim. or so. For if, on the one hand, there are fewer molecules impinging on the warm body (which is adverse to the carriage of heat), yet on the other the mean length of path between collisions is increased, so that the augmented motion is carried further. The number of steps by which the temperature passes from the warmer to the cooler body is diminished, and accordingly the value of each step is increased. Hence the increase in the difference of velocity before and after impact may make up for the diminution in the number of molecules impinging. It is therefore conceivable that it may not be till such high exhaustions are reached that the mean length of path between collisions becomes comparable with the diameter of the case, that further exhaustion produces a notable fall in the rate at which heat is conveyed from the case to the thermometer.

The above experiments show that there *is* a notable fall, a reduction of pressure from 5 M. to 2 M. producing twice as much fall in the rate as is obtained by the whole exhaustion from 760 millims. to 1 millim. We may legitimately infer that each additional diminution of a millionth would produce a still greater retardation of cooling, so that in such vacua as exist in planetary space the loss of heat—which in that case would only take place by radiation—would be exceedingly slow.

RECENT HISTORY OF ELECTRIC LIGHTING.

By HENRY MORTON,
President of Stevens Institute of Technology.

On the 17th of October, 1878, in fulfilment of a promise made a year before, the present writer read before the American Association of Gas Engineers an address in which (alluding to the reports then resounding in the daily papers as to Mr. Edison's new form of electric lamp) he said, after quoting an account of a similar report current twenty years before in France:—"These promising experiments, as we all know, reached no successful results. I would by no means, however, have it inferred that better success can never be attained. On the contrary, there are several very promising directions for experiment, on one of which no doubt Mr. Edison is at present embarked; but the difference between a promising line of experiment and a successful result, as all the world's history teaches us, is often a distance of many years, to say the least." An opinion so little calculated as this, to aid in "bearing" gas stocks, naturally excited no little disgust in certain quarters, and I had the pleasure of seeing myself very handsomely abused by those whose approval would have been a very doubtful compliment.

Looking back over the space of two years which has elapsed since those words were uttered, and remembering the events which have transpired in this connection, I feel that I have much more reason to congratulate myself than have those who took an opposite view.

Not only has "the promising line of experiment on which Mr. Edison was then engaged been separated by many years from a successful result," but it has, within this time, been totally abandoned by him for another, which other is, to say the least, still under approval, and the discredit and ridicule which has been cast from all sides has shown how unfortunate and unwise were the reckless and unfounded assertions of the would-be Prophets of the Press.

Notwithstanding these drawbacks, however, continual progress has been made in the development of the art of lighting by electricity, and we have to regret that what would otherwise have been a dignified march of advancing science, reflecting nothing but credit upon this country and its inventors, has been degraded to some extent into a carnival rout of grotesque and absurd extravagances.

In recording the progress which has been made in the art of electric lighting, the most important practical results have been achieved by those who have devoted their attention to the application of the electric arc. The great difficulty which presented itself two years ago in this subject was the unsteadiness of the light, its great liability to extinction, and the actual cost of maintaining it. By improvements in the carbon rods as to their purity of composition and uniformity of structure, a great increase in steadiness has been attained, and by other improvements in the mechanical arrangements of the self-adjusting lamps, this amelioration has been likewise assisted.

Among those who have done most for the improvement of the carbon rods, I would mention Wallace and Sons, of Ansonia, and among the most marked improvements in electric lamps I would cite those of the Brush Co., of Cleveland.

The economical efficiency of the electric generators or dynamo-electric machines has also been increased, and in this regard the best results, so far as accurate tests which have been published indicate, have been obtained by Mr. Edison, whose machine, as tested by Profs. Young and Bracket, shows the very high efficiency of 84·1 per cent of motive force or energy converted into electric current. The machines of other inventors appear to be only a few per cent behind this, if indeed some of them are not found to equal it when accurately measured.

In consequence of these improvements and of the energy with which the several manufacturers have brought these machines to the notice of those likely to use them, their employment in factories, stores, workshops, and hotels has become very general, and has undoubtedly proved of great advantage and convenience, although instances are not wanting (as, for example, in the great cotton mills at Lowell, Mass.), where the electric lighting was abandoned after a thorough test. There is, however, so wide a field in which this light has manifest advantages over any other means of illumination that we may confidently look for a vast increase in its introduction in all parts of the country.

Turning next to the production of light by incandescence we find that this branch of the art also has made marked advances. This art had its beginning as early as 1845, when in the patents taken out in England by King, the representative of the American, Mr. Starr, we find described the use of platinum wires and thin rods of carbon, the latter inclosed in a torricellian, or absolute, vacuum. Extensive experiments in the same line were made by Mr. Moses G. Farmer, the widely known electrician, about the year 1858, and again a little later, by Dr. Isaac Adams, the inventor of the process of nickel plating, now widely used; but the costliness of an electric current derived from a galvanic battery prevented the earlier experimenters from reaching any commercial success, and even with the cheap electricity of to-day, there is still a limit set upon this sort of lighting, by its relatively wasteful expenditure of electric force and consequent costliness.

Great progress has, however, been made here also.

Mr. Edison supposed he had solved the problem two years ago when he had re-invented the earlier methods of Starr, Farmer, and others, but very soon finding, as they had done, the insufficiency of his solution, he attacked the problem afresh with an energy, persistence, and originality of resources worthy of all praise. In the course of his researches he has brought to light a number of astonishing results, and has discovered whole classes of new properties in platinum, and other metals, and lime, magnesia, zirconia, and other substances.

Had his enthusiastic friends only claimed for him from time to time what he had actually done, his fame would have been unparalleled as a discoverer, and without blemish.

Unfortunately, too many prophecies have been indulged in, and the credit of really great achievements has been overshadowed by the failure of still greater assertions and promises.

After the abandonment of the platinum lamp, Edison next took up the carbon-in-vacuo lamp of Starr, and as before, his friends shouted "Eureka" at the beginning and not at the end of his search.

The carbon lamp, as first made by him, has met the fate of its predecessors, and has been abandoned by him likewise; but from its ashes have arisen, both in his and other hands, improved descendants, which show much better chances of survival.

Having found it practically impossible to make his paper carbons sufficiently durable when heated to such an intensity as would render them even possibly economical as sources of light, he has ransacked nature for some material which would fill these hard conditions, and in a certain variety of bamboo has found something which he believes will yield him a fine hair-like thread of carbon with which he can obtain the light of 16 candles from a single lamp, at an expense of 3400 foot-pounds of energy a minute. This would represent about 9 such lamps, or about 155 candles per horse power.

This is a decided improvement in efficiency over his horse-shoe lamps of last year, and with a durability, as is claimed, of six months, will carry his system a long step forward on the roads toward success.

Starting, also, with the idea of Starr as to the use of a strip of carbon in a vacuum, Mr. Maxim has proceeded along a different road.

In place of searching through nature for a substance which would yield a perfect lamp-carbon on heating, Mr. Maxim has sought to make a perfect carbon strip in his lamp by building it up on any imperfect form as a substratum. To accomplish this he has taken a strip of carbonised paper as his groundwork, and has plated this over with a metallic layer of graphitic carbon, by heating the original carbon strip by a current in an atmosphere of gasoline vapour. Under these circumstances, not only is a sheet-like coating of this peculiar carbon deposited all over the strip, but, if there is a weak spot anywhere in it, the carbon will be first deposited at this point (on account of its getting hotter than the rest), and thus an irregular or defective carbon strip will be repaired and rendered perfectly uniform.

By this process two objects are attained. In the first place a good carbon strip is secured, no matter what imperfections may have existed in the original paper or other carbon foundation. In the second place, the outer surface of the strip is composed of that variety of carbon whose resistance to heat and mechanical injury surpasses all others. All familiar with the material will know, that what is technically called "gas carbon" (*i.e.*, the carbon deposited in the outlet retorts by the decomposition of the issuing gas), of gas is a metal-like substance, the cutting, breaking, or shaping of which is very difficult, on account of its combined hardness and toughness. It very rapidly spoils the best steel files and saws, and to break it with a hammer is almost a hopeless undertaking.

I have also known a weighed quantity of it to be placed with the fuel in a cupola furnace where iron was being melted, and to have been taken out again after some hours not sensibly diminished in weight.

This is the material of which the surfaces of the carbon strips in the Maxim lamps are composed, and should they prove as enduring as is hoped, it is to this that we should naturally credit their resistance.

I have measured a number of these lamps with such results as the following:—

Lamp A.—Resistance cold 20·4 ohms, and when yielding in its best position the light of 50 standard candles its resistance was 8·3 ohms. The current required to heat it to this degree was one of 4·07 webers, and the energy expanded was therefore about 5850 foot-pounds. This would represent about five and a half (5½) such lamps to each horse-power of energy of current employed, or an efficiency of about 275 candles per horse-power. Taking 70 per cent of this as the average for all positions we would have about 192 candles per horse-power of current. Now the amount of actual horse-power in the steam engine required to develop one horse-power in the lamps will vary with the dynamo-electric machine, the number of lamps in use, and many other conditions, but we may assume 60 per cent as a safe allowance and this would give us about 115 candles per actual horse-power from the engine as the efficiency of this lamp when run at 50 candles.

Another lamp (D) whose resistance cold was 115 ohms when yielding a light of 52 candles required a current of 1·35 webers, and had a resistance of 64 ohms when emitting the above-mentioned amount of light.

The electric energy consumed in this case was therefore about 5131 foot-pounds per minute, or about 6·4 such lamps to a horse-power, or at a rate of 330 candles per horse-power of current.

Taking 70 per cent as the average light in all directions, reduces this to 231 candles, and assuming 60 per cent as the efficiency of the conversion from steam power into electric current in the lamp, brings it to 138 candles per horse-power of mechanical force developed in the steam engine.

We see therefore in this case also a marked advance upon the state of the art a year ago.

Undoubtedly the advance now made is sufficiently encouraging to authorise the experiments which are being made to apply these lights on a practical scale in locations where they have peculiar advantages, as, for example, in the vaults of the Safe Deposit Co., New York. When that experiment has been carried on for a few months, we shall know much more than we do now as to the actual running cost of the system, but anything like a general adoption of the same in place of gas is still an affair o the future about which prediction is premature.

THE DETECTION OF STARCH AND DEXTRIN.

By SPENCER UMFREVILLE PICKERING, B.A., Oxon.

In conducting some experiments in which it was necessary to ascertain the presence or absence of starch in a liquid containing various amounts of dextrin, the few facts here described were brought to light, and may, perhaps, be of sufficient interest to warrant their publication.

When a solution of starch which has been coloured blue by the addition of iodine is heated, it is found that the temperature at which the colour disappears varies with the intensity which it possessed before heating. Thus, for instance, 100 c.c. of a rather dark iodine-starch solution on being heated gradually in a flask became perfectly colourless at 58° C., and, on being cooled, showed a slight reappearance of colour at 49° C., whereas an opaque blue solution did not lose its colour till heated to 99° C., and became visibly coloured again when cooled to 63° C. Similarly variable results were obtained by experimenting on iodine-starch solutions in sealed tubes, the temperatures of reappearance being much more constant (generally about 50° C.) than those of disappearance; this no doubt is due to the fact that, the stronger solutions having been heated to a higher temperature than the weaker ones in order to effect the disappearance, a greater quantity of the iodine present in them would have been converted into hydriodic acid, and this would tend to an equalisation of the amounts of iodine present on cooling in the various cases. Owing to this conversion of iodine into hydriodic acid, the solutions on cooling, as might be expected, are considerably lighter than they were before heating, and their intensity naturally depends to a great extent on the rapidity with which they have been cooled; even a very weak iodine-starch solution which has been heated may be made to re-assume its colour if cooled very quickly.

The amount of starch which may be recognised by means of the iodine reaction varies, of course, with the bulk of liquid operated on. Using about 200 c.c. the weakest solution which gives an easily discernible blue tint in a beaker contains about 0·0001 per cent of starch; while if small quantities are examined in a test-tube this percentage must be doubled in order that the colour may be rendered visible. The green colour which is noticed when a large quantity of iodine is added to a weak solution of starch, appears to be due simply to the combination of the proper yellow colour of the free iodine with the blue colour of the iodine-starch.

When two weak solutions of iodine, to one of which some starch has been added, are exposed to the air in an uncovered beaker, the iodine in both cases disappears entirely in the course of a few days, but more slowly from the solution which contains the starch; hence the iodine which disappears (owing partially to its volatilisation into the air and partially to its hydrogenation) seems to be retained to a certain extent by the presence of starch. The presence of iodine has a reciprocal action in the preservation of starch. A solution of starch, which, in a few days, is converted into dextrin, may be preserved unaltered for a long time—possibly for an indefinite time, if an excess of iodine is present in it.

When a sufficient quantity of iodine is added to a solution of dextrin, a deep brown colour is produced; the coloured compound which is here present is in a state of true solution, whereas in the case of starch it will, as is well known, settle entirely to the bottom of the liquid in deep blue flocks, leaving the supernatant solution quite colourless, and these flocks on agitation are disseminated again so as to form an apparent solution. The dextrin reaction with iodine is not nearly so delicate as that of starch; the weakest solution which gave any discernible colour on being tested contained 0·005 per cent of dextrin, and in this case the colour could only be detected by using about 200 c.c. of the solution, and comparing the colour with that of some iodine solution of the same strength as that to which the dextrin had been added.

With starch, the first drop of iodine which is added produces a permanent colouration. With dextrin, however, this is not the case; the colour produced by the first drops disappears instantly and entirely. A considerable quantity must be added before a moderately permanent colour is produced, and the reaction, owing to which the iodine disappears in this way, will continue for six or seven days. Whether the dextrin disappears or not at the same time has not been ascertained, although it seems most probable that it should do so.

When a solution of iodine-dextrin is heated, the colour becomes lighter and gradually disappears, as in the case of iodine-starch, but the temperature at which this disappearance takes place is considerably lower. An opaque brown solution on being heated in a flask became colourless at about 81° C., and, on cooling, regained its colour with considerable diminution in intensity) at 64° C. A solution of one-quarter the strength of the preceding one lost its colour at 52°, and regained it on cooling at 34° C.; here also, as in the case of iodine-starch, we find that the coloured principle does not become colourless at any particular temperature, but its disappearance is dependent on its original intensity.

The dextrin usually met with in commerce contains a considerable amount of starch, which, however, may be entirely converted into dextrin by prolonged heating at 140° to 160° C. for several hours, after which it gives the pure brown reaction with iodine above mentioned.

When iodine is added in excess to a mixture of starch and dextrin, the colours produced are blue, violet, purple, claret, red-brown, or brown, according to the various proportions in which the two substances are present. When the iodine is added gradually to the mixed solutions the colours produced, both temporary and permanent, follow the same order as those above mentioned, the blue colours appearing first, and the red ones only on the addition of larger amounts of iodine. Conversely, when the coloured solution is allowed to stand, the red tints disappear first, and the blue ones last. Obviously, therefore, the gradual addition of iodine affords an easy and delicate means of detecting starch in the presence of even a large amount of dextrin. Another way in which starch may be detected in similar cases, is to add an ample sufficiency of iodine to produce a permanent colour, and then to heat the liquid; the brown iodine-dextrin is decomposed at a comparatively low temperature, while the blue iodine-starch remains till the heat is raised considerably higher, and again, on cooling, the blue tint reappears long before the brown or red tint does: even when there is not sufficient starch to yield satisfactory results by this method, it may often be detected by the liquid being of a more bluish tint after the heating than it was before it.

O. Knab (*Chem. Centr. Blatt*, 1872, 492) found that some dextrin which he had prepared by repeated (ten times) precipitation with alcohol gave the reaction of a mixture of dextrin and starch, and hence concluded that it still contained some of this latter substance. It appears superfluous, however, to raise an impure preparation to the dignity of a chemical compound by giving it a distinct name—dextrin-starch—as Knab does. On leaving a mixture of solutions of starch and dextrin for some days, Knab found that, whereas the addition of iodine had at first caused a deep blue colouration, after a time nothing but the red or brown colour of iodine-dextrin was produced, and hence draws the somewhat startling conclusion that starch under the influence of dextrin is converted into dextrin. A simpler and more probable conclusion from these experiments would surely have been, that at the end of the few days during which his experiments lasted, the starch had suffered that spontaneous decomposition to which it is, as is well known, so prone, leaving in solution nothing which would give a colouration with iodine but the unaltered dextrin.

Dextrin and starch, it appears, give entirely different reactions with iodine; the former combines with the halogen to form a brown soluble substance, whereas the latter forms with it a deep blue insoluble body; and these two reactions are so distinct that presence of either of the reagents may be easily detected in a solution containing both of them.

The fact that the addition of iodine to dextrin produces only a transitory colour at first, and that an excess of it is necessary to give a permanent tint, will, no doubt, explain the various discordant statements which exist as to whether any colour is produced by the mixture of these two substances or not, and will probably render unnecessary the theory of there being two or three different dextrins as proposed by Mulder and by Griessmayer.

Detection of Iodine in Bromine and Metallic Bromides.—A. Jorissen.—A few drops of the bromine in question are placed in a small porcelain capsule, 30 c.c. of a solution of potassium chlorate, saturated in the cold, are added, and the liquid is boiled till colourless. The solution is then poured into a test-tube, allowed to cool, mixed with a few drops of a solution of morphine sulphate and a little chloroform. If the chloroform takes a violet colour, iodine was present in the sample. The morphine solution is prepared by dissolving 0·5 grm. morphine in an excess of dilute sulphuric acid, and diluting to 50 c.c. In examining potassium bromide the solution is mixed with 2 or 3 drops of pure bromine water, and a few c.c. of a cold saturated solution of potassium chlorate, and further treated as above.—*Zeitschrift fur Analytische Chemie.*

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, December 16, 1880.

Prof. H. E. Roscoe, President, in the Chair.

The following certificates were read for the first time:—B. Akroyd, H. Baker, M. M. Bird, H. P. Cooper, H. W. Fenner, T. S. Judson.

The President then called on Mr. Ruffle to read a paper "*On the Estimation of Nitrogen by Combustion, including the Nitro-compounds.*" For strictly correct chemical purposes the nitrogen in a substance is estimated by Dumas's method; in analytical practice the process ueually employed is the well-known soda-lime method of Will and Varrentrapp. It has long been known that with some compounds containing nitrogen this process gives very imperfect results. Thus, sodium nitrate, when burnt with soda-lime, gave an acid instead of an alkaline distillate. It has been proposed to add various substances to the soda-lime to obtain better results. The author has tried the effect of numerous devices of this nature. Mixing with sugar, a current of hydrogen, of olefiant gas, of marsh-gas, tartaric acid, citric acid, benzoic acid, sodium formate, sodium acetate, hydrogen sulphide, ferrous sulphate, sodium sulphide, sodium sulphite, sodium hyposulphite, potassium hyposulphite, barium hyposulphite, barium hydrate instead of lime, mixtures of caustic soda with various reducing substances, sulphur, iron, charcoal, tin, zinc, antimonious sulphide, antimony, arsenic, arsenious acid, sodium hyposulphite: all, however, yielded results much too low or were inconvenient. A mixture of sodium hyposulphite and sodium hydrate yielded theoretical results, but had the great objection that it could not be obtained as a powder, only as a wet mass. After many trials it was found that a mixture containing—

2 molecules of sodium hydrate,
1 molecule of pure lime (from marble),
1 molecule of sodium hyposulphite,

the first two being slaked together, dried, and finely powdered, gave very satisfactory results with all varieties of guano, gunpowder, nitrates of soda, potash, and silver, picric acid, sodium nitro-prusside, East Indian linseed, milk residues, dried flesh, &c. The author uses ½ inch lap-welded steam-piping, bored out to ⅝ inch, in 22 inch lengths, for combustion tubing. The substance is mixed with 1½ grms. of a mixture of equal parts of sulphur and wood-charcoal. 13 grms. of the above soda-lime mixture and 21 grms. of powdered hyposulphite of soda are weighed, and mixed. 5 grms. of this mixture are placed at the back of the combustion-tube, then the mixture containing the substance, then the rest of soda and hyposulphite, and finally 18 grms. of ordinary soda-lime. The combustion is commenced 4 inches from the front end, and continued gradually until the evolution of gas ceases: it is maintained at a red-heat for ten minutes. The average time of a combustion is forty-five minutes. As results may be quoted the following:—

	Theory.	Found.
Potassium nitrate ..	13·86	13·86
Silver nitrate	8·23	8·03
Sodium nitrate.. ..	16·47	16·32 mean of 5 analyses
Picric acid	18·34	17·91 ,, 6 ,,

In answer to Mr. Perkin, the author stated that he had not used the method with any organic nitro-compound other than picric acid; it did not succeed with aniline.

Mr. Warington said we must not too hastily conclude that the soda-lime process does really fail: there were many eminent chemists who contended that when fairly conducted it gave good results. If the temperature was too high the ammonia formed was decomposed again; errors also crept in when air was aspirated through the tube when hot. Care should also be taken to have a good layer of soda-lime in front of the mixture. He did not think that it was quite fair to conclude that the results obtained by the soda-lime process must be wrong because those by Dumas's method were higher. The question was one of very high practical importance in the analysis of manures.

Dr. Voelcker testified to the value of the author's process when mixtures of ammonia salts and nitrates had to be dealt with. He could strongly recommend it. The points to be attended to in the soda-lime process were plenty of soda-lime and complete combustion without too high a temperature.

Mr. Ruffle said that he had carefully attended to the precautions just mentioned in the combustions he had made with soda-lime.

The President then said that Dr. Carnelly had been good enough to promise to show the experiments concerning the effect of pressure on the fusing-point, of which the Fellows present had doubtless heard. The author did not propose to read a paper, as his results were published elsewhere, but only to show the experiments.

Dr. Carnelly then explained the device which he had adopted in order to secure and maintain a vacuum in the case of ice. For the success of the experiment the tension must be below 5 millimetres. The apparatus consisted of a wide glass tube ¾ inch in diameter, and about 5 to 6 feet high. This is placed in a vertical position, and is connected at its upper end with a strong glass flask placed horizontally, and surrounded with a freezing-mixture. The apparatus having been inverted and filled with mercury, the lower end of the tube is closed with the thumb, and placed under the surface of a layer of mercury about 10 inches deep. On withdrawing the thumb the mercury sinks in the tube to the barometric height, and a large Torricellian vacuum is obtained, which is surrounded, as far as the flask is concerned, with a freezing-mixture. A small quantity of boiled water is now introduced, which rises to the top of the mercurial column, and surrounds the bulb of a thermometer suspended inside the tube. The water is then frozen, and the depth of the layer of mercury in which the tube stands reduced to about 3 ins.; in consequence the mercury in the tube sinks, and leaves a detached column of ice with the thermometer bulb in its centre. This column acts as a cork, shutting off the large vacuous space above from the small vacuum below. By carefully heating the tube the ice is melted round the circumference of the plug, and a fine annular opening is made between the ice and the inside of the glass tube. This restores the communication between the upper and lower portion of the vacuum. As soon as this is effected, any aqueous vapour which is formed is at once condensed by the freezing-mixture, and the vacuum is kept intact. Under these circumstances the author has made the ice so hot that the thermometer in the centre of the cylinder stood at 180° C. before the ice melted. In the experiment shown to the Society the thermometer only rose to 30° C. when the cylinder (which was too large and therefore too heavy) dropped off the thermometer. To prove that the ice was really hot the author has contrived and carried out some experiments, in which the cylinder of hot ice was dropped into a small calorimeter filled with water; the temperature rose when the ice was introduced, whereas if ordinary ice it would of course have been lowered. The author then showed two experiments with camphor and mercuric chloride, which were perfectly successful. The camphor was contained in a glass tube closed at one end and connected at the other with a Sprengel pump. On heating the tube the camphor melted, but on starting the Sprengel pump the camphor, as the pressure decreased, solidified, though the heating was continuous. The mercuric chloride was similarly raised many degrees above its ordinary melting-point, when kept under diminished pressure, without liquefying; but on allowing the atmospheric pressure to enter, by cutting the tube, the solid mass immediately melted and began to boil.

Prof. McLeod suggested that the cylinder of ice might be used as its own calorimeter on admitting the atmospheric pressure. If the ice were at 180° C. it should liquefy into water at about 100°.

Prof. Ayrton pointed ont that although these results seemed at first sight at variance with the experiments of Sir William Thomson and the well-known calculations of James Thomson, according to which an increase of pressure lowers the melting-point, there was really no direct opposition between them, because the constants on which Carnot and James Thomson based their calculations were only constants at temperatures at and below 32°, and we had no evidence until these experiments of Dr. Carnelly as to the value of these constants at higher temperatures. Under ordinary conditions an increase of pressure produces a slight diminution of the melting-point, yet it was perfectly in accord with the principles of thermodynamics to suppose that at temperatures above 32° a slight diminution of pressure might enormously raise the melting-point.

Mr. T. Brown said that he did not quite understand how removing the pressure from the outside of the ice could affect the ice in the interior, and it was difficult to see why the ice in the interior should not liquefy if raised to 180°, even if the diminution of pressure prevented the liquefaction of the surface layer.

A vote of thanks to Dr. Carnelly from a crowded meeting was then passed by acclamation.

Dr. Armstrong then read a joint paper "*On Some Naphthalene Derivatives*," by Mr. N. C. Graham and himself. It was a continuation of the authors' former paper, and dealt especially with experiments they have made to establish the law of substitution for β-naphthol. In their previous communication they have shown that Merz and Ebert are correct in regarding the naphtholsulphonate produced by fusing naphthalene-β-disulphonate with potassium hydroxide as identical with that obtained from β-naphthol, and have pointed out that in this acid the radicles are probably in the relative positions 2 : 3. The action of bromine on the potassium salt of this acid is now described. The product of the action of a single molecular proportion is a highly characteristic bromo-β-naphthol-sulphonate, $C_{10}H_5Br(OH)(SO_3K)$, which crystallises in colourless plates, sparingly soluble in cold water. If two molecular proportions of bromine are employed α-dibromo-sulphonate, $C_{10}H_4Br_2(OH)(SO_3K)$, is formed, which is also colourless, but more soluble and less characteristic in appearance than the mono-bromo-salt. By the action of three or four molecular proportions of bromine, however, coloured salts containing less bromine than those resulting from the employment of a less amount of the halogen are produced. The product of the action of $3Br_2$ is probably a mixture of the products of the action of $2\,Br_2$ and $4Br_2$. The latter crystallises in lustrous deep orange plates, moderately soluble in water, forming a deep red-coloured solution. Potassium and bromine determinations agree with the formula—

$$C_6H_4(O)_2(OBr)(SO_3K),$$

which is that of a potassium bromoxyl-α-naphtha-quinone-sulphonate. Bromine apparently has no action on this salt. Having obtained a quantity of what they presumed to be the dihydroxy-naphthalene of Ebert and Merz, the authors converted it into a sulpho-acid by treatment with twice its weight of bromine. The product, however, was entirely different from that recently described by Griess, being incapable of forming colours with diazo-compounds. On treatment with bromine, nitric acid, &c., it yields a number of highly characteristic bromo-nitro- and bromo-derivatives. The authors were thus led to doubt whether Ebert and Merz are correct in stating that only one of the two naphthalene-disulphonates is convertible into a dihydroxy-naphthalene.

The Society then adjourned to January 20, when a paper "On Some Hydrocarbons Present in Resin Spirit," by H. E. Armstrong, will be read.

PHILOSOPHICAL SOCIETY OF GLASGOW.

Chemical Section.

November 16th, 1880.

Mr. J. Mactear, F.C.S., F.I.C., President, in the Chair.

Fifteen gentlemen were elected, and four were proposed for the first time, as Associates of the Section.

The Secretary opened a discussion "On the Action of Sulphuric Acid upon Lead" by reading the following paper communicated by Mr. James Napier, jun., "*On the Action of Concentrated Sulphuric Acid on Lead at Ordinary Temperatures.*" About a year ago our firm (James Napier and Co.) shipped a number of lead cases containing concentrated sulphuric acid. These cases were enclosed in wooden boxes bound with hoop-iron—a form of package for exporting acid well known in the trade, and which has been in use for many years. The vessel with these cases on board had been to sea for more than a month when, through stress of weather, it was put back to Greenock for repairs, and we were informed shortly afterwards that the cases of acid we had put on board were being unshipped, and were in a most deplorable condition. On inspection we found that nearly all the lead cases, originally cubical in shape, were now swollen, from internal pressure, almost to a spherical form, and all the wooden boxes were broken and burst asunder. We requested Mr. R. R. Tatlock to investigate the matter for us, and for that purpose he took samples of the gas, the acid, and the lead for analyses, and we also furnished him with cuttings from two lead-pans which had been in use at our works for several months concentrating acid to 146° Twaddell. The following extracts are from Mr. Tatlock's report:—

"The cases were made of sheet-lead of about 7 lbs. to the square foot. They contained each about 2 cwts. of concentrated sulphuric acid, were hermetically sealed by the fusion together of the edges of the lead, and were packed in boxes made of wood ⅞ of an inch thick. It was at once apparent that the lead cases had expanded and bulged to an extraordinary extent, so much so that some of them were approximately spherical, and had forced asunder the wooden boxes in which they were contained. The effect was manifestly due to internal pressure, and on puncturing one of them with a pocket-knife a jet of gas rushed out with great force, and continued for about one minute. A sample of this gas was collected for analysis (sample No. 1); a sample of the acid also taken from the same case (sample No. 2); a sample of lead was also cut off from an empty case, which was said to have exploded on the application of a flame (sample No. 3); and also a sample of lead which seemed to have given way spontaneously (sample No. 4). On the same day I received from Messrs. James Napier and Co. two samples of lead labelled as follows:—'Cut from a pan which has been in constant ush for more than six months, considered a good pan' (sample No. 5); and, 'Cut from a pan still in use, and which has been going about six months: Lang's lead' (sample No. 6). I have made an analysis of each of these samples excepting No. 4, which it was considered quite unnecessary to analyse, with the following results:—

Sample No 1.—Gas from Bulged Case.

This was found on analysis to consist exclusively of pure hydrogen gas.

Sample No 2.—Sulphuric Acid.

Pure sulphuric acid	99·78
Sulphurous acid	0·02
Sulphate of lead	0·13
Sulphate of lime	0·07
	100·00
Specific gravity	1·842
Equal to degs. of Twaddell's hydrometer	168½

Samples Nos. 3, 5, and 6.—Sheet-lead from exploded case, and two samples of lead from Messrs. James Napier and Co.

	No. 3. Per cent.	No. 5. Per cent.	No. 6. Per cent.
Lead	99·9595	99·962	99·503
Copper	0·0370	0·037	0·077
Zinc	absent	absent	absent
Iron	0·0020	0·001	absent
Antimony	absent	absent	0·420
Tin	absent	absent	absent
Silver	0·0035	not estimtd.	not estimtd.
	100·0000	100·000	100·000

In addition to these analyses the following experiment was made.

Measured portions, having equal surfaces, were cut from the three samples of lead, of which the above are analyses, and immersed in good concentrated sulphuric acid. Chemical action immediately took place in the case of every sample, with effervescence, due to disengagement of hydrogen gas. The action soon ceased, however, in the case of Napier's pan lead, whereas in the case of the lead from the exploded case, it was still going on after 48 hours. The extent of the chemical action can be best judged by noting the bulk of the gas given off from a square foot of each of the samples of lead as follows:—

Cubic inches of hydrogen evolved from a square foot of lead surface by immersion in pure sulphuric acid in 48 hours:—

	No. 3.	No. 5.	No 6.
Hydrogen in cubic inches	41	16	⅓

From all these results the following conclusions may fairly be drawn:—1. That the sulphuric acid was good concentrated vitriol of first-rate quality, and free from any objectionable or exceptional property. 2. That the bulging of the cases was due to the evolution of hydrogen gas caused by the action of the sulphuric acid on the lead of the cases which contained it. 3. That any concentrated acid would have produced the same result with the same lead. 4. That the chemical action within the cases was produced by the extreme purity of the lead. This is quite apparent, from the fact that the impure lead was practically unacted upon, and although one of the samples received from Jas. Napier and Co. is a lead of great purity, and has been in use for chemical purposes for six months, I am clearly of opinion that that fact does not affect the correctness of this conclusion, as sulphuric acid is very far from being without action upon it, and I do not consider it a good lead for chemical purposes. The proportion of hydrogen given off in the 48 hours is the best test for the suitability of the lead for chemical purposes. The fact that concentrated sulphuric acid of any kind could act upon very pure lead producing hydrogen in quantity, at ordinary temperatures, has hitherto been unknown to me, and, so far as I am aware, to manufacturers of sulphuric acid generally, most of whom would at present, I believe, be surprised to know that such was the case. In future there will be no difficulty in distinguishing lead that is suitable for chemical purposes from that which is not, even without the necessity of making chemical analyses; but hitherto this has not, so far as I am aware, been considered necessary. — (Signed) Robert R. Tatlock, F.R.S.E., F.I.C., F.C.S."

On immersing pieces of lead in cold concentrated acid, the writer observed that the small bubbles of gas made their appearance first in lines in the direction in which the sheet had been rolled. It occurred to him then to try if thin sheets which had been passed repeatedly through the rollers were more readily acted upon than thick ones. Mr. James Stoddart, of the Clyde Lead Works, kindly supplied samples of various thicknesses, rolled from the same block of lead. The results were very anomalous, but showed no marked difference between the thick and thin sheet. Very wide differences, however, did occur between pieces cut from different portions of the same sheet, as will be seen from the following results:—The method employed was, simply placing several square inches of the lead in a small bottle fitted with a rubber stopper and bent tube, filling the bottle up to the neck with concentrated acid, and collecting the gas generated. The sheets were made from an English pig lead desilverised by the Pattison process. It was not analysed, but was evidently a soft pure lead somewhat similar to No. 5 in Mr. Tatlock's report.

Sheet Rolled to about 3 lbs. to the Square Foot.

Conditions.	No. of Hours in Acid.	Cub. in. Gas per Sq. Foot.	Remarks.
Dull surface as received.. ..	10	8·09	In every case the gas was nearly all given off during the first hour or so.
Dull	20	8·47	
„	12	9·52	
Scraped clear ..	40	6·66	
„ „ ..	2	12·80	Had been acted on by acid and re-scraped.
Average		9·10	

Sheet Rolled to about 4 lbs. to the Square Foot.

Conditions.	No. of Hours in Acid.	Cub. in. Gas per Sq. Foot.	Remarks.
Dull	20	16·19	Rapid and energetic action.
„	2½	5·23	
„	Very little action.		
„	12	12·58	
„	12	11·42	
„	12	9·14	
Scraped	1½	4·75	
„	3	4·75	
„	45	4·28	
Average		8·54	

Sheet Rolled to about 5 lbs. per Square Foot.

Conditions.	No. of Hours in Acid.	Cub. in. Gas per Sq. Foot.	Remarks.
Dull	3	9·71	
„	12	10·85	
„	6	10·28	
Scraped	—	15·95	
Average		11·49	

Sheet Rolled to about 6½ lbs. per Square Foot.

Conditions.	No. of Hours in Acid.	Cub. in. Gas per Sq. Foot.	Remarks.
Dull	18	13·71	Brisk action.
„	18	3·41	Very little action.
„	18	11·42	In all cases the action was practically over in an hour or so.
Scraped	18	10·85	
Average		9·84	

Sheet Rolled to about 8 lbs. per Square Foot.

Conditions.	No. of Hours in Acid.	Cub. in. Gas per Sq. Foot.	Remarks.
Dull	24	11·42	
Scraped	24	4·75	
Average		8·08	

We offer no explanation of the cause of this irregularity in the above results, but the facts agree with our experience of the behaviour of some of our lead concentrating pans. We have frequently observed that certain pans did not wear equally, but gave way suddenly in local patches or spots. After this Mr. Stoddart kindly consented to prepare for us samples of the same brand of lead with small quantities of antimony, copper, zinc, &c., added. These were experimented on in precisely the same way, with the following results:—

Sample No. 1 consisted of 99·25 of the above brand of lead and 0·75 of antimony.

Four pieces, each 5 inches square, were separately immersed in the strong acid, and at the end of 24 hours neither of them had given off as much gas as could be conveniently measured. A very slight action was observed for a minute or two after immersion, but it soon ceased. The gas given off from all four was collected together and measured; it was equal to ¼ of a cubic inch per square foot.

Sample No. 2 consisted of 99·25 same lead and 0·75 antimony.

Four pieces, each 5 in. square, were treated separately same as last. A very slight action was perceived at the moment of immersion, but after 5 hours in the acid not a single bubble of gas had come over from any of them, and all action had ceased.

Sample No. 3 consisted of 98·88 same lead, 0·75 antimony, and 0·37 copper.

Four pieces treated as before gave a slight reaction at first, and about one small bubble of gas passed over from each, in all equal to about one-tenth of a cub. in. per foot. After some hours' submersion these four pieces were cleaned and a clear bright surface scraped upon them, and again immersed in fresh acid. As before, a slight action was observed, lasting only for about one minute, and about the same quantity of gas was given off as before.

Sample No. 4 consisted of 99·63 same lead and 0·37 copper.

Four pieces treated as before gave, in the case of two of them, a very considerable reaction, and very little with the other two; probably the alloy was not equal throughout. The total gas given off equalled 1·42 cub. in. per square foot. The action lasted only a short time.

Sample No. 5 consisted of 99·63 same lead and 0·37 zinc.

Four pieces as before all gave a pretty equal action which did not last long; the gas given off equalled fully 2 cub. ins. per square foot. One of these pieces was scraped and again immersed in strong acid; a much brisker action took place on the clean surface, and gas equal to 3·4 cub. ins. per square foot was given off. A sample of a hard Bedminster lead was treated in the usual way and gave equal to 1·42 cub. ins. per square foot. Samples composed of 97 of the soft lead and 3 of the hard Bedminster, and of 98½ of the soft and 1½ of the hard, gave from ½ to 1 cub. in. gas per square foot. A sample cut from a large sheet composed of 99½ of the soft lead and ½ of the hard, gave a rapid and continuous action with the acid, far exceeding that of the two former samples made on the small scale. We intended to carry these experiments much further, but have not had sufficient opportunity. So far as the above experiments go, though not altogether satisfactory, they distinctly show that a small admixture of antimony, from ½ per cent to ¾ per cent, makes the best lead for chemical purposes.

Mr. W. G. Chrystal (of Messrs. John and James White) exhibited specimens of various leads cut from pans used by the firm in vitriol manufacturing. He said:—The firm to which I belong being, like other makers of vitriol, frequently troubled by the unsatisfactory way the leaden pans used in the concentration of the vitriol, stand, I determined to examine how the different sheets and brands stood the action of pure, strong, concentrated acid in the cold. For this purpose I took 1000 grms. of each beat out into thin plates of uniform size and thickness, so as to present the same surface in each case. These were placed in bottles containing in each 1750 fluid grms. strong pure sulphuric acid for exactly one month:—

	Loss in weight. Per cent.
The first specimens exhibited were cut from a pan which gave way in a very short time, and which was full of holes. Sample from this showed	31·20
Sample taken from another sheet of the same maker as the foregoing, being Glasgow rolled lead, before being used showed	15·60
Sample from another sheet as the above two, by the same maker..	11·60
Sample from a sheet of English rolled lead, before being used	6·40
Sample from another sheet as above	6·40

Various Brands of Lead.

L.L.	"Llanelly," Welsh lead	25·36
L.H.	"Lead Hills," Scotch lead	12·94
G.S.	"Greenside," Cumberland lead ..	6·14
L.C.	"La Cruz," Spanish lead	23·19
B.M.	"Bedminster," Bristol lead	8·22

Mr. Chrystal exhibited several other specimens, some from pans, with large holes and with a peculiar honeycomb appearance, others from chambers showing cracks caused by the expansion and contraction of hard lead.

Mr. C. Hope gave analyses of specimens of lead cut from a vitriol pan which had been in use for some time at the St. Rollox Works. One of the specimens had been attacked, the other was entirely uncorroded. Analyses showed little difference in the composition of the specimens:—

	Corroded.	Uncorroded.
Lead	99·53	99·60
Copper	0·22	0·21
Zinc	0·17	0·13
Tin	absent	absent
	99·92	99·94

Dr. Wallace suggested, with reference to these two specimens, that the lead from which they had been cut had originally contained a larger proportion of zinc, but that the zinc, having crystallised out, as it were, in the lead, had been first attacked, and the greater part dissolved.

Mr. Mactear concurred in this view, and referred to the difficulty of getting a lead of uniform composition.

Mr. Stoddart (Clyde Lead Works) said that he had made experiments in this subject, and had found that a mixture of lead with zinc of fairly uniform composition could be obtained. When the desilverisation by the zinc process was carried out properly, no zinc remained in the lead; the lead was, in fact, nearly chemically pure. He suggested that a little dross accidentally mixing with the metal would afford a weak point at which the action of the sulphuric acid would begin, the oxide of lead being more readily attacked than the metal.

Mr. Ferguson mentioned that he had been connected with the lead trade for 25 years, and that the difficulties lead manufacturers had to contend with seemed to have increased rather than to have diminished within that time. He attributed this in great part to desilverizing by the zinc process, but thought that when the corrosion resulted in the formation of large holes in the lead it was to be attributed to oxide having been rolled out with the metal, as suggested by Mr. Stoddart.

Mr. R. R. Tatlock said it would be interesting to know if the durability of lead depended in any way upon the temperature to which they were exposed. About five years ago he had received for analysis samples of lead which had been exposed to a high temperature, and which had given way in a most extraordinary manner. He found that the lead was exceptionally pure, and, as the fact that pure lead is acted upon by sulphuric acid more readily than lead containing certain impurities was not generally known at that time, the firm on whose behalf he analysed the specimens would hardly credit his statement.

Mr. Mactear concluded the discussion. He reminded the section of a paper read by him last session (printed in Chemical News) which contained the results of experiments made by himself in this subject, and which also gave an account of what had been previously done in the matter. With regard to the difficulty of obtaining uniform alloys, Mr. Chandler Roberts had found that even gold and silver could not be uniformly alloyed. Again, brass had been found not to be a true alloy. He did not believe that zinc could be combined with lead, and if it separated out in the lead it would afford a weak point at which the action of the acid would begin. He said that a distinction must be drawn between lead for the chambers and lead for the pans. The pans were exposed to a varying temperature. With regard to the lead employed for the latter he held it had been proved :—1. That chemically pure lead was unsuitable. 2. That lead containing certain impurities, and especially lead containing zinc, was unsuitable. 3. That the presence of certain metals, especially antimony, seemed to render the lead more durable. 4. That the most suitable lead had yet to be discovered, and that the subject required further investigation.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Note.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 21, November 22, 1880.

Researches on Nitrogen Sulphide.—E. Demarçay.—This substance acts upon the chlorides of titanium, tin, phosphorus, arsenic, &c., producing one or more derivatives. The author has obtained SNCl in splendid crystals by mixing nitrogen sulphide with two or three volumes of chloroform, and passing chlorine gas through the liquid.

Phytolaccic Acid.—A. Terreil.—This acid exists naturally as a potassium salt in the fruits of *Phytolacca kæmpferi* and *Ph. decandra.* The free acid is amorphous and may be evaporated to dryness without decomposition. It neither precipitates silver nitrate, nor barium chloride, nor the salts of lime.

Determination of the Fatal Dose of Carbon Oxide for Various Animals.—M. Gréhant.—Air containing 1-300th of its volume of carbon oxide proved fatal to a dog when inhaled for fifty minutes. With another dog of the same size the fatal dose was 1-250th. A rabbit resisted various proportions up to 1-60th. A sparrow perished with 1-500th.

Observations on the Production of Pellagra, attributed to the Dietetic Use of Maize.—M. Fua.—The author shows from comparative instances that sound maize does not produce pellagra.

Zeitschrift für Analytische Chemie. Part 3, 1880.

Determination of Free Oxygen Dissolved in Water.—J. König and C. Krauch.—Already inserted.

Contribution to the Characteristics of the Alkaline Earths and of Zinc Oxide.—Dr. G. Brügelmann.—The alkaline earths and zinc oxide if their hydrates, carbonates, and nitrates are heated to complete decomposition, are obtained in the following forms and of the following specific gravities. Lime is obtained amorphous from the hydrate and carbonate, but in regular cubic crystals from the nitrate ; in either case of the sp. gr. 3·25. Strontia is obtained from the hydrate and carbonate amorphous, and of sp. gr. 4·5, but from the nitrate in regular crystals and of sp. gr. 4·75. Baryta is obtained from the hydrate in optically one- or two-axial crystals, of sp. gr. 5·32 ; but from the nitrate in regularly cubic crystals of sp. gr. 5·72. Magnesia is always obtained in the amorphous state, and of sp. gr. 3·42. Zinc oxide is obtained amorphous from the hydrate and carbonate of sp. gr. 5·47, but from the nitrate in hexagonal pyramids of sp. gr. 5·78.

Contribution to a Knowledge of Saponification of Fats.—Dr. von der Becke.—The name fat is generally applied to a mixture of the tri-glycerides of palmitic, stearic, and oleic acids. As regards the animal fats this assumption has been in all cases verified, but the vegetable fats display certain not unimportant deviations. J. König, J. Kiesow, and B. Aronheim, in saponifying vegetable fats, obtained invariably less glycerine than is required for forming the glycerine-ethers of the fatty acids—a fact pointing to the conclusion that free fatty acids must be present, since the quantity of cholesterine occurring in the plants is too small to combine with the fatty acids. For saponification potassium and sodium hydrate were used along with the other basic oxides, the latter substances being considered equal in value to the former, the only difference being that the products in the one case are termed "soaps," and in the other "plasters." It was assumed hitherto that the tri-glycerides, like other ethers, were completely decomposed by the above-named ethers into salts of the fatty acids and glycerine, and that equal quantities of glycerine were obtained in all cases. For the saponification of fats and the separation of the products, J. König had proposed a process which consists essentially in treating the fat operated upon with an excess of lead oxide in presence of water at 90° to 100°. The author when attempting at his request to saponify cacao-butter in this manner—in order to discover a process detecting the sophistications of this product—found that it could not be saponified with lead oxide, at least not in this manner. It was found on further experimentation that the quantity of glycerine obtained on saponification with potassium hydrate was in all cases considerably the highest. In the easily saponifiable fats, butter, lard, and olive oil, the difference was found less manifest, but it was much more distinct in those which are hard to saponify. Cacao-butter and tallow, if saponified with lead oxide, yield scarcely traces of glycerine. A mixture of an easily saponifiable fat like butter with cacao-butter gave the same quantity of glycerine as if butter alone were employed. It is possible that the reaction when once set up may extend itself. Hence it appears that in the case of some fats the method of saponification with oxide is not trustworthy, and that when the accurate determination of the proportion of glycerine in a fat is required, the saponification must be effected with potassium hydrate.

The Determination of Glycerine in Commercial Samples.—Dr. W. Lenz.

Volumetric Determination of Lead.—W. Diehl.—Already inserted.

Further Communications on the Reversion of Ferriferous and Aluminiferous Superphosphates.—Dr. C. F. Meyer.—Already inserted.

Apparatus for Preventing Liquids from Evaporating too far, and Safety-Contrivance for Water-Baths and Drying Apparatus.—Max Süss.—This apparatus cannot be intelligibly described without the accompanying woodcut.

Determination of Sulphur in Iron Pyrites.—Bernhard Deutecom.—On oxidising pyrites with nitric acid and precipitating the sulphuric acid from the ferriferous solution, slightly acidified with hydrochloric acid, the author always obtained a barium sulphate contaminated with iron, and still the results were too low. He therefore adopted the following process :—1 grm. pyrites was mixed in a large covered crucible with 8 grms. of a mixture of equal parts potassium chlorate, sodium carbonate, and sodium chloride. The crucible is heated at first gently so as to dry the contents, which are afterwards melted at a high temperature. The mass when cold is treated with boiling water, and the solution together with the deposit is introduced into a measuring-flask of 200 c.c.

filled up, filtered, and the sulphuric acid is determined in aliquot parts, say 50 c.c. The insoluble residue does not retain any sulphuric acid. In this manner the use of nitric acid is evaded. The decomposition of the potassium chlorate is complete.

The Electrolytic Determination of Nickel and Cobalt.—H. Fresenius and F. Bergmann.—Already inserted.

Electrolytic Determination of Silver.—H. Fresenius and F. Bergmann.—Already inserted.

Photometric Apparatus.—M. Gouy.—This memoir cannot be reproduced without the accompanying woodcut. In using Bunsen's photometer A. Töpler employs the following arrangement in place of the "grease spot":—Between two pieces of parchment paper, as thin as possible and equally translucent, is laid a moderately thick piece of common paper with a circular hole in its middle, and the three papers are laid between two colourless plates of glass. The translucent spot allows of as exact adjustment as the grease spot; but the latter, if viewed under a certain angle and caused to disappear between two sources of light, becomes visible again if viewed under another angle, a defect from which Töpler's arrangement is said to be free. According to J. M. Eder the separation of mercurous chloride which takes place under the influence of light in mixed solutions of mercuric chloride and ammonium oxalate is chiefly due to the ultraviolet rays of light, and is so regular that the quantity of the mercurous chloride deposited may serve as a measure for the intensity of these rays.

Rapid Method of Filtration.—J. de Mollins.—This apparatus cannot be described without the accompanying illustration.

Use of Glass-Wool in Filtration.—F. Stolba and R. Böttger.—Both these authors point out that glass-wool is attacked by various liquids, including hot water.

Detection of Alcohol in Ethereal Oils.—A. Drechsler.—The author employs as reagent a solution of 1 part potassium bichromate in 10 parts nitric acid of sp. gr. 1·30. Alcohol, if present, is at once betrayed by the pungent odour of ethyl nitrite.

Zinc Chloride as a Reagent for Alkaloids, Glycosides, &c.—A. Jorissen.—The author finds that the following bodies produce characteristic reactions with pure zinc chloride:—Strychnine, bright rose; thebaine, yellow; narceine, olive-green, delphinine, brownish red, berberine, yellow; veratrine, red; quinine, pale green; digitaline, chestnut-brown, salicine, violet-red; santonine, violet-blue; cubebine, carmine red. In case of strychnine the reaction can be produced with 1 decimilligrm. of the hydrochlorate. Brucine and aconitine, if present, interfere. To obtain the blue colouration characteristic of santonine, the mixture during evaporation must be continually stirred with a glass rod drawn out to a point. Digitaline gives first a green solution, similar to that produced by heating with hydrochloric acid. After evaporation there remains upon the porcelain a chestnut-brown spot which quickly blackens. The salicine reaction can be used for detecting the fraudulent addition of this body to quinine sulphate. Albumenoid substances, if heated for a time with the zinc chloride solution, leave a violet stain upon the porcelain, which may be distinguished by its instability from the colourations mentioned above. As a rule it quickly blackens. The author's method of operating is as follows:—A solution of the alkaloid or its hydrochlorate is evaporated to dryness upon the water-bath, say in the inside of the lid of a porcelain crucible; two or three drops of the test-solution—1 grm. fused zinc chloride in 30 c.c. concentrated hydrochloric acid and 30 c.c. water—are placed upon the residue, and afresh dried up on the water-bath. The colouration begins at the outer edge and spreads inwards as the water is expelled.

Journal de Pharmacie et de Chimie.
July, 1880.

Curare from French Guaina.—G. Planchon.—A botanical paper.

Gases Retained by Occlusion in Aluminium and Magnesium.—M. Dumas.—The substance of this paper has been already noticed. The gas occluded by aluminium is pure hydrogen. In magnesium the hydrogen is accompanied by small proportions of carbonic oxide and carbonic acid.

On Saccharine.—Eug. Peligot.—Saccharine is dextrorotatory; its power is expressed by 93 °5′, that of ordinary sugar being 67°·18′. The rotatory power is the same whether the saccharine is derived from starch, sugar, or levulose.

The Spectroscope Applied to the Chemical and Pharmaceutical Sciences.—M. Coulier.—Continued from the May number.

Insalubrity of American Pork.—J. L. Soubeiran.—Among the parasites observed in American pork are *Tricocephalus dispar*, *Stephanurus dentatus*, *Echinorhynchus gigas*, *Cisticercus cellulosæ*, *Fasciola hepatica*, and *Distomum lanceolatum*. *Trichina spiralis* is not uncommon in American hams. The author then mentions a special contagious disease which in 1878 destroyed 260,000 swine in North Carolina alone. At Chicago, diseased animals are killed and prepared for exportation without scruple.

Detection and Determination of Heavy Mineral Oils, Resin Oils, Fatty Oils, and Resin in Commercial Oils.—A. Remont.—Already abstracted.

Wines Mixed with Grape Sugar.—A. Schmitz.—The non-fermentible part of the grape sugar which is introduced into wines, if administered to dogs by way of subcutaneous injection, produced vomiting and other morbid symptoms. The author thinks that these residues contain a poison similar to that present in potato-oil.

Volumetric Determination of Quinine Sulphate.—G. Kerner.—To obtain a saturated solution of quinine the substance is ground up at the temperature of the operation with a quantity of water insufficient to dissolve it, agitating the mixture frequently and letting it settle for twelve to eighteen hours. The pure solution of ammonia is used of the spec. grav. 0·92. The quinine sulphate under examination is dissolved as follows:—5 grms. are ground up in an agate or porcelain mortar with cold distilled water, so as to make a homogeneous mixture, and the whole is poured into a bottle, using altogether 50 c.c. of water, and is left to itself for twelve to eighteen hours without rise of temperature, but with frequent and brisk shaking. After this the bottle is set in cold water. No account need be taken of the external temperature if the two solutions (quinine and ammonia) are standardised at the same temperature. A tall burette graduated into twentieths of a c.c. renders it possible to recognise 1-100 c.c. The author measures with a pipette 10 c.c. of the saturated solution of quinine sulphate into a test-tube and titrates as follows:—5 c.c. of ammonia are run at once into the quinine, the tube is closed with the finger, and inverted two or three times without shaking. The greater part of the quinine is precipitated and re-dissolved, but the liquid remains turbid. The ammonia is then run in, drop by drop, three-tenths, two-tenths, one-tenth, of a c.c., until after two inversions the liquid remains clear. The end of the reaction is indicated by the perfect limpidity of the mixture. The simultaneous titration of the standard solution of quinine shows the exact value of the ammonia and the surplus of the same solution of ammonia necessary for the titration of the sample indicates the proportion of cinchonidine sulphate present; o. 288 c.c. ammonia at 0·92 correspond to millgrm. crystalline cinchonidine sulphate.

COMPOSITION AND QUALITY OF THE METROPOLITAN WATERS.

November, 1880.

	Total Solid Matter.	Oxygen required by Organic Matter, &c.	Nitrogen as Nitrates, &c.	Ammonia.	Hardness. Clark's Scale. Before Boiling.	Hardness. Clark's Scale. After Boiling.
	Grs.	Grs.	Grs.	Grs	Degrees.	Degrees.
Thames Water Companies.						
Grand Junction	22·42	0·071	0·146	0·001	15·4	2·4
West Middlesex	21·83	0·128	0·146	0·001	15·4	2·4
Southwark and Vauxhall.. ..	21·97	0·088	0·167	0·000	15·4	2·0
Chelsea	21·75	0·044	0·156	0·000	15·4	2·4
Lambeth	23·03	0·031	0·167	0·000	15·4	2·4
Other Companies.						
Kent	32·02	0·000	0·413	0·000	22·4	5·6
New River	22·91	0·004	0·198	0·000	15·4	2·4
East London	22·53	0·077	0·177	0·000	15·4	2·4

(The results are stated in grains per imperial gallon of 70,000 grains.)

Note.—The amount of oxygen required to oxidise the organic matter, nitrites, &c., is determined by a standard solution of permanganate of potash acting for three hours.

The water was found to be clear and nearly colourless in all cases but the following, when it was slightly turbid, namely;—The Grand Junction Water.

C. Meymott Tidy, M.B.

MISCELLANEOUS.

Royal Institution.—Prof. Dewar will give the first of his Christmas lectures (adapted to a juvenile audience) "On Atoms," at the Royal Institution, on Tuesday next, December 28, at three o'clock.

Safety for Cheques: a New Invention.—Every person who has to pay away a cheque is naturally anxious that it should not be tampered with before being presented at his bankers. For this purpose not a few ingenious and complicated processes have been devised, all of which fail to baffle the patient subtlety of the modern forger. There are, *e.g.*, cheques where the part to be written on is tinted with aniline violet, whilst the back bears a design in ultramarine, and the paper itself is impregnated with Prussian blue. It might be thought the application of an acid to such a cheque for the purpose of removing the writing could not fail to decompose the ultramarine, whilst on the other hand, treatment with an alkali must destroy the Prussian blue and alter the tint of the ground. Yet we have seen the writing entirely removed from an experimental cheque drawn upon such paper, whilst the respective tones of the white, the blue, and the violet were pronounced by an experienced colourist to be entirely unchanged. Such being the case we think there is good scope for the simple and happy invention of Mr. A. A. Nesbit, F.C.S. He points out that in order to remove writing-ink solutions must be used which are either slightly acid or either alkaline. He proposes, therefore (Specif. No. 2184, A.D. 1880), to tint cheques with a dye which is affected by both acids and alkalies, but which takes one colour in acids and another in alkalies. He then prints upon the document partly in an acid and partly in an alkaline ink, so that the characters, designs, lines, &c., are apparent only in virtue of their respective alkalinity or acidity. If a forger moistens such a document with a dilute acid with a view to discharge any part of the writing, the colour takes the acid tint, and the words, lines, or other design disappear, and if it is placed in an alkaline solution the whole becomes alkaline, so that the words, &c., are not restored. If an alkaline solution were applied first the inscription would still disappear, and could not be revived by the subsequent application of an acid. Even if a perfectly neutral solvent were found capable of removing writing-ink it would still be impossible to alter such a cheque, as the acid portion of the lettering or design might be made a little stronger than the alkaline part, or *vice versa*, in which case the document, on an immersion in an alkaline solution, would be blurred. So far as we can perceive, this invention bids fair to put a stop to a most dangerous kind of fraud, and we have no doubt that it will be welcomed in financial circles.

MEETINGS FOR THE WEEK.

Tuesday, 28th.—Royal Institution, 3 p.m. "Atoms," Prof. Dewar.
Thursday, 30th.—London Institution, 7.
—— Royal Institution, 3. "Atoms," Prof. Dewar.

TO CORRESPONDENTS.

Addresses Wanted.—A correspondent would like to have the addresses of a few firms who are refiners of cobalt and borax.

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- Chloride of Sulphur,
- Acetone,
- Chloroform,
- Aldehyde,
- Chlorate Baryta,
- Arsenic Acids,
- Fruit Essences for Confectionery & Liqueurs,
- Perchloride of Iron,
- Sulphite and Hyposulphite of Soda,
- Phosphates of Soda and Ammonia,
- Ethers,
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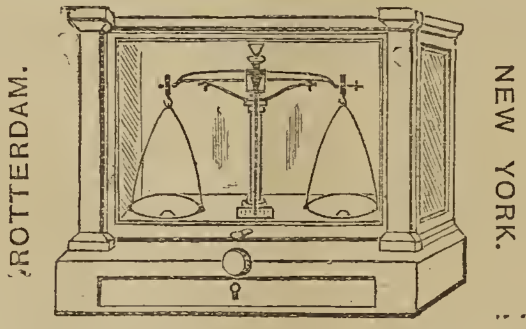

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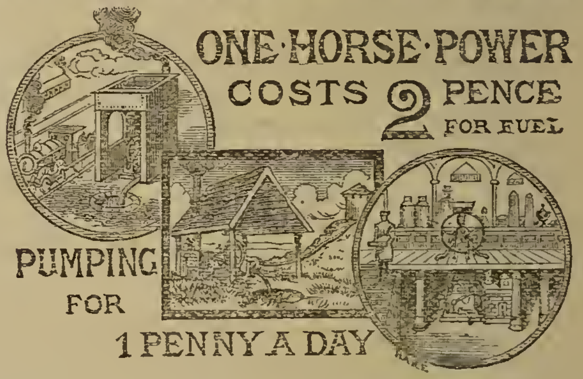

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THE CHEMICAL NEWS.

Vol. XLII. No. 1101.

ON CHICKEN CHOLERA: STUDY OF THE CONDITIONS OF NON-RECIDIVATION AND OF SOME OTHER CHARACTERISTICS OF THIS DISEASE.*

By M. L. PASTEUR.

In the communication which I had the honour of presenting to the Academy in the month of February last, I announced, among other results, that chicken cholera originates in a microscopical parasite; that there is an attenuated virus of this disease, and that one or more inoculations of this attenuated virus may preserve chickens from death when inoculated with the virus of maximum virulence. On account of the striking similarity that these two forms of virus present with the effects of variola and vaccine in man, it becomes interesting to ascertain not only if the immunity from the more aggravated form of virus is absolute, for the regions of the body which have undergone the preventative inoculation, but also if this immunity exists in the system, no matter what portion of the animal may have been inoculated, and what may have been the manner of introducing the virus.†

To explain with brevity the results which I have to communicate, I may be allowed to use the word *vaccinate*, to express the act of inoculating a chicken with the attenuated virus. This being admitted, I may state, as the result of many experiments, that the effects of vaccination are very variable. Some chickens are little affected by the most virulent virus after one inoculation of the attenuated virus; others require two such inoculations, and even three. In every case, the preventive inoculation does some good, because it acts in a certain measure. Vaccination, then, may be of several degrees; but we may always succeed in completely vaccinating a chicken, which means that we can bring it to such a condition that it becomes incapable of being affected by the most virulent virus.

To make this matter clear, I will now give the results of experiments:—I take eighty new chickens (I call *new* those which never suffered before with chicken cholera). On twenty of these, I inoculate the most virulent virus—and they all die. Of the sixty that remain, I take another lot of twenty, and I inoculate them with that quantity of the most attenuated virus which the point of the needle will take up ‡—and not one dies. Are they then vaccinated for the aggravated form of virus? Some are and some are not, for if I afterwards inoculate these twenty chickens with the most virulent virus, six or eight of them will not die, although they may be ill, while in the first case every inoculated chicken died. I take again from the remaining chickens another lot of twenty, and these are vaccinated with the attenuated virus exactly as the preceding lot, and, a week afterwards, they are again vaccinated in the same manner. Are they now safe from the virulent virus? We now inoculate these twenty chickens with this virulent virus, and, instead of there being six or eight which do not die, there are twelve or fifteen. Finally, I take the twenty remaining chickens, and vaccinate them successively three or four times. If now I come to inoculate them with the most virulent virus, not one will die. In this case, chickens are brought to the condition of animals who are incapable of suffering from chicken cholera.

As to the cause of non-recidivation, I find it impossible to resist the idea that the microscopic germ, which causes the disease, finds in the body of the animal conditions suitable to its development, and that to satisfy the necessities of its life, the germ alters certain substances, or destroys them, which comes to the same thing, whether it assimilates them, or whether it consumes them with oxygen borrowed from the blood.

When complete immunity has been reached, the most virulent germ may be inoculated into any of the muscles without producing any effect. This means that the cultivation of the germ has become impossible in these muscles. They no longer contain food for the germ.

It is impossible to convey the impression that one receives from observing such phenomena. Here are twenty chickens which never had this disease. I inoculate them in their pectoral muscles or, still better, in the muscle of the thigh, so as to observe with greater ease the effect of the inoculation. The next day all the chickens are lying down; they are very lame and seem overcome by sleep. The inoculated muscle becomes of enormous size, and is profusely filled with the parasites. From time to time, a chicken dies, and, at the end of forty-eight hours, they are all dead. We may take also twenty chickens, previously vaccinated several times, and inoculate them at the same time as the others, with the same virus, in equal quantities. The next day and the next, they are all alive and in good health; they eat and cackle as usual; the cocks crow; the inoculated muscles present nothing abnormal. There is not even a sign to show where the skin was punctured. This healthy condition remains permanent.

We may now inquire whether the impossibility of cultivating the parasite is not limited to the muscles which have been inoculated. This may be answered by introducing the deadly virus in the blood vessels and in the digestive organs. I have taken ten chickens, never before inoculated, and ten others inoculated several times with the mild virus. I have then injected the worst form of virus in the jugular vein of all these chickens. The first ten have died rapidly; many of them within twenty-four hours. The ten vaccinated chickens, on the contrary, have only been slightly ill from the incision of the skin and of the jugular vein, and were soon in good health. This shows that the blood of these ten chickens was itself *vaccinated*, which means that previous cultivation had deprived it of the materials fit for further developments of the germ.

As to the introduction of the parasite in the digestive organs, I have imitated the epidemics which depopulate poultry yards, by introducing the parasite in the food of the chickens. On the 11th of March I brought together twelve chickens, bought at the market that very morning, and twelve others, previously vaccinated several times. Every day I gave to these twenty-four chickens a meal of the diseased muscles of chickens, who had died from chicken cholera. Through the combs of the twelve chickens which had not been vaccinated I passed a platinum wire, so as to distinguish them from the other twelve. On the next day, the unvaccinated chickens began to sicken and die. On the 26th of March, the experiment terminated. Seven of the chickens that had not been vaccinated have died, and a *post mortem* examination reveals the fact that the disease was introduced in the system, either through the first portion of the alimentary canal, or more frequently, through the bowels, which were highly inflamed, and sometimes ulcerated, in a manner which recalls the lessons of typhoid fever.* The five other un-

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* Translated from the *Comptes Rendus de l'Academie de Sciences*, of April 26th, 1880, page 952, by P. Casamajor. The translation of the first paper of this series appeared in the Chemical News, vol. xli., page 4 (July 2nd, 1880).

† From all I have seen and read of vaccine in man, and from my experiments on chicken cholera, I infer that *vaccine* rarely acts as a complete preventative. There are cases cited of vaccinated persons who have had the variola, and there are even cases of persons who have had it, afterwards, as much as three times.

‡ There are degees of attenuation as well as of virulence. I will give explanations in a future communication.

* The blood is full of parasites, and the interior organs are frequently covered with pus and false membranes, particularly next to the intestinal pockets, through which the germ seems to have penetrated.

vaccinated chickens are more or less ill, one seriously so. As to the twelve vaccinated chickens, not one has died, and to-day [*] they are all alive, and in good health. We may now sum up the results as follows :—

It is the life of a parasite, in the interior of the body, which causes the disease known as chicken cholera and which causes death by this disease. When the cultivation of this parasite cannot take place in the body of a chicken, the disease does not show itself. The chicken is then in the constitutional condition of animals which chicken cholera cannot attack. Animals, in this condition, may be said to be born vaccinated for this disease, because the fœtal evolution has not placed in their bodies the proper food of the parasite, or because substances, which could serve as such food, have disappeared while they were yet young. We must not wonder that there are constitutions more or less apt to receive inoculations of certain kinds of virus, for, as was announced in my first note, the broth of beer-yeast is entirely incapable of supporting the life of the parasite of chicken cholera, while it is well adapted to the cultivation of a multitude of microscopical germs, notably of the bacteridia of carbuncular disease.

The explanation to which we are led by the facts already mentioned, of the different degrees of constitutional resistance of some animals, as well as of the immunity which chickens acquire by preventive inoculations, must seem a natural one, if we take into consideration that every cultivation modifies the medium in which it takes place. In the case of ordinary plants, the soil is modified; in the case of parasites, the animals and plants on which they live are also modified. The same thing happens with the liquids in which they live, in the case of ferments and other microscopical germs. The modifications which take place have this character in common, that new cultivations of the same species in these media soon become difficult or impossible. If chicken-broth is used for cultivating the germ of *chicken cholera*, and if, after three or four days, the liquid is filtered, to separate all the germs, and furthermore, if after this fresh quantities of the germs are placed in the filtered liquid, it will be found incapable of producing the feeblest development. Perfectly limpid at first, the liquid remains indefinitely limpid.

We are led to believe that the cultivation of the attenuated virus in a chicken places its body in the same state as that of the liquid which can no longer sustain the life of the germ of disease. We may extend the comparison still further, for, if we filter the broth on the second day of the cultivation, instead of on the fourth, the filtered liquid will still permit the cultivation of the germ, but less readily than at first. This may enable us to understand that the cultivation of the attenuated germ in the body of a chicken may not have removed all the food for the germ. The remainder may allow a fresh cultivation of a feebler kind. This is the same as a first *vaccination*. Subsequent inoculations will remove progressively all the materials for the cultivation of the parasite. Through the action of the circulation, a time will come when any new cultivation on the animal will remain unproductive. Then the disease cannot recidivate, and the subject becomes perfectly vaccinated.

It may seem astonishing that the first cultivation could have stopped before all the food of the germ has been destroyed; but we must not forget that the germ is aerobian,[†] and that, in the body of an animal, it does not find the same conditions as in an artificial medium of cultivation, in which there are no obstacles to its propagation. In the body, on the contrary, it finds opposition from the cells of the organs, which are also aerobian, and are continually absorbing oxygen.

We might also account for the fact of non-recidivation by admitting that the life of the germ, instead of destroying certain substances in the body of an animal, on the contrary, adds other substances which act as an obstacle to its further development. The history of the life of these inferior beings, of all beings in fact, authorises this supposition. The excretions due to vital functions often prevent vital functions of the same nature. In some fermentations, antiseptic products are formed while fermentation is going on, and even by the action of ferments, and these products put an end to further action, even if there are still substances left capable of undergoing fermentation. In the cultivation of our germ, there might, in the same way, be substances formed whose presence might explain non-recidivation and vaccination.

Our artificial cultivation of the parasite will enable us to examine this hypothesis. If we prepare an artificial cultivation of the germ of chicken cholera, we may evaporate the liquid *in vacuo* while cold, then bring it back to its original volume by the addition of chicken broth. If the extract contains a poison which destroys the germ, and if the presence of this poison is the cause of its non-development, the cultivation of the germ cannot take place in this liquid. On the contrary, the development does take place without difficulty. We cannot then believe that, during the life of the parasite, there are substances produced which prevent its further development. This is a corroboration of the opinion which we have expressed on the cause of non-recidivation in certain virulent diseases.

ON THE
MANUFACTURE OF SULPHATE OF SODA BY THE DIRECT PROCESS.[*]

By JOHN HARGREAVES.

In compliance with the invitation of our Secretary, I have the pleasure of placing before you the following paper on the Manufacture of Sulphate of Soda by the Hargreaves and Robinson direct process.

This process was first brought before the public about ten years ago, and since that time has slowly but surely made its way, its superiority over the old process being now clearly established in every point.

As the chemistry of the process is so well known, I will confine myself exclusively to its technicalities.

The first working apparatus erected consisted of four brick chambers of 6-ft. cube. With this arrangement difficulties arose in consequence of leakage of the gases from one chamber to another without passing through the columns of salt, from the chambers into the flues, and *vice versa*. These, together with the leakage of air, necessitated a very large consumption of fuel. To overcome these evils a second experimental apparatus—consisting of four cast-iron cylinders, each 10-ft. diameter by 10-ft. high—was erected in a double line. This practically got rid of the leakage. But the small amount of material and the shortness of the column, together with a relatively large amount of radiating surface, showed the advisability of increasing the size of the apparatus. With this object two other cylinders of the same dimensions were added to the series. This addition, although giving improved results, still fell short of the object in view, and the cylinders were afterwards pulled down.

The next apparatus consisted of eight cylinders of 12-ft. diameter, by 12-ft. high, which were erected in a single line: each cylinder having two fire-places and two discharging doors placed on opposite sides. The sulphurous acid pipe was placed on one side, and the circulating pipe connecting each of the end cylinders on the other side of the line of cylinders.

This arrangement gave a long column of salt, but exposed too much cooling surface, and consumed more

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[*] April 26th.

[†] Pasteur divides germs and other microscopic organisms into *aerobians* (requiring air to live) and *anaerobiæ* (which do not require air).—*Translator.*

[*] A Paper read before the Members of the Faraday Club, in Liverpool, December 15th, 1880.

fuel than was desirable. But this was afterwards greatly reduced by erecting another line of eight cylinders so arranged as to form a double row with one fire-place and one discharging door to each cylinder. This reduced the fuel to about one-eighth of that previously required per ton of sulphate, for, while the production of the plant was doubled, the total fuel consumption was reduced to about one-fourth. This most remarkable result was another confirmation of the fact that, the larger the quantity of material operated upon, the smaller the proportion of fuel required.

Four sets of plant, having a total of thirty-eight cylinders of 15-ft. diameter by 12-ft. high, were next erected by several firms. Each set was arranged in two rows with an archway betwixt, having two fires and two drawing doors to each cylinder for the convenience of quick discharging and re-heating. Twenty cylinders of the same size, but arranged back to back, and with only one fire-place and drawing-place to each cylinder, were afterwards erected by the Atlas Chemical Company. The working of these cylinders proved that it was quite practicable to discharge the contents of each through one drawing-door. All the foregoing experience pointed in the direction of still further increasing the dimensions of the cylinders, and arranging the apparatus compactly, so as not to expose an excessive amount of radiating surface. Convinced of this fact, the same firm decided to erect another set of cylinders of larger dimensions than any yet attempted—viz., twelve cylinders of 18-ft. diameter by 12-ft. high, each containing 55 to 60 tons of sulphate. Several important improvements were introduced into this apparatus. In the previously erected plants the gas, after descending through the mass of partly-converted sulphate, was conducted from the bottom of each cylinder to the top of the next by means of an external pipe connected to the lower part of the cylinder. In this apparatus these external pipes are replaced by D-shaped pipes arranged in the interior of the cylinders, by which leakage of air into the upcast pipe is entirely prevented, and, besides, has the advantage of placing all the connecting joints at the top of the cylinders where they can be got at. The brickwork is also simplified, and now consists of a 9-in. ring round each cylinder, with an outer wall enclosing the whole series, the remaining spaces other than the fire-places and flues being filled up with concrete to prevent loss of heat. The hydrochloric acid is taken from the top of the circulating pipe through a movable wrought-iron syphon to a pipe fixed above the two lines of cylinders. This has considerable advantages over the earlier system of taking the exit gases through the drawing-doors, and thence by an underground flue to the exhausters, us it effects considerable economy in the amount of iron used in construction, and decreases the number of joints.

This plant also contains the recently-improved firing arrangements, by which the heat from the fire-places is more evenly distributed, and local heating of the cylinders rendered almost impossible, no matter how carelessly the workman may fire.

Respecting the question of fuel required to maintain the temperature of the cylinders, I may here note that one of the best conducted plants is now fired almost exclusively, not with coal, but *cinders obtained by riddling the ashes from other furnaces in the works*, and of this inferior fuel less than 2½ cwts. is required per ton of sulphate made, and consequently the cost of repairs to fire-places is almost *nil*.

The salt was originally prepared by moistening with water, and forming into lumps by drying on floors heated from beneath, and afterwards broken into small pieces by a mill. This preparation is now performed entirely by machinery. The moistening is effected by the waste steam from the engine supplying the motive power. This steam passes into a box in which the salt is agitated by revolving arms. It then passes on to an endless band formed of a series of iron plates, where it is stamped and cut into suitable pieces. It then enters the drying oven, where, after being brought into direct contact with the products of combustion, it arrives at the other end in a suitable condition for putting into the cylinders. It is important that the prepared salt be sufficiently porous to allow the diffusion of the gaseous reagents, and also free from dust, so as not to close the passages through which the gases circulate in traversing the cylinders.

The kind of salt varies with the purpose for which the sulphate is required. Ground rock, surface evaporated salt, sweepings from salt works, and other cheap sources of salt are largely used for the process where sulphate is made for alkali manufacture.

When it is required to use the sulphate without refining for the manufacture of plate glass, it is necessary to use salt containing only traces of iron.

The salt prepared by Mr. Otto Pohl's method works exceedingly well in the direct process. His system consists in evaporating the brine by bringing the products of combustion into direct contact with it in one vessel, while above there is a pan in which another quantity of brine is heated from below. It is found much more economical than the ordinary system of salt manufacture both as to fuel, labour, and working space, and, hence, affords a supply of salt at a much lower cost. I have here samples of this salt kindly furnished by Mr. Pohl.

Any carbonaceous matter contained in salt used for the direct process is quickly burnt out in the cylinders, leaving a perfectly white sulphate.

When a cylinder is filled with salt it is made the exit of the series, and quickly becomes elevated to a working temperature (which may be estimated at 400° C.) by the hot gas coming from the previous cylinders, and also by the action of the small fire attached. As soon as the formation of the sulphate commences, the temperature rises rapidly, rendering the further application of external heat unnecessary until—in consequence of the small amount of chloride remaining—the heat, generated by chemical action, diminishes.

As the salt becomes converted into sulphate it is brought nearer and nearer to the source of sulphurous acid, the proportion of which, passing the cylinder, increases daily until it becomes the first cylinder in the series, when it receives the red-hot gas coming direct from the burners, which quickly decomposes the remaining chloride.

The time occupied in decomposing the contents of a cylinder varies with the kind of salt and the size of the pieces employed, but under ordinary circumstances should not exceed twenty-one days, and has often been done in less than fourteen days.

When the charge is decomposed the cylinder is isolated by closing the dampers connecting it with the sulphurous pipe and the succeeding cylinder, which latter in its turn is made the first in the series. The drawing-door is now opened, and the sulphate discharged; after which the cylinder is re-filled, and again made the last in the series.

The amount of cubic capacity required in the cylinder per ton of sulphate per week is about 170 to 180 cubic feet.

The pyrites burners, with the exception of being better protected against heat losses, are the same as used in the old process.

The draught from the burners is obtained by means of exhausters placed on the passage from the cylinders to the condensers. The waste steam from the engine driving these, after passing through a superheating coil, heated by the waste products of combustion coming from the cylinder fires, is mixed with the sulphurous acid in the burners to supply the necessary water vapour for the decomposition of the chlorides. Besides being an economical source, this has the advantage of automatically regulating the supply of steam to the sulphurous acid, which must vary with the speed of the engine driving the exhausters.

Neither the hydrochloric nor sulphur acids have any appreciable effect upon the iron-work of the apparatus, as the temperature is always in excess of that required to keep then in the gaseous condition.

The condensers used are same as in the the old process, but, the gas comes in a continuous stream, there is no necessity for the frequent variation of the supply of water to the condensers which is required to condense the acid from pots.

The advantages claimed are briefly as follows :—

1st.—Although the cost of apparatus is about the same as that required for the old process, there is this difference: That whereas the life of a vitriol chamber may be estimated at—say ten years, the cylinders used with ordinary care in the direct system show every sign of lasting three times that period without renewal.

2nd.—Less fuel, being about one-half that required by the old process.

3rd.—Less labour, and, with the exception of the burner men, and one intelligent person to look after the plant, no skilled labour is required. Hence, less difficulty in dealing with the workmen.

4th.—Less repairs. In the instance of one apparatus which has been working several years, I am informed that this item is less than 6d. per ton of sulphate made.

5th.—No nitrate of soda necessary.

6th.—Cheaper qualities of salt—not available for the old process—are used.

7th.—Less sulphuric amongst the hydrochloric acid. Consequently less annoyance from sulphate of lime in the Weldon process.

8th.—No leakage of hydrochloric acid into the fire flues, the whole of it passing through the exhausters.

9th.—The sulphate when drawn emits no hydrochloric or sulphuric acid to annoy the workmen.

10th.—Sulphate is made containing only very small quantities of chloride and free acid, and, therefore, especially adapted for the manufacture of 76 per cent or other high strength caustic, for which it it is largely used. The manufacturers, whilst guaranteeing this quality to contain less than 0·5 per cent chloride of sodium, often supply it with less than 0·2, and even as low as 0·5 per cent chloride. I need hardly say that sulphate of such a quality commands a decided preference over that made by the old process, and in practice sells for about 2s. 6d. per ton over the ordinary market rate.

Sulphate made by this process is also used without refining for the manufacture of plate glass, and is superseding that of the old process which has to be refined at an additional cost of 25s. and upwards per ton before it is fit for this purpose.

Lastly, a reduced cost of production, which may be fairly estimated at 15 to 20 per cent.

In conclusion, the process is now rapidly extending. Eight sets of apparatus are now at work, three others are in course of construction, giving an aggregate capacity of production of about sixty-seven thousand tons per annum. Arrangements are also being made for the immediate erection of additional apparatus.

WATER FROM A HOT SPRING, NEW BRITAIN.*

By A. LIVERSIDGE,
Professor of Geology and Mineralogy in the University of Sydney.

The sample of water forming the subject of this note was collected from a hot spring in one of the islands of the New Britain group by the Rev. George Brown, Wesleyan missionary, to whom my thanks are due for the trouble taken in safely bringing it to Sydney.

The water was of a yellow tinge, and smelt of sulphuretted hydrogen : at first it was neutral to test-papers, but afterwards became very faintly acid, probably from the oxidation in part of the sulphuretted hydrogen.

No attempt was made to determine the amount of sulphuretted hydrogen and other gases, inasmuch as the quantity of water was but small, and moreover on account of the time which had elapsed since its collection (some few months), it was thought that the amounts of gases present at the time of examination would afford but little information as to the actual quantities contained by the water when freshly collected.

The residue left on evaporation to dryness at 100° C. amounted to 36,312 parts per 1,000,000, or 2541·84 grains per gallon, which is about the same as average sea-water.

Composition of the Residue, dried at 100° *C.*

	Per cent in residue.	Parts per million of water	or	Grains per gallon.
Silica	0·200	72·6		5·08
Alumina and iron sesquioxide ..	0·440	159·7		11·18
Calcium sulphate..	1·394	506·2		35·43
Calcium chloride ..	2·240	813·4		56·93
Magnesium chloride	4·710	1710·3		119·72
Sodium chloride ..	87·320	31707·6		2219·53
Potassium	traces	—		—
Combined water ..	3·696	1342·2		93·97
	100·000	36312·0		2541 84

WATER FROM A HOT SPRING, FIJI ISLANDS.*

By A. LIVERSIDGE,
Professor of Geology and Mineralogy in the University of Sydney.

When at Kandavu, Fiji Islands, in 1876, I heard of the boiling springs at Savu Savu, but to my great regret my stay of three or four days only at Kandavu was too short to allow me to visit them, and I am indebted to the kindness of Dr. T. D. Bromlow, R.N., for the sample of water forming the subject of this note.

The water was contained in clear glass bottles, well corked and sealed. I mention this because the sample had evidently been collected with great care. On more than one occasion I have received samples of mineral waters which proved to be worthless for chemical investigation simply because insufficient care had been exercised in the collection and bottling. This was often a source of regret to me, since much trouble must have been taken to procure the samples, and still more to get them safely to Sydney. When distant from towns it is, of course, not always possible to obtain glass-stoppered bottles, but when procurable there is nothing so convenient and suitable as the large half-gallon bottles known as Winchester quarts.

The water was clear and colourless, after the deposition of the small amount of matter which it had in suspension, free from smell, but with a strongly marked saline taste ; to test-papers it was neutral, or but very faintly alkaline. On evaporation to dryness the filtered water left a very white, extremely deliquescent, residue, which on ignition fused but did not blacken, thus showing the absence of any appreciable amount of organic matter.

The specific gravity of the water was found to be 1·0064 at 60° F.

The total quantity of solid matter in solution, weighed after drying the residue at 110° C., was found to be 8·320 parts per million, or 582·40 grains per gallon ; but after driving off the combined water at a dull red heat, the residue was reduced to 7813 parts per million, or 546·91 grains per gallon—*i.e.*, it lost 6·09 per cent.

The rarer elements were carefully sought for in this residue by means of the spectroscope, but none were found. The total quantity of water at my disposal was but small—some four pints : perhaps a larger quantity would have enabled one to detect their presence. Neither iodine nor bromine could be found, although carefully sought for.

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* Read before the Royal Society of New South Wales, Sept. 1, 1880.

* Read before the Royal Society of New South Wales, Sept. 1, 1880.

Composition.

	Per cent in residue.	Parts per million of water	or Grains per gallon.
Silica, insoluble ..	1·681	133·3	9·20
Silica, soluble	0·074	5·8	0·40
Alumina and traces of iron sesquioxide ..	0·534	41·7	2·92
Aluminium chloride	1·646	128·6	9·00
Phosphrc. acid (P_2O_5)	traces	—	—
Calcium chloride ..	46·754	3652·9	255·70
Calcium sulphate ..	4·770	372·7	26·09
Magnesium chloride	0·154	12·0	0·84
Sodium chloride ..	42·171	3294·8	230·64
Potassium chloride..	1·756	137·2	9·60
Carbonic acid	traces	—	—
Loss	0·460	34·0	2·52
	100·000	7813·0	546·91

From the above it will be seen that the salts in solution consist of chlorides for the most part, and that the chlorides of calcium and sodium largely preponderate over the others: the amount of calcium chloride is unusually large.

No mention was made by Dr. Bromlow of the temperature of the water.

NOTE ON THE ANALYSIS OF SHODDY AND WOOL WASTE.

By JOHN HUGHES, F.C.S.

For some time past it has been the practice for manure manufacturers to purchase shoddy at so much for each unit of ammonia per cent per ton, the ammonia being calculated from the yield of nitrogen as determined by the usual combustion process with soda-lime.

If the samples are fairly drawn, carefully prepared for analysis, and the analytical operations be skilfully performed, the results obtained should be so satisfactory that there should not be a greater difference than one-tenth to two-tenths per cent in the ammonia determinations of duplicate samples. Unfortunately, however, the difference between the percentage of ammonia found by the seller's chemist and that found by the buyer's chemist frequently amounts to as much as one, or even two, per cent; and, as each unit of ammonia is valued at from 6s. to 7s., a very serious loss is incurred by one of the two parties to the contract.

During the past eight years a great number of samples of shoddy have been submitted to me for analysis, and I am fully convinced that manufacturers have frequently been called upon to pay for what they have never received.

In this note, therefore, I wish to call particular attention to one source of error introduced in the preparation of the sample for analysis, namely, that some chemists do not take the trouble to determine the proportions of water and dry matters (containing the nitrogen) present in the shoddy as received by the analyst. The practice too frequently adopted is to turn out the sample and proceed to cut it up into a fine condition, without any regard to the loss of moisture which goes on during this process to a greater or less extent, according to the temperature of the laboratory.

When the shoddy is dry and contains not more than 16 per cent of water, this loss is inconsiderable and need not be noticed, but in the damp samples with 26 to 30 per cent of water, the loss of moisture during the making of the sample frequently amounts to 10 per cent or *more*, if the temperatute of the room is at all high and the shoddy be much handled during cutting-up.

The consequence is that the percentage of ammonia is given for the partially dried sample rather than for the shoddy as received. From this cause alone the ammonia is stated 1 per cent too high in many cases.

It is the duty of the analyst to report the analytical results for the sample *as received*, and he should take all necessary precautions to correct any possible sources of error, either in the preparation of the sample or in the analysis itself. He must be aware of the great pecuniary importance attached to the analysis of all samples officially sealed in duplicate.

The following is the plan adopted by myself in the preparation of samples for analysis:—

The shoddy is turned out of the bottle and any large lumps pulled to pieces, after which about 40 grms. are weighed out and water determined by drying in a water-bath; a portion of the sample is returned to the bottle in case of reference, while the remainder is cut up very fine for analysis.

The nitrogen determination is made in duplicate, also a water determination that we may ascertain how much moisture has been lost in the preparation of the sample and make the necessary correction in the ammonia results.

Thus, suppose that the original damp shoddy as received contained 26 per cent of water and 74 per cent dry matter, and the prepared sample, as analysed, 16 per cent of water, 84 per cent of dry matter, yielding 9·50 of ammonia; we have only, therefore, to calculate how much ammonia would be yielded by the 74 parts of dry matter present in the original sample, as received, thus:—

Shoddy as Analysed.			Shoddy as Received.		
Dry Matter.		Ammonia.	Dry Matter.		Ammonia.
84	=	9·50	74	=	8·37

It will be seen that there is 1·13 per cent more of ammonia found in the prepared sample analysed than in the original sample as received. Now, at 7s. per unit of ammonia on a delivery of 50 tons, this would amount to £19 15s. 6d., which sum, the purchaser is asked to pay on behalf of ammonia which he has not received, and which consequently will not appear in the manufactured manure.

In the following table the respective analytical statements of two chemists are recorded, in reference to duplicate sealed samples of shoddy:—

	Buyer's Chemist.				Seller's Chemist. Results given on Certificate.	
	Shoddy as Analysed.		Shoddy as Received.			
No.	Water.	Ammonia.	Water.	Ammonia.	Ammonia.	Water.
1.	29·67	7·13	38·58	6·23	8·00	Not stated
2.	15·64	9·13	20·72	8·57	9·69	—
3.	18·30	9·16	24·01	8·53	9·14	—
4.	22·48	8·36	27·52	7·83	9·00	—
5.	15·30	10·00	25·57	8·81	8·59	—
6.	15·02	8·89	22·46	8·12	9·38	—
7.	13·76	9·22	21·14	8·43	9·54	—
8.	18·08	8·59	26·40	7·72	8·65	—
9.	24·08	7·66	30·46	7·02	8·51	—
10.	Very dry	9·75	Very dry	9·75	9·63	—
11.	15·30	8·79	17·76	8·54	10·08	—
12.	Very dry	9·55	Very dry	9·55	9 95	—

It will be understood that the results stated on the certificate of the Buyer's Chemist are those included in the column headed, "Shoddy as received." The percentage of water has been estimated by direct determination, and the proportion of ammonia present in the sample received has been carefully calculated from the nitrogen found in the partially dried "Shoddy as analysed."

The Buyer's Chemist, however, gives no details as to the percentage of water present in the sample; and from the high ammonia results it would appear that the shoddy must have dried considerably during the cutting-up process.

It will be noticed that only in two cases (Nos. 5 and 10) does the Seller's Chemist give a lower percentage of ammonia, and that in all the other ten instances his results are higher, in many of them there being an excess of at least 1 per cent more ammonia.

Not a little singular is it to observe that in Nos. 3, 7, and 8 the results for the sample, *as analysed* by the Buyer's Chemist *do agree* remarkably with the figures given by the Chemist of the Seller. Also, let it be noted how closely the results of both chemists agree in the two samples, Nos. 10 and 12, which are described as being very dry.

Finally, we must observe that with 38·58 per cent of water, No. 1 is represented by one chemist as containing 8·00 of ammonia, and by the other only 6·23 per cent. Now, as the average quality of good shoddy only shows about 9 per cent of ammonia associated with 15 per cent of water, and 85 per cent of dry matter, it is impossible that 8 per cent could be found in shoddy containing 38·58 per cent of water, while, on the other hand, if it did contain 8 per cent ammonia, it follows that there could not have been 38·58 of water in the sample analysed by the Seller's Chemist.

With a view of removing such discrepancies, I beg to make the following suggestions :—

1. That samples of shoddy should be drawn as fairly and impartially as possible ; that the buyer and seller's representative should not be allowed respectively, to select samples, but that official samples should be taken by a lad (blindfolded, for a wool dealer could easily select a superior quality for his sample.
2. That each sample should consist of at least 1 lb., that four of such 1 lb. samples should be drawn, placed in wide-mouthed jars or bottles, and officially sealed ; two samples being given to the buyer and two to the seller.
3. That the chemist be requested to state on the analysis certificate the proportions of *water* and (nitrogen equal to) *ammonia* found in the sealed sample as received.

If these suggestions be followed, I venture to hope that the analytical results will show a much greater uniformity and tend to reflect more credit on the profession of analytical chemist, while the practical results will be still more satisfactory to the manufacturer, who will only pay for the ammonia actually received ; he will also be better able to control the percentage of ammonia in the manufactured manure. At the same time the regular determination of the percentage of moisture will tend to discourage the too common practice of adding water to increase the weight, while hoping, through the negligence of the chemist, to escape detection.

If necessary let the price per unit of ammonia be raised, but by all means let us endeavour to obtain correct analyses.

Analytical Laboratory, 79, Mark Lane, London, E.C.,
November 29, 1880.

DETECTION OF STARCH-SUGAR MECHANICALLY MIXED WITH COMMERCIAL CANE-SUGAR.*

By P. CASAMAJOR.

(Second Paper.)

In a previous communication on the same subject,† read before the American Chemical Society at the meeting of March, 1880, I gave several processes for the detection of starch-sugar in commercial sugars. One of these consisted in adding to the suspected sugar a quantity of cold water, somewhat less than its own weight, and stirring the mixture for a few seconds. If starch-sugar is present, it will be seen in the shape of white chalky specks.

Quite lately a sample of yellow refined sugar was given to me which was supposed to be adulterated by being mixed with starch glucose. By applying the test just mentioned, there seemed to be left a few small chalky specks, which dissolved after standing a minute or two, making it very uncertain whether any starch glucose was present. Upon repeatedly trying the same test the result was always doubtful.

I was then led to treat the suspected sugar by a liquid capable of dissolving sugar, but without any solvent action on starch-glucose. After many trials, I found that methylic alcohol of such density as to mark 50° by Gay-Lussac's alcohometer answered the purpose very well, if previously saturated with starch-sugar, as this solution dissolves cane-sugar, either white or yellow, very readily, but does not dissolve starch-glucose.

Methylic alcohol at 50°, saturatad with starch-sugar, gives a solution of specific gravity = 1·25. 100 c.c. of methylic alcohol at 50° dissolves 57 grms. of dry starch-sugar, the volume of the solution being 133 c.c. A solution of starch-sugar in ethylic alcohol does not answer so well, because ethylic alcohol does not dissolve so readily the gummy matters found in soft sugars, which are those generally chosen for adulteration with glucose.

To test the presence of starch-sugar in a commercial cane-sugar, the suspected sugar should, in the first place, be thoroughly dried, as otherwise any water present will weaken the alcohol, and enable it to dissolve more starch-sugar. It should then be stirred for about two minutes with the saturated solution of starch-sugar in methylic alcohol. After this, the residue is allowed to settle, and the clear solution poured off. The residue may then be washed with a fresh quantity of the same solution. After stirring again and allowing the residue to settle, there will remain, if any starch-sugar is present, a certain quantity of chalky white specks, accompanied by a fine deposit, formed by the starch-sugar present in powder or fine grains. These finer particles are never seen when water is used for detecting the presence of starch-sugar, as they dissolve in water very readily. It seems probable that by using this solution of starch-sugar in weak methylic alcohol, the starch-sugar in an adulterated sample could be estimated quantitatively by a process somewhat analogous to that of Payen for estimating cane-sugar.

Not having had any occasion for such a process I have not determined experimentally the degree of approximation obtainable in this way.

The methylic solution of starch-sugar should be poured on a filter, after it has dissolved all it can from a commercial sugar, and the residue should be washed out with the same solution, and everything poured on a weighed filter. After all the liquid has run off, the filter and the residue may be rapidly washed with the strongest methylic alcohol found in commerce, which tests 92½° by Gay-Lussac's alcohometer, and which dissolves starch-sugar with great difficulty.

By a dexterous use of this process it seems probable that very approximate results may be obtained, although what is said here is merely in the nature of a suggestion to those who may have use for quantitative results.

A TUBULATED CRUCIBLE FOR USE IN ESTIMATING VOLATILE PRODUCTS OF IGNITION.*

By F. A. GOOCH.

In many processes of analysis which involve the collection and direct estimation of volatile products of ignition (such, for example, as the determination of carbon in irons and steels, or of carbon and hydrogen in organic compounds), the use of combustion-tubes of glass or porcelain or platinum, either with or without boats of platinum or

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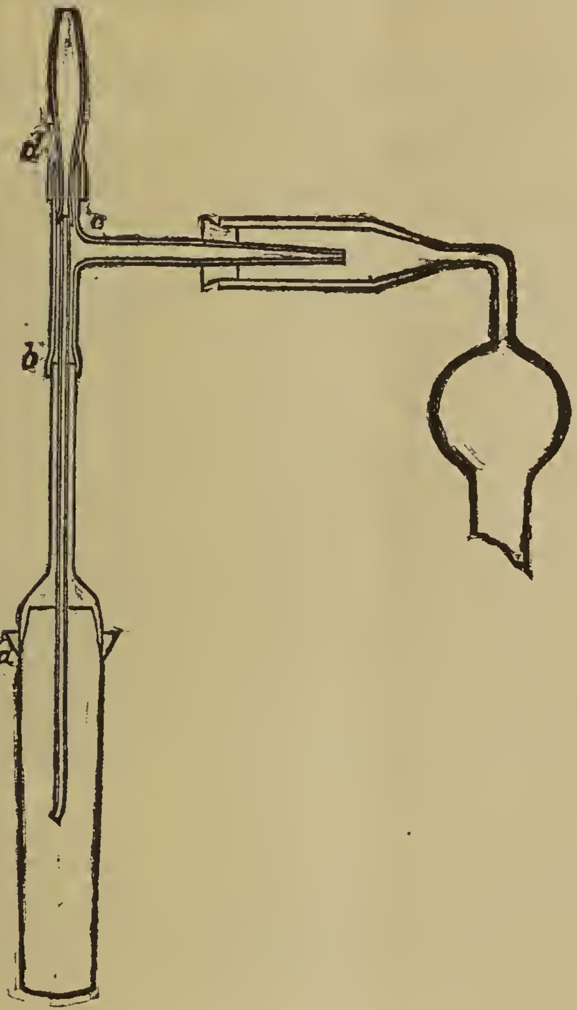
* A paper read before the American Chemical Society, Nov. 4, 1880.

† Chemical News, vol. xli., p. 221 ; *Journal of the American Chemical Society*, vol. ii., p. 111 ; *Sugar-Cane*, vol. xii , p. 283.

* *American Chemical Journal*, vol. ii., No. 4.

porcelain as accessories, in connection with proper absorption apparatus, is attended with satisfactory results; but accuracy in processes of this kind which require the application of high temperatures, particularly when the substance under examination is submitted to ignition in contact with a flux or other reagent (as, for instance, in Lipöcz's method of determining the water of constitution of silicates by fusion with sodic carbonate) is not easily secured. It is for use in such processes that the apparatus whose description is appended was devised. Its practical working has been found to be satisfactory, its management easy, and the results of analysis accurate. Its range of applicability to processes, both actual and prospective, whose object is the determination of volatile products of ignition, promises to be extensive.

A platinum crucible shaped, as shown in the figure, in the form of two truncated cones placed base to base, has its upper rim folded twice upon itself so as to present a smooth conical surface on the exterior and an up-turning flange. A conical cap of platinum prolonged upward in a tube fits the outer surface of the crucible near the flange, *a*, in a ground joint. Over the upper end, *b*, of the platinum tube one end of a Ʇ-tube of lead glass is fused, and the other end of the same branch of the Ʇ-tube is connected by means of a piece of rubber tubing, *d*, with a glass tube into whose lower end, *c*, is fused a slender platinum tube which extends downward into the crucible and terminates in a slight turn to one side; or, in special cases in which the use of a rubber joint is not permissible, the connection between the inner and outer tubes may be made by fusing the slender platinum tube directly into the upper end of the Ʇ-tube. The horizontal branch of the Ʇ-tube is drawn out a little and passes through the stopper of an absorption-tube, as shown in the figure. An air-tight joint is made between the cap and the crucible by melting into the flange some easily fusible material like the neutral anhydrous sodic tungstate, or sodic meta-phosphate, the former being preferable in most cases; but the seal may be varied, of course, according to the nature of the case in hand.



In using the apparatus the substance to be ignited is put into the crucible, the cap is adjusted, the inner tube set in place and the rubber joint made, the horizontal branch of the Ʇ-tube connected with the absorption apparatus, the opening in the upper end of the inner tube closed by a cap made of rubber tubing stoppered with a bit of glass rod, and finally the seal melted into the flange with a blowpipe-flame.

As the apparatus now stands it should be air-tight. Next, the cap is removed from the top of the inner tube, and the latter connected with an arrangement for forcing (or drawing) pure air or other gas, according to the purpose of the experiment, through the crucible and absorption-tubes, and heat is applied to the crucible. The temperature may be raised until the lower part of the crucible shows a white heat without fusing the seal of sodic tungstate within the flange.

To take the apparatus to pieces after the completion of the operation it is only necessary to disconnect the absorption apparatus, remove the inner tube, re-melt the seal, and lift the cap. Hot water removes the adhering sodic tungstate very easily.

Some of the dimensions of the particular apparatus which I have in use are as follows:—

Capacity of crucible	22·0 c.m.^{3}
Diameter of crucible at top	1·6 c.m.
,, ,, ,, flange	1·9 ,,
,, ,, ,, bottom ..	1·7 ,,
Height of crucible without cap ..	9·0 ,,
,, ,, tube above cap	5·0 ,,
Diameter ,, ,, ...	0·6 ,,
Length of inner tube of platinum ..	13·0 ,,
Diameter ,, ,, ,, ..	0·2 ,,
Weight of platinum in apparatus ..	48 grms.

THE FOG QUESTION.

A new phase of what is called "Sanitary Reform" has just been brought forward. We have been told that the fogs of London and of our other large cities are a nuisance, and though the news is somewhat of the stalest we are listening as if to some novel revelation and beginning to look out for a remedy. Here, therefore, arises a danger lest in our well-meant endeavours to get rid of fog we bring down upon ourselves even greater evils.

The favourite proposal at present is "smoke-consumption," as it is called. It is suggested that "if local authorities had the power to levy a tax upon every fireplace so constructed as not to consume its own smoke, the smoke nuisance would disappear in a very few years." Now we do not for a moment dispute that the normal city fog, yellow or black in its colour, owes most of its disagreeable attributes to the products of the combustion of coal. But will smoke-consumption free us from all these evils? The irritating effects of a dry fog upon the eyes and the mucous membrane of the nose, &c., is admittedly due to the evolution of sulphurous acid derived from the oxidation of the sulphur present in all coal. No smoke-consumption can free us from this unwelcome gas. On the contrary, the more completely every particle of coal is burnt the more certain we are that all its sulphur will be converted into sulphurous or sulphuric acid. We have known manufacturing establishments where nothing but colourless transparent vapour escaped from the chimneys, but the condition of vegetation in the neighbourhood, and the acid reaction of water in which the leaves of adjacent trees had been steeped, showed but too plainly that the enemy was not less formidable because no longer perceptible. So long as we burn the same quantity of coal within the metropolitan area so long, in calm and cold weather, we shall have a suffocating atmosphere. The only true remedy for this evil is to obtain from coal its practical maximum of duty, and thus immensely to decrease its consumption. Now it is exceedingly doubtful whether any kind of firegrate or stove exists which will answer both these purposes combined. To minimise the quantity of coal needed to heat our houses, the German or Dutch fireclay stove, with its winding flues which enable the combustion gases to expend almost their entire heat before entering the chimney, are probably the best contrivances known. We are glad to find that Dr. Alfred Carpenter, in his paper read before the Society of Arts, urges that fireclay should supersede iron in all possible parts of a firegrate. But though these stoves greatly decrease the consumption of coal, they are not necessarily smoke-consuming, and might very possibly not be accepted by the smoke inspectors who in due time are to lord it over our homes and hearths. On the other hand, many smoke-consuming appliances which we have seen assuredly do little towards diminishing the quantity of fuel required, and the consequent amount of sulphurous acid given off. A charge brought by Dr. A. Carpenter against coal-smoke is the following :—"It is a main factor in the production of a high rate of mortality arising from the absence of ozone, or of ozonised oxygen, or depreciation of oxygen capable of acting upon changing organic matter." That the absence of ozone involves a high rate of mortality is an unproved assertion, nearly all the observations that have been made on the subject being inconclusive. Moreover, it can be experimentally proved that ozone irritates the organs of respiration, the very parts chiefly affected by our town fogs. Statistics do not warrant the inference that "zymotic" disease is promoted by coal-smoke. It is even probable that containing as it does coal-tar products and traces of carbolic and cresylic acids it may act as a disinfectant.

There seems to us an obvious difference between dwelling-houses and factories which ought to be considered before applying the Smoke Prevention Act to the former. A factory, if not the freehold of the occupant, is generally held under a long lease. Hence the occupier may with obvious fairness be called upon to undertake such structural alterations as the public good seems to require. But in a dwelling-house the tenant has in a large majority of cases no permanent interest. It is scarcely compatible with justice to compel a man either to submit to a tax which is in reality a penalty or to sink perhaps more than a year's rent in altering the fire-grates of a house which he may have to quit at six months' notice, and perhaps to repeat the outlay in another dwelling! Then there is the pleasant prospect that the inspector may be induced by dealers in stoves to pronounce the arrangements for smoke-consumption unsatisfactory, and the expense and the trouble will have to be incurred afresh! In prospect of such things we believe that all save the wealthy will prefer to run the risk of bronchitis rather than the certainty of being harried for the carrying out of this new sanitary reform.

There is one cause of smoke and general nuisance which has sprung up of late in all the outskirts of London which have a clay sub-soil, namely, "ballast" burning. This is carried on in the northern suburbs in the most high-handed manner and quite close to dwelling houses. A ballast heap gives off more smoke than a hundred dwelling-houses. Such smoke, too, is far worse in quality. The poorest coal mixed with the contents of the dust-bins serve for fuel, and as the heat is never fierce, the volatile products are not decomposed but escape into the atmosphere. In certain states of the weather, a plot of land of hundreds of square yards may be seen completely shrouded by the smoke and vapours from a single heap. This nuisance should be the first evil to be suppressed in the endeavour to improve the atmosphere of London. Next, sharper attention should be paid to locomotives, steamships on the river, and the portable engines used by building speculators and others, many of which dispense clouds of black smoke without any interference. It might be fairly enacted that all houses built after a certain date should be fitted with some improved fire-grate, and that other houses on the expiration of a tenancy should be reformed before being let again. Now the appliances for the use of gas as a fuel have been so greatly improved by certain inventors, among whom Mr. Fletcher, of Warrington, may claim the foremost place, it is very certain that cooking by gas would increase greatly if the price were reduced. It strikes us that the gas companies would find in this respect liberality the best policy, since lower charges would lead to a vastly increased consumption.

CORRESPONDENCE.

THE SPECIFIC ROTATORY POWER OF CANE- AND INVERT-SUGAR.

To the Editor of the Chemical News.

Sir,—In the Chemical News, vol. xlii., p. 257, is a communication of Mr. J. Steiner on "The Specific Rotatory Power of Cane- and Invert-sugar," which is interesting from the fact that he understands his subject and states his points with clearness. I must, however, take exception to the following passage, which contains erroneous statements :—"Clerget found that 16·35 grms. of pure sugar after inversion, and cooling to 0° C., deviate the light to −44 divisions of Soleil's scale. Tuchschmidt, by a series of careful observations, confirmed this (finding −44·16), and Casamajor merely repeated partly what Tuchschmidt had said before him."

I have not before me the paper of Clerget in the *Annales de Chimie et de Physique*, but I have the impression that Clerget's experiments did not extend any lower than 10°. Dr. Tuchschmidt does mention, however, that at 0°, −16·471 grms. of sugar in 100 c.c., when inverted, will give −44. This number, −16·471, is the one used in Soleil's old instrument. At any rate, by calculating from Clerget's table what the deviation would be at 0, we find −44.

Dr. Tuchschmidt found that the law is expressed by $-44{\cdot}16+0{\cdot}50578\,t$ instead of $-44+\frac{t}{2}$. This may seem a confirmation of Clerget's results, and I have shown that for temperatures between 10° and 40° C., the results of both formulæ are nearly the same. For higher temperatures, the differences are quite appreciable. For the temperature at which the deviation becomes 0, the formula $-44+\frac{t}{2}$ gives 88° C., while Tuchschmidt's formula gives 87·3° C. I have ascertained experimentally that 88° C. is the correct temperature.

As to my own labours, it is an error to state that I merely repeated partly what Dr. Tuchschmidt had said before me. If Mr. Steiner will take the trouble of reading my paper and that of Dr. Tuchschmidt, he will see that he is entirely mistaken. In order to present my results with clearness, I was obliged to go over the whole subject, and Dr. Tuchschmidt's paper contains some things which are also in mine. At the time of writing my paper, I was not aware of the results of Dr. Tuchschmidt, which were called to my attention by Dr. Behr, and I noticed them in a foot note.

The object of the researches which I undertook on the deviation of inverted sugar, as affected by temperature, was beyond the range of the labours of either Clerget or Tuchschmidt. My object was to ascertain experimentally at what temperature the deviation of inverted sugar becomes 0. In the books this temperature is given as 90° C., and one experimenter has placed it at 92° C. If either of these numbers was correct, the law deduced from

Clerget's table, which gave 88° C., must suffer a change above 35° C., which was the highest temperature observed by Clerget. Was there such a change, and, if there was, at what temperature did it occur? I determined the deviations due to inverted sugar, and gave the results of 68 observations, made between 14° and 92° C. The result of these was that the law deduced from Clerget's table was found correct all the way to 92°. The deviation at 88° was found to be 0, and at 92° it was +2. Previous to these researches, the deviations above 41·8° (the highest temperature observed by Tuchschmidt) had not been investigated, except that the statement was made that the deviation of inverted sugar became 0 at 90° or 92°. Nobody had inquired whether this had anything to do with the law contained in the table of Clerget.—I am, &c.,

P. Casamajor.

Brooklyn, December 7, 1880.

THE THALLEIOQUIN TEST AGAIN.

To the Editor of the Chemical News.

The very spirited defence of the claims of H. A. Meeson as put forward by J. Denham Smith in the Chemical News, vol. xlii., p. 183, as discoverer of the thalleioquin test, require some explanation at my hands. When such authority as your Henry Watts, B.A., F.R.S., F.C.S., author of probably the best chemical dictionary in the English language, should be so woefully ignorant of the claims of this Guy student as not to give him credit for the discovery in his article on Quinine, and yet possibly breathing the atmosphere of the same city, is it marvellous that a writer over 3000 miles away should be be*guy*led into following the common herd and awarding the palm to the Frenchman? The fact is that the writer has never had the pleasure of M. André's acquaintance, and it is *just possible* that M. André may be ignorant of the existence of the writer. Under the circumstances, it would be as great a presumption for him to accept the office of scientific executor as for Mr. Smith to tender it. The facts, however, as set forth by Mr. Smith, are sufficient to establish the priority of Meeson's claim, and unless another applicant appears claiming still greater priority, this leaf shall undoubtedly be restored to his chaplet.—I am, &c.,

Chas. Fredk. Zeller.

Philadelphia, December 11, 1880.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Note.—All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances, l'Académie des Sciences. No. 22, November 29, 1880.

Note Relative to a Memoir on the Vision of Material Colours in Rotatory Motion and of Speeds estimated in Figures by means of General Morin's Apparatus named the "Turning Plate" for the Observation of the Laws of Motion.—M. Chevreul.—This note is substantially an expression of regret for the delay in the appearance of the memoir in question.

Spontaneous Oxidation of Mercury and of Metals.—M. Berthelot.—Mercury, as well as iron, zinc, cadmium, lead, copper, and tin, undergoes on exposure to the air a superficial oxidation, very slight, and restricted by the difficulty of renewing the surfaces and by the want of contact which results from the layer of oxide formed at the outset. For the oxidation to continue this layer must be constantly removed, as is the case with rust of iron formed in moist air, or for lead hydrocarbonate produced in distilled water.

Propagation of Light.—M. Gouy.—The author shows that there is not for a given homogeneous source a determined speed of light independent of the manner in which the amplitude is caused to vary.

A New Electric Property of Selenium, and the Existence of Tribe-electric Currents Properly so-called.—R. Blondlot.—The author has observed a new electric property of selenium which may be shown by the following experiment:—To one of the poles of a capillary electrometer there is attached, by means of a platinum wire, a fragment of selenium which has been recently heated, and to the other pole a platinum foil. If the selenium is brought in contact with the platinum, holding it by means of an isolating handle, the electrometer remains at zero, as might be expected from the symmetry of the circuit; but if the selenium is rubbed against the surface of the metal the electrometer deviates strongly, the deviation obtained being equal to that produced by a sulphate of copper element.

Action of Phosphorus upon Hydriodic and Hydro bromic Acids.—A. Damoiseau.—With hydriodic acid and white phosphorus the latter melts and becomes covered with a reddish layer of biniodide, whilst phosphonium iodide sublimes. With red phosphorus even at 100°, there is produced merely a small quantity of phosphonium iodide. Upon dissolved hydrobromic acid, phosphorus does not react in the cold. At from 100° to 120°, phosphonium bromide sublimes, but no phosphorus bromide is produced.

On Waldivine.—C. Tanret.—Waldivine, $C_{36}H_{24}O_{20}$, is a neutral principle, without rotatory power, very sparingly soluble in cold water, freely soluble in chloroform, insoluble in ether, and remarkable for the ease with which it is decomposed by alkalies.

Proximate Analysis of Peat: Its Chemical Constitution.—C. E. Guignet.—Peats contain the acids formerly known as crenic and apocrenic, a waxy matter, soluble in benzine, a quantity of paraffine, chlorophyll, and certain glucosides.

Certain Optical and Visual Phenomena.—M. Trève.—If the flame of a lamp is viewed through a narrow slit, the lustre of the flame and the effects of diffraction vary much according as the slit is vertical or horizontal, the light being much more considerable in the latter case.

Revue Universelle des Mines, de la Metallurgie, &c., Tome 7, No. 2, March and April, 1880.

Distribution of Copper in Primordial Rocks and Sedimentary Deposits.—L. Dieulafait.—Noticed under the *Comptes Rendus.*

Australian Guanos.—A report on the guano of the islands Malden, Lacépède, and Huon. These guanos are poor in potash and nitrogen, but rich in phosphates.

Analysis of Superphosphates.—L. Chevron.—In acting upon a superphosphate made from bone-black or from the phosphate of Caceres with a solution of ammonium citrate of sp. gr. 1·09, there is no occasion to take into account the time of action or the fluctuations in the temperature of the laboratory. In the analysis of a bone-black superphosphate, an excess of citrate must be avoided—20 c.c. are sufficient for 2 grms. of the sample. An excess of the reagent dissolves part of the phosphoric acid of such tricalcic phosphate as has escaped the action of sulphuric acid in the manufacture of the superphosphate. The phosphate of Caceres is much less sensitive to the action of the citrate than the phosphate of bone-black, and here from 20 to 100 c.c. may be taken to 2 grms. of the sample.

Tome 7, No. 3, May and June, 1880.

This issue contains no original chemical matter.

Tome 8, No. 4, July and August, 1880.

This issue contains no papers on chemical subjects.

Bulletin de la Société d'Encouragement pour l'Industrie Nationale.

No. 78, June, 1880.

Report Delivered by M. F. Le Blanc on an Apparatus for the Industrial Analysis of Gases Invented by M. H. Orsat.—This useful apparatus cannot, unfortunately, be intelligibly described without the two accompanying figures.

Report Delivered by M. F. Le Blanc on M. Coquillon's Apparatus for the Rapid Determination of Fire-damp in the Atmosphere of Mines.—This paper also cannot be reproduced without the accompanying illustration.

No. 79, July, 1880.

The Society has awarded the Le Blanc prize of 1000 francs for the utilisation of manufacturing refuse to M. Vincent, for his process for obtaining methyl chloride from the *vinasses* of the beet-root sugar manufacture.

A sum of 1000 francs has been awarded to M. J. A. Martin for his mixtures for rendering textile articles, paper, &c., uninflammable. His ordinary mixture for light goods is:—Pure ammonium sulphate 8 kilos., ammonium carbonate 2 kilos. (5), boric acid 3, pure borax 2, starch 2 (for which may be substituted 0·400 kilo. dextrine, or the same weight of gelatine), and water 100 kilos.

A silver medal has been awarded to M. Idrac for his process of drying timber.

Les Mondes, Revue Hebdomadaire des Sciences.

No. 9, July 22, 1880.

New means of Rendering Tissues Water-proof.—The process consists in passing the tissues under a press containing a "peculiar composition" which is kept secret. Cloth, paper, &c., thus made water-proof remain permeable to air.

No. 10, July 29, 1880.

It appears that more than a third part of the vines in France have been already destroyed by the phylloxera. The departments of Haute Savoie and Jura are now attacked.

Determination of Chicory in Ground Coffee.—M. Prunier.—The author weighs out 2 grms. and separates the finer powder by sifting through fine silk. This powder which, as microscopic examination proves, is composed entirely of pure coffee, is set aside. That which remains on the sieve is macerated with a few grms. of water in a test-glass. After some hours it is thrown upon a piece of cloth stretched out and crushed with the fingers. The grains of coffee resist the pressure, whilst those of chicory reduced to a paste by soaking in water, penetrate into the cloth, and adhere to it. On drying the cloth it is easy to detach the coffee, which, after desiccation at 100° and addition of the fine powder separated at first, gives the weight of pure coffee. The chicory is calculated as loss.

No. 12, August 12, 1880.

The Marquis Tommasi has succeeded in sending a message across the Atlantic with two Minotto elements.

An Application of Accidental Images.—J. Plateau.—The author, from some experiments performed by his son, concludes that the *apparent* distance of the full moon is only 50 metres from the observer.

No. 13, September 2, 1880.

"Floral Manure."—This compound, which is said to consist of nitrogen, phosphoric acid, magnesia, and sulphur in a very concentrated state, is said to have given, in experiments made at Saint Ouen, results superior to other manures, though it was applied to pure sand and its rivals to garden soil. It is applied in solution, 1 or 2 grms. to a litre of water.

A New Electric Motor and its Applications.—M. Trouvé.—The description would not be intelligible without the six accompanying illustrations. Referring to an experiment to be made with this apparatus by MM. Trouvé and Gaston Tissandier, M. Moigno makes the characteristic remark:—"I shall not be present; the cassock of the author of the "Splendeurs de la Foi" would distress M. Gaston Tissandier too much!"

The Electrophone.—L. Maiche.—An illustrated memoir.

Researches on the Property which Solid Bodies Possess of becoming Soldered together by means of Pressure.—W. Spring.—This property appears to be a function of hardness, but appears to depend also on another element. All crystalline bodies unite easily. Amorphous bodies are divided into two groups, the ciroids—wax, pitch, &c.—which become welded together by pressure, and the aciroid, such as amorphous carbon, which cannot.

Certain Properties of Flames.—M. Neyreneuf.—Already noticed.

No. 14, September 9, 1880.

Fire- and Water-Proof Paper.—There is prepared a mixture of two-thirds ordinary paper pulp, and one-third asbestos. The whole is then steeped in a solution of common salt and alum, and after being made into paper is coated with an alcoholic solution of shellac.

Tome 53, No. 1, September 16, 1880.

Resistance of the Air.—A discussion concerning a paper by M. Le Dantec (*Mondes*, Sept. 1).

Study on the Gramme Machine.—M. Pelleux.—The first portion of a memoir, to be completed in a succeeding number.

No. 2, September 23, 1880.

This number does not contain any chemical matter which has not appeared elsewhere.

No. 3, October 14, 1880.

M. Maiche informs the editor that his electro-electrophone speaks through resistances equal to 20 million kilometres, and that he can cause an ordinary telegraph to act against these prodigious resistances.

Disinfection of Animal Matters, Blood, &c.—M. Vautelet.—The author proposes a treatment with aluminium sulphate, sulphuric and nitric acids, used in certain proportions,—a process efficacious, but certainly not novel.

Reimann's Färber Zeitung,

No. 25, 1880.

This issue contains a notice of the trade list of the Urda Company of Stockholm, who offer ammonium vanadiate at the greatly reduced price of £15 per kilo.

Sodium phosphate is recommended by Dr. Allrich as useful in dyeing with the new azo-colours. It is added to the extent of 4 per cent of the wool to be dyed along with the neceesary colouring-matter in water of 30° to 35°, and after the yarns have been turned for about ten minutes and taken out, alum is added to the extent of 6 per cent, the yarn is re-entered, and the water slowly raised to a boil.

No. 26, 1880.

This number contains an account of the Exhibition of the German Woollen Trade, at Leipzig.

No. 27, 1880.

With reference to the Sidney Exhibition it is mentioned that the firm Laer and Son, who received the first prize for woollen yarns, are not manufacturers at all, but merely buy yarns and send them to be dyed or printed!

No. 28, 1880.

This number is principally devoted to an account of the Leipzig and Dusseldorf Exhibitions.

No. 29, 1880.

At a meeting of the Société Industrielle of Mulhouse, it was stated that tin sulphocyanide, formed by the double decomposition of calcium sulphocyanide and tin oxalate, is found very useful in calico printing.

E. Dollfus, on treating the diazo-derivative of orthotoluidine with nitric acid, obtained a binitro-cresol, which melts at 86° and crystallises in yellow needles. It is distinct from the two binitro-cresols which have been examined by Martius and Wichelhaus.

No. 30, 1880.

M. A. Poirrier, of Paris, writes that the azo-colours, orangé, rocceline, chrysoine, and ponceau, were first discovered by Roussin, who deposited an account of his researches in a sealed packet in the care of the Academy of Sciences, April 8, 1876.

For printing cotton with the azo-colours, Dr. Allrich proposes to dissolve 100 grms. of the colour in five times its weight of water; then makes up a solution of sodium stannate or aluminate at 15° B., to every litre of which are added 20 grms. alizarin oil. Of this mixture 150 grms. are incorporated with the colour, which is then thickened with starch and printed. After printing the pieces are steeped for an hour in lead or barium acetate or barium chloride at 5° to 10° B., and washed in cold water.

Chemisches Central-blatt.

No. 24, 1880.

Influence of Increased Supplies of Water on the Transformation of Matter in the Animal System.—Jacques Mayer.—Augmented excretion of nitrogen does not accompany augmented excretion of water.—*Med. Central Blatt*, 18, p. 276.

Detection of Arsenic in Non-metallic Liquids.—H. Hager.—The author recommends his process for evolving arseniuretted hydrogen, and bringing the gas in contact with paper moistened with silver nitrate.

Determination of Silicon in Iron and Steel.—T. M. Brown.—One grm. iron or steel is placed in a porcelain crucible with 25 c.c. nitric acid of 1·2 sp. gr. When the reaction is over 25 to 30 c.c. dilute sulphuric acid—1 part acid and 3 water—are added, and the solution is heated till the nitric acid is entirely or nearly expelled. When the residue is sufficiently cool water is cautiously added, and the contents of the capsule are heated till the crystals are perfectly dissolved. The solution is then filtered as hot as possible, and the residue washed first with hot water, then with 25 to 30 c.c. hydrochloric acid of sp. gr. 1·20, and finally again with hot water. After drying and ignition the silica is obtained snow-white and granular.

Contribution to Electrolysis.—L. Schucht.—The author describes the electrolytic determination of unanium, thallium, indium, vanadium, palladium, molybdenum, selenium, and tellurium. For qualitative analysis he uses a strong test-glass, 10 to 12 c.m. high, and 1·5 c.m. wide, fitted with a cork coated with paraffin. Two platinum wires, 1½ m.m. in thickness, pass through the cork down to the bottom, and are connected above the cork with the polar wires of the battery by means of small binding screws. This decomposition tube may be held in a wooden clamp. After the current has passed through the solution to be analysed for ten to fifteen minutes, the stopper with the wires is drawn out, without interrupting the current, and the deposited metal is determined by its colour, lustre, solubility in acids, &c. The manner of decomposition and the slight or strong evolution of gas is noticed. The solution is completely precipitated, rendered alkaline, and again electrolysed, after the wires have been cleansed. Copper is recognised by its colour, mercury by the precipitated globules, nickel and cobalt by their lustre and sparing solubility in acids, zinc and cadmium by their colour and solubility in potassa. The formation of peroxides is characteristic for lead, silver, bismuth, thallium, manganese. Bismuthic acid is gradually formed, whilst the peroxides of lead, silver, and thallium are deposited at the beginning of the precipitation. Silver peroxide dissolves in ammonia with liberation of nitrogen. The decomposition of the alkalies and alkaline earths is best effected in a U-tube. The hydroxides of the latter are separated in a voluminous form; those of calcium and magnesium in white crusts. The hydroxides of barium, stontium, and the alkalies dissolve on the negative wire.—*Berg- und Hütten Zeitung*, 39, 121.

Determination of Gold and Silver in Alloys, after Quartation with Cadmium.—Fr. Kraus.—Two portions of the alloy, each of 0·25 grms., are weighed off and placed with the cadmium in small porcelain vessels. A piece of potassium cyanide is melted in a porcelain capsule over the flame, and the metal thrown in. The melting together takes place readily, and is complete in a few minutes. By changing with two or three porcelain capsules, and having a vessel with warm water at hand, in which the melt is dissolved when sufficiently cool, twenty to thirty meltings can be executed in an hour. The two metallic granules are now thrown together into a small long-necked flask, in which is nitric acid of sp. gr. 1·30; a piece of wood charcoal is introduced to prevent bumping—which would rupture the globules—and heat is gently applied. The first solution lasts rather long, according to the proportion of gold; *e.g.*, an hour in case of fine gold. The solution is poured off, the boiling repeated with nitric acid of sp. gr. 1·3 for ten minutes, the liquid again poured off, the globules rinsed with hot water, boiled for five minutes with water, which is poured off, and the flask filled with water is inverted into a porous earthern crucible, dried, ignited strongly, proceeding as in cupellation. In most cases the globules can be weighed separately. Silver is determined in the solution of titration with ammonium sulphocyanide according to Volhard's method.

No. 26, 1880.

Preparation of Liquids of Determinate Specific Gravity without the use of Tables of Weight.—E. Mylius.—The author proposes a formula for this purpose.

The Bulk of Yeast-Cells as a Factor in Natural Degeneration.—J. Brynych.—The smaller yeast-cells are much more liable to disease than the large ones.—*Listy Chem.*, 4, 229.

Annales de la Société des Sciences Industrielles de Lyon,

No. 2, 1880.

This number, contrary to what might have been expected, does not devote a line to dyeing, tissue-printing, or the manufacture of colours, and in fact contains no chemical matter at all.

No. 3, 1880.

This number, again, contains no reference to the chemical arts and manufactures.

MEETINGS FOR THE WEEK.

Monday, 3rd.—London Institution, 5.
Tuesday, 4th.—Royal Institution, 3 p.m. "Atoms," Prof. Dewar.
Wednesday, 5th.—Geological, 8.
Thursday, 6th.—London Institution, 7.
—— Royal Institution, 3. "Atoms," Prof. Dewar.
—— Royal, 4.30.
Friday, 7th.—Geologists' Association, 8.
Saturday, 8th.—Royal Institution, 3. "Atoms," Prof. Dewar.

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ROYAL POLYTECHNIC.—Utopia, a Grand Fairy romance. The Electrical Railway; Great Lightning Inductorium; a Lecture on Scientific Wonders, by Mr. J. L. King. Musical Sketches, by Mr. Eric Lewis. Christmas and its Customs, by Mr. Frank Heath. Humorous Recitals, by Madame Hickson. Blondin, Leotard, Diving Bell, &c. Admission to the whole, 1s. Open from 11 till 5, and from 6 till 10.

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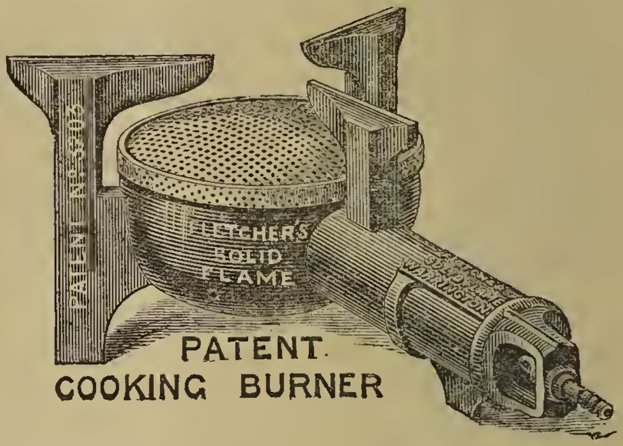

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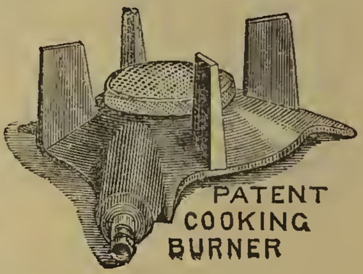
Illustrated List of Gas Apparatus for Domestic Use (Cooking, &c.) free by post.

Illustrated List of Gas and Petroleum Heating Apparatus for Laboratory use; Furnaces, Foot and Automatic Blowers, Hot and Cold Blast Blowpipes, Ingot Moulds, &c., Price 2d.



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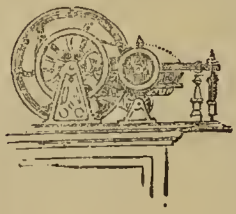
4 & 6, MUSEUM STREET, WARRINGTON.

Carr's Patent Paint and Drug Mill.

Approved by some of the most eminent Chemists, and awarded the Silver Prize Medals of the Manchester and Liverpool, and the Middleton Agricultural Shows.

From Messrs. E. H. Smyth & Co., 21 Duke Street, Edinburgh, and 69, Coleman Street, City, London.—The small Levigator Mill we recently got from you works most efficiently, and is a great economiser of time.



The chief feature in this machine is, that although it can be turned with ease by a boy, it has immense frictional power for levigating purposes. This is caused by the runner being driven at a much greater speed, but in the same direction, than the bowl. The surfaces being Minton ware, they are clean, smooth, and durable.

Testimonials on application.

£11 nett cash, delivered in Manchester.

The diameter of the bowl is 18 ins., and it and the runners are made of Minton ware. It is useful for Painters, Chemists, Grocers, and Cooks, for triturating and mixing Medicines, Vegetable Powders, Ointments, Paints, Sugar, Salt, Spices, Herbs, Meat for Potting, and will grind the coarsest Emery into a nearly Impalpable Powder in a few seconds. The pan can be tilted for emptying, as shown in the dotted lines; it can be worked by hand or power.

SOLE MAKERS—
W. H. BAILEY & Co., Albion Works, Salford, Manchester.

ASBESTOS.

H. W. JOHN'S MANUFACTURING CO. have appointed WITTY and WYATT Sole European and Colonial Agents for their well known ASBESTOS Fire-proof Paints and other materials, which for twenty years have been largely used in the United States, South America, China, Japan, &c.

ASBESTOS RETORT CEMENT is Fire- and Acid-Proof; when subjected to intense heat will vitrify without shrinking, thus rendering it of great value in repairing retorts, pipes, and other chemical appliances.

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