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THE

CHEMICAL NEWS

AND

JOURNAL OF PHYSICAL SCIENCE:

(WITH WHICH IS INCORPORATED THE "CHEMICAL GAZETTE")

A Journal of Practical Chemistry

IN ALL ITS APPLICATIONS TO

PHARMACY, ARTS, AND MANUFACTURES.

EDITED BY

WILLIAM CROOKES, F.R.S.

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THE COLLEGE OF PHYSICIANS AND PHARMACY.

After a long slumber, the London College of Physicians, a few years ago, awoke to a sense of its position, its opportunities, and, possibly, its responsibilities. Recognised, legally and socially, as the head of the medical profession in England, it had slept on its dignity, while other and younger corporations had made rapid advances over the ground it should have occupied. Aroused at last, however, by the Medical Act of 1858, it has since made vigorous, and so far successful, efforts to recover some of the ground lost by its former apathy or pride. The College has recently begun to license a new order of general practitioners, and, to render the qualification complete, now Examiners in Surgery.

But to what end, some of our readers may ask, who have seen that we have hitherto kept clear of the thorny ground of medical politics, do we call attention to these facts? It is to recommend the College of Physicians to go one step further. At the present time it includes within its pale all branches of the medical art but one—pharmacy. At the present time, however, and under its present charter, the College can exercise considerable authority over pharmaceutists and druggists—at all events, in the precinct of London. The Censors of the College have the power to visit the shops, to inspect the stock, to examine the druggist as to the composition of the medicines, and even, it would seem, to summon druggists before them, and inflict a fine for the sale of "corrupt" medicines, or for compounding medicines "not agreeable to the prescript or direction given." It would be but a slight modification of this authority if the College were now to exercise a more general and direct control over pharmaceutists. The power we have just mentioned has been allowed to fall into abeyance, and, besides, is hardly in keeping with the spirit of these times. But a far better guarantee for the purity of medicines, and the intelligence of pharmaceutists, might be obtained, if the College were to institute a new order of "Licentiates in Pharmacy," and so include within its authority all degrees of the medical profession. The natural alliance of the physician with the pharmaceutist is very close, but we have no doubt that there are physicians who will stand aghast at our proposal. We shall not stop now to characterise the sentiment which inspires such, nor answer some objections which might be fairly raised. We shall only remark, that pharmacy was a respectable art when surgery was in the hands of barbers and old women, and when physicians, except in so far as they were pharmaceutists as well, were held but of small account. In modern times, too, improvements in pharmacy have quite kept pace with improvements in the practice of medicine. We hope, then, that no false pride will prevent the College of Physicians from carrying out the proposal we submit. To be a "Licentiate in Pharmacy of the College of Physicians" would be an honour which young pharmaceutists would be proud to possess, and for which they would earnestly work. The public, too, would recognise and have confidence in such a diploma; and, lastly, medical men themselves would have a double security that the Licentiate in Pharmacy would not stray from the legitimate province of his business, and occupy the place of the prescriber.

TECHNICAL CHEMISTRY.

On the Manufacture of Alcohol from Coal-gas, by M. Mallet.

Thénard said, in his "Treatise on Chemistry," in 1827—"It is certain that the greater portion of vegetable substances are composed only of hydrogen, carbon, and oxygen; but, nevertheless, we cannot form any of those substances from these elements. This powerlessness of chemistry has often rendered results doubtful in the eyes of persons unacquainted with science, although really profound thinkers. J. J. Rousseau, when following the course of chemistry with Rouelle, said that he would only believe in the analysis of flour when he saw chemists able to reproduce it. The great writer would, doubtless, in the present day, use different language." These reflections of Thénard's are very judicious. Rousseau would certainly have had his conviction disturbed if he had witnessed the beautiful experiments of Lavoisier and of Meunier in 1783 and 1785, as well as the more recent ones of Lefevre-Gineau, Foucroy, Vauquelin, and Seguin, on the synthesis of water. The philosopher of Geneva would, most probably, have believed in chemical analysis after seeing 500 grammes of water produced by means of oxygen and hydrogen, in the proportions indicated by the analysis.

But what would Rousseau, and even Thénard, have said, if they had been fortunate enough to see alcohol manufactured from these elements? This production is one of the wonders of the chemistry of the present day, which is not yet at the end of its marvellous developments. The attention of chemists and manufacturers has, therefore, been awakened during the last two months, because it is now proposed to transplant this industry from the loboratory into the manufactory.

Since the question of the artificial manufacture of alcohol has seriously occupied attention, it has been discovered that, as early as 1854, M. Castex, of Puteaux, took out a patent for a means of obtaining alcohol from the smoke of oil, fat, and of all other organic matter. The following is an extract from the patent given in the *Cosmos*:—"In burning these organic matters, the smoke which is liberated may be absorbed by concentrated sulphuric acid. This sulphuric acid, mixed with water and distilled, produces alcohol. To facilitate the absorption of the smoke, it is caused to pass through a body impregnated with sulphuric acid, or through coke moistened with the acid. When the acid no longer absorbs smoke, the body impregnated with it may be washed for the purpose of extracting it; and this weakened acid gives alcohol on distillation. Before delivering the ordinary lighting gas for the purpose of being consumed, it may be treated by this method."

The specification of the patent is drawn up in such singular terms, that it is clear M. Castex's knowledge of chemistry is not very extensive. As to making use of lighting gas before its consumption for the production of alcohol, it is a perfect impossibility. The gas would no longer give any light; and we do not recommend gas companies to make any use of the idea broached by M. Castex, for the consumers would soon rise in arms against the application of such a process.

The patent was taken out by M. Castex on the 8th of December, 1854; and on the 15th of January, 1855—that is to say, little more than a month afterwards—M. Bertholet explained for the first time to the *Académie des Sciences* his mode of producing alcohol synthetically. The nearness of these two dates may give rise to some reflections, from which we abstain, because we have not now to examine the question of the validity of the patent. For the scientific world, M. Bertholet is the first author of the discovery; and the silence of M. Castex is a mode of acquiescence which has some importance.

Let us now speak of the experiments of our celebrated chemist. It is not with smoke that M. Bertholet produced alcohol, but from the bicarburetted hydrogen, or olefiant gas. He introduced into a flask twenty-six litres of this gas, a quantity of concentrated sulphuric acid, and three kilogrammes of mercury—the use of which in the operation is, we apprehend, for the purpose of minutely dividing the particles. He then agitated the whole together for a considerable length of time. After about 53,000 agitations, it was found that the whole of the gas had been absorbed by the acid, and that a real combination was formed, producing an acid, to which the name of sulpho-vinic or sulpho-ethylic was given.

In treating this acid with water, a new reaction was produced. The bicarburetted hydrogen set at liberty combined in its nascent state with water, forming alcohol, leaving the sulphuric acid combined with the addition of a certain quantity of water—that is to say, diluted—and, according to the formula of equivalents, we have $C_4H_4 + 2HO = C_4H_6O_2$. The alcohol produced is expelled from the liquid by heat.

Up to the present time, M. Bertholet has made no attempt practically to utilise this curious experiment. Possibly, he was frightened by the difficulty which attended the absorption of carburetted hydrogen by even the most concentrated sulphuric acid; and he has contented himself with sending to the International Exhibition a litre of alcohol thus produced from its elements, which may be seen in the glass case of M. Ménier, No. 204, Class II. of the French Department.

What M. Bertholet has not attempted, M. Cotelle, of St. Quentin, has since realised. Like M. Castex, he has had the idea of employing coal-gas for the production of alcohol, but not before using it for the purpose of giving light. Coal-gas, well purified, contains light carburetted hydrogen, bicarburetted hydrogen, carbonic oxide, hydrogen, and certain carburets, more or less well determined—such, for example, as a trace of propylene, aniline, and even, perhaps, of acetylene. Of these various gases or vapours, the bicarburetted hydrogen is, with propylene, the sole useful element for the purpose proposed. Unfortunately, it exists only in very small proportions. The richness of coal-gas in bicarburetted hydrogen depends evidently, in the first instance, on the nature of the coal distilled, and afterwards upon the manner in which it is distilled. If certain old and not very numerous analyses are to be relied upon, the proportion of bicarburetted hydrogen may be from 8 to 12 per cent.; but under the conditions in which the manufacture now takes place, we consider these figures as much too high, and believe that 3 or 4 per cent. at the utmost would be much nearer the truth. We speak of the gas manufactured in retorts from the coals of Belgium and the north of France. As to the gas manufactured in coke-ovens, we do not believe it has yet been analysed. However this may be, the following is the method which M. Cotelle appears to have adopted:—

After having indicated in his patent many means which he has probably tried, he condenses the gas, and purifies it completely from its sulphuretted hydrogen and ammonia, and finally passes it through concentrated sulphuric acid, which dehydrates it as much as possible. By means of a pump or exhauster, the gas is directed to the base of an earthenware or glass column, filled with plates or diaphragms, pierced with small holes, upon which the sulphuric acid at 66° descends, and which is, consequently, minutely divided. The column which M. Cotelle used when we witnessed the operation, had only fourteen plates or diaphragms, and a single passage of the acid did not suffice to absorb all the bicarburetted hydrogen of the gas, and, at the same time, to saturate all the acid. According to the inventor, a cascade of six plates would be required to accomplish these two objects at one operation—the absorption of all the bicarburetted hydrogen, and the saturation of the sulphuric acid.

Let us suppose this desideratum obtained. The carburetted hydrogen and the propylene are absorbed in the column, and there remains from 96 to 97 per cent. of light carburetted hydrogen, hydrogen, and oxide of carbon, without value for the purpose of giving light, but capable of being used for the purpose of heating. It is principally with the object of so using them that the pump or exhauster is required, so that the gas may be drawn off or propelled in any direction without increasing the pressure upon the retorts.

The sulpho-vinic, or sulpho-ethylic acid, which M. Cotelle calls his *vinasse*, is treated with five times its volume of water, and the mixture is then submitted to the action of a current of steam, which carries with it the alcohol produced. These vapours are condensed, and the alcoholic liquor is obtained. This is distilled on a little lime to saturate the sulphuric acid which has been carried with it, and the products of this distillation are rectified, and alcohol of 90° is obtained.

The sulphuric acid which has been diluted by the operation so as to reduce it to 20° or 25° of strength, must be concentrated to 66° if required to be used over again, or some other use must be found for it in its diluted state. This use of it, advantageously if it is possible, involves the establishment of another factory on a large scale, even for a distiller producing 30 hectolitres (660 gallons) per day; for the quantity of sulphuric acid at 66° to be employed is considerable. It amounts to about 1500 kilogrammes (30 cwt.) of acid for each hectolitre (22 gallons) of alcohol produced, or 45,000 gallons (45 tons) of acid for 30 hectolitres (660 gallons) of alcohol. Should it be desired to concentrate the diluted acid, apparatus must be provided such as is used in the sulphuric acid factories. This, truly, is not a manufacturing impossibility; but what will be the cost of concentrating for 1500 kilogrammes of acid—that is to say, for one hectolitre of alcohol?

This expense of concentration appears to us, in the actual state of chemical industry, to be a grave difficulty in the way of the success of the new process, even when the complete absorption of the bicarburetted hydrogen is easily effected, as well as the simultaneous saturation of the sulphuric acid; and we are quite sure that it would be necessary to make a new estimate of the cost price which the journals of St. Quentin, with laudable zeal, but with too much precipitation, had fixed at 25 francs per hectolitre of alcohol of 90°. Any serious estimate of the cost price must have been premature, as M. Cotelle was not provided with suitable apparatus to determine it.

Twenty-five francs per hectolitre for alcohol of good taste (it is said to be exempt from all empyreumatic flavour), the selling price of which was 70 francs! Here was enough to set all the speculators in a ferment, and to cause serious reflections to all distillers, and even the manufacturers of sugar and agriculturists. Purchasers presented themselves from all quarters, anxious to treat with M. Cotelle for his patents, French as well as foreign, on condition, be it well understood, that he satisfied them on the subject of cost price. The French patent was sold, it is said, for 20,000 francs, and the English one for 480,000 francs. A company was formed at St. Quentin, with a capital of 16,000 francs, which was subscribed in an hour, for the purpose of constructing a real manufactory, and solving the problem on a manufacturing scale. Since then—that is to say, about two months ago—the enthusiasm has abated, the matter has been examined with more *sang froid*, and it is generally admitted that the cost price can only be ascertained as the result of a manufacture on a practical scale, and not by approximative experiments in a laboratory.

(To be continued.)

On the Formation of Acetylide of Copper in Copper Tubes for conveying Lighting Gas, by M. Crova.

It is well known, that copper tubes, long used to conduct lighting gas, have sometimes, during the cleansing of the interior, occasioned very dangerous and sudden explosions, occasionally proving fatal to the workmen. In one of the scientific reviews, an accident of this kind is recorded as happening at Philadelphia.

Acetylide of copper having the property of detonating by an elevation of the temperature or by a blow, and the presence of acetylene in lighting gas having been recently proved by M. Berthelot, I have tried to ascertain whether acetylene, in presence of air, could combine with copper, and so form acetylide of copper.

By passing a mixture of air and moist acetylene through a glass tube containing bright copper turnings, this metal tarnishes rapidly, assumes iridescent colours, and finally becomes black; but as this alteration takes place only on the surface of the metal, it is very limited.

By using copper reduced by hydrogen, I extended the surface of the metal. A small quantity of metal was placed in two flasks containing a mixture of equal volumes of air and acetylene, and also in one of the flasks a drop of ammonia. The two flasks, well stoppered, their necks plunged in water, were left to themselves for two days. They were then carefully opened, and the water entering them absorbed nearly half the gaseous volume. The absorption seemed to be somewhat greater in the flask containing a little ammoniacal vapour. The copper turned black. After washing and drying, I found that it contained a notable quantity of acetylide of copper; for, heated with hydrochloric acid, it disengaged acetylene, and, thrown on a heated metallic plate, a louder explosion resulted than with pure acetylide, and without deposit of carbon. This difference will be readily understood, for the acetylide formed during my experiments, finding itself in presence of excess of oxide of copper, the carbon and hydrogen of the acetylide were entirely destroyed.

It is evident that copper, in presence of air and acetylene, is spontaneously transformed into acetylide of copper containing excess of oxide. As lighting gas contains acetylene, a little air, and perhaps even some ammoniacal vapours, it is obvious that acetylide of copper may be formed in tubes which have served to conduct lighting gas.—*Comptes Rendus.*

PHARMACY, TOXICOLOGY, &c.

*On the Preparation of Artificial Mineral Waters.**

(Concluded from page 295.)

Sulphurous Waters.

In the preparation of artificial sulphurous waters for drinking purposes, it is recommended to employ the crystallised monosulphide of sodium, and it is stated that it is unnecessary and objectionable to pass free sulphuretted hydrogen into the solution. The atmospheric air dissolved in the water is always sufficient to effect the decomposition of some of the sulphide of sodium, and set free some sulphuretted hydrogen. We extract the formula for Baréges water, to show the dose of sulphide of sodium prescribed:

Baréges Water.

Sulphide of sodium, crystallised .	0·200 grammes.
Silicate of soda	0·100 ,,
Chloride of sodium	0·072 ,,
Distilled water	1 litre.

In the preparation of sulphur baths, the authors recommend a polysulphide of a definite composition to be used, instead of the variable compound obtained by fusion; and, as no particular advantage is known to be derived from the use of a salt of potassium, the sulphide of sodium is prescribed as being the cheaper compound. The following is the preparation recommended for the next edition of the Codex:—

Monosulphide of sodium, crystallised .	125 grammes.
Flowers of sulphur	60 ,,
Distilled water	100 ,,

Dissolve the sulphide of sodium in the water, then add the sulphur, and heat gently until the latter is dissolved. The solution obtained in this way will contain exactly 100 grammes of penta-sulphide of sodium, which is sufficient for a bath.

Products derived from Mineral Waters.

The substances derived from mineral waters which are employed in medicine may be arranged in two classes. The first comprises the deposits formed spontaneously, either on the soil in the neighbourhood of the springs, or in wells or basins in which the water is confined. In the second class may be placed the saline products obtained by the evaporation of the waters. "Chemists have always recognised the fact, that the extraction of all the salts in a mineral water, and with the molecular arrangement assigned to them by Nature, is an operation beyond our art. By submitting a natural water to a moderate temperature, it loses the character

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* Abridged from a Report to the Société de Pharmacie of Paris, by MM. Chatin, Poggiale, and Lefort. *Journal de Pharmacie et de Chimie*, t. xli. p. 370-410.

of a natural water, and can only be considered as a solution of artificial salts; for the mode of combination of acids and bases has been completely changed by the evaporation of the liquid and the volatilisation of the gas. The salts, therefore, even when obtained from the natural source, as at Vichy, can only be looked on as artificial products."

Bicarbonate of soda, prepared with care, is, consequently, to be preferred to Vichy salts for the preparation of Vichy pastilles or soda lozenges, and the authors recommend the following formula:

Bicarbonate of soda	36	grammes.
Powdered sugar	1000	,,
Gum Tragacanth	10	,,
Water	110	,,

The above mass is flavoured by the following mixture of essences:

English oil of peppermint . .	0·20	grammes.
Oil of orange-flower . . .	0·10	,,
,, lemon	0·30	,,
,, rose	0·10	,,
,, aniseed	0·25	,,
Tincture of vanilla	0·60	,,

The essential oils are to be dissolved in three times their weight of alcohol, and mixed with the lozenge paste while it remains very soft. It is stated, that the soda pastilles are much better preserved in card-boxes than in closely-stopped bottles. In these latter they are said to become damp, and acquire a disagreeable alkaline taste.

COURT OF QUEEN'S BENCH.

December 10, 1862.

Simpson and Others v. Wilson and Another.

This was an action, directed by the Court of Chancery, to try the validity of Medlock's patent for improvements in the preparation of red and purple dyes. (See Chemical News, vol. vi. page 302.) The declaration, after stating that Medlock's patent is now the property of the plaintiffs, alleged that the defendants had infringed the said patent, by manufacturing and selling a red dye of the colour, commonly known as magenta or roseine, made according to the process described in the specification of the said patent. To which the defendants pleaded—

1. Not guilty.
2. That Medlock was not the first and true inventor.
3. That the manufacture was not new.
4. That the manufacture was not new, nor an improvement in any way useful or beneficial to the public.
5. That the patentee did not, within six calendar months after the date of the patent, file an instrument in writing, particularly describing the nature of the invention, and the manner in which it was, or might be, performed.

Counsel for the plaintiffs, Sir Fitzroy Kelly, Mr. Grove, Q.C., Mr. Bovill, Q.C., Mr. Drewry, and Mr. J. A. Russell. For the defendants, Mr. Hindmarsh, Q.C., and Mr. Day.

In the absence of Sir F. Kelly the case was briefly opened by Mr. Grove, after which the following evidence was called for the plaintiffs:—

Mr. G. C. Nicholson (examined by Mr. Bovill): I am one of the plaintiffs, and a member of the firm of Simpson, Maule, and Nicholson, manufacturing chemists, of the Atlas Works, Newington. I have given much attention to the study of chemistry and the preparation of dyes. I conduct the chemical department of our business, and am well acquainted with the body called "aniline." It is an oily basic liquid produced from coal tar, and has been known in chemistry for about thirty years. Between 1845 and 1850 I constantly worked on aniline to see what results could be produced from it. In 1859, or early in 1860, I discovered the peculiar properties of aniline in combination with arsenic acid. They produced a very brilliant dye, which is now called "magenta." I patented the discovery, and afterwards found out that Dr. Medlock had previously patented the same thing. Our firm afterwards purchased Dr. Medlock's patent. Before the time I have mentioned no such colour had been produced by aniline and arsenic acid. I was then, and am now, satisfied of the novelty of the invention. (A copy of Medlock's specification was here put in, and read to the Court). By the process described magenta colour is produced, which, by treatment with different preparations, gives different shades of colour. In my judgment this process was new. The colour is largely used, and has become a very valuable article *. I know Heilman's patent. (See Chemical News, vol. vi., p. 302). My attention has been drawn to it since this action was brought. (Specification put in). That patent professes to treat aniline with various salts and substances,—200 at least, but it may include 500. I have tried several of the things mentioned; an immense number of them produce no colour whatever. A person would have had to work, perhaps, six months to find out what it was produced the colour according to that specification. It was only a provisional specification; and no one was aware of it except the Clerk of the Patents. Arsenic acid is a combination of metallic arsenic with five equivalents of oxygen, and containing water of constitution, not water mechanically mixed with or adhering to it. The water is in chemical combination with it. Anhydrous arsenic acid is arsenic acid absolutely free from water.

Lord Chief Justice Cockburn: You start with saying that arsenic acid is a combination of the metal with oxygen?—Witness: And water.

Lord Chief Justice Cockburn: Well, suppose you want to distinguish dry arsenic acid from the arsenic acid you have been speaking of, what would you call it?—Witness: I should call that dry arsenic acid—that which I have just mentioned: the combination of arsenic, oxygen, and water is dry.

Lord Chief Justice Cockburn: Then there is no difference between arsenic acid and dry arsenic acid?—Witness: No.

Mr. Bovill: The distinction is between anhydrous and dry.

Lord Chief Justice Cockburn: You do not mean by dry that from which the water, which has been mechanically united with it, is removed?—Witness: I should call this dry, my lord (holding up a bottle of arsenic acid).

Mr. Bovill: Although it contains water?—Witness: Yes; water as part of its chemical constitution. Anhydrous acid is that from which all water has been driven off. The term "anhydrous" implies, free from water. In our process we use what is called dry arsenic acid, which means arsenic acid that contains a certain proportion of water in its constitution. Arsenic acid is produced by dissolving arsenious acid—the white arsenic of commerce—in nitric acid, and boiling the solution down until it solidifies into a dry powder. In boiling down, a large quantity of water is driven off, and that brings it to the state of what we call dry arsenic acid, leaving, still, a certain portion of water.

Lord Chief Justice Cockburn: Let me see that I distinctly understand you. You say that the driving off the quantity of water brings the acid to the state of dry arsenic acid?—Witness: Yes, my lord.

Mr. Bovill: Which still contains a certain portion of water which is not driven off?—Witness: Yes, exactly.

Lord Chief Justice Cockburn: That is, it retains it in chemical combination?—Witness: Yes.

By Mr. Bovill: The portion of water driven off in boiling

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* It is necessary for us to remark here that our report is confined to the chemical evidence.

was, in mechanical combination, water of solution. We cannot produce any effect on aniline by arsenic acid until water is driven off down to the point at which it becomes dry arsenic acid. Arsenious acid will not do it; but if we take arsenious and nitric acids, and drive off the water, we produce dry arsenic acid, which will act on aniline.

By Lord Chief Justice Cockburn: Arsenious acid is converted into arsenic acid by the action of nitric acid. The nitric acid parts with a portion of its oxygen, which is superadded to the arsenious acid, and then we get arsenic acid. The water driven off in boiling is not that which enters into chemical combination—it is water of solution or in mixture.

Lord Chief Justice Cockburn: Suppose you did not drive the water off at all, but having got arsenic acid you used it without going on boiling?—Witness: We should not get colour until we boiled it down for the purpose of getting all the mechanical water, if I may so speak, out of it.

Lord Chief Justice Cockburn: You must get it into a dry state?—Witness: We must get it into a dry state prior to the dye being formed.

Lord Chief Justice Cockburn: Then you must get it it into an anhydrous state, must you not?—Witness: No, certainly not.

Lord Chief Justice Cockburn: Not if you drive the water off?—Witness: The mechanical water, I am speaking of.

Lord Chief Justice Cockburn: However, you say you get no results until you come to the dry arsenic acid?—Witness: Until we boil off the water of solution from dry arsenic acid.

By Mr. Bovill: No water but that in the nitric acid is used in making arsenic acid; and all of this is driven off except what remains in chemical combination.

Mr. Bovill: Now then, in using the dry arsenic acid with aniline, after you have produced it, do you use the dry arsenic acid for the purpose of producing colour?—Witness: We do not mix the dry arsenic acid with aniline.

Mr. Bovill: Just describe the process.—Witness: Having got our dry arsenic, we mix it with aniline, and heat the mixture. The application of heat produces the melted mass which contains the magenta dye, and which, when dissolved in water and boiled down, produces the pure solid magenta. I should say that any nitric acid in the mixture would have no effect in producing the dye.

Lord Chief Justice Cockburn: What I understood you to say just now was this:—If you take arsenious acid and nitric acid, and combine those with heat, you will get, as you say, arsenic acid, the nitric acid parting with a portion of its oxygen, and increasing the strength of arsenious acid until it brings it up to the state of arsenic acid. Then, as I understand, before that combination will act on aniline to produce the dye, you must drive off the superfluous water of solution, so as to bring the arsenic acid to the state of dry arsenic acid?—Witness: Exactly so, my lord.

By Mr. Bovill: If any person were to take arsenious acid, nitric acid, and aniline, and put them all together, I do not think any effect at all would be produced; but, when the three are subjected to heat until the water is driven off, the dye would be produced, if the operation were conducted properly: if done improperly, it would blow your head off. The dye is produced, because in that process the arsenious acid is converted into dry arsenic acid.

Mr. Bovill: Does your lordship follow it?

Lord Chief Justice Cockburn: I do not, indeed. I have attended to it, but I have not been able to understand it.

Sir F. Kelly: There are three states of this arsenic acid; the one with the water belonging to it in the operation, which, when the water is driven off, becomes dry arsenic acid. Then, to make anhydrous, the small quantity of water which is in chemical combination must be got rid of also; but for the purpose of this operation it is quite enough if you make it dry, which is not, strictly speaking, anhydrous, yet, for all the purposes of commerce and of this manufacture, is dry.

Mr. Bovill: Then, as I understand it now, you produce the dye by taking the dry arsenic acid and mixing it with aniline?—Witness: Yes.

Mr. Bovill: You may equally make the dye by procuring arsenious acid, aniline, and nitric acid, mixing them together, and then driving off the water; and the reason why you produce it is, the water being driven off, you leave dry arsenic acid—not dry to the sense of touch, but chemically dry arsenic acid?—Witness: Yes.

Mr. Bovill: And what is it produces the colour? Is it the action of dry arsenic acid on the aniline?—Witness: Yes, it dehydrogenates the aniline. There is no difference between the processes, whether you take dry arsenic acid and mix it with aniline, or take the two things from which arsenic acid is made.

Lord Chief Justice Cockburn: In fact, in the one instance you start with dry arsenic acid already prepared, and in the other you prepare it in the first stage of the operation.

By Mr. Bovill: The addition of a metallic oxide in a mixture of the three would produce no effect as regards the colour. There was no process before by which the colour could be produced so beautiful and so cheap. Many thousand gallons of it are sold.

Cross-examined by Mr. Hindmarsh: I am well acquainted with the various modes of producing this dye. One of the colours produced is old, the other new. The purple was new at the date of this patent. The red had formerly been prepared. The processes in use before consisted, to the best of my belief, in the application of two oxidising agents, nitrate of mercury and nitric acid. I have read the various patents relating to the production of the colour. They are very numerous. I believe only two relate to the use of oxides. Bichromate of tin will not produce the colour. I have read a specification of Brooman's, in which he proposes to use a good many anhydrous salts. I believe I have read another in which he proposes to use anhydrous bichromate of tin. Bichloride of mercury is not an oxidising agent; it contains no oxygen. It may, perhaps, be a dehydrogenating agent. Iodine and five hundred other things have been so described. Arsenic acid was used in cotton print works before the date of this patent. I have never seen it used, but I know it is used as a discharge in place of tartaric acid. The mode of making arsenic acid I have described is mentioned in books on chemistry. I do not remember that the result is, in Graham's book, described as a perfectly anhydrous acid.

Mr. Hindmarsh: So far as you know, is there any book which describes the making of arsenic acid, which would not produce the material positively dry or anhydrous? Witness: I do not know.

Sir F. Kelly: That is rather a comprehensive question. If we are to have the book put in, in which the manufacture has been described, I apprehend the passages should be read in order that we may understand the question; but to ask the contents of every chemical book is too comprehensive.—Witness: One book will contradict another.

Lord Chief Justice Cockburn: Do you know of any process given in scientific works on chemistry in which nitric acid is used where the result would not be anhydrous?—Witness: If I were to perform the operation I should produce a dry arsenic acid.

Lord Chief Justice Cockburn: The question put by Mr. Hindmarsh relates not to dry but anhydrous acid.—Witness: Let me read the passage: "This acid is obtained by heating powdered arsenious acid in a basin with an equal quantity of water, and adding to the mixture at the boiling point nitric acid in small quantities so long as ruddy fumes escape. An addition of hydrochloric acid is generally made to increase the solubility of the arsenious acid, but it is not absolutely necessary. The solution of arsenic acid is then evaporated to dryness to expel the remaining nitric and hydrochloric acids, but the dry mass is not heated above the melting point of lead, otherwise oxygen gas is emitted, and arsenious acid reproduced." I say that, if I were to perform the experiment according to these instructions, I should get, according to the temperature I used, dry arsenic acid or the anhydrous acid.

Mr. Hindmarsh: You stopped short; go on with the passage.—Witness reading: "Arsenic acid thus obtained is milk-white and contains no water." That is not absolutely correct. It may be white, and contain water or no water. When nitric acid and arsenious acid are mixed together there may be no nitric acid or arsenious acid present: they may destroy each other. Nitrous and arsenic acids are formed. It is not necessary to apply a high degree of heat to drive off the nitrous acid. There may be something wrong in the description given by Mr. Graham. I know "Fownes' Chemistry." I do not know the process given for making arsenic acid. (Book handed up and description read.) "Powdered arsenious acid is dissolved in hot hydrochloric acid, and oxidised by the addition of nitric acid, the latter being added as long as red vapours are produced: the whole is then cautiously evaporated to complete dryness." We may differ about the complete dryness. If aniline is mixed with dry arsenic, and the mixture is allowed to stand, no colour will be produced, nor would there if water was present. The application of heat is necessary for the production of the colour. If the arsenic acid were anhydrous, practically no colour would be produced. Medlock's process, as we perform it, produces the colour. On the large scale, we do not boil the arsenic acid to dryness—to an absolute powder, or perfect dryness. We use it in a fluid state. We mix, in fact, a solution of dry arsenic acid and aniline together. The weight of water present is considerably less than that of the arsenic acid. There is about 30 per cent. of water and 70 per cent. of arsenic acid. The presence of water at the time of mixing is not absolutely essential to ensure success.

By the Lord Chief Justice Cockburn: We use water, because it is not so dangerous to the workman; it is more convenient, and it is cheaper. We do not get more dye, but it saves the expense of boiling down.

By Mr. Hindmarsh: It is essential that an arseniate of aniline should be formed in the process; but mechanical water is not necessary for the formation of the arseniate. I am quite sure that I have made arseniate of aniline without water.

By the Lord Chief Justice Cockburn: The bottle I have in my hand contains dry, but not anhydrous acid, and I can produce arseniate of aniline by mixing it with aniline. The water present is water of constitution, but not mechanical water, and the whole of it enters into the aniline to form arseniate of aniline. Mechanical water would not combine with the arseniate of aniline produced.

By Mr. Hindmarsh: I do not know in what proportions arsenic acid and aniline unite to form arseniate of aniline. We use in practice about equal proportions. The whole of the arsenic acid, practically, is then converted into arseniate of aniline. There may be free aniline. I do not know to a per centage how much water there is in what we call dry arsenic acid. Our workmen know the exact point at which to stop the boiling. Arsenic acid is very deliquescent, and rapidly absorbs water from the atmosphere.

Mr. Hindmarsh: Suppose arsenic acid to be made either according to Graham or Fownes, and you wanted to keep it dry, would you not exclude the atmosphere from it?—Witness: According to Graham, it is anhydrous, and, therefore, I do not think it necessary to exclude the atmosphere.

Mr. Hindmarsh: Does he not say it absorbs moisture?—Witness: That may be an error.

Mr. Hindmarsh: But does he not say so?—Witness: It cannot be anhydrous, and yet have a great affinity for water—I am speaking of anhydrous arsenic acid. Mr. Graham may be mistaken. I applied for a patent, but, as far as I recollect, I did not mention solution of arsenic acid. In substance, unquestionably, I mentioned solution of arsenic acid, because I found that answer my purpose at the time.

Mr. Hindmarsh: Is not this what you described? "In carrying out this invention, I take a strong solution of arsenic acid, to which I add aniline, toluidine, cumidine, or mixture of the same. I then heat the mixture in a still, or other convenient vessel, until the colour is sufficiently developed."—Witness: That is my provisional specification. I have made the colour with dry arsenic acid. I cannot recollect whether I added water to the experiment. Dry arsenic acid is not mentioned in my specification. Some months after we purchased Medlock's patent, I became aware of Delaire and Girard's process. They propose to use definite quantities of arsenic acid, aniline, and water, and apply heat to the mixture (see Chemical News, vol. vi., p. 314), for the purpose of converting aniline into a red dye. I have tried the process experimentally with certain definite quantities of aniline, arsenic acid, and water. When the whole of the water is driven off, the result is, to convert substantially the whole of the aniline into red dye. Substantially, we use that process. We do not use the exact proportion of water the patent states; we use the smallest quantity we can possibly do with, which saves time in boiling off. The use of water is not necessarily essential to the conversion of acid and aniline into the arseniate of aniline. The arseniate of aniline would be formed whether the arsenic acid was in a solid or a liquid state. It is easier and cheaper to use the liquid solution of arsenic acid, and it is less dangerous than it is to boil that liquid down to a dry state. The main point is saving the men's health, but I admit that it is a little cheaper.

Re-examined by Sir F. Kelly: Dry arsenic acid is known as an article of commerce. We had manufactured and sold it before the date of Medlock's patent. I believe it is largely used in calico-printing. We do not mix that dry arsenic acid with aniline. Practically, we use the solution—the dry arsenic acid in solution in water.

Lord Chief Justice Cockburn: Does it continue, when in a state of solution, to be dry arsenic acid?—Witness: No, my lord.

Sir F. Kelly: Dry arsenic acid is still in existence, though in a state of solution.

Lord Chief Justice Cockburn: No, no; arsenic acid in solution is not dry arsenic acid.

Sir F. Kelly: It is dry; at least, so we consider.

Lord Chief Justice Cockburn: The witness does not say so.

Sir F. Kelly: If your lordship will allow me, I think I can show that it is. The distinction is as perfect as possible. (To the witness): In the solution, does the water at all combine with what you call "dry arsenic acid"?—Witness: No more than in a solution of common salt in water.

Lord Chief Justice Cockburn: The solution implies that it is only a mechanical combination.

Sir F. Kelly: Whereas, the water which exists in the dry acid, and which distinguishes it from anhydrous, is in chemical combination?—Witness: In constitution. That is the distinction between water of constitution and water of solution. There are three conditions or states of arsenic acid:—There is the anhydrous, or acid absolutely free from water; there is the dry, which has a certain proportion of water, but in constitution or combination; and what we may call the wet, or acid held in solution. We can, and have used the acid in the second condition for the production of the two dyes; but it is more advantageous to use the solution with about one-third, or thirty per cent., of water. In this case, all the mechanical water comes off before the colour is produced; and it is, undoubtedly, the dry arsenic acid of commerce which operates on the aniline to produce the colour. We used water and arsenic acid nearly a year before I saw Delaire and Girard's patent. We have often sold dry arsenic acid to Dr. Medlock. I had no knowledge of Heilman's patent until I saw it in print. When arsenic acid is made anhydrous, the atmosphere has scarcely any effect on it. The description of the anhydrous acid given by Kopp (*Annales de Chimie et de Physique*, T. xlviii., p. 109), is quite correct. "The different acids, raised to a dull, red heat, furnish the anhydrous acid. This is no longer an acid, but is an inert body, having no action on litmus, insoluble in water, ammonia, &c. It may remain exposed to moist air for days without becoming wet." When books speak of the acid being deliquescent, they mean the dry acid, not the anhydrous. The part of Dr. Medlock's specification relating to the application of heat is not accurately expressed; but no practical man would be misled by the description. He would see that the heating was essential, and must be resorted to.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

December 18, 1862.

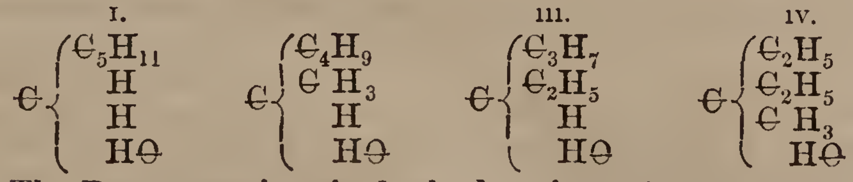
Professor A. W. Hofmann, *LL.D., F.R.S., President, in the Chair.*

The minutes of the previous meeting having been read and confirmed, the following gentlemen were balloted for, and unanimously elected fellows of the Society:—Mr. Edmund J. Mills, Assistant in the Chemical Laboratory of the University of Glasgow; Mr. Alexander T. McChattie, Mr. John Hooker, and Mr. William A. Dixon.

Dr. Frankland made an interesting verbal communication to the Society in reference to the subject-matter of a letter addressed to him by Mr. Wanklyn, in which were described the results of experiments recently made by that gentleman, in conjunction with M. Erdmeyer, upon "Mannite, and some of its Derivatives." By treating mannite with hydriodic acid, the iodide of the hexyl radical was directly formed, with production of water and liberation of free iodine, the change being expressed by the following equation:—

$$\text{Ȼ}_6H_8(H\text{Ɵ})_6 + 11HI = \text{Ȼ}_6H_8I + 6H_2O + 10I.$$

The action of oxide of silver upon this iodide then furnished the corresponding alcohol, which had, however, a lower boiling point, 136° C., than the hexylic alcohol previously described by M. Faucher, which boiled at 151° C. It was, therefore, isomeric, and would probably bear the same relation to the body last-named that the new amylic alcohol of M. Wurtz, which boils at 105° C., bore to the ordinary fousel oil of twenty-seven degrees higher. The substance prepared by Mr. Wanklyn is described as being readily decomposed into hexyline and hexylic acid, and incapable of forming a sulpho-acid. In reviewing the constitution of hexylic alcohol, Dr. Frankland sketched upon the board a series of possible formulæ, by the first of which he preferred to represent the atomic arrangement of this body. They were as follows:—



$$\begin{array}{cccc} \text{I.} & & \text{III.} & \text{IV.} \\ \text{Ȼ}\left\{\begin{array}{l} \text{Ȼ}_5H_{11} \\ H \\ H \\ H\text{Ɵ} \end{array}\right. & \text{Ȼ}\left\{\begin{array}{l} \text{Ȼ}_4H_9 \\ \text{Ȼ}\,H_3 \\ H \\ H\text{Ɵ} \end{array}\right. & \text{Ȼ}\left\{\begin{array}{l} \text{Ȼ}_3H_7 \\ \text{Ȼ}_2H_5 \\ H \\ H\text{Ɵ} \end{array}\right. & \text{Ȼ}\left\{\begin{array}{l} \text{Ȼ}_2H_5 \\ \text{Ȼ}_2H_5 \\ \text{Ȼ}\,H_3 \\ H\text{Ɵ} \end{array}\right. \end{array}$$

The President inquired whether the action of ammonia upon this body had yet been made the subject of experiment?

Dr. Frankland stated that no information had been given him on this point.

Dr. Odling thought it was premature to be engaged already with theoretical considerations regarding the possible constitution of a body about which so little appeared to be known. With the progress of experiment, certain reactions might, in due time, be discovered, by which the atomic arrangement would be indicated with a greater degree of certainty; but even with the aid of such further information it was often difficult to arrive at an ultimate decision in regard to the real nature or constitution of organic bodies, since, by varying the conditions, widely different radicals or combinations were in turn eliminated, and the inquirer was perplexed as much as ever in attempting to select that reaction or series of changes which would best explain the true constitution of the substance under investigation. He was himself almost tempted to believe that the simple expression of the kind and number of elementary atoms in a compound body would be generally the most appropriate formula by which a body could be described.

Dr. Frankland remarked, that the provisional announcement of a series of possible formulæ appeared to incite to further investigation, and sometimes these hypotheses were corroborated by the results of more extended research. He begged to instance the theory of the ammonium type, originally proposed by the President, which had been fully substantiated by later experiment, and had done so much good service in classification and discovery.

The President then gave a most interesting account of some experiments he had lately been making upon a colouring matter shown in the French Department of the International Exhibition. The substance was in the form of beautiful crystals of a rosanilic character, with a green metallic reflection; and, by the kindness of M. Ménier, he was put in possession of a sufficient quantity to enable him to undertake their chemical examination. He soon identified them as being the chinoline blue of Mr. Greville Williams; they were called, however, "cyaniline," or "cyanine"—a commercial name, which would lead one to an erroneous conception regarding their nature, since they were not basic in their character, nor did they contain aniline. The solution in water or alcohol had a magnificent blue colour, being, however, more fugitive than beautiful; and, as evidence of the commercial importance of rendering this dye permanent, it might be stated, that the Société de Mulhouse had offered a premium of 10,000 francs to any one who should succeed in accomplishing this object. He would candidly admit, however, that the result of his own experiments had not been practically successful, and that he was not yet in a position to claim the prize. To return to the chemical question, the crystals were found to contain iodine, and, on more minute examination, were shown to consist of two bodies in intimate admixture, and very difficult to separate. He proceeded to decompose the iodide by the action of oxide of silver upon the alcoholic solution, and the base, or bases, so obtained were converted into chlorides, and treated with bichloride of platinum in such a way as to effect partial precipitation; and by comparing the first product with the last there was evidence of a separation into distinct bodies having been effected. The platinum salts respectively were decomposed by sulphuretted hydrogen, and yielded products from which the original composition of the crystals could be deduced. The two iodides were represented by formulæ* as follows:

I. $C_{30}H_{39}N_2I$.
II. $C_{28}H_{35}N_2I$.

Referring then to Mr Greville Williams' specification, he ascertained that the blue dye was produced by the action of iodide of amyl upon the chinoline bases. This series of bases would be described by the following formulæ:

Chinoline	C_9H_7N.
Lepidine	$C_{10}H_9N$.
Cryptidine	$C_{11}H_{11}N$.
Vertidine	$C_{12}H_{13}N$.

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* Throughout Dr. Hofmann's communication the atomic weight of carbon is represented by 12.

If now the first-mentioned component of the crystals be viewed as an ammonia, it becomes related to the iodide of amyl-lepidine ammonium, and the second compound to the iodide of amyl-chinoline ammonium, thus—

$$\text{I.}\quad 2(C_{10}H_9N) + 2(C_5H_{11}I) = C_{30}H_{40}N_2I_2.$$
$$\text{II.}\quad 2(C_9H_7N) + 2(C_5H_{11}I) = C_{28}H_{26}N_2I_2.$$

It will be seen that the union of two equivalents of iodide of amyl furnishes, in each instance, compounds differing only from those present in the crystals by the elements of hydriodic acid; and Von Babo, who analysed some of these products in 1856, obtained this splendid colouring matter by the action of potash upon the first of these iodides. Only half of the iodine is removed, the reaction being as follows:—

$$2(C_{10}H_9N,C_5H_{11}I) + KO = KI + HO + (C_{30}H_{39}N_2)I;$$

and by the aid of the salts of silver the corresponding oxide, chloride, bromide, etc., could be produced. On the other hand, it was possible to destroy the blue colouring matter by the action of hydriodic acid; and, on cooling a concentrated solution, yellow needle-shaped crystals were obtained.

$$C_{30}H_{39}N_2I + HI = C_{30}H_{40}N_2I_2.$$

The latter is the same, therefore, as the iodide of amyl-lepidine ammonium, but double the formula, or

$$2(C_{15}H_{20}NI).$$

The corresponding chloride forms double compounds by uniting with two equivalents either of bichloride of platinum or terchloride of gold. It became interesting to study the action of heat upon the iodide, in order to ascertain whether it would split up in a similar manner to the iodide of tetrethyl-ammonium. Under these circumstances, it was anticipated that it would be resolved again into iodide of amyl and two equivalents of lepidine, with formation of amylene as the residual product. A tube surrounded with ice was adapted to the apparatus in which the distillation was conducted, and the expected product was duly collected, which had the boiling point, and all the characters of ordinary amylene. The following equation correctly represented this charge:

$$C_{30}H_{39}N_2I = C_5H_{11}I + 2(C_{10}H_9N) + C_5H_{10}.$$

In conclusion, he was happy to remark that the theory of ammonias which he propounded in 1852 proved true also of the tinctorial ammonias.

Mr. Church stated that he had been for some time past engaged, in conjunction with Mr. Greville Williams, with the chemical examination of these same products. They had, however, been working upon the dye obtained from pure chinoline, which crystallised in the form of square prisms. The results were generally the same, although higher in the series, than those announced by Dr. Hofmann. The blue dye was of so changeable a nature, and faded so rapidly on exposure to sunlight, that Mr. W. H. Perkin had no difficulty in taking a photograph upon a surface prepared with the dye.

Mr. Frederick Field read a paper on "*Some Reactions of the Hyposulphites.*" The author referred to the use of hyposulphite of soda as a solvent for the chloride and iodide of silver in photography, and to certain facts in reference to these bodies, which formed the subject of a communication to the Chemical News.† Besides those already named, there were many other substances, ordinarily insoluble, which could be readily dissolved in the same manner. Sulphate of lead was very soluble, even the natural crystals of this substance being rapidly attacked. The chromate of lead was almost insoluble, and the author was induced to attempt the separation of chromate and sulphate of lead in this way, which would have served a useful purpose in the analysis of chrome iron ore, when, after fusing with bisulphate of potash, the soluble chromate, together with a large amount of sulphate, were present in the same solution. It was found, however, that the accuracy of the process was impaired by the slight solubility of the chromate of lead. The iodide of lead dissolved freely in hyposulphite of soda, forming a colourless solution. The suboxide of copper and the hydrated protoxide were very soluble. On heating such solutions, black sulphide of copper was precipitated. The scarlet iodide of mercury was likewise very soluble, and, on boiling this solution, the sulphide of mercury was thrown down in the red modification. Sulphate of lime was soluble to a considerable extent in hyposulphite of soda, and not re-precipitated on boiling—a circumstance which led the author to believe that this salt might be used with advantage in steam-boilers to prevent incrustations of sulphate of lime. The sulphates of baryta and strontia were, on the other hand, perfectly insoluble.

The President, after coinciding with Mr. Field in the opinion, that the method of fusion with bisulphate of potash was unsatisfactory in the analysis of chrome ores, stated that he preferred to employ a mixture of borax and caustic potash, which, according to his experience, acted more energetically upon these refractory minerals than any kind of flux in common use.

The meeting was then adjourned until Thursday, January 15.

SOCIETY OF ARTS.

November 26, 1862.

Dr. A. W. Williamson, *F.R.S., in the Chair.*

Mr. B. H. Paul, Ph.D., read a paper on the "*Utilisation of Peat, with reference more particularly to the Manufacture of Hydrocarbon Oils.*"

The application of peat to some useful purpose is a subject which has at various times attracted considerable attention; a vast amount of inventive ingenuity has been bestowed upon it; it has given rise to very sanguine, and I may say, in some cases, very exaggerated expectations, and, as a natural consequence, it has been a source of proportionate disappointment. Notwithstanding the numerous attempts and proposals that have been made for utilising peat, very little has been done as to inquiring into and elucidating what are really its capabilities and disabilities as a material for use in the arts. But a knowledge of these circumstances is an essential preliminary to any successful application of it, and I hope to be able, in some degree, to contribute to the acquisition of such a knowledge by bringing before this Society the results of several years' practical experience in the prosecution of this subject.

Taking it for granted that the existence of enormous deposits of peat in various parts of the kingdom is sufficiently well known, and having regard more especially to its technical value, it will be unnecessary for me to enter into any consideration of the origin and formation of peat, or of the different views entertained on that subject. It will be sufficient for my purpose to consider peat as it exists now; and with regard to this point there are two modes in which it occurs, which I believe to be of importance as regards its application to useful purposes. In one case we find peat deposits in the form of what are called peat-bogs, masses of peat of considerable superficial extent, and generally of great depth, 20 ft., 30 ft., and sometimes upwards of 100 ft. deep, where the uppermost layers are of a loose, fibrous, or grassy texture, and saturated with water to such an extent, in some instances, as to be incapable of affording any support to the foot. When the water is drawn off from these bogs by drainage, the peat is generally found to vary in character according to the depth at which it is situated, gradually becoming darker in colour, more compact, and having less evident indications of vegetable structure. At the bottom of such bogs the peat is generally a black pasty mass of a clayey consistency.

In the other case we find, situated on the slopes of

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† *Vide* Chemical News, vol. iii. p. 17.

mountainous country, peat deposits, which are never of very great depth, generally from 12 ft. to 2 ft., and where the peat is sufficiently solid to be walked upon with ease. In these deposits the peat is of a more uniform texture and character throughout than in bogs, although there is always a greater or less difference between the peat at the surface and that at the bottom. These deposits of mountain peat are very common in the Highlands of Scotland and in some parts of Ireland.

Mountain peat offers very much greater facilities for cutting than bog peat, and it is generally of much better quality. Bog peat, when dried, has very much the appearance of pressed hay; it rarely has a density of more than 300 or 400—water being 1000,—and the cubic foot weighs only from 15 lb. to 30 lb.; it would, perhaps, be useful to distinguish it by the term "turf," from the true mountain peat, which when dried is dark brown or quite black, with little or no remains of plants in it; capable of taking a high polish when rubbed, and of a density greater than that of water, the cubic foot weighing from 53 lb. to 78 lb.

The method of cutting peat in the Highlands of Scotland is very different from that adopted for cutting peat from bogs. In the first place, trenches are opened at distances of about ten yards apart; and, according to the nature of the ground, these trenches are made from 50 to 400 or 500 yards long. After removing the surface sod at the places where the trenches are to be cut, for a width of three feet, along the whole line of the trench, the peat cutter digs out the peat with a peculiar-shaped tool, in slices of about a foot square, and three or four inches thick. As fast as these slices are cut, another man takes them off the peat iron, and throws them on the surface, so as to spread them out as much as possible. In this way prisms of peat, measuring three feet in width and depth, are cut out at intervals of ten yards, and the number of slices cut in each trench are just as many as a man can throw on both sides of the trench, without shifting his position, except from one end of the trench to the other, as the cutting advances.

In succeeding years the peat is cut from the two banks thus formed in each trench, to a width of only eighteen inches, and a depth of three feet. The advantage of this system of cutting is, that there is no necessity for removing the peat by barrows to the spreading-ground, a proceeding which is attended with considerable expense for labour. When the peat is cut in this way from a bank 150 yards long, it will give 75 cubic yards of wet peat, and the number of slices into which this is divided will be about 8000. Then, as the banks are ten yards apart, there are five yards' width of drying-ground to each bank, or a superficial area of 6750 square feet to each bank of 150 yards long. Cutting it in this way every year, it would take ten years to remove the whole of the peat to a depth of three feet. As the banks are cut away in successive years the area of spreading-ground on the surface is reduced, and some of the peat has to be spread at the bottom of the trench, the area of which increases as that of the banks' surface is reduced by the cutting.

The peat cut to a width of eighteen inches, and a depth of three feet, from a bank of 150 yards long, is what is called an iron's work, and the 75 yards of peat so cut yields about ten tons of dry peat, so that to cut 7000 or 8000 tons of dry peat would require 750 irons' work, or banks about 64 miles in length, and extending over an area of about one-fifth of a square mile. This area of ground would supply 7000 or 8000 tons every year for ten years.

The cutting and spreading of peat in this way forms but a proportion of the cost of the dry peat. A far more considerable portion of its cost results from the labour of collecting the dry peat and bringing it to the place where it is to be used. Herein lies one of the greatest difficulties of employing peat on any very extensive scale. Whatever mode may be adopted for collecting the dried peat to one spot for use, the cost of carriage will increase in proportion to the increase in the quantity of peat consumed at that spot. Thus, for instance, in the case of a factory consuming 7000 tons annually, it would be requisite to carry the peat, on the average, a distance of one-tenth of a mile; if the quantity consumed were 70,000 tons, it would be requisite to carry it an average distance of half a mile; and if the quantity consumed were 300,000 tons a year, it would have to be carried an average distance of two miles, or a mile and a-half, inasmuch as the cutting ground would extend over an area of eight square miles.

The extent to which this disadvantage affects any particular instance of the use of peat will depend very much on the skill exercised in laying out the ground for cutting the peat, and in disposing the banks and tram-roads, or other means for conveying the peat to the place where it is to be used; but it is a disadvantage which can only be reduced by such means within the smallest possible limits, and which is quite inseparable from the use of peat on a large scale.

Another prominent difficulty attending the use of peat consists in obtaining it in a dry state, fit for use as fuel or otherwise. Mountain peat, as it occurs naturally, contains as much as 80 per cent. of water, even when it has been well drained, and bog peat often contains very much more. Consequently, to obtain one ton of dry peat, five tons of material have to be dug and spread, and four tons of water have to be got rid of by evaporation. When mountain peat is cut in slices, as I have described, and spread out on the ground during dry weather, the drying goes on rapidly, the surface of the pieces acquire a kind of skin, which is not wetted again by rain, and the peat, in the course of a week, is sufficiently hardened to be handled. The pieces are then set up on edge, so that the air may play on both sides, and, in the course of six weeks or two months, they are dry enough to be stacked or heaped up. But, unfortunately, peat districts are generally remarkable for a very moist atmosphere and for a great frequency of rain. In the Highlands of Scotland, and in the Hebrides, on an average, there is rain four days out of six, and it is only during the months of May, June, and July that you can expect to have any continuance of weather favourable for drying peat. It is necessary, therefore, to obtain the utmost advantage of that period for the drying of the peat; and to do so, the peat must all be cut before the end of May at latest. On the other hand, if the peat is cut during frosty weather, and becomes frozen, it crumbles to powder when the thaw comes, and for this reason it is not safe to commence the cutting at all before April, or even May. As a rule, it might be said that the month of May is the only time available for cutting peat in the Highlands of Scotland, and more especially in the Hebrides, so as, on the one hand, to avoid the destruction of the peat by frost, and, on the other hand, to insure the best possible chance of getting it well dried.

Notwithstanding the general moist condition of the air in those places, the boisterous winds which prevail are very efficacious in drying the peat; and if, during the months of May and early part of June, the peat has got a certain amount of drying, and a skin has formed on the surface of the pieces, it may be considered safe, whatever kind of weather there may be afterwards. It may then remain on the ground, set up in little heaps, till the autumn, and will get the advantage of whatever dry weather there may be. Of course, even in this case, the quality of the peat will depend on the weather; but if the cutting is not finished by the end of May, there is always less probability of getting the peat in good condition.

It will be evident, from these circumstances, that the cutting of peat, to supply a factory consuming any large quantity, must be an affair requiring considerable management, so as to get the work done in the short space of time available for it, and the difficulty of effecting this increases in proportion to the quantity of peat required to be procured.

Two men working together, one cutting and the other casting the peat, will, in good weather, get through about one iron's work in a day, equivalent to ten tons of dry peat; so that, if they were able to work every day during May, they would cut from 200 to 300 tons of peat; and to get 10,000 tons cut and spread, 100 men would be required for the whole month; and to get 300,000 tons cut and spread, would require 3000 men to be employed for the whole month. It is unnecessary to dilate upon the difficulty of getting such a large number of men together for the work, and of organising a system for measuring the work done, and carrying on the general supervision of the peat cutting on such a large scale; but I may mention, that there are circumstances connected with the habits of the people in those districts which are in some degree favourable to the carrying out of such an operation. The people are almost all fishermen, and the fishing season does not commence until the end of May or June; so that it would be possible to obtain many of these men before they go to the fishing, and thus the inconvenience of employing a large number of men for a short period would not be so great there as it would in most other instances. Moreover, these people are accustomed to hutting themselves, with no small degree of comfort, in huts or bothies built of the surface sods of the peat, and they live in these as a rule throughout the Hebrides; so that a squad of 200 or 300 men find, on the ground where they are going to work, the materials for their encampment, and it is interesting to see the dexterity and quickness with which they construct these bothies.

(To be continued.)

NOTICES OF PATENTS.

2474. *Treating Oils obtained by the Distillation of Bones.* J. Stuart, Poplar, London. Dated October 4, 1861. (Not proceeded with.)

This process consists in digesting crude bone-oil with hydrochloric or diluted sulphuric acid; then to separate the highly coloured acid, and add thereto a sufficient quantity of alkali to precipitate a brown colouring matter, which may be used as a pigment. The oil, after this treatment, is mixed with carbonate of potash and oxide of iron, introduced into an iron retort, and subjected to distillation, when a refined bone-oil passes over, the residual product in the retort serving for the preparation of ferrocyanide of potassium. For each gallon of oil, one pound of hydrochloric acid, half a pound of carbonate of potash, and six ounces of oxide of iron may be used, but the inventor does not restrict himself to these proportions.

2525. *Artificial Manure.* T. Tidmarsh, Dorking. Dated October 9, 1861.

For the manufacture of a manure of special excellence the inventor collects any available refuse matters, whether of animal or vegetable origin, such as peat, turf, stubble, bones, hide, &c., and reduces them to an alkaline ash by burning. With this he further mixes a variety of nitrogenous materials, such as farmyard manure of different kinds, and to every ton weight of the mixture adds four bushels of quicklime and twenty-five pounds of oil of vitriol!!

2527. *Charging Illuminating Gas with the Vapour of Hydrocarbons.* W. J. Williams, Warnford Court, London. Dated October 10, 1861. (Not proceeded with.)

This invention consists in causing illuminating gas, in its passage from the meter to the burners, to pass through a series of perpendicular rows of threads or cords saturated with the liquid hydrocarbon, by which it becomes charged with the vapour of the same; and as the gas is liable to become overcharged, and cause a waste of the hydrocarbon, besides being in cold weather further troublesome by obstructing the flow of gas in the pipes, the inventor causes the gas to pass through a condenser, wherein the excess of hydrocarbon vapour is separated, and the liquid resulting from the condensation flows back to the evaporating-chamber, whilst the gas in a properly charged state passes on to the burners.

2540. *Method of Obtaining Ammoniacal Salts and other Valuable Products from Liquors or Substances containing Ammonia, and for utilising the Residuum.* C. N. Kernot, West Cowes; and M. D. Rucker, Fenchurch Street, London. Dated October 11, 1861.

The patentees claim the precipitation of ammonia from solution by sulphate of zinc, or other sulphates, or by the superphosphates of lime and magnesia, as prepared by the action of sulphuric or other acid upon bones, coprolites, apatite, or phosphorite.

2550. *The Application of a New Material in the Manufacture of Paper and Cardboard.* V. Pirson and A. Deyeser, Brussels. Dated October 12, 1861. (Not proceeded with.)

The inventors propose to use the Spanish trefoil or lucerne for the manufacture of paper pulp, treating it in the same manner as linen and cotton rags, with which it may sometimes be used with advantage. The plant is picked over by hand, boiled with caustic alkali, washed, bleached and macerated, until the fibre is uniform in colour and quality.

Grants of Provisional Protection for Six Months.

3044. George Smith, Cobden Street, Stewart's Lane, Battersea, Surrey, "Certain improvements in obtaining colouring matter."

2134. William Maugham, Prospect Place, Wandsworth Road, Surrey, "Improvements in the manufacture of effervescent beverages."—Petition recorded July 28, 1862.

3066. Edmund Small Cathels, Shrewsbury, Shropshire, "Improvements in apparatus used in the manufacture of gas."

3069. Samuel Roberts, Sheffield, "An improvement in frames for containing stoppered bottles and jars."

3085. Christopher Binks, Parliament Street, Westminster, "Improved methods of obtaining oxygen and chlorine gases."

3093. Jacques Arbos, Barcelona, Spain, "Improvements in generating certain gases for lighting and heating, and in apparatus employed therein."

3117. George William Oldham, Moll Spring, Honley, near Huddersfield, Yorkshire, "Improvements in preparing and dyeing silk waste, flax, hemp, Indian or China grass, or other similar fibrous substances."

3157. John Moule, Seabright Place, Hackney Road, Middlesex, "An improved method of deodorising mineral oils and hydrocarbons."

3159. Albert Louis Woolf, Birmingham, "A new or improved metallic alloy."

3167. Thomas Marwood Elton, St. Luke's Soap Works, Golden Lane, Barbican, London, "Improvements in the manufacture of soap, and in the machinery employed therein."—Petitions recorded November 25, 1862.

3172. Joseph Francis Foveaux, Strand, London, "Improvements in apparatus for pulverising or dividing liquids into spray."—A communication from Amatus Luer, Paris.

3175. Alfred Vincent Newton, Chancery Lane, London, "An improved mode of preparing oxide of zinc as a pigment."—A communication from George Lewis, Philadelphia, U.S.—Petitions recorded November 26, 1862.

3252. James Braddock, Droylesden, Lancashire, "Certain improvements in machinery or apparatus for effecting the separation of impurities from the water employed in steam boilers, and also for effecting the circulation of the said water."

Notices to Proceed.

2134. William Maugham, Prospect Place, Wandsworth Road, Surrey, "Improvements in the manufacture of effervescent beverages."—Petition recorded July 28, 1862.

2170. Elijah Freeman Prentiss, Birkenhead, Cheshire, and Robert Adam Robertson, Liverpool, "Improvements in obtaining products from rock oil, coal, coal tar, and other like mineral substances, in a more or less pure and deodorised state, and in the apparatus to be used therefor, and which is also applicable to distillation in general."—Petition recorded July 31, 1862.

2235. Thomas De la Rue, Westbourne Terrace, Middlesex, "Improvements in the manufacture of pigments and writing inks."—Petition recorded August 9, 1862.

CORRESPONDENCE.

Black Dye from Aniline.

To the Editor of the Chemical News.

Sir,—In reply to a notice in the Chemical News of December 20, 1862, and signed by Mr. W. Cort Wright, I beg to say that I have seen a black from aniline, dyed and printed in July, 1860, which was discovered by Mr. John Lightfoot, jun., Broad Oak Print Works, Accrington, a swatch of which I beg to enclose.—I am, &c.

J. C. Derbyshire.

Church, near Accrington.

Red Chalk.

To the Editor of the Chemical News.

Sir,—I have made analyses of several varieties of red chalk. On my return to town I will look over my notes, and will endeavour to send you, for insertion in the Chemical News, an abstract of my results. One of the specimens, in very hard nodules, contained at least four times the amount of sesquioxide of iron found by Mr. Clapham. I can confirm his remark as to the almost complete absence of sulphate of lime from the red chalk, an observation almost sufficient to negative the hypothesis, that the ferric oxide of this formation is derived from iron pyrites, especially as no separate crystallisations of gypsum have been as yet detected in the stratum in question.

I am, &c. A. H. Church.

Double Equivalents.

To the Editor of the Chemical News.

Sir,—I was much surprised on observing the onslaught made on Gerhardt's notation by parties signing themselves "F.R.S.," and "F.C.S."

Their attack abounds in strong expressions, which should be avoided in all calm reasoning. Their proofs are not to the point, and, therefore, whether "facts" or not, are valueless in this case.

They take water as an example. Why single out this compound? Why not take peroxide of hydrogen? It is composed of H and O in the ratio of 1 to 16; therefore, by reasoning analogous to theirs, these "must naturally be the equivalents for these bodies," viz., the numbers 1 and 16.

They admit that their equivalent for H occupies two volumes, whilst that for O occupies only one volume. Distinctions are never drawn without reason. What reason is there for one here? The equivalents of the chlorine group compared with the equivalent for H are synonymous with = volumes of the bodies in the gaseous state. What reason have we for saying that such is not the case with the oxygen group? If there be no difference, then the formula for water is clearly $H_2\Theta$, or 2 equivalents of H to 1 equivalent of Θ.

The formula Ꞓ_6H_7N, when $\text{Ꞓ} = 12$ is just the same practically as the formula $C_{12}H_7N$, where $C = 6$. The one serves as well as the other for calculating the composition of aniline. Surely 6 times 12 = 12 times 6.

The remainder of their article consists of strong statements, for which they give no proof.

Practically, it matters not which system be chosen. See Chemical News, vol. i., p. 105, "The Radical Theory in Chemistry." Theoretically, I think that Gerhardt's notation is more systematic, and develops analogies better than any other. See Chemical News, vol. i., p. 220, "On Acids and Salts." In conclusion, I am prepared to prove my statements at greater length, either in your columns or privately. I undertake, without fear, to show that organic chemistry is anything but "a chaos."

I am, &c. T. Fairley.

To the Editor of the Chemical News.

Sir,—Your correspondents, "F.R.S." and "F.C.S.," will render great service to rational chemistry, by continuing to show up, in the trenchant style which characterises their last letter, the wretched absurdities involved in the so-called polyatomic views, which now jostle us at every turn, and even threaten to "push us from our stools."

But I could wish they had proceeded further in their attacks upon unnecessarily multiple equivalents, and had striven to explode the older fallacy of making carbon 6 instead of 3.

To adopt their own happy language, I defy any chemist to show the slightest advantage to be gained by this doubling, or to refute the following simple facts, which are directly opposed to it:

The equivalent of hydrogen being taken = 1, we observe that when hydrogen and carbon unite to form marsh gas, 1 lb. of H unites with 3 lbs. of C; and, therefore , being the equivalent of H, 3 must naturally be the equivalent of C. If we take volumes, we find 2 vols. of H combine with 1 equivalent, or 3 parts, of C; here, also, the 2 vols. of H weigh 1 lb., whilst the C with which it is combined weighs 3 lbs. The marsh gas thus formed equals 1 vol.; and if (to establish a similarity between this gas and organic vapours) we make this 1 vol. equal to 4 vols., we have evidently in these 4 vols. 4 equivalents of C = 12, and 4 equivalents of H = 4. If, therefore, we double the equivalent of carbon, we must also double that of hydrogen, and, consequently, the equivalents of all the elements, without exception. It is evidently more simple to double nothing at all! If we wish to represent 4 vols. of marsh gas, can it not be done by writing C_4H_4?

We have hitherto allowed our doubling friends too much their own way in these matters, ever letting them obtrude their nonsensical views without contradiction. But to quash this growing heresy, a very different plan must be pursued. For my part I would gladly join with "F.R.S." and "F.C.S." in a determined effort to extinguish utterly all new doctrines, which we do not comprehend, and cannot, for the lives of us, appreciate.

I am, &c.

Not an Ephesian.

London, December 24.

Estimation of the Soluble Sulphides in Crude Sodas.

To the Editor of the Chemical News.

Sir,—I have seen in your journal a process given by M. Lestelle for the estimation of sulphide of sodium in soda ashes and in "vat liquors" by means of nitrate of silver in ammoniacal solution, and, with your permission, I wish to point out to your readers its total inapplicability for the purpose. All crude sodas in which sulphide of sodium is present contain, also, sulphite of soda; and the liquors, besides these salts, often contain hyposulphite of soda—

sometimes sulphite and hyposulphite are present, but no sulphide. Premising this, I will now give the reactions of these salts with nitrate of silver under the conditions proposed by M. Lestelle.

A solution of sulphite of soda with ammonia, when heated, and ammoniacal nitrate of silver added, gives, at first, no permanent precipitate; but in a few moments a grey precipitate of metallic silver appears. The change is thus represented—

$$NaO,SO_2 + AgO,NO_5 = NaO,NO_5 + SO_3 + Aq.$$

Hyposulphite of soda, when treated as above, gives no permanent precipitate, but in a short time, longer than in the case of sulphite, a brown coloration is visible, which, especially if more silver salt be added, rapidly accumulates until a deep black is produced. The subjoined equation shows this change—

$$NaO,S_2O_2 + AgO,NO_5 = NaO,NO_5 + SO_3 + AgS.$$

A mixture of a solution of sulphite and hyposulphite gives at once, under the same circumstances, a coloured precipitate, the greater part of which re-dissolves, but a black residue remains; and by continuing to add nitrate of silver, the whole quickly becomes quite black, and in appearance is not distinguishable from the precipitate produced in a solution containing sulphide only. The precipitate in this case is a mixture of sulphide of silver and metallic silver.

A solution containing sulphide of sodium, sulphite, and hyposulphite of soda, gives at once a black precipitate of AgS, but after the decomposition of the whole of the sulphide of sodium, shown by the addition of acetate of lead to a portion of the solution. When more silver salt is added, the production of a dark brown precipitate, quickly becoming black, continues until the other salts are decomposed.

It is evident, therefore, that M. Lestelle's process cannot yield accurate results. The method is not, indeed, applicable to the detection of a sulphide in such compounds. The best mode of determining the amount of sulphide in such cases is by means of carbonate of cadmium, as recommended some time ago by Werther in the *Jour. fur Prakt. Chem.* I am, &c.

J. W. Kynaston, F.C.S.

Gloucester Street, Dublin.

Analysis of Soap.

To the Editor of the Chemical News.

Sir,—I have delayed addressing you again on the subject of the analysis of soap, in the hope of being able to make further experiments, and so render my remarks more correct; but business engagements, to my regret, prevent me, so I have thought it best to give the result of my experience, even though it be imperfect.

In the first place, I would remark that nearly all, if not the whole, of the soap used in the domestic wash-tub contains rosin. Nor can this addition of rosin to yellow soap be considered an adulteration, as I have noticed you to call it, in your generally good remarks upon new patents. If it is present in undue quantity, then I would call it an adulteration; and, in that case, the consumer would very soon complain of the soap wasting away too fast.

On the Estimation of the Fatty Matter.—If the cake of fat, obtained by adding dilute sulphuric acid to a soap containing rosin (known as pale yellow, &c.), be weighed, the exterior surface being previously dried, it will give about 3 per cent. more fat than really is present, owing, I presume, to the rosin retaining a small quantity of water. So that, in estimating the fatty matter in all soaps containing rosin, it is necessary to heat the fat obtained in order to expel the water before weighing. Whilst expelling the water, it is requisite to stir well to prevent any of the fat being blown away by smart little explosions, which take place so long as water is present, and the absence of which shows that the water is all evaporated, at which point it is better to stop, or some of the fatty matter may be driven off.

On the Estimation of the Water.—The water bath is an extremely tedious affair, and the results very unsatisfactory. I consider the method given by Mr. O'Neill, in his "Dictionary of Calico-printing," preferable, if the soap be well stirred, of simply weighing out 100 grs., and heating in an evaporating basin, even until the soap begins to brown a little. It is, according to my experience, more correct than the old plan, and has the advantage of being done in a few minutes, and is very useful in affording a near approximation to the real amount of water present.

On the Estimation of the Alkali.—This requires very delicate management to get correct results. There is about 6 per cent. of real soda in good hard soap. In estimating it directly by means of test acid, it is difficult to tell the precise moment when sufficient acid has been added. And it is evident that to be half per cent. wrong will make a very important difference. Also the presence of silicate and carbonate of soda, in very many of the cheaper soaps, will diminish very much the value of an estimation of the alkali in the ordinary manner.

To me it appears that the real test of the value of a soap is, the amount of fatty matter it contains; the fat costing more than twenty times as much as the alkali. This important and most expensive article to the soap-boiler is the one that can be the most easily estimated, and that with certainty; for soaps containing no rosin (that is, curd soaps), in the manner usually described; and for yellow semi-transparent soaps containing rosin, with the addition of re-melting the cake of fat obtained, and driving off by heat the water held by the rosin.

In conclusion, allow me to say, that I have offered no suggestions for the better estimation of the alkali, because I wish for further experience; and, in the meantime, I shall be glad to learn from any of your correspondents on the subject. I am, &c. Technologist.

MISCELLANEOUS.

Royal Institution.—The following lectures will be delivered by Professor Frankland, F.R.S.:—On Tuesday, January 6, at 3 o'clock, on "Air and Water" (juvenile lecture). On Thursday, January 8, at 3 o'clock, on "Air and Water" (juvenile lecture).

Royal Polytechnic Institution.—This Institution, still under the able and enterprising management of Mr. Pepper, opened for the holidays, as usual, on boxing-night. It seems somewhat derogatory to the Polytechnic to class it among places of entertainment; but in the highest and best sense it is a place of real entertainment—great amusement being blended with solid instruction. The new experiments introduced by Mr. Pepper in his "Strange Lecture" are of a most striking character; and the spectres in the scene of the "Haunted Man" have wonderfully substantial appearance. They must be seen to be appreciated, and the sight will greatly puzzle the beholder to imagine how such a spectre, so real, and yet so unsubstantial, can be thrown into the middle of the room. This experiment will be the most startling novelty produced at any place of entertainment this season. The other arrangements are almost equally good: some scenes of the pantomine being excellently managed, and exceedingly diverting. Mr. Pepper, we think, may calculate on another very prosperous season.

ANSWERS TO CORRESPONDENTS.

Quæro.—We cannot find the reference.

M. P. T.—The matter is still under consideration.

Received—J. C.; G. B.

W. D. P.—We regret that we are unable to accommodate our correspondent in the way he deserves.

J. H.—A thermometer can be inserted by a contrivance well-known to makers of stilts.

THE CHEMICAL NEWS.

Vol. VII. No. 162.—January 10, 1863.

THE DISCOVERY OF THE METAL THALLIUM.

In another part of this Number, we print at length a translation of the report by M. Dumas on M. Lamy's latest observations on thallium. Our readers will hardly expect, interested as we are in this subject, that we should publish this report without comment. It will be seen that M. Dumas claims for M. Lamy the discovery of the *metal* thallium. M. Le Verrier had already made the same claim for M. Lamy in the journal *La France* for October 22, 1862; and, in reply to a letter of our own in answer to M. Le Verrier, published in the *Cosmos* for December 5, 1862, M. Lamy has since advanced the same claim for himself.

Now, as M. Lamy states (*Cosmos*, December 19, 1862, p. 681) that it is "priority of publication which constitutes priority of invention," we are induced to give a short *résumé* of dates in support of our own claim to the discovery—not only of the new element, but of its metallic character. Our readers will remember that it was in the Chemical News for March 30, 1861, we first announced "*The Existence of a New Element,* PROBABLY *of the Sulphur Group.*" The word *probably* is here of some importance, as showing the doubts we had at the time of the exact nature of the new body—doubts which were further indicated in the title of our next paper—"*Further Remarks on the* SUPPOSED *new Metalloid,*" in the Chemical News for May 18, 1861. Subsequent research soon proved to us that thallium was, in fact, a true metal, but the publication of this discovery was deferred.

M. Lamy's claim for priority of publication, and, consequently, priority of discovery, as advanced by himself, is founded on a communication made to the *Société Imperiale des Sciences, de l'Agriculture, et des Arts*, of Lille, on May 16, 1862. On May 1, 1862, however, the International Exhibition opened, and there, in a case, deposited some days before, and open to the inspection of the numerous scientific men of all countries who were present on the occasion, was displayed several grains of the new body, with the following label—"*Thallium, a New* Metallic *Element, discovered by means of Spectrum Analysis.*" Besides this there was a card, on which was written "*Chemical Reactions of Thallium, by which it is distinguished from every other known element. It appears to have the character of* A HEAVY METAL, *forming compounds which are volatile below a red heat. It is reduced from its acid solutions by zinc in the form of a dense black powder, difficultly soluble in hydrochloric acid, readily soluble in nitric acid.*" The above, we contend, was a publication in the widest sense of the word, and in this publication the metallic nature of thallium was distinctly asserted. The metal, it is true, was exhibited in powder, just as it was obtained by precipitation by means of zinc, but was none the less the pure metal. It was there for the jury of chemists to examine if they thought proper. It was not examined chemically by the jury; no one tested it; and yet M. Lamy, in his letter to the *Cosmos*, has the hardihood to assert, that Mr. Crookes "contented himself with exhibiting to the public and the international jury of Class II., as thallium, some centigrammes of a black powder which was not thallium." We shall make no remark on this assertion of M. Lamy; but, as some of our readers may be inclined to ask why the metal was not exhibited in the form of a button, we shall be excused for going into some detail.

The source from which we extracted the metal, and the compounds exhibited, was sulphur from the Spanish pyrites mentioned in our paper of May 18, 1861. This sulphur contained no more than *one or two* grains of thallium in *a pound*. The metal and compounds we exhibited represented in all about twenty grains of the metal, and the difficulty of extracting this quantity from upwards of fifteen pounds weight of sulphur will be fully appreciated by all chemical readers, when we inform them that the whole of the sulphur had first to be dissolved in nitric acid. We may contrast this, in passing, with the source from which M. Lamy derived his metal, as described by M. Dumas,—namely, the residues of a sulphuric acid manufactory, "which contained thallium in tolerably large quantity, and in a form which made it easy to extract."

Ignorant, at the time, of any richer source of thallium, and having in previous fusions of the precipitated metal discovered that it was rapidly volatilised and lost by oxidation, as described by M. Dumas, it was hardly likely that we should risk the loss of the whole of our small specimen for the sake of exhibiting it in a button; it was, therefore, placed in the Exhibition in the form of powder as precipitated. We might refer to our laboratory note-book, which is open to inspection, to prove that we had obtained the metal and fused it in September, 1861; but as a note in a private book does not constitute a publication, we found no claim on this. Nor do we on the fact, that Mr. Williams saw the metal in our laboratory in January, 1862, as mentioned by him in the Chemical News, vol. v., p. 350. But it may be evidence that we were "aware of the metallic nature of thallium, and acquainted with the essential properties of the new body," to state, that early in April, 1862, we had the following labels printed by Silverlock (as can be proved by a reference to the books of that firm) for the metal and the salts at that time we had prepared:—

Thallium (θαλλος)—Oxide of Thallium—Sulphide of Thallium—Basic Chloride of Thallium—Iodide of Thallium—Sulphate of Thallium—Chloride of Thallium—Nitrate of Thallium—Ferro-cyanide of Thallium—Cyanide of Thallium—Phosphate of Thallium—Carbonate of Thallium—Chromate of Thallium—Thallium, Sublimed—Oxalate of Thallium.

It is sufficient for us, however, that the metal, labelled and described as a metal, was in the International

Exhibition, at its opening, on the 1st of May, 1862, to prove priority of publication to M. Lamy's communication made at Lille, on the 16th of May, 1862.

The fact, that the metal was in the Exhibition, rendered it quite unnecessary for us to do what M. Dumas says we should have done after seeing M. Lamy's specimen. Our metal and two products, the peroxide and sulphide, had been in the Exhibition some time, with the descriptive cards we have quoted; and with regard to M. Dumas' insinuation, that we borrowed from M. Lamy some, if not all, of the materials for the paper read before the Royal Society a few days after we met that gentleman, it may suffice to say, that, as M. Lamy only spoke French, a language we ourselves speak but imperfectly, it was not possible that either of us could have profited much by the interview.

We have no wish to detract in the least from the great merit of M. Lamy's researches. We estimate as highly as any one the skill and industry with which he has worked out the compounds of thallium. But it must not be supposed, as M. Lamy seems to suppose, that we ourselves remained idle during the fourteen months which had elapsed since we remarked the green line in the spectrum. With the limited means at our disposal, and amid other pressing occupations, we had, and have since, been continually engaged in investigating the properties and compounds of the new metal; and all we need say to M. Lamy is, that we heartily congratulate him on his successes, and envy him nothing but his opportunities.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Report on a Memoir of M. Lamy relating to Thallium,[*] *by M.* Dumas.

At the origin of human societies, the arts of procuring fire at will, of cultivating corn, and of extracting metals, were considered so great benefits that the inventors of these arts were ranked among the gods.

At the present day, the metals are so numerous, that the discovery of a new simple body of this class is less astonishing to ordinary men, although the scientific interest attaching to the discovery has not at all diminished. So far from that, in proportion as new metals are pointed out, the characters which appertain to them throw, by comparison, a strong light on the characters, similar or opposite, which are found in the older metals.

As soon as the bold and felicitous labours of Bunsen and Kirchhoff had shown, beyond doubt, that, in studying natural products by spectrum analysis, it was possible to discover traces of metals which ordinary analysis was powerless to recognise, rubidium and cæsium were considered by all chemists as only the two first terms of a long series of new elements. Every one understood that the residues of manufactures, in which, by the elimination of known and useful products, were concentrated the inappreciable traces of useless and unknown substances that the matter originally worked sometimes contained, offered a mine worth exploring.

It was, therefore, natural enough that Mr. Crookes in England, and M. Lamy in France, should submit to spectrum analysis the products of the combustion of iron pyrites, which for some years have played such an important part in replacing sulphur in the manufacture of sulphuric acid; and it is easy to understand, when one has seen it, that the beautiful green line produced in the spectrum by the new body which forms the subject of this report, could not have escaped the observation of either the one or the other.

But, in our opinion, it is neither the process by which the new metal was recognised, nor the material which furnished it, that commends it to our notice. Spectrum analysis has completed its proofs, and manufacturing residues have long since been recognised as fruitful mines to explore. But thallium is destined to mark an epoch in the history of chemistry by the astonishing contrasts exhibited between its chemical characters and physical properties. It is no exaggeration to say, that, in regard to the classification generally accepted for the metals, thallium offers an assemblage of contradictory properties which entitles it to the name of a metallic paradox—the ornithorincus of metals.

We shall not detain the attention of the Academy on the history of its discovery. No one disputes that Mr. Crookes first saw, on the 30th of March, 1861, the green line characteristic of thallium in certain selenium residues; that he recognised it again in the products of a specimen of sulphur from Lipari, and in those of a pyrites from Spain; and that he described and named thallium as a new simple body.

Nor will any one dispute that M. Lamy was the first to isolate thallium, and establish, in the sequel, that it was not a metalloid analogous to selenium and tellurium, as Mr. Crookes, who had never obtained it free and pure, thought; but that it was, in fact, a true metal. M. Lamy announced his discovery to the Société Imperiale of Lille on the 16th of May, 1862, and on the 10th of June he submitted to the jury of chemists in London, in the presence of Mr. Crookes, a beautiful ingot of thallium. If the latter gentleman considered that he had any rights to preserve, he should at once, as is usual in such cases, have taken the members of the jury to his laboratory, and exhibited his notes and his products, instead of silently listening to the communication of M. Lamy, and depositing at the Royal Society, eight days afterwards, a note indicating that he had long been aware of the metallic nature of thallium, and was acquainted with the essential properties of the new body.

The historical point which engages us—for, in chemistry, the discovery of each new simple body has its legend or its history—is determined by two authentic dates: one of these is the 30th of March, 1861, on which day Mr. Crookes announces the existence of a new body which he believes to be non-metallic, characterised by the brilliant green line; the other is the 16th of May, 1862, the day on which M. Lamy makes known the metal as a metal, and who alone possesses it.

It was in the sulphuric acid manufactory of our learned *confrère*, M. Kuhlmann, among the sediment of the leaden chambers fed by Belgian pyrites, that M. Lamy discovered thallium in tolerably large quantity, and in a form which made it easy to extract; for, by a little manipulation, it could be brought to the state of sulphate or chloride, from which combination the metal itself can be easily separated by means of zinc, which takes its place, and precipitates it in crystals, in the same manner as lead.

The Academy will permit us to draw attention to the importance which attaches, in cases of this kind, to positive characters, like those given by spectrum analysis. We shall see, as we proceed, that, beyond his certain conviction and natural penetration, a sure guide was necessary to M. Lamy, to prevent him from going astray in the first steps of the study. In fact, if the green line had not been there to prove incessantly that he was not dealing with lead or a plumbiferous alloy,

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[*] *Comptes-Rendus*, lv., 866.

how many chemical reasons were there for thinking that such was the fact! This metal, which is separated like lead from solutions of its salts by means of zinc, presents the appearance of lead. It has nearly the same colour as lead; is scratched and cut like it. It makes a streak on paper like that which lead produces; it has the same density, and very nearly the same melting point. It possesses the same specific heat. Its solutions are precipitated black by sulphuretted hydrogen, yellow by iodides and chromates, and white by chlorides, just as those of lead are. We do not then hesitate to assert, that, without the aid of spectrum analysis, this curious and important metal must have remained unrecognised; that, even with this help, it was easy to be mistaken; and that M. Lamy has given proofs of great sagacity, when he places, without hesitation, a metal so much resembling lead in its essential properties beside the alkali metals, potassium and sodium, which it resembles so little.

Thallium is a perfect metal, endowed in the highest degree with a metallic lustre, as is seen on examining a freshly-cut surface, or on heating a bar strongly in hydrogen, and allowing it to cool in that gas. It is less blue than lead, less white than silver, and, in its colour, more resembles tin or aluminium than any other metal. It softens at 100° C., and, if kept for some time at that temperature, a crystalline structure becomes apparent in the ingot: this is shown by the appearance of a beautiful watering (*moiré*), produced when the metal is moistened with water, which cleanses the surface like acids.

Before the blow-pipe, thallium exhibits some characteristic phenomena. It melts rapidly, and oxidises, giving off odourless fumes, of a whitish colour, but mixed with reddish or violet tones. It continues to give off the fumes a long time after the heating has ceased. When the principal globule has cooled, it is found to be surrounded with small globules of the volatilised metal. In a closed tube, it melts in the flame of a spirit-lamp, oxidises rapidly, giving an oxide which, when hot, calls to mind the appearance of rubies (metallic sulphides), and which, when cold, more resembles litharge: this is a compound of the protoxide of thallium with the silica of the glass. A globule of the metal, heated over a spirit-lamp in a bulb tube open at both ends, and inclined to facilitate the passage of air, soon melts, forming a layer of the ordinary brown fused oxide, but, at the same time, giving off abundant fumes, which condense a short distance from the bulb as a reddish or violet amorphous powder. When a globule of the metal is placed on a cupel heated to redness, and then plunged into oxygen, the metal burns brilliantly, and oxidises, the fused oxide sinking into the cupel. This oxide is either the peroxide of thallium, or a mixture of the proto- and peroxide of thallium.

M. Lamy has discovered that thallium forms two oxides: the protoxide, a strongly alkaline base, like soda and potash; and the peroxide, which gives up oxygen when heated with strong acids, and may be converted into a chloride, which, when heated, gives up a part of its chlorine. Chemists, however, will notice, that the protoxide of thallium, which corresponds to potash, so far from having, like potash, a great affinity for water, loses its water readily when heated, or even when cold in a vacuum. There then remains the reddish anhydrons oxide, while the hydrated oxide is yellowish white: the oxide is hydrated or dehydrated with equal facility. It will be further remarked, that the peroxide of thallium, in the experiments of M. Lamy, has given no sign of the formation of oxygenated water.

Thallium burns in dry chlorine. It forms three chlorides, one of which corresponds to common salt, another to sesquichloride of iron, while the third is a bichloride corresponding to corrosive sublimate. The protochloride is white, fusible, slightly soluble, and, when prepared in the moist way, is precipitated in large dense flocculi like chloride of silver. Thallium can also form higher chlorides than the bichloride, but their composition is not definite.

The proto-iodide and proto-bromide only have been studied; they resemble the corresponding compounds of lead. Cyanide of thallium is soluble; but a crystalline precipitate of this salt is formed when concentrated solutions of cyanide of potassium and of a salt of thallium are mixed.

The sulphide of thallium obtained by precipitation is brownish black; it resembles sulphide of lead. In whatever way obtained, it easily oxidises in the air, and is converted into the soluble and colourless sulphate.

Thallium is very slowly attacked by hydrochloric acid even when concentrated and boiling. It is, on the contrary, rapidly attacked by nitric and sulphuric acids. The latter, concentrated and hot, dissolves it with a rapidity which contrasts with the slowness with which the same acid attacks lead.

In relation to the action of acids, thallium presents a complete opposition of characters to aluminium, the latter being quickly dissolved by hydrochloric acid, which does not attack the former, and resisting nitric acid, which easily dissolves thallium.

In the state of protoxide, thallium forms soluble and crystallisable salts with carbonic, nitric, sulphuric, and phosphoric acids. The carbonate is a very characteristic salt.

The salts formed by the protoxide of thallium with organic acids, which have been studied by M. Kuhlmann, jun., are the oxalate, binoxalate, tartrate, paratartrate, malate, citrate, formiate, acetate, and some others of less importance. All these salts are soluble, and, according to M. La Prevostaye, some of them are isomorphous with the corresponding salts of potash.

Thallium, then, is a new metal well characterised. It is distinguished from all other reputed simple bodies by the beautiful green line it gives in the spectrum, and which corresponds to the line 1442 in the typical spectrum of Kirchhoff. From the examination of the solar spectrum, we may conclude that thallium does not exist in the solar atmosphere.

Thallium undoubtedly forms one of the family of alkaline metals, the number of which has been doubled by recent discoveries. At the beginning of this century, only two of these metals were known, potassium and sodium. Forty years ago lithium was added to the number; and within the last three years three others have been discovered, rubidium, cæsium, and thallium, all three by spectrum analysis.

From this we may be allowed to hope that the number of these metals, and of metals in general, is destined, by the application of this new method of analysis, to receive a rapid and considerable extension.

Among alkaline metals, thallium occupies the opposite extremity of a scale of which lithium forms the first term, and the equivalent weights mark the different degrees. The weights are, in fact, as follows:—

Lithium	7
Sodium	23
Potassium	39
Rubidium	85
Cæsium	123
Thallium	204

On this point it has been remarked,—

1. That the equivalent of sodium is exactly the mean of the equivalents of potassium and lithium, $\frac{39+7}{2}=23$;

2. That by adding double the weight of sodium to the weight of potassium, we obtain the weight of rubidium: $46+39=85$;

3. That by adding twice the weight of sodium to twice the weight of potassium, we get nearly the weight of cæsium: $46+78=124$;

4. That by adding double the weight of sodium to four times that of potassium, we obtain nearly the equivalent of thallium: $47+156=202$.

These considerations are of a nature to attract the attention of chemists; and without attributing to them a value that the actual numbers would not justify, they show the interest which attaches to the careful comparison of the equivalents of bodies belonging to the same family.

The alkaline metals have this peculiarity, that to bring them under the law of Dulong and Petit—that is to say, to make their atomic heats equal the atomic heats of other metals,—it is necessary to halve their atomic weights. Thallium does not escape this rule. Its equivalent $=204$; but its specific heat, as determined by M. Regnault (who appends a note on this subject to this report), being equal to 0·03355, it is necessary to reduce the atomic weight to 102. In the same way as potash has for its atomic formula K_2O, the protoxide of thallium would have for its formula Tl_2O.*

The atomic volume of thallium will be equal to 8·5; and if we do not compare it with the volumes of sodium and potassium, it is because these present extraordinary anomalies which have not yet received sufficient attention from chemists.

In conclusion, we may remark that the series of alkaline metals actually known contains a body which possesses so light an equivalent that it may be placed near to hydrogen—that is to say, lithium; and also a body, thallium, which has so heavy an equivalent that it may be ranked by the side of bismuth, a metal which possesses the highest of equivalents.

We see that the discovery of new bodies extends the circle of our knowledge, not only by the facts with which they enrich practical science, but especially on account of the prospects revealed by the study of them, the laws they lead us to ascertain, and that freer and more general aspect under which we are taught to regard the properties of individual substances, their analogies, differences, and classification, and even their nature and essence.

For these reasons, and taking into consideration the difficulties overcome by the author, the clearness and importance of his results, we have the honour to propose to the Academy that his memoir form part of the "*Recueil des Savants Etrangers.*"

TECHNICAL CHEMISTRY.

On Tannate of Rosaniline, by M. E. Kopp.

One of the most interesting combinations of rosaniline, and one which plays an important part in the application of this beautiful colouring matter, is that which it forms with tannin and gall-nut or quercitannic acid.

Tannate of rosaniline, if not the most insoluble salt, is, at least, one of the least soluble in water among rosaniline salts. It is produced by adding a solution either of pure tannin or freshly prepared gall-nut to a watery solution of a neutral, or only slightly acid, rosaniline salt, especially if operating on a salt with an energetic mineral acid (sulphate, nitrate, hydrochlorate). When the salt of rosaniline is too acid, the larger part of the excess of acid should be neutralised by an alkaline carbonate.

The insolubility of tannate of rosaniline can be utilised for the precipitation of this body, from solutions too weak to be evaporated or saturated with advantage by an alkaline neutral salt, with the view of precipitating the salt of rosaniline by this saturation.

The physical properties of tannate of rosaniline vary according to whether it is precipitated by means of weak or concentrated, cold or hot solutions.

By precipitating a salt of rosaniline from a hot and sufficiently concentrated watery solution, by a concentrated solution of tannin, tannate of rosaniline is obtained in the form of a pitchy, resinous mass, of a very dark red-brown tint. When the temperature is raised sufficiently high, the precipitate itself can even be entirely melted, in which case, the mother-waters can be decanted; but then they generally have an intense red colour, especially if excess of tannin has been employed in the precipitation. It would thus appear that there are several combinations of tannin with rosaniline, and that bi- and tri-tannate are more soluble than mono-tannate. The salt, melted and then solidified by cooling, sometimes presents, when dried, beautiful golden metallic reflections.

By using cold and sufficiently diluted solutions, and by avoiding a notable excess of tannin, tannate of rosaniline forms either a flaky or pulverulent precipitate, very divided, and of a magnificent carmine-red colour, rivalling the carmine of cochineal. It should then be washed, and dried with gentle heat. The mother-waters are often quite colourless.

To obtain a precipitate charged as much as possible with rosaniline, it must be shaken with fresh solutions of aniline red, previously sufficiently purified, until these become colourless, or the shade of the precipitate deepens.

Tannate of rosaniline, when heated, becomes darker, and assumes a violet tinge; by raising the temperature, it becomes brown, and then decomposes. In alcohol, wood-spirit, and acetic acid, it dissolves with an extremely dark carmine-red colour. Energetic acids change the red tint to orange-yellow, and the material is dissolved. By the addition of water the red tint is restored, but verging a little to violet. The whole often seems to remain in solution, but after a certain time a new precipitate of tannate of rosaniline, more or less violet-red, is formed; nevertheless, the liquids generally remain red, sometimes intensely so, especially if the tannate has been strongly saturated with rosaniline.

Tannate of rosaniline decomposes and loses its colour under the influence of energetic caustic alkalies. The mixture assumes a tarnished and dirty colour after the alteration of the tannin by atmospheric oxygen in presence of the alkali. In mordanting or printing tissues with tannin, whether pure or associated with metallic salts or other organic substances, and then tinting them in a bath of aniline red, it is during the formation of tannate of rosaniline that the printing becomes apparent, and fixation takes place.

This salt will serve directly in printing tissues, on account of its solubility in acetic acid. The acetic solution being sufficiently thickened (by gum arabic, traga-

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* M. Dumas persists in using the symbol "Th," which we have already shown has been adopted for Thorinum.

canth, fecula, or starch, &c.), the pattern is printed and vaporised, and the tannate of rosaniline becomes fixed on the tissue. During these operations, it is only necessary to guard against imparting too much depth to the red colour, which tends to give it a brownish or violet tint.

By boiling tannate of rosaniline with hydrochloric acid, there is at first solution without alteration, for the addition of much water gives rise again to a red precipitate, while the liquid itself retains its dark red colour. But, if the boiling is continued, alteration takes place. When evaporated to dryness in a water bath, the residuum consists of a mixture of red, tarnished violet, and of a blackish, insoluble matter.

By treating hot tannate of rosaniline by soda or potash, a portion dissolves, and the liquid takes a blackish-grey colour. But, even after prolonged contact, the red colouring matter is recovered, either on the filter or in the liquid, and retaining all its properties, on the addition of acetic acid.

One of the most curious reactions of tannate of rosaniline is that which takes place with rough commercial wood spirit under the influence of a very small quantity of mineral acids (hydrochloric, nitric, sulphuric). By pounding the tannate with from three to four times its weight of wood spirit, a thick liquid results, of the darkest carmine-red colour; then, by adding from one-twentieth to one-tenth of the volume of wood spirit, or hydrochloric or nitric acid—or, better still, alcohol saturated, cold, with hydrochloric gas, this solution not turning black, as is speedily the case with crude wood spirit saturated with hydrochloric gas—the red tint becomes successively more and more bluish, and finally becomes almost a pure blue.

The transformation can be stopped at a given moment, and a violet, more or less verging on red or blue, obtained at will. For this purpose, the wood spirit must be previously acidulated and added by small portions to the tannate of rosaniline, on each addition pounding and stirring the mixture until it becomes quite dry.

Produced in this way, the violet, when dry, presents extremely brilliant cupreous and golden metallic reflections. If the colour be still too red, a fresh portion of acidulated wood spirit is added, and the mixture is again pounded and stirred until dry, which takes place in a very short time.

By adding, in the first instance, a larger proportion of wood spirit and acid, a very bluish violet is immediately produced, especially if the mixture is slightly heated.

A temperature too high must, however, be avoided, as the colours easily lose their purity and brightness.

Aniline violets, and blues, produced in this way are almost insoluble in water; so that the small quantity of acid they retain can be taken away by washing in cold water. To obtain these colouring matters in cake it is only necessary to add alcohol or wood spirit to the dry colour, to dissolve, or, at least, dilute it, and then water containing a little carbonate of soda. The violet or blue matter coagulates in large and very voluminous flakes, which are collected on a filter.

To dye silk or wool these violets or blues are dissolved in alcohol or wood spirit, and the solution diluted with tepid water. It is true that a portion of the colouring matter is precipitated in the bath, but in so divided a state that it combines very easily with the textile fibre. In dyeing small portions of silk successively in the same violet bath, the first pieces are more reddish violet, and the last more bluish violet; a fact which seems to show that the violet colouring matter is a mixture of blue and red or violet-red, and that the latter combines more easily with silk than the blue colouring matter, and is more readily soluble in dissolvents.

For printing, the violets and blues are dissolved in acetic acid; the solution is thickened, the pattern printed, dried, vaporised, and washed.—*Repertoire de Chimie.*

On the Manufacture of Alcohol from Coal-gas, *by M.* MALLET.

(*Continued from page* 3.)

Some persons have not been sparing in their criticisms of the process, and even of the inventor, and have gone so far as to pretend that M. Cotelle had imposed upon the public; that he had never made alcohol from coal-gas, and that the brandy, which he had given his visitors to taste, was manufactured from molasses. Till, however, we have proof to the contrary, we cannot believe that M. Cotelle has lent himself to such charlatanism. For all actions there is logically a motive. What can have been the motive of this inventor? The illusion could not last long, and all his future prospects must be irreparably destroyed, should he have lent himself to any such fraud. M. Cotelle must, therefore, be regarded as acting in good faith; and even should it be proved that he has grossly deceived himself as to the cost price of his alcohol, or as to the possibility of introducing this system of manufacture, he is entitled to our praises for the trouble he has given himself, and for the sacrifices he has imposed upon himself and his copartners, if he has any. Pioneers often fail in their task, but they render incontestable service to society.

The following ideas, upon the most advantageous conditions on which the manufacture of alcohol can be effected, and upon the chances, more or less brilliant, which this new process has of success, may not be without interest:—

Sulphuric acid, even concentrated to 66°, absorbs bicarburetted hydrogen with difficulty. This is indisputable; but in this process the gas is not used in a pure state, as in coal-gas (and even in gas produced from cannel, schist, and Boghead) it exists in a very small proportion, so that it is in a state of great subdivision, and, consequently, more easily subject to the action of the sulphuric acid. If the process is applicable, it is clear that gas will not be expressly manufactured for it, even from cannel or from Boghead. In some manufactures, these gases are to be obtained as a species of waste product. Thus, in coke-ovens, immense quantities are suffered to be lost; and certainly the manufacturers of metallurgic coke will not hesitate to adopt such furnaces as will allow the gas and, at the same time, the other sub-products, to be collected, if a market can be found for them even at a low price. There are some coals, such, for instance, as those of Commentry, certain varieties of St. Etienne, and of Mons, which may produce coke of a tolerably good quality, and a certain proportion of bicarburetted hydrogen. It is to such coals that a preference would be given. Generally, also, the manufacturers of oils of schist, who distil either Boghead or the schists of Autun, or of the Allier, derive little profit from the gas which they produce in spite of themselves, and this gas is much richer than the ordinary coal-gas in bicarburetted hydrogen. These are the sources to which, in our opinion, the manufacturers of alcohol by the new process should, in the first instance, look for their materials.

This use of gas, the refuse product of coke and schist-oil manufactories, is all the more to be sought for, as the quantity of coal to distil, with special reference to the manufacture of alcohol, may be very considerable. The cost of production published in the papers of St. Quentin indicates a consumption of two tons of coal for the manufacture of a hectolitre of alcohol; but, as we have before said, the quantity of coal to be employed depends evidently upon the richness of the gas given by it on distillation in bicarburetted hydrogen. If the gas contains 12 per cent of bicarburetted hydrogen (we leave aside the propylene), two tons may suffice for the production of a hectolitre of alcohol at 90°, if the whole of the carburetted hydrogen is properly employed. We submit that 1765 cubic feet of this gas are necessary for the production of this quantity of alcohol (M. Cotelle only reckons upon 353 cubic feet). Supposing the gas produced from coke-ovens at the rate of 7063 cubic feet per ton of coal (and this product is the maximum, in the present state of such apparatus), the quantity of coal necessary for the production of a hectolitre of alcohol will be as given in the following table:—

Richness of Gas in Carburetted Hydrogen.	Volume of Gas in Cubic feet.	Coal distilled in Pounds.
12 per cent.	14,692	4,588
11 ,,	16,104	5,020
10 ,,	17,658	5,525
9 ,,	19,601	5,682
8 ,,	22,073	6,890
7 ,,	25,216	7,871
6 ,,	29,419	9,186
5 ,,	35,317	11,025
4 ,,	44,146	13,781
3 ,,	58,838	18,374
2 ,,	88,292	27,562
1 ,,	176,585	55,125

The quantity of coal being given by the formula $x \times 7063 \times a = 50$, a representing the quantity of bicarburetted hydrogen contained in the gas.

The gas produced in schist-oil manufactories which distil Russel's Boghead No. 1, contains from 12 to 14 per cent. of bicarburetted hydrogen; and its richness should, therefore, be four times greater than that of the coal-gas manufactured in retorts.

The price of the raw material of the alcohol will depend, therefore, essentially on the price at which the manufacturers of coke will sell either the cubic mètre of gas or the cubic mètre of carburetted hydrogen; for they may easily sell only the part useful for the manufacture of alcohol—the surplus of the gas serving for the ordinary consumption of the ovens. These are our opinions upon the most advantageous manner of obtaining the raw material.

We will not return to the difficulty which the absorption of the bicarburetted hydrogen by sulphuric acid presents; but it may be useful to go further into the question of the expense of concentrating acid of 22° into acid of 66°. The large manufacturers of sulphuric acid admit that the concentration to 66° of the chamber acid, which weighs 52° to 53°, costs them at least a franc per 220 pounds of acid at 66°. To this expense it is necessary to add the cost of concentration from 22° to 52°. What will it be? As this operation is not carried on in ordinary manufactories of sulphuric acid, we can only give an approximate estimate; we, therefore, prefer to abstain. For a daily manufacture of 660 gallons of alcohol, 99,225 pounds of sulphuric acid of 66° are required. The concentration of this acid, diluted to 22°, with the loss consequent on it, would be an operation of extreme importance. Besides the cost of concentration, some account must be kept of the loss in acid caused by the distillation of the alcohol from the *vinasse* by means of steam, which necessarily carries with it a certain quantity of acid; and also causes some expense in the manipulation of this acid. Since M. Cotelle's experiments have been under consideration, we have always thought, and still think, that the question of the acid is one of the great difficulties, and perhaps the greatest, in the way of the successful realisation of the new process.

Another consideration, but of another class, must not be omitted. The manufacture of alcohol with bicarburetted hydrogen gas, upon a certain scale, would necessarily be followed by a fall in the price of alcohol, and, consequently, a reduction in the price of molasses, which would lead to a diminution in the cost of manufacture. Besides this, well-informed persons pretend that the Champonnois system applied in farm-houses to the distillation of beet-root, and the utilisation of the refuse on the spot, will allow alcohol of 90° to be produced at a cost of 1s. 8d. per gallon, profit included.

It will be seen that the question is complex, and it may be dangerous to form any positive opinion, at the present day, upon the new process. It is wise, therefore, to wait for the practical experiments which the Company Cotelle are about to make at the pit's mouth in the use of the gas from coke-ovens. Parties interested will, nevertheless, do well not to fall asleep in their quietude. The alarm is given; numerous inventors are about to, or do already, occupy themselves with the question, which will be closely examined under all its phases, and M. Cotelle should not be the last to seek actively to improve his process. The word "impossible" has long since ceased to exist in France in all that relates to industry; and the production of alcohol, by means of gas, may, one day or another, take its place among our manufacturing pursuits. This would be one of those wonders to which modern chemistry has accustomed us.—*Journal de l'Eclairage au Gaz.*

COURT OF QUEEN'S BENCH.

December 10, 1862.

Simpson and Others v. Wilson and Another.

(Continued from page 7.)

Dr. Odling (examined by Mr. Grove): I have made myself acquainted with the manufacture of the magenta colours from aniline, and have read Medlock's specification. The word "dry" I should say is used in its ordinary sense, in opposition to moist or wet, and it is applied by chemical authors indifferently to hydrated and anhydrous bodies. The word "anhydrous" is applied to bodies which do not contain water of constitution or combination, and it is used as well with bodies which are wet, as with bodies that are dry. Dry lime used for gas purifying contains 39 per cent. of water, and yet is generally called dry in scientific books, as contra-distinguished from wet lime or lime mixed with water. In a large number of other instances the salts may be wet when in a liquid or dissolved state, but they become anhydrous when the water of constitution is driven away.

Lord Chief Justice Cockburn: I understood Mr. Grove's question to be—is it true of substances, that they may be in solution, that they may be dry, or that they may be anhydrous?—Witness: I said yes: I meant lime might be in solution, but dry lime in solution would not be dry. The chemical compound would be there still, but being wetted with water it would no longer be dry, and I should not apply that term to it.

By Mr. Grove: If I drove off the water, though it then contained water of constitution, I should call it dry.

Lord Chief Justice Cockburn: The specification speaks of dry arsenic acid, and the plaintiff says, "I no longer use it dry, I use it in solution."

Mr. Grove: "And then I make it dry in the process." Still the dry is the thing which really effects the operation, and it would have misled the public if the term "wet" had been used, because wet arsenic acid never produces the colour. You may make it dry beforehand or afterwards.

Examination continued: I agree with Mr. Nicholson that it is the dry acid *per se* which produces the effect; neither the anhydrous nor the wet. Practically, the anhydrous does not produce it. The invention, to the best of my knowledge, was new at the date of Medlock's patent. Other substances were used to produce the colour, but not arsenic acid. It is a valuable invention. Arsenic acid is the most manageable and cheapest of all the agents used to produce the colour. I have read Heilman's specification. It includes an enormous number of substances. A large number of those substances will produce the colour. I agree with Mr. Nicholson that Medlock's process is, undoubtedly, a practical one.

Lord Chief Justice Cockburn: Suppose the patent had said nothing about "dry," but had simply used the term "arsenic acid," what should you then have understood?—Witness: I should have understood the same body.

By Mr. Grove: If the patent had simply said arsenic acid, but had not given any process for driving off the adventitious water, the result would not have been produced. Either dry acid must be used, or an extra process for making it dry must be used. If the mixture were heated to near its boiling point, according to Medlock's specification, it will immediately assume a purple colour. If you had to drive off the water, the colour would not be developed for a long time. The dry acid is no doubt a valuable instruction. On June 7 and August 6, 1861, I received samples of dye from the plaintiffs. I analysed them, and found they were aniline colours containing small quantities of arsenious and arsenic acids. From knowing that such colours were produced from aniline and arsenic acid, I inferred that they were made by means of arsenic acid, or something substantially the same. As far as my experiments have gone, I believe that the colours could only have been produced by either using the arsenic acid in what has been called the dry state, or by using it in solution, and evaporating until it has reached that state. Chemically, these two methods are substantially quite the same.

Cross-examined by Mr. Hindmarsh: I have read Delaire and Girard's specification. I have no doubt their process would produce the colour. Arsenic acid is very deliquescent. The dry mass left on evaporating the solution, as directed in Graham's book, would be very deliquescent; but when the mass was heated to the melting point of lead, there would be a decomposition. The two acids produced in the two ways are very different. I knew arsenic acid as a manufacturing article before the date of this patent. It was used in calico printing as a substitute for tartaric acid. Perfectly dry arsenic acid and aniline mixed together will not, so far as I know, produce the colour without heat. I have tried, and failed to produce any colour by mixing dry arsenic acid and aniline, and allowing them to stand together. I have kept them together for a considerable time. I think I have read all the published processes for the production of aniline dyes. Several of them are dry processes, in which no water is to be used; several of them are processes, in which oxides or chlorides of metals are to be used either cold or hot; but, as far as I know, those that are used produce a colour which is not the same body. If I took perfectly dry arsenic acid and aniline, and mixed them together, I should undoubtedly produce colour as described in Medlock's specification. By perfectly dry acid I mean acid containing water of constitution—dry acid as it is sold, which contains about 12 per cent. of combined water, but is perfectly dry. I have bought such acid of Griffin, and of Jackson and Townson. Dry arsenic acid in the ordinary state in which it is sold contains from 11 to 12 up to 13 per cent. of water. In making the colour I believe it is advantageous to form an arseniate of aniline, but it is not necessary. In Delaire and Girard's process undoubtedly the arsenic acid is converted into arseniate of aniline, and in Medlock's a large portion is. In Medlock's process a red and a purple dye are produced. Delaire and Girard's process is for a red dye. They have a separate process, in which a larger quantity of aniline is used for producing the violet or blue. I have tried Delaire and Girard's first process. A pasty substance is formed, which may contain what is called water of crystallisation. The mixture is then put into a still and heated, the effect of which is to drive off a large quantity of water, and then convert the material into colour. The great bulk of the arsenic acid is then deoxidised and converted into arsenious acid.

By the Lord Chief Justice: The oxygen acts upon the aniline, and converts it into colour. In the first stage arseniate of aniline is formed, which, when heated, breaks up.

By Mr. Hindmarsh: The combination of aniline is called "rose aniline," or generally "magenta." Chemically, it is probably an arseniate of rose aniline; but this is broken up in the purification. Arseniate of aniline cannot be formed save in the presence of constitutional water. It contains, no doubt, the elements of water, but these elements are not added to it in the shape of water. You cannot make arseniate of aniline by adding anhydrous arsenic acid to aniline; but if you add water you can. When you do this you add the elements of water in the shape of water. Water, or the elements of water, are necessary to the formation of the arseniate. The arseniate is usually produced, and it is advantageous to produce it; but it is not necessary to the formation of the colour. I can produce the colour without the formation of the arseniate.

The Lord Chief Justice: We are dealing with a given process: in that given process you say the arseniate of aniline is always formed?—Witness: Undoubtedly.

Re-examined by Mr. Grove: Arseniate of aniline always contains the elements of water; but whether they exist in the shape of water is a disputed point. All are agreed that they exist in chemical combination, and not as mere moisture. Water of combination is often termed "basic water." The water of dry arsenic acid is basic water. The constitutional water in dry arsenic acid is sufficient for the production of arseniate of aniline. The water of solution has nothing to do with the production of that salt. The reaction of arsenic acid on aniline is a complex reaction a long way from being understood. I had never seen anhydrous arsenic acid until I made some recently. What is called anhydrous in books is not always anhydrous; it is what we call dry. If it were anhydrous it would not be deliquescent. Anhydrous acid is not properly an acid; it has not the reactions of an acid. The anhydrous acid could not be bought.

By the Lord Chief Justice: The patentee, I suppose, used the word "dry" to distinguish from wet. The advantage of using a solution of arsenic acid is that it mixes more speedily, and you get rid of the difficulty of evaporating the acid to dryness beforehand.

A Juror: Are you of opinion that the process described in the specification for the use of dry arsenic acid correctly describes the process wherein Mr. Nicholson states he actually uses arsenic acid in solution?—Witness: The process is substantially the same—the order is somewhat different.

The Juror: Are you of opinion that it is correctly described in this specification?—Witness: Mr. Nicholson's colour is produced by dry arsenic acid.

The Juror: If you were instructing a class, and had to describe this process to them, would you tell them that they must use dry arsenic acid when you meant them to use it in solution in water?—Witness: I should tell my class that it did not matter whether they took the acid dry, or whether they took it in solution, and dried it during the process.

The Juror: Then if dry acid is to be taken in its natural sense as opposed to wet, how would you suppose that the use of it in the moist state in solution was a use of it as specified here in the dry state?—Witness: Simply because, before the action of the two bodies can take place, the water is to be removed.

Dr. Frankland (examined by Mr. Russell): I am generally acquainted with the process for making magenta dyes. I have read Medlock's specification, and understand the process. I agree with Dr. Odling in the definition of the term "dry" as distinguished from anhydrous. I think the distinction is quite recognised among chemists. Dry arsenic acid is a well known substance, and has been known in commerce now for many years. It contains from 11 to 13½ per cent. of water. I have made magenta dye by Medlock's process, and have found that the use of arsenic acid in the dry state will produce the colour. I have likewise used arsenic acid in solution, and found that in that state it produced the desired result. In the experiment to which I allude the arsenic acid in solution was mixed with the aniline, and heat was applied to the mixture. No colour was produced until a considerable quantity of water had distilled over. I then measured the quantity distilled over, and found it just equal to that I added to the dry acid. The dry acid undoubtedly appears to be the efficient agent in producing the result. I never knew of any such process as Medlock's previous to the publication of his specification.

Cross-examined by Mr. Hindmarsh: To make the colour it is necessary that the arsenic acid and aniline first combine, and for that you must have water I do not mean water *plus* what is in it. The article sold commercially as dry arsenic acid, and which I have bought myself, contains from 11 to 13½ per cent. of water. I have said in an affidavit that "the condition under which alone the red dye is produced is that the arsenic acid shall be dry—that is, shall not contain much more than 5 per cent. of water, which is commonly called dry arsenic acid." No doubt an arseniate of aniline is produced in Delaire and Girard's process. I do not know whether in the process the whole of the aniline is converted into red dye. If I were to take a dry alkali to dehydrate alcohol, I should probably take the artificially dried; I certainly should take the salt in the dry state, deprived of water of crystallisation. The words "arsenic acid" alone would be a less definite name for the material. If I were simply told to take arsenic acid, I should consider myself at liberty to take any form of the acid. A solution of arsenic acid would be naturally included under that designation, just as nitric acid commonly means a solution of nitric acid. I should be guided by the nature of the process I was to perform as to the kind of arsenic acid I took; but the first which would present itself to my mind would be the commercial article. By dry arsenic acid I should not understand more dry than the ordinary acid when dry.

Mr. Hindmarsh: Do you mean that the word "dry" would import nothing to your mind?—Witness: No; I do not think it would in this case, except that it would utterly preclude the use of liquid acid, which the other name would not.

Re-examined by Sir F. Kelly: I purchased arsenic acid from Messrs. Page and Tibbs; I sent an order for dry acid. Whether I sent for arsenic acid or dry arsenic acid, I believe I should get the same thing. I estimated the water in one sample, and found it to contain about twelve or thirteen per cent. I never heard that arsenic acid is sold in solution, nor anhydrous. I know of no other arsenic acid than the "dry" as an article of commerce. The amount of water in this may differ in different samples, as it is very deliquescent. It depends upon the heat applied, and the degree it may have been exposed to the atmosphere. In Medlock's process it must be dry, not anhydrous, nor contain water of solution. Dry caustic soda contains thirty per cent. of water. Any water that a substance may contain chemically combined does not affect the wetness or dryness of the substance; it is only that which is not chemically combined that affects the wetness or dryness.

By Mr. Hindmarsh: I should think that about half the thirteen per cent. of water might be driven off without injuring the acid, or without preventing it from combining. If you drive off the whole, it is then almost incapable of combining, and, therefore, it is injured.

Dr. Hoffman (examined by Mr. Grove): I have paid a great deal of attention to aniline. For the purpose of this patent, I should have taken the commercial arsenic acid containing water of hydration, that is to say, water of combination—combined water. I certainly should not have taken chemically anhydrous acid. I do not believe I could have got anhydrous acid in commerce. I have never seen it, as far as I know, in my whole professional career. With all respect to Mr. Graham, I must say that there is a contradiction in his description of arsenic acid. The anhydrous acid is described by those who have especially devoted their attention to it, as a substance which does not attract water—is not deliquescent. I have analysed dry arsenic acid, and found 13½ per cent. of water in it. I bought my samples of Hopkin and Williams. I gave no directions for their manufacture. I agree with the other witnesses that dry arsenic acid and aniline mixed and heated will produce the colour. I also agree that a solution of arsenic acid will not produce it unless you continue the heat so long as to drive away the water of solution. According to the best of my knowledge, Medlock's specification is correct in treating dry arsenic acid as the substance which produces the colour; and if it had been said—"take a solution of arsenic acid," it would have been necessary to state that the colour would only appear after a certain length of time: it would appear only when the water added for the purpose of solution had been evaporated.

Cross-examined by Mr. Hindmarsh: It is necessary to employ arsenic acid containing a certain amount of water, such as the article called dry arsenic acid. I do not believe that the formation of arseniate of aniline is absolutely necessary, although in practice it generally takes place. I am not aware that the process goes on much more rapidly when there is a complete formation of arseniate of aniline. I have tried Delaire and Girard's process; it is simply taking the dry process with a certain quantity of water. There are, no doubt, practical advantages in using the acid in solution. Arsenic acid is generally obtained from solution, and it is desirable not to go to the expense of evaporating the solution. I have only performed the operations on a small scale, and am not able to give much practical evidence.

Dr. Medlock (examined by Mr. Grove): I am a practical chemist, and the inventor of this process. For the purpose of my invention, I bought dry arsenic acid, as such, from the plaintiffs, and from Bailey, of Wolverhampton. I ordered nothing special, but bought the article of commerce. For the invention the acid must not be anhydrous, but should be dry. It may be dried in the first instance, or after having been mixed with the aniline.

By a Juror: If I had contemplated using a solution of arsenic acid, I should have said that the solution would answer, but the acid must be made dry. I should have directed the mixture to be boiled till all the water was expelled.

Mr. R. M. Hands (examined by Mr. Russell): I am a dyer at Coventry. Dr. Medlock, at my request, made experiments to find out a means of producing magenta dyes. He made some of his experiments at our works, and some in his own laboratory, and I assisted him. I did not try arsenic acid in solution. My belief is that arsenic acid will not produce the desired result until all water but that in combination has been got rid of. We have followed the process according to the specification, and manufactured the colour largely.

Cross-examined by Mr. Hindmarsh: We do not make our own arsenic acid.

Mr. R. Warrington (examined by Mr. Grove): I perfectly agree with the evidence given by the scientific witnesses. The term "dry" is a very loose expression; it does not necessarily mean anhydrous.

Cross-examined by Mr. Hindmarsh: What would you understand by arsenic acid?—Witness: You might have it in solution, or dry, or in all sorts of forms.

Mr. Hindmarsh: If you were told to use arsenic acid, would you use it wet?—Witness: No, I should use it dry.

Mr. Hindmarsh: Then, do you mean to say that the word "dry," to your mind, means nothing?—Witness: It means nothing but dry. The term "dry" is used very loosely in reference to chemicals. Sometimes it is used to signify a material deprived of the water of crystallisation, and sometimes only part of the water of crystallisation.

Mr. Hindmarsh: And sometimes part of the water of hydration, too?—Witness: That would depend on whether the hydrate was a wet substance. I do not know of such cases. Arsenic acid is a substance soluble in water in all proportions. Hydrated oxide of iron is insoluble in water. If a person added the word "dry" to a material like arsenic acid, I should not imagine that he meant more dry than the ordinary state of the acid. He says dry acid in distinction to acid in solution; and how am I to know what quantity of arsenic acid the party intends to use if he does not say "dry?" I do not say that he must not use it in solution, but he must state dry in order to show what quantity he is to use. I should understand the direction to use dry arsenic acid as a direction not to use it in solution; but in practice it may be much more efficient to use it in solution.

Re-examined by Mr. Grove: The patentee means certain proportions as useful for the purpose, and you could not ascertain those proportions unless you were first to get dry acid.

This closed the scientific evidence for the plaintiffs.

Mr. Hindmarsh, for the defendants, contended that the patent was bad, seeing that the specification did not clearly describe the process, inasmuch as the application of heat was given as an alternative operation, while the evidence of the plaintiffs' witnesses proved that it was absolutely necessary to the production of the colour.

The Lord Chief Justice said there never was a more unfortunately drawn specification, but the case had better go on.

Mr. Hindmarsh then addressed the jury, after which the court adjourned.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, December 16, 1862.

E. W. Binney, *F.R.S., F.G.S., President, in the Chair.*

Dr. Joule read the following letter addressed to himself:

"Varagnes, near Annonay, Ardèche, France,
"November 25, 1862.

"Dear Sir,—I am much obliged to you for the disinterestedness you have maintained in reference to the discovery of the great principle that my uncle, the celebrated Montgolfier, revealed to me some sixty years ago. Since that time I have passed my whole life in studying this question.

"Much merit is due to you, Sir, for your defence of another's rights, especially after the compositions which you have published, and are, without doubt, the best explanations yet written of the great principle so continually occupying you, myself, M. Grove, M. Mayer, and many others. Among the latter there are many who endeavour to persuade the public (every one in his interest), that the world owes the great advancement of the dynamical theory of heat solely to their works; but the public do not like being led in this kind of logic, and look with indifference upon the pretended authors of these discoveries, at the same time raising the merit of those who, like yourself, take a pleasure in rendering justice to those who wait patiently until just and impartial men judge them by their works and not by their words.

"For more than sixty years I have studied the question of identity of caloric and movement, and yet have perceived no advancement of this subject; nothing that either has been said or written has helped me towards the conviction I have arrived at. Indeed, almost all the experiments since then have proved this a fact (which formerly no one endeavoured to contest), viz., that immediately movement disappears, heat is produced, reciprocally. We cannot, however, praise too much your studies, and those of the physicists who have followed you, having witnessed the details of this great principle, and calculated the different circumstances with the utmost accuracy. All the attempts in giving a plausible explanation of the phenomenon in question have generally conveyed this opinion, that the molecules of bodies, as well as those of ordinary matter, are liable to Newton's attraction, and, consequently, must be separated from each other by spaces infinitely larger than they occupied before; then, they must keep their distances from each other by virtue of laws, if not identical, at least analogous with those which govern the celestial bodies, and maintain in fixed boundaries the distances between the planets and the stars, which determine their course.

"Such were the ideas I had, when addressing a letter on this subject to the celebrated and most learned Sir John Herschel, when President of the Royal Astronomical Society, the most distinguished man of our time for his philosophical genius and his wise and judicious manner of treating sciences, well known to me through the connexion I have had with him.

"We are indebted, Sir, there is not a doubt, to the solid and enlightened spirit that characterises the English learned, for the development made in your country on this interesting question, which occupies the most learned men in the world. I have endeavoured to engage, by numerous papers, the attention of the members of the French Academy of Sciences, my colleagues, upon this important subject, but without avail. But I take the liberty of asking you to communicate this letter to the members of the Institution of which you are a member; and should I obtain its publication, I hope my studies will be known by all your learned colleagues, and contribute to enlighten the opinion of those who pursue this branch of science.

"The letter I addressed, on the 2nd of April, 1824, to Sir John Herschel has been translated into English, and was sent to Sir David Brewster, who inserted it, on the 24th of April, 1824, in the 20th number of the *Edinburgh Review* (page 280), in which I expressed an opinion that the cohesion of the bodies constituted on the surface of the earth may be liable to attraction, which will tend to join in their centre of gravity the composed molecules. I also explained the great difficulty I had in attaining this result. Indeed, if the particles of invisible and invaluable bodies could maintain their distances by the same laws as the celestial bodies, the former would necessarily be perceived, for they would be obliged to collect themselves in a symmetrical and regular manner about their centres of gravity, and make their movements apparent to our senses. I have understood since that a law exists, unknown to me, determinating the organised parts of bodies to keep a distance and to preserve the permanent forms they exhibit. Therefore, I studied incessantly to solve a question of so great an interest; and in 1836, after twelve years passed in study and meditation, I was fortunate enough to succeed. Indeed, an attentive examination of the manner of action exercised reciprocally by the particles, and generally by all the bodies, brought this result: If systems of two or several molecules, being comparatively in repose, traverse other systems comparatively in movement, the former separate the latter from another, losing a part of their own movement. Consequently, the question of cohesion was for me implicitly resolved; and, after eleven years' further reflection, I began to publish (in 1848) the series of papers I have read in the Institute, and in which I proved, under the name of 'Distension,' the new result of Newton's attraction applied by myself.

"Then, making myself better acquainted with this law, I published, in 1855, a large volume upon 'Cohesion,' and, in 1859, another upon the 'Origin and Propagation of Force,' in which I have perfectly explained these principles, trusting to my own proofs and experience. Moreover, I published in 1861, in the form of a letter addressed to M. Tramblay, director of the *Cosmos*, a third paper in reference to the same subject. But yourself, sir, a physicist of the greatest reputation, and who have taken quite a different direction in science not attempted by any other, must know very well how difficult it is, even for those who sincerely wish the triumph of truth, to leave ideas and old practices in which they have grown up. To attain this result we must have confidence in ourselves. New ideas, however clear and judicious they may be, are never adopted immediately, but by degrees, so that those who are occupied with the same question, already explained by their predecessors, fancy themselves the discoverers and authors. After several generations, however, when the tomb is closed above all, historians and commentators will re-establish the truth of the facts, and render them due justice.

"Mr. Faraday, your fellow-countryman, gave me that great satisfaction, and his powerful testimony has paid me more than I ever could hope, for the oblivion with which several of my contemporaries wished to cover me, who endeavoured to uphold one another without attaining anything but ridicule, the public always being just and free from prejudice, even if the question is above their intelligence.

"Let us console ourselves, Sir, should our efforts rest in obscurity, and not be valued as we suppose just; let us be assured that those who will follow us will know how to sift the true from the false, that they will adopt the one and reject the other; and that, if the opinions we have exhibited are just and exact, they will be duly valued one day. But we shall not complain for the present to see our reputation injured by errors we have incurred, perhaps at the first moment, for these errors will have the definitive result of classing us according to merit in the history of science.

"I have the honour to be, with the greatest respect,
"Sir,
"Your most obedient, humble servant,
"Seguin, Aîné."

In connection with a conversation on the solutions of different salts with which almost all the waters in England are contaminated, and the practical evils resulting from these impurities to steam-boilers, Mr. Spence stated that various means had been adopted, with only partial success, for preventing incrustation on boilers caused by these impurities. For some years he had adopted a simple plan, which is perfectly successful, both at Goole, where the Aire and Calder canal has a considerable amount of lime salts, and also at his Newton works, where the water from the Rochdale canal is not only impregnated with lime salts, but apparently also with sulphate or chloride of iron. The plan is as follows:—On the suction part of the water-pipe by which the boiler is supplied, that is, between the force pump and the canal, a small vessel is placed, capable of containing about two gallons of water; a pipe of half an inch diameter, with a stop cock, communicates from the water-pipe with this vessel; every day the boiler man puts into this vessel 1 lb. or 1½ lb., as found sufficient, of soda ash, and dissolves it in water in the vessel, and then, when pumping into the boiler, turns the small cock, and in three or four minutes all the solution is taken up and passed through the force pump into the boiler, and this is daily repeated; the consequence is, that not the slightest crust forms on the boilers, and no chipping is ever required, the salts being all decomposed, and the earthy and metallic bases thrown down as mud, which may be blown off or cleaned out periodically, if the boiler is a flued one. These two boilers are cleaned out monthly, and a broom and shovel are the only tools required. The plan is easily adopted, perfectly efficient, and very generally applicable.

Dr. Joule drew attention to the great sacrifice of life by steam-boiler explosions. He believed that, in nearly every instance, rupture took place simply because the iron, by wear or otherwise, had become unable to withstand the ordinary working pressure. Various hypotheses set up to account for explosions were worse than useless, because they diverted attention from the real source of danger. He believed that one of these hypotheses—that which attributed explosions to the introduction of water into a boiler the plates of which are heated in consequence of deficiency of water—was quite inadequate to account for the facts; although weak boilers might be exploded at the moment of starting the engine, in consequence of the swelling of the water through renewed ebullition throwing hot water over the heated plates. The absolute necessity of employing the hydraulic test periodically had been pointed out so frequently that he considered that the neglect of it was highly criminal.

A paper was read by Mr. Thomas Hopkins, M.B.M.S., on the "*Influence of the Earth's Rotation on Winds.*"

PHYSICAL AND MATHEMATICAL SECTION.

November 13, 1862.

Robert Worthington, *Esq., F.R.A.S., President of the Section, in the Chair.*

A paper by Mr. Thomas Heelis, F.R.A.S., entitled "*Notes of Observations on η Argûs,*" was read.

A paper by Mr. Thomas Heelis, on "*Hydrometric Observations of the Water of the Mediterranean,*" was read.

Mr. Baxendell, F.R.A.S., mentioned the following observations made by himself and Mr. Richard Dale, on October 24, 1862, at Mr. Worthington's observatory:—The night was remarkably clear. There was no moon, nor any decided indication of aurora in the north; but there was so much light in the atmosphere that the country around could be distinctly seen, and houses and hedgerows at a distance clearly perceived. This apparent luminosity of the atmosphere continued for two hours, and the night then became very dark, but still remained very clear. Mr. Baxendell afterwards noticed that objects at a distance were seen much more distinctly on this occasion than when the moon was seven days old.

MICROSCOPICAL SECTION.

November 17, 1862.

J. G. Lynde, *F.G.S., M. Inst. C.E., in the Chair.*

Capt. Randall, late of the barque *Brazil*, forwarded eight soundings, taken on the north coast of the Brazils.

Mr. Thomas Heelis presented a specimen of the *Echeneis remora*, or sucking fish.

Mr. J. Parry presented a number of cells and rings in cardboard. They were very smooth and sharply cut, without the bur usually produced by punching out cells. Mr. Parry explained that they were cut in a lathe, twenty to thirty together, the outside cuttings only presenting an appreciable bur.

Dr. Roberts called attention to the aid that might be received in the examination of the structure of animal and vegetable tissue by the use of colouring materials. Magenta is peculiarly adapted for this purpose, in consequence of its solubility in simple water and its inert chemical character. The nuclear structures of animal cells are deeply tinted by magenta, and by its use the nuclei of the pale blood corpuscles, of pus globules, of the renal and hepatic cells, and of all epithelial structures, are brought out in great beauty, tinted of a bright carbuncle red. The red blood discs are tinted of a faint rose colour, and a darker red speck, not hitherto noticed, is to be observed on the periphery of the corpuscle. It undergoes some changes when treated with tannin, and subsequently with caustic potash, but this point is still under investigation. Dr. Roberts exhibited drawings and mounted specimens to illustrate his views.

Mr. John Leigh, M.R.C.S., exhibited a case of microscopical dissecting instruments, by Messrs. Wood, of Manchester, which were highly approved of for completeness and finish.

Mr. Thomas H. Nevill exhibited, with dark ground illumination, some fine specimens of *Conochilus volvox.*

NOTICES OF PATENTS.

2562. *Apparatus employed in Reducing Straw and other Vegetable Substances in the Manufacture of Pulp for Making Paper.* F. B. Houghton, Kensington. Dated October 14, 1861.

For the purposes of this invention the patentee employs a cylindrical-shaped boiler, within which a shaft, with blades, is kept constantly rotating, whilst the straw or other vegetable material is being submitted to the action of the caustic lye. Steam is introduced into the cylinder to maintain the ebullition, and pipes are provided for the purpose of supplying the water necessary to wash the reduced pulp prior to its removal from the boiler.

2574. *Re-working Waste Vulcanised India-rubber.* T. Forster, Sparrow Hall, Streatham. Dated October 16, 1861.

For this purpose the vulcanised India-rubber is reduced to a suitable condition by crushing between rollers or otherwise, and is then mixed with sulphur and gutta-percha. Whenever desirable, pigments may be incorporated with the before-mentioned mixture.

It has become a common practice to re-work perished vulcanised goods by some such process as that just described. The specification of Mr. J. R. Hunt, of earlier date,* claims the union of finely-divided India-rubber with masticated gutta-percha.

2580. *Apparatus for, and Method of, Increasing the Illuminating Power of Gas.* W. Smith, Salisbury Street, Adelphi, London. A communication. Dated October 17, 1861.

This invention consists in the use of a small chamber or vessel, charged with benzol, or other liquid hydrocarbon, and provided with a system of capillary tubes, by means of which the evaporation of such liquid is promoted during the passage of the gas.

The apparatus is very similar in principle to that described by Mr. W. J. Williams † for effecting the same purpose.

2597. *Apparatus for the Simultaneous Manufacture of White Lead and Vinegar.* C. D. Abel, Southampton Buildings, Chancery Lane. A communication. Dated October 18, 1861.

This invention relates to the simultaneous manufacture of white lead and vinegar, and is carried out by erecting a lead-corroding room above that in which the vats are placed, wherein the formation of vinegar is being conducted; the acetic acid escaping from the vinegar tubs is allowed to rise into the lead-corroding room, and there assists in converting the metallic lead into carbonate of lead.

A very dangerous suggestion! for there is every probability of the vinegar becoming itself contaminated with lead.

2551. *Compound to Prevent the Incrustation and Sediment of Calcareous Matters in Boilers.* E. T. Hughes, Chancery Lane, London. A communication. Dated October 12, 1861. (Not proceeded with.)

This composition is prepared by mixing together soda crystals and alum, dissolved in water, the former being employed in excess; red ochre and the refuse starch from potatoes are then, in the form of a paste, added to the other ingredients.

The soda and red ochre would, undoubtedly, serve a useful purpose, but the addition of an organic matter like potato-starch would, on the contrary, be detrimental, and likely to induce frothing.

Grants of Provisional Protection for Six Months.

3131. James Steart, St. James Road, Blue Anchor Road, Bermondsey, Surrey, "An improved method of extracting the fibre from *Zostera marina* and other aquatic vegetable productions."—Petition recorded November 21, 1862.

3221. Peter William Reuter, Buckland Crescent, St. John's Wood, Middlesex, "The preparation of a new compound to be used for dyeing and printing purposes.'—A communication from Edward Reuter, Paris.

3237. Richard Kingsman Cautley, Thorney, Cambridgeshire, "Improvements in electro-thermal baths."—A communication from Mark House, Cleveland, Ohio, U.S.

3247. Alfred Frederick Eden, Threadneedle Street, London, "Improvements in apparatus for taking minute photographic pictures and magnified pictures of microscopic objects."—Petitions recorded December 3, 1862.

3296. Victor Mirland, Frameries, Belgium, "Improvements in manufacturing paste with the dried pulp of rhubarb, to be used as preserve."

3298. William Clark, Chancery Lane, London, "Improvements in photographic apparatus."—A communication from Jean Baptiste Sabatier-Blot, Boulevart St. Martin, Paris.

3320. John Robert Breckon, Darlington, and Thomas Douglas, Pease's West Colliery, Crook, Durham, "Improvements in the manufacture of fire-bricks and other articles usually made from fire-clay."

Notices to Proceed.

2233. Alexis Jean Moreau and Adolphe Ernest Ragon, Bernard Street, Russell Square, London, "Improvements in the mode of, and apparatus for, treating bituminous and carbonaceous substances for the purpose of obtaining the various products, volatile, liquid, and solid, which they contain, and also in the treatment and application of such products."

2234. Alexis Jean Moreau and Adolphe Ernest Ragon, Bernard Street, Russell Square, London, "Improvements in the manufacture of gas and coke."—Petitions recorded August 9, 1862.

2266. James Dickson, Tollington Road, Holloway, Middlesex, "Improvements in obtaining sodium from certain sources of that metal."

2314. John Cimeg, Great James Street, Bedford Row,

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* *Vide* Chemical News, vol. v., p. 138.

† *Vide* Chemical News, vol. vii. page 10.

London, "Improvements in depositing silver and other metals on fabrics and other materials."

2346. James Mackay, Glasgow, Lanarkshire, N.B., "Improvements in the manufacture of soap powder."—Petitions recorded August 22, 1862.

2357. Martin Kenneth Angelo, Gloucester Place, Portman Square, London, "Improvements in apparatus used in the manufacture of shellac."—Petition recorded August 23, 1862.

2425. Joseph Mosheimer, Manchester, "Certain improvements in machinery for amalgamating gold and silver, or the ores thereof."—Petition recorded September 2, 1862.

3157. John Moule, Seabright Place, Hackney Road, Middlesex, "An improved method of deodorising mineral oils and hydrocarbons."

3247. Alfred Frederick Eden, Threadneedle Street, London, "Improvements in apparatus for taking minute photographic pictures and magnified pictures of microscopic objects."—Petition recorded December 3, 1862.

CORRESPONDENCE.

Preservation of Stone.

To the Editor of the Chemical News.

Sir,—Pressure of business had prevented my usual perusal of the Chemical News until a few days since, when I found the descriptive particulars of a process for indurating stone claimed by Mr. Jesse Rust. I feel sure, from a sense of justice, you will grant us what space you can afford, to set us right with your readers with regard to this process. In the Chemical News of November 9, 1861, your courtesy gave us space for the explanation of our process, which we would ask your readers to peruse side by side with that claimed by Mr. Jesse Rust, in your journal of December 6, 1862. We further hand you the specification of Mr. Rust's patent, for which he received provisional protection only; and it would appear that he relinquished this *bonâ fide* mode of procedure for the more questionable one of working a *secret* process. The part of this secret process which he divulges, in order to secure, if possible, the confidence of the public, leads us to believe it to be a colourable imitation of our own, which has been legally patented, and openly and fully submitted to the scientific public generally and the chemical world, especially through the medium of your columns and those of your contemporaries. If you can insert so much of Mr. Rust's specification as to show that his process consisted of the use of *two* solutions, separately and apart, as Nos. 1 and 2, whilst our own was for the use of agent and re agent in the same solution, it will the more plainly be seen why and wherefore he allows his patent to lapse, and take up with a process having our own speciality as its principal feature, as also whence that feature was obtained. —I am, &c.

Sanders Trotman, *pro* Bartlett Brothers & Co.

"*Provisional Specification, June 6, 1861, left by Jesse Rust, of Lambeth Glass Works, Carlisle Street, Lambeth, in the County of Surrey, glass manufacturer.*

"*An Improved Composition or Preparation for Hardening and Preserving Stone and Cement.*

"The invention relates to the making and applying a solution of caustic baryta, or bicarbonate of magnesia and fluoride of silica, otherwise fluo-silicic acid, or fluoride of alumina, to stone and cement, to harden the same and preserve them from decay.

"My invention consists in preparing the solutions Nos. 1 and 2, hereafter mentioned, and applying them to the above purpose, and rendering stone and cement better able to resist the effects of water and the atmosphere and the acids therein. The plan I adapt is as follows:—

"I first prepare a solution of caustic baryta, or bicarbonate of magnesia, or both, which I call No. 1; I then prepare a solution of fluoride of silica, otherwise fluo or hydro-fluo-silicic acid, or a fluoride of alumina, which I call No. 2. I can also use potash or soda as a solvent for the silica or alumina, but I prefer using hydro-fluoric acid as a solvent for the silica or alumina, or both, and I claim the using and adapting hydro-fluoric acid for this purpose as part of my invention.

"Having prepared my two solutions, I proceed first to wash or saturate the stone or cement to be operated on with solution No. 1; when I use the caustic baryta solution, I prefer using it hot; I then proceed to wash or saturate the same with solution No. 2; solution No. 1 forms a base, and by adding solution No. 2, a decomposition takes place, and an insoluble substance, or fluo-silicate of baryta, or fluo-silicate of magnesia, whichever is used, is formed in the pores of the stone or cement. These operations I repeat until the pores are filled up, when, the surface being hardened, weak acids have no action upon it."

Potash Lye.

To the Editor of the Chemical News.

Sir,—Will you permit me to put the following queries to your chemical correspondents, through the medium of your useful publication:—

Which is the best way of producing potash lye for soap making?

The best way to free them from salts, &c., so as to get them perfectly pure?

Also the acid,—and best strength (Twaddle's hydror.) to prove them caustic?—I am, &c. Jno. Jones.

Manchester.

Red Chalk.

To the Editor of the Chemical News.

Sir,—When residing at Lynn, some eight or nine years ago, I analysed several specimens of the Hunstanton red chalk. I found the peroxide of iron to vary from 9 to 17 per cent.

I cannot conceive that the chalk could have been coloured by the decomposition of iron pyrites. If so, what has become of the sulphuric acid?

Augite is a silicate of magnesia, containing from 10 to 14 per cent. of magnesia. It also contains protoxide of iron, lime, and generally a small quantity of protoxide of manganese.

Glauconite, or green sand, is a silicate of iron and alumina, never containing less than 5 per cent. of alumina. Some specimens also contain potassa and lime. According to Dana, the largest amount of iron found in glauconite is 24·3 per cent., and the smallest amount of silica, 46 per cent.

I, therefore, think it impossible that the red chalk could be formed by the decomposition of either augite or glauconite; for the augite contains magnesia, and too little iron,—the glauconite contains too much alumina, and twice as much silica as iron.

It is a very interesting subject, both to chemist and geologist; and I trust some of your readers who have leisure will endeavour, by analysis and investigation, to discover the true origin of the red chalk.

I am, &c. W. M. Lupton.

Hoddesdon.

MISCELLANEOUS.

Chemical Society.—The next meeting of this Society will be held on Thursday next, at eight o'clock, when the following paper will be read: "On the Atomic Weights of Nickel and Cobalt." By Dr. W. J. Russell.

THE CHEMICAL NEWS.

VOL. VII. No. 163.—January 17, 1863.

UTILISATION OF PEAT.

A PROJECT has just been set on foot for this purpose, which, if the account given of it could be accepted as correct, might be of important influence on three very considerable branches of industrial art—viz., the supply of fuel, the manufacture of iron, and the manufacture of gas. The latter two, if they are not in a state of perfection, are, at any rate, carried on in this country according to such a thoroughly organised system, that any proposed means of modification relating to them, either as regards the materials employed, or the mode of procedure, must necessarily be of a very unquestionable and well-established character as to its practicability. The use of coal as fuel is likewise so universal in this country, and the principles of its application are so well understood, that there seems to be little room for alteration or improvement beyond the adoption of means for its more economic use, and for its purification. It is, therefore, somewhat surprising to find a proposal made for substituting peat for coal as fuel for steam-navigation and other purposes, for iron smelting, and as a material for producing gas. This, however, is the nature of the project referred to for the utilisation of peat. Under the title of the "Condensed Peat Company," a proposal is made to submit peat to a preparative treatment of pugging and moulding, by which it is to be rendered compact, and as dense as coal. The cost at which it is stated, in the prospectus of this proposed Company, that peat may be so prepared, is not to exceed that of coal at the pit's mouth. The grounds on which this project has been put forward are stated to be the probable failure of the supply of coal, the freedom of peat from sulphur, and some others of less importance. As regards the first of these, it may safely be said, that, if apprehended failure of the supply of coal is in any way well founded, that failure must be of such remote probability, that the suggestion of a substitute is, at any rate, premature at the present time. The freedom of peat, or rather of some varieties of peat, from sulphur, is certainly an advantage in its favour; but it must be remembered, that there are means of purifying coal, and separating the sulphur from it, which are quite equal to meeting any requirements that may arise for a fuel free from sulphur. This object is certainly not more easily attainable by the use of peat as fuel, than it is by the purification of coal.

But there is another reason assigned for the proposed introduction of peat for the purposes above named—viz., the assumption that it will do double the duty of coal, or, in other words, that it has twice the fuel value of coal. If this were the case, no doubt there would be room for an entire revolution of those branches of industry in which the use of fuel is an essential element. This, however, is not the case. If there is any subject connected with the arts which is thoroughly well understood, and beyond all question, it is the comparative value of different kinds of fuel. The principles which determine the value of fuel are well known, and practical experience has fully confirmed the results arrived at, with regard to this point, by scientific investigation. Those results have established the fact, that peat is a very much inferior fuel to coal, though superior to wood; and it is well known that, though peat is used in some places as steam-fuel for engines and for smelting purposes, that is done only where coal cannot be had. Even in such cases, it is often found preferable to import coal, than to have recourse to the peat which abounds in many localities where fuel is required.

The mechanical preparation of peat that is now proposed to be adopted for the purpose of improving its texture and physical characters as a fuel, has no kind of influence on its chemical composition; and it is this composition which constitutes the inferiority of peat as a fuel, when compared with coal.

This view of the subject is founded—not upon a comparison of ordinary air-dried peat with coal—but upon the comparison of absolutely dry peat with average coal. There is no kind of doubt or uncertainty as to the composition of coal or of peat in that condition. These data are generally recognised and well established, quite independent of opinion, and altogether above the necessity for belief. The comparison is, therefore, a comparison of facts, and its result is quite in accordance with the universal experience, that absolutely dry peat has not much more than half the calorific power of ordinary coal.

From this point of view, then, alone, it is evident that peat cannot compete with coal for those purposes which involve a large consumption of fuel. Besides this, the characters of bulk and texture of ordinary peat, even when it is made absolutely dry by kiln-drying, are such, that it cannot be substituted for coal. Undoubtedly, peat can be so prepared as to become applicable, in this respect, to the purposes for which coal is used; but, as before remarked, that preparation will only affect these characters of bulk and texture; it will not in any degree influence its calorific power,—that will still remain little more than half the calorific power of coal.

But then, in regard to such a preparation of peat as would render it absolutely dry, of a density equal to that of coal, and of such texture that it would equally well bear a blast, its practicability, in a commercial point of view, must be considered. The question of the cost of such preparation naturally arises, and is of quite as much importance as the actual value of the material produced by it. Taking all the circumstances of the case into consideration, it is by no means unreasonable to regard with great suspicion the assumption, that this prepared peat can be produced at the average cost of coal at the pit's mouth, especially in the absence of any statement of the data on which this assumption is founded. The reasons for doubting such an assumption become very much stronger when it is remembered that, in bringing a ton of coal to the pit's mouth, there is only a ton of material to deal with; while, on the contrary, to produce a ton of prepared peat, at least five tons of material have to be dealt with—to be dug, carried, pugged, moulded, and then dried; and that, for every ton of peat so prepared, four tons of water have to be evaporated. It is quite inconceivable that all these operations could be carried out at the same cost that a ton of coal can be brought to the pit's mouth; and, even if this were possible, the difference in the calorific power of peat and coal would still make the peat twice as costly as coal for fuel.

Numerous other circumstances might be brought forward, all tending to strengthen the general conclusion as to the impracticability of preparing peat so as, in a general way, to admit of its being used in the place of coal; but, to any one capable of appreciating the value of the facts above mentioned, they will be amply sufficient. It would, indeed, be altogether puerile to enter into argument on this subject, if it were not that the project of preparing peat to supersede coal has been brought forward in sober earnest, with some show of authority in support of it, and with some pretence of proof. This project is a striking illustration of the fact that, during periods favourable for commercial speculation, no scheme is too wild—no project too absurd, to be seriously proposed, and eagerly entered into. Indeed, the attraction which any enterprise presents to a certain class of minds, appears to be directly proportionate to its wildness and absurdity. A rational and practicable undertaking is far too tame and flat to be received with favour by them; and, as there is always a multitude of people who delight to dabble in matters they do not understand, and who, sensible and prudent enough in their proper sphere, are quite incapable of forming a correct judgment with regard to subjects relating to the arts, or involving scientific or technical facts, it is not to be wondered at that we have a constant succession of projects brought forward and entered into, which are mere phantasms, and which, after a very short time, naturally terminate in disappointment and loss. Belief being a condition attained and produced so much more easily than rational conviction, people, impressed with the desire of profit, are readily influenced by the most futile projects, the most exaggerated statements, and the most impudent assertions. When an idea has once been entertained by such people, they are so entirely possessed by it, that no facts, no arguments, are of any avail in manifesting to them their delusion; nothing but experience will bring them disillusion. The utilisation of peat has been a fertile ground for such mistaken attempts; and the attainment of any really useful results has been much more seriously retarded by such attempts, and their natural failure, than by any inherent difficulties in the utilisation of peat, although these, it must be admitted, are unquestionably considerable.

There is no doubt that, whenever the utilisation of peat is taken up in a reasonable way, with a due regard to what can be effected with it, and by carefully avoiding attempts to effect impossibilities, such an undertaking will be followed by successful results. It is even possible that peat may be used for smelting iron, and as fuel for various other purposes; it is, however, equally certain that such applications of peat can never become general, but must, in the nature of things, be confined to certain limited districts, and can be carried out only under exceptional circumstances.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Presence of Rubidium in Beetroot, by M. Lefebvre.

From the analysis of a raw beetroot salt by M. Grandeau, and several others made at Corbehem, we can give, approximatively, the amount of chloride of rubidium contained in two acres of ground.

Thus, two acres contain, on an average, 40,000 kilogrammes of beetroots, giving—

	Kilogrammes.
Sugar	2350
Molasses	1177
Crude potash	128*

One kilogramme of raw salt contains, on an average, 1·75 gramme of chloride of rubidium; thus, two acres would produce 226 grammes of this salt.

The amount of rubidium varies with the nature of the soil, and seems always to correspond to the quantity of potash contained in the raw salt. Several careful analyses give us the following results:—

Raw Salt of Leus, Béthune.

Water and insoluble matters	26·22
Sulphate of potash	12·95
Chloride of potassium	15·87
Chloride of rubidium	0·13
Carbonate of soda	21·52
Carbonate of potash	23·40

Raw Salt of Dunkirk.

Water and insoluble matters	19·82
Sulphate of potash	9·88
Chloride of potassium	20·59
Chloride of rubidium	0·15
Carbonate of soda	19·66
Carbonate of potash	29·90

Raw Salt from Sugar Factory at Allert (Somme), sensibly calcareous earth.

Water and insoluble matters	17·47
Sulphate of potash	2·55
Chloride of potassium	18·45
Chloride of rubidium	0·18
Carbonate of soda	19·22
Carbonate of potash	42·13

Raw Salt of Soissons.

Water and insoluble matters	13·36
Sulphate of potash	3·22
Chloride of potassium	16·62
Chloride of rubidium	0·21
Carbonate of soda	16·54
Carbonate of potash	50·05

M. Grandeau, who was the first to demonstrate the presence of rubidium in beetroot, came, last April, to spend a few days at Corbehem. By the aid of his spectral apparatus, he found that the chloride of rubidium we had already prepared (about 30 or 40 grammes) was quite pure. Since then, we have extracted it in considerable quantities, and have shown in London a series of salts composed of sulphate, chloride, carbonate, nitrate, and bitartrate of rubidium.

We have since prepared alum and chromate.

The following is our method for isolating the rubidium contained in the residuums of our large saltpetre manufactory (according to our analysis we find 4·90 grammes of chloride of rubidium to the kilogramme of mother-water):—

The cold leys, of a density of 40° B., are incinerated

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* These numbers show the average of the two last years.

with wood sawdust, to burn their sulphur and destroy the detonating compound.

The carbonaceous residuum is recovered by water; to eliminate the greater part of the salts, and produce a liquid rich in rubidium, the solution is evaporated to one-tenth.

These waters then have a density of about 35° B., and contain carbonates, sulphates, chlorides, but especially sulphides, hyposulphites, bromides, and iodides, which must be eliminated before the precipitation by chloride of platinum can be effected.

For this purpose, an excess of hydrochloric acid is added to the liquid, which saturates the carbonates and decomposes the sulphides and hyposulphites, forming an abundant deposit of sulphur. Nitric acid is added, drop by drop, to the filtered and boiling liquid until it ceases to produce violet or red vapours, which shows that all the bromine and iodine is displaced by the chlorine.

The waters then contain only sulphates and chlorides, and are ready to be treated. They are diluted to 5° B., and boiled. Then a saturated and boiling solution of double chloride of platinum and potassium is added. The precipitate thus formed is composed chiefly of chloroplatinate of rubidium and a little potassium, which is got rid of by a few washings in boiling water, leaving very pure chloroplatinate of rubidium to be reduced under heat by a hydrogen current. The mixture is extracted by water, which dissolves the chloride of rubidium.—*Comptes-Rendus.*

TECHNICAL CHEMISTRY.

Method of Assaying Rough Soda (Black Ash), for the use of Soda Manufacturers, arranged by W. Gossage *in* 1836.

I observe that the assaying of rough soda, or black ash, has attracted some notice latterly, as evinced by your columns. I, therefore, send you the details of a method of assaying this compound, which I introduced for the use of soda manufacturers in the year 1836, and which I have continued to use since that period with satisfactory results.

It is essential that the mode of assaying adopted in the manufacturer's laboratory should be capable of being rapidly executed, and require the least possible amount of calculation. I consider that my method fulfils completely both these requirements.

Assuming the equivalent number of real soda as 32, and operating upon such a proportion of solution as contains the soluble matter of 32 grains of rough soda, or some multiple of this quantity, and employing test solutions, of which the equivalents are 100 or 1000, the results obtained indicate directly the per-centage of real soda contained in the material subjected to assay in its different states of combination, no calculation being required unless some multiple of 100 measures has been used, or the test solution has an equivalent of 1000, and in such case the calculation is of the greatest simplicity. I need scarcely notice that this simplicity is the best safeguard against errors.

Prepare a standard hydrochloric acid of such strength that 100 measures shall be equivalent to 54 grains of anhydrous carbonate of soda; also a solution of chloride of barium, of which 1000 measures shall be equivalent to 72 grains of sulphate of soda; and a solution of nitrate of lead, of which 1000 measures contain 112 grains of protoxide of lead; and a solution of nitrate of silver, of which 1000 measures are equivalent to 60 grains of chloride of sodium. (At the above date, these numbers were generally accepted as expressing the equivalents of the substances mentioned. Since that time some variations have been made in these numbers.)

Take 320 grains of a sample of rough soda, in fine powder, and mix it with 1000 measures of distilled water, at about 100° F., in a closed bottle. Digest, with frequent agitation, for two or three hours, and filter the solution. Call this solution *A*, of which 100 measures may be considered to contain the soluble matter of 32 grains of rough soda; the increment in bulk occasioned by the addition of the dissolved matter to the water is too small to require notice in a manufacturing assay.

B.—Take 100 measures of the solution *A*, and apply heat to it; then add, from a graduated burette, standard hydrochloric acid sufficient to effect exact neutralisation at a boiling temperature. We may assume that 25 measures of acid have been required, and this will represent 25 of real soda (NaO) as soluble alkali, in all conditions, contained in 100 of the rough soda.

C.—Add to *B*, after neutralisation, a few drops of nitric acid, and then add standard solution of chloride of barium until all the sulphuric acid present is converted into sulphate of barytes. We may assume that 10 measures have been required for this purpose. This 10 measures being divided by 10 (the equivalent of the barytic solution being 1000 in place of 100) gives 1 of real soda, in the state of sulphate of soda, contained in 100 of the rough soda.

D.—Take 200 measures of the solution *A*, and add to it standard solution of nitrate of lead until all the sulphur of the sulphide of sodium present has become exactly precipitated as sulphide of lead, which point is shown by the addition of a further quantity of lead solution, just ceasing to give a brown precipitate. We may assume that 5 measures of lead solution have been required for this purpose. Then $5 \div 10 \div 2 = 0{\cdot}25$, which represents the equivalent of soda present, as sulphide of sodium, in 100 of rough soda.

E.—Add to *D*, after precipitation of sulphide, just sufficient solution of chloride of barium to give a mixture, of which a filtered drop will show a small quantity of barium in solution. This removes the carbonic and sulphuric acids, at the same time neutralising the carbonate of soda, and leaves the caustic soda still in solution with a small portion of caustic baryta. Add sufficient water to make up the mixture to 400 measures. Filter, and take 200 measures of the filtrate. Neutralise this exactly with the standard hydrochloric acid. We may assume this has required 8 measures. Then 8 represents the proportion of soda, as caustic soda, present in the liquor obtained by digesting 100 of rough soda in water.

F.—Take 100 measures of *A*, and saturate with nitric acid, applying heat, and adding a slight excess of acid. Then add standard solution of nitrate of silver exactly until no further production of chloride of silver is occasioned. We may assume that 10 measures have been required. Then $10 \div 10 = 1$, which represents the equivalent of soda present, as chloride of sodium, in 100 of rough soda.

We may thus state the result centesimally, as indicated by the assumed quantities of test solutions required:—

B indicates the presence of 25 of real soda as soluble alkali.

From this deduct 0·25 of soda present as sulphide *D*; also

8 of soda present as caustic E—8·00, together .	8·25	
And we have . . .	16·75	NaO present as $NaOCO_2$ = 28·27 carbonate of soda.
Also by E we have .	8·00	NaO present as NaO = 8·00 caustic soda (anhydrous).
Also by D we have .	0·25	NaO present as NaS = 0·3125 sulphide of sodium.
Also by C we have .	1·00	NaO present as $NaOSO_3$ = 2·25 sulphate of soda.
Also by F we have .	1·00	NaO present as NaCl = 1·866 chloride of sodium.

The first column expresses the relative equivalents of soda contained in the different compounds of sodium present in the rough soda.

COURT OF QUEEN'S BENCH.

December 10, 1862

Simpson and Others v. Wilson and Another.

(Continued from page 21.)

After Mr. Hindmarsh's address to the jury, the following evidence was called for the defendants :—

Mr. Dugald Campbell (examined by Mr. Hindmarsh) : I have paid particular attention to manufacturing chemistry, and am acquainted with the various processes used for producing the dyeing material called magenta. I have read the specifications of the patents granted prior to Medlock's. Some of them relate to dry, and some to wet processes. In some of them dry metallic oxides are used to act on aniline. I have read Medlock's specification, and am very well acquainted with arsenic acid. It consists of one atom of arsenic and five atoms of oxygen. It is a very deliquescent substance. If exposed it may all run into a liquid in twenty-four hours, depending on the hygrometric state of the atmosphere. It is very deliquescent in the anhydrous state. I heard a passage read yesterday from a book which said that the anhydrous acid was not deliquescent. That book is wrong. I will explain how the mistake may have happened. When arsenic acid is heated above a certain temperature, the arsenic acid loses oxygen, and becomes arsenious acid, which is not a deliquescent body. Now, the gentleman who made the experiment (Kopp) heated the arsenic acid to a red heat, and so must have converted it into arsenious acid or white arsenic. I have ascertained, by experiment, that anhydrous arsenic acid is deliquescent. In most books on chemistry arsenic acid is described by the symbols AsO_5. Graham's is a very good book ; but they are all very much alike. You will first find "arsenic acid, AsO_5 ;" then you have the mode of preparing it, by which you get AsO_5.

Lord Chief Justice Cockburn : Really, we cannot go into AsO_5. We are not here for the purpose of listening to a chemical dissertation. I shall be glad if the jury understand it. I do not, and do not desire. I do not see what it has to do with the question.

Examination resumed : Arsenic acid among chemists means a dry or anhydrous acid. I am not aware of any distinction used in chemical works between the use of the terms "anhydrous" and "dry." They are used as convertible terms, and I can give many instances of this if required. When applied to a deliquescent material I should understand the term "dry" to mean anhydrous ; and if applied to a crystalline body, not deliquescent, I should understand the same thing. Dry means anhydrous. I do not know a chemist who would dispute it in the instances I can give. One instance is, if you are told to take dry carbonate of soda to fuse a silicate, you would drive off all the water, and would not leave a trace. If in this patent it was stated, instead of dry arsenic acid, take dry carbonate of soda, the first thing a chemist would do would be to drive off all the water. I have made experiments with regard to the operation of arsenic acid on aniline.

By the Lord Chief Justice : I have done it with both anhydrous and hydrated acid—with arsenic acid generally.

By Mr. Hindmarsh : Anhydrous or dry arsenic acid will not act on aniline in the slightest degree. I have kept them together for twelve months without any action. I have the bottles here. I have also subjected the mixture to heat. There was no action—the aniline was blackened a little, but there was no other action. Aniline distils at about 460°, and if that heat is applied to the mixture, the only effect is to drive off the aniline. To produce a first class red dye it is, in my judgment, necessary to have the arsenic acid in solution. You can get colour without its being in solution ; but unless it was in solution it would not be commercially available for producing this colour. You must, under those circumstances, necessarily destroy a large quantity of aniline. If the arsenic acid contains water of hydration it may still be solid, and on heating the aniline with this, fusing the two together, you do get colour, but it is not a practicable process for the production of a commercially available colour. When a solution of arsenic acid and aniline are mixed together, they give out heat, and form a solid crystalline mass, which is arseniate of aniline containing three equivalents of water. It is impossible to produce this without the presence of water. [The witness here added a solution of arsenic acid to aniline in a beaker, and handed it to the Lord Chief Justice.]

Examination continued : I have read Delaire and Girard's specification. The production of the arseniate of aniline is described in it. There is nothing in Medlock's specification indicating the production of anything of that description. Before Delaire and Girard's patent I think it was known that such a product would result from the union of those two bodies. It was known among chemists. I have tried Delaire and Girard's process with various quantities of water, and with the precise quantities given in the specification. These proportions gave a perfect result ; there was a very large yield of colouring matter. I only obtained the red colour. The effect of using smaller quantities of water was to get less colour and more tarry matter. I cannot say what the tarry matter was ; it was valueless. It was not colour. I cannot say that the proportions given by Delaire and Girard are the best ; they are the best I have experimented with, but there may be better known. As far as my experience goes, the directions relating to temperature must be observed to ensure the conversion of substantially all the aniline into colouring matter.

Lord Chief Justice Cockburn : We are not trying Delaire and Girard's patent. Does this gentleman say he has tried the experiment with the dry arsenic acid, such as the plaintiff propounds the meaning of the term to be, and it has failed ?—Mr. Hindmarsh : I understand so.

Lord Chief Justice Cockburn : We have heard nothing of that kind yet.

Examination resumed : In my experiments, I have used 10, 20, and 40 per cent. of water with the arsenic acid. With 10 per cent. I got colour, but not in great quantity. With that proportion I do not think the process would be commercially available. When I say 10 per cent., I mean 100 of the anhydrous acid and 10 of water, and the other per-centages in the same way. [Samples of wool were produced dyed with colours produced with the above percentages of water. The strength of colour in the specimens was in proportion to the water employed.] When 20 per cent. of water was used there was a great destruction of aniline. In the operation of the acid and water upon the aniline by means of the wet acid, the acid is never reduced to the state of what is called dry arsenic acid.

Mr. Hindmarsh : Is the acid never reduced by the action of heat or otherwise to a state of dry arsenic acid?—Witness: Never.

Lord Chief Justice Cockburn (to Mr. Hindmarsh): You must use the word "anhydrous." You put it to the witness in one way, and he accepts it in the sense of dry. It is never reduced to the condition of anhydrous acid, but it is reduced to the condition of acid having a certain quantity of water in it.

Examination continued: In the course of the operation water is driven off. Before all the water is driven off, the arsenic acid ceases to be arsenic, and becomes arsenious acid. If the arsenic acid and aniline were used in the proportions given by Delaire and Girard, there would be no arsenic acid left in the colour at all. At the time of the action of the arsenic acid on the aniline it becomes arsenious acid. It is arsenic acid that operates on the aniline.

Cross-examined by Mr. Grove: I do not know of any instance of the term "dry" being used in another sense than "anhydrous," except where it is applied to substances that cannot be rendered anhydrous by any temperature. For instance—we say, commercially, "dry potash," which means hydrate of potash. You cannot get rid of the water. I should call dry lime, as a chemist, CaO—one equivalent of lime. When they speak of "dry-lime gas-purifiers," they mean hydrate of lime; but whenever dry lime is taken by a chemist—as, for example, in the preparation of anhydrous alcohol—the lime is invariably anhydrous. "Dry lime," to a gas-maker, would not mean anhydrous.

Mr. Grove: Supposing I were to tell a person to use dry tartaric acid, would not that contain water?—Witness: It contains water which you cannot drive off without destroying the tartaric acid.

Mr. Grove: By driving off the water you would make what you call "anhydrous tartaric acid"?—Witness: It would no longer be tartaric acid. I am not aware it is called "anhydrous tartaric acid."

Mr. Grove: Just attend to this passage:—"By carefully continuing the action of heat on tartaric acid, it may be obtained *anhydrous*; it is then white, amorphous, and insoluble in water; but by the protracted action of water, or by boiling, it reverts to its ordinary condition by the resumption of two atoms of water." Is that a correct or incorrect expression—"anhydrous tartaric acid"?—Witness:—I should say it was very likely correct.

Mr. Grove:—Then, after giving the components, the author says, "Dry tartaric acid has no action on carbonate of soda." I will give you a definition from the same book. [Extract from Taylor and Brande's "Chemistry" read.] Do you agree to that definition?—Witness: No, I do not know much about it. Dr. Taylor can explain it. If you were to tell me to take dry lime in a process, I should take anhydrous; not in gas-purifying, because I know it is wet in gas-purifiers. It is called the "dry lime process," to distinguish it from the wet, where milk of lime is used. "Dry slacked lime" means "hydrate of lime," which contains water, but is a dry powder or dry lime to appearance. All the water that can be got rid of, short of that which is part of its constitution, may have been got rid of.

Lord Chief Justice Cockburn: We heard from the chemist at Apothecaries' Hall that he sent an order to a manufacturing chemist for dry arsenic acid. I am curious to know whether the acid he received was anhydrous or contained water. That would be the practical test of the meaning of the term among manufacturing chemists. It is with that class of persons that a patentee is dealing—not with professors of the science, who treat the term in its scientific signification.

Mr. Hindmarsh: If it be a material which is commonly used for manufacturing purposes it would be so; but I think your lordship will find that this was a material merely kept by experimental chemists.

Mr. Warrington recalled, and examined by the Lord Chief Justice: Have you ascertained whether the arsenic acid is hydrated or not?—Witness: It is hydrated: it contains between 12 and 13 per cent. of water. From the same manufacturer we might have got a hundredweight. I only sent for a small quantity for my experiments.

Cross-examination of Mr. Dugald Campbell resumed: I have known anhydrous arsenic acid sold before I was consulted in this case. I got some fifteen years ago from Dymond's. I cannot say I tested it for water, but I did heat it in a test tube, and saw it contained a little water. I ordered simply arsenic acid. I was working with Mr. Graham on the arseniates. Arsenic acid is used in calico-printing. What I have seen used was in a pasty state. I have spilled some in my laboratory over night, and in the morning have found it all gone to liquid. If it were kept in a bottle with a cork, in a short time it would all run to a liquid. In the description of Kopp's process, as read by Sir F. Kelly, I understood that he heated it to a red heat: "Near to an obscure red, short of red heat," is much about the temperature that would decompose arsenic acid. A temperature of 400° would render the acid perfectly anhydrous. I am sure of it: I have made the experiment dozens of times.

Mr. Grove: Dr. Miller places the decomposition at above redness, the anhydrous a little below redness, as Kopp does, and the monhydrate at 400°. Is he correct?—Witness: My own experiments were, that if I heated above the melting point of lead it would lose oxygen. The melting point of lead is between 600° and 700°. Still I have tried even by keeping it at 400° a sufficient time.

Mr. Grove: Is this correct, speaking of the same substance—"The last equivalent of water is expelled by a dull red heat?"—Witness: That is not agreeing with my experiments. These books do not agree with my experiments. I say it is decomposed a little above the melting point of lead. I would rather not say what is a low red heat. I am in the midst of some experiments with regard to that. I have seen a low red heat estimated at 400°, but I will not give a decided opinion, as I may have it brought against me at some other time on another matter. One of the temperatures given in Delaire and Girard's specification is the evaporating temperature of water; the others are above it. The result in that process, by an operation above 320° F., and lasting four or five hours, is a dry mass, or nearly so. I should say it was very nearly anhydrous. In the experiment I made just now, the aniline and arsenic acid were not mixed in chemical equivalents; if they had been there would have been no liquid present. There may have been water which would have been driven away by heat. Here is arseniate of aniline (handing up a bottle) which is absolutely solid.

By the Lord Chief Justice: It is not anhydrous; it contains, I should think, 40 per cent. of water.

By Mr. Grove: It certainly contains water, but it would not change if you kept a stopper on it. The process will succeed with 10 per cent. of water, but not so well as when you take more. You do not get anything like the amount of colour. I heard Mr. Hands say he had used "tons" of the dry acid, and sold the colour. I do not know if that is probable; it depends on the price he got for it. If he took Medlock's patent, and made it by that, he would be just losing half his aniline to begin with.

Lord Chief Justice Cockburn: The case was opened that these articles were produced according to Medlock's patent. Now, Medlock's patent speaks of "dry"—I will not say anhydrous arsenic acid, for that is in dispute—and Dr. Medlock tells us himself that he used the term "dry" in contradistinction to a solution of arsenic acid. The things which are produced here are produced by arsenic acid in a dissolved state.

Mr. Grove: Yes, my lord; but Mr. Hands stated that they have been produced by the other process. Here (showing a specimen of dyed silk) is one that is absolutely produced by the dry.

Examination resumed: Witness: I only know of one hydrate of arsenic acid, not three. I have made a large number of experiments on that point. I found it to contain two equivalents, or about thirteen per cent. of water. By following Delaire and Girard's specification, I produced a good result. I made a mistake when I said aniline distilled at 460°; I should have said 360°. I have tried to get colour from the tarry matter I mentioned. I did not get a purple colour, only a dirty sort of brown. I do not know the exact mode of purifying the tarry matter. I do not think what I got was worth treating.

Re-examined by Mr. Hindmarsh: It was not a blue dye like that mentioned in Medlock's specification.

Mr. Warrington again re-called and examined by the Lord Chief Justice: I got my arsenic acid through Messrs. Dunn and Heathfield. I cannot say that they manufactured it. I do not think they got it of the plaintiffs. I do not know where they got it. Dunn and Heathfield are well-known chemical manufacturers.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

SOCIETY OF ARTS.

November 26, 1862.

Dr. A. W. Williamson, *F.R.S., in the Chair.*

(Concluded from page 10.)

Having now described the mode of obtaining the peat, and pointed out the two great difficulties involved in the supply of a large quantity of it for the purpose of fuel or for any other application, I will now request your attention to the nature of this material when it has been dried, and, in the first instance, as regards its application as fuel.

Even in the most favourable seasons, the air-dried peat retains a considerable amount of water—from 20 to 30 per cent.—as will be seen from the following results or analyses of different kinds taken from stacks a year old:—

Lews Peat.	Air dried.	
	Per-centage of water.	Weight of a cubic foot.
1. Light brown fibrous turf	40	25 lbs.
2. Blackish brown fibrous peat	26·71	53 ,,
3. Black peat, very dense..	25·39	54 ,,
4. Black peat, very dense..	31·60	65 ,,
5. Brown peat	28·74	71 ,,
6. Brownish black peat	27·76	78 ,,

This water cannot be separated from the peat except by kiln-drying; but, in order to illustrate the effect of this moisture on the value of peat as fuel, I will, at first, suppose that it has been so dried as not to contain any water. In that state the composition of peat may be taken as generally represented by the following proportions as compared with coal:—

—	Peat.	Coal.				
		Welsh.	Newcastle.	Lancashire.	Scotch.	Derbyshire.
Carbon	60	83·78	82·12	77·90	78·53	79·68
Hydrogen ..	6	4·79	5·31	5·32	5·61	4·94
Oxygen	32	4·15	5·67	9·53	9·69	10·12
Ash	2	4·91	3·77	4·88	4·03	2·65
	100					

Combustion, or that chemical process by which heat is generated from ordinary fuel, consists in the combination of carbon and hydrogen with atmospheric oxygen.

The amount of heat produced by the combustion of any kind of fuel depends, therefore, on the amount of carbon and hydrogen it contains. The amount of heat produced by any particular fuel, or its calorific power, is expressed by comparison with the amount of heat produced by the combustion of carbon, which is taken as unity. Hydrogen gas, when burnt, produces an amount of heat three times as great as that produced by the combustion of an equal weight of carbon to carbonic acid. The calorific power of hydrogen is, therefore, three times as great as that of carbon. The per-centage composition of a fuel being known, it is easy to determine its relative calorific power, that of carbon being equal to 1000. When the combustible portion of the fuel consists of carbon only, as in coke or charcoal, the per-centage of carbon expresses the calorific power or relative fuel value as compared with pure carbon. When the combustible portion of the fuel consists of carbon and hydrogen, the per-centage amount of hydrogen multiplied by three, and added to the number expressing the per-centage of carbon, gives the calorific power of that fuel as compared with carbon; but when the fuel contains oxygen besides carbon and hydrogen, a portion of either or both of these constituents equivalent to the amount of oxygen contained in the fuel must be regarded as already in combustion with oxygen, and, therefore, as ineffective for the production of heat. In such cases it is only the surplus carbon and hydrogen, over and above what are equivalent to the oxygen of the fuel, which can produce heat by combustion. Therefore, the greater the amount of oxygen in any fuel, the smaller will be the calorific power. In this respect there is a great difference between coal and peat. The presence of a large amount of oxygen in fuel affects the calorific power in two ways, viz., by reducing the per-centage amount of carbon and hydrogen, and by rendering a portion of those constituents ineffective for the production of heat. For this reason the calorific power of absolutely dry peat is only 660, while that of coal is from 966 to 903.

Hence, it will be evident that the maximum capability of peat as fuel is necessarily far below that of coal, even when the peat is absolutely dry. But, as I have already pointed out, that degree of dryness cannot be attained except by kiln-drying, and the ordinary air-dried peat of good quality contains one-fourth its weight of water. Here, then, is a further reduction of the calorific power of this substance, by one-fourth, or to 495; about one-half that of coal.

In some cases where peat is needed for fuel, it is essential to have it quite dry, and then it is worth while to kiln-dry it; but there is no saving effected by so doing. The 25 per cent. of water separated by kiln-drying requires for its separation a determinate quantity of heat, and a proportionate consumption of fuel, which is equally consumed without useful effect, whether the peat be used as fuel in the air-dried state, or whether it be kiln-dried before it is used.

These simple considerations will be sufficient to show what a palpable delusion it would be to suppose that peat could possibly be in any way equal to coal in fuel value.

The greater bulkiness of peat as compared with coal is another circumstance which operates against its application as fuel. The average of coal has a density corresponding to 80 lb. to the cubic foot, while air-dried peat has a density corresponding to only 64 lb. to the cubic foot. A cubic foot of coal, in the state in which it is used, contains about 60 lb., whereas peat in the same way weighs only 30 lb. to the cubic foot; so that with only half the calorific power it takes twice the space, and to produce a given effect with air-dried peat, it would require twice the weight and four times the bulk of the coal to produce that effect.

Hence has arisen the idea of compressing peat. It is notorious that no success has attended any of the attempts to carry this idea into practice, and that this should be the case is very easily intelligible. Absolutely dry peat of the very best quality has a fuel value of 660 as compared with coal at 960; in order, therefore, that equal bulks of coal and peat should have the same fuel value, a cubic foot of peat must contain nearly one and a-half times as much in weight as a cubic foot of coal, or nearly 116 lbs. to the cubic foot, corresponding to a density of 1800 lbs. Whether such a compression of peat is or is not possible in practice I will not pretend to say, though I consider it very improbable; and even if it were effected so as to be of practical utility, there would still be the disadvantage attending the use of peat as fuel, that its calorific power would be only two-thirds that of coal, and that one and a-half times as much must be used to produce the same effect.

As regards the use of peat for fuel, it now remains only to consider what are the circumstances under which it can be used for this purpose, and under which there is an advantage in using it rather than coal. I can best illustrate this by a case within my own experience. During the last four years I have had occasion to manufacture a large quantity of bricks in one of the western islands of Scotland, and for that purpose required fuel for raising steam to drive the brick machinery and for burning the bricks. Coal could be delivered at the port of Stornoway at about 18s. per ton, and, as the works were at some distance inland, there was a cartage amounting to 4s. per ton, making the cost of the coal 22s. per ton. But I found that the peat, of which there was abundance close to the works, was capable of raising steam well, and of being used for burning the bricks, and that, taking it to have only half the fuel value of coal, and even with very imperfect arrangements for bringing it in from the moor, I could for 8s. put down at the boiler, or at the kiln, a quantity of peat equivalent to one ton of coal, thus making a difference of 14s. between the use of a ton of coal and the use of peat equivalent to it. As the burning of the bricks required about half a ton of coals per thousand, this was equivalent to a saving of 7s. per thousand in the cost of the bricks. In this case, therefore, there was an unmistakable advantage in using peat as fuel, and the advantage would have been still greater if there had been a more efficient means of bringing in the peat from the moor. In the case to which I now refer, this cost as much as the peat itself cost on the moor, or about 2s. per ton.

In the town of Stornoway, however, it is found to be more advantageous to use coal at the gasworks, and as fuel for the steam-boiler at the slip, and for general purposes, since there is no organised system for supplying the peat from the moors, which are three or four miles distant from the town, the consequence being that the gathering and cartage of the peat costs as much as 4s. or 5s. per ton over and above the cost of cutting and drying, or, in all, 6s. or 7s. per ton. At that cost it is evidently better to use coal, which is so much more easily obtained, and which, being double the fuel value of peat, is not much dearer.

From my own experience in the use of peat as fuel, I consider that, wherever it can be had on the spot, and, with a fuel value one-half that of coal, can be put down at a cost of 4s. per ton at the place where it is to be used, it may be advantageously substituted, when coal, under the same circumstances, costs more than 10s. per ton; but if coal can be had for 10s. per ton, or less than that, there would be a disadvantage in using peat.

When the place where peat is to be used is far distant from the moor where it is cut, the cost of its carriage, under the most favourable circumstances, amounts to twice as much as the carriage of coal, because, the fuel value being only half that of coal, two tons of peat are required for one ton of coal. This necessarily limits the use of peat as fuel to places near the moors where it is cut.

Besides the compression of peat, various other modes of improving it for use as fuel have been tried; the general principle of all these modes of treatment is the kneading or pugging of the peat, so as to give it a more uniform and compact texture and greater density. Peat which has been prepared in this way, will have, when dried, a density sometimes equal to that of coal; but I have never been able to perceive how these operations can be advantageously applied to peat, for the following reasons:—In the first place, it must be remembered that, to obtain a ton of dry peat, it is necessary, in the kneading or pugging, which is intended to improve its texture, to operate upon five tons of material. Supposing that to be rendered practicable by suitable mechanical contrivances so as not to cost more than it is worth, or more than is proportionate to the consequent improvement of the peat, it must be remembered that this kneading or pugging of the peat does not separate the water—it does not dry the peat. This—which is the greatest difficulty of all in regard to the use of peat—still remains to be done; and, even admitting that some of the water may be separated by the pugging, there will be, at least, three tons of water to evaporate in order to obtain one ton of dry peat.

The idea of employing heat to evaporate that water,—of drying the peat artificially, is quite out of the question, since the consumption of fuel for that purpose would be quite disproportionate to the value of the peat obtained. The only plan of drying that is practicable is air-drying, and to dry peat by exposure to the air it must be spread out over a large surface. Every ton of dry peat will require 75 square yards of drying ground; and if the quantity of peat required every year is 30,000 tons, the area of the cutting ground will be one square mile at least. Now, if the peat is to be submitted to the operation of kneading or pugging, and has then to be dried by exposure to the air, it must, if the pugging machinery is fixed, be carried to the machine, and then carried back to the spreading ground. This carriage to and fro will amount to ten times as much as the carriage of the dry peat itself, and must evidently add considerably to the cost. If, on the other hand, the pugging machine be locomotive, another obstacle to the adoption of this plan of treatment arises from the necessity of carrying out the cutting and spreading of the peat within a very limited time, as I have already pointed out.

Now, when we consider all these circumstances, and compare the cost of applying this treatment to peat with the results effected by its application, I think it must be obvious to any one acquainted with the peat districts, with the use of machinery, and with the value of fuel, that the attempt to apply such treatment to peat is like breaking a fly upon a wheel, that the means are totally disproportionate to the end, and that the use of peat as fuel is altogether dependant on local circumstances, the principal of those circumstances being the want of coal, or the high price of it, and the presence of an abundance of peat of good quality. These are the circumstances which would determine the consumer of fuel in choosing peat or coal. It is entirely a question of cost. To the landowner there may be, in some cases, other inducements to promote the use of peat in the place of coal, such, for instance, as the employment of a population which would otherwise be in idleness; the desire to clear away the peat and make land available for agricultural purposes; but these circumstances are all incidental, and of a nature foreign to the true merits of the question as to the value of peat as fuel.

I am quite convinced, however, that there are many places in the Highlands of Scotland, and perhaps also in Ireland, where the concurrent influence of a variety of circumstances favourable to the application of peat as fuel is sufficiently great to admit of a vast amount of good being effected by carrying out the cutting of it on a large scale. Thus, for instance, steam communication between Glasgow and the various ports of the Western Islands of Scotland is still very expensive, on account of the necessity of sending out the coal for the return passage of the steamers. The cost of the coal consumed in the steamers running between Glasgow and Stornoway is about 80*l.* each passage, and there is much more than a mere probability that a judicious and liberal-minded application of capital would be successful in establishing the use of peat as fuel in those steamers on their return passage to Glasgow. The applicability of the peat for this purpose is indubitable. I have employed peat as the only fuel for steam-boilers during the last four years, and have found it to answer admirably. It has also been tried by Mr. James Napier, of Glasgow, on board his steamer, the *Lancefield*, and he is of opinion that it might be used in the place of coal. The fact of its being only half the fuel value of coal would in this instance be counterbalanced by the advantage of its cost being less than that of coal. Besides this, the steamers running between Glasgow and the Western Islands are chiefly supported by the freights from Glasgow; their return cargoes are sufficiently small to admit of their using peat as fuel, although a greater weight and bulk would be needed of it than of coal. Moreover, the peat being a natural production and encumbrance of those islands, its use for this purpose would at once be a means of establishing a productive industry, and also of affording what is so much needed there—a greater opportunity for employment; while, at the same time, the condition of the people would be bettered, and, by the removal of the peat, land would be cleared for cultivation, and the climate improved and rendered less unfavourable to vegetation. At present, however, the peat deposits of these islands and of the Highlands generally, though containing the elements of social amelioration, of industry, and of wealth, lie like a huge inanimate chaos, burying the land which might yield abundant harvests, preventing the labour of the inhabitants, and hindering the development and maturing of the crops on those few patches of ground which are yet cultivated.

The next application of peat, and that to which I purpose to refer more especially in this paper, is the manufacture of oils and paraffine from it by distillation.

It will, doubtless, be remembered that, in the year 1849, great interest was excited in Parliament, and throughout the country, by the announcement, that a method of obtaining valuable products from the peat of the Irish bogs had been discovered, and that a company had been formed for the purpose of carrying out, on a large scale, the manufacture of those products from peat in Ireland.

The proposed undertaking was very warmly supported by the press, and was described in a popular journal as constituting an Irish El Dorado.

This mode of working peat was devised by Mr. Rees Reece. It consisted in distilling the peat in a kiln, much in the same way as Lord Dundonald, in 1781, distilled coal for tar, oil, &c., with this difference, that Mr. Reece employed a kiln, constructed more in the form of an iron smelting furnace at the bottom, and that he employed, as in such iron furnaces, a blast of air for the purpose of maintaining the combustion of the peat at the lower end of the kiln, by which means heat was produced for distilling the peat at the upper part of the kiln. By thus distilling peat a tar was obtained, which, on subsequent distillation and treatment, yielded oil and paraffine.

In consequence of the very great public attention directed to this project, an investigation of the subject was instituted, at the suggestion of Lord Clarendon and the Chief Commissioner of Woods, by the chemical officers of the Museum of Irish Industry, under the direction of Sir R. Kane. The results of that inquiry were published in a Blue Book in the year 1851.

Among the products which were shown to be obtainable from peat were paraffine and certain hydro-carbon oils, which it was proposed by Mr. Reece to use as solvents of India-rubber and for lubricating machinery.

These oils and paraffine were obtained, in the first instance, as tar, by the distillation of the peat; and the results given in Sir R. Kane's report go to show that there is no serious difference in the nature and amount of the produce, whether the distillation of the peat be conducted in close retorts or in kilns, as proposed by Mr. Reece.

The substance of the remainder of Mr. Paul's paper was read by him at the last meeting of the British Association, and has already appeared in the Chemical News.

PHARMACEUTICAL SOCIETY.

Wednesday, January 9.

Mr. P. Squire, *President, in the Chair.*

It was announced that the Pereira Medal for 1862 had been obtained by Mr. Sanger.

A fine specimen of an *Astragalus* was exhibited, with the tragacanth *in situ* as it exudes from the incisions made into the shrub. It was presented to the Society by Mr. Pertwee, of Smyrna.

The subject for the evening was "*The Use of Methylated Spirit in Pharmacy:*" but there was a very small attendance of members to discuss this important question.

In opening the discussion Mr. Deane read a letter from an active member of the Society, at Leeds—Mr. Reynolds—who would have been present at the meeting if he had not been prevented by his lectures at the Leeds School of Medicine. Mr. Reynolds said in his letter that, as the cost of alcohol was at least twenty per cent. of the pharmaceutist's outlay for drugs, the subject was a most important one to members of the Society. Methylic alcohol was allowed to be added to spirit to make it nauseous, but it had been found by manufacturing chemists that methylated spirit could be purified. He would not enter into the question of purification, but would remark that, when slightly flavoured, the taste of methylated spirit was hardly distinguishable. The poor were almost entirely supplied with methylated medicines; but the manufacture of sweet spirit of nitre was the chief field for the use of methylated spirit. If manufacturing chemists would tell the extent to which the methylated spirit was used, the members of the Society would stand aghast at the quantity. In Leeds there was hardly a druggist who did not use it, and there was good evidence to show that 12,000 gallons per annum were used in the manufacture of what are called "Indian spirits." One chemist alone uses 1000 gallons of sweet spirit of nitre in the production of these compounds, and there were few whose hands were quite clean from the use. [A sample of one of these compounds was exhibited. It was called "Cordialised and Highly-concentrated Indian Brandee," and was evidently a strong spirit, flavoured with sweet nitre, and sweetened—much more of the nature of a liqueur than a medicine. It is retailed in Leeds at 3d. per ounce.] Mr. Reynolds proceeded to state that by the use of methylated spirit all distinction between pure and impure medicines was broken down, and he called upon the Society to make a vigorous protest against its employment. It was easy to see why the subject had received so little attention. In the first place, the Excise were gainers, inasmuch as, having put an end to illicit distillation, they had no trouble now to find stills; and secondly, because the Pharmaceutical Society had not spoken out plainly. The fact was, that at least half the members of the Council were interested in the *legitimate* use of methylated spirit, and were afraid that, if attention were called to the subject, the boon might be withdrawn; but, on this matter, he suggested that the manufacturing members of the Council should stand aside, and allow the purely pharmaceutical members to act.

Mr. Deane, having read the letter of Mr. Reynolds, said that the Government had made a mistake in permitting the use of methylated spirit for the manufacture of sweet spirit of nitre, and a still worse mistake in sanctioning the use of the spirit in medicines for the army and navy. If the Government paid for pure spirit, they had first of all received the duty, so no saving was effected by the use of methylated spirit. On the contrary, it having been noticed that the mortality of soldiers and sailors was not affected by the use of methylated medicines, the spirit had come into general use in hospitals and dispensaries. Its use in the drug trade, too, was very extensive: poor druggists were obliged to use it to save themselves from starvation. He suggested that a short Act of Parliament should be passed rendering it illegal to use methylated spirit in any medicine intended for the stomach, and of which alcohol formed an integral part. Spirituous extracts might, of course, still be made with it, for in these the spirit was distilled off; but for all other internal remedies, and even for liniments, the use should be stopped.

Mr. Haselden read a speech deprecating the use of methylated spirit, and suggesting that a report of this meeting should be widely published, and public meetings held to discountenance the use.

Mr. Whipple doubted whether the Government had given its sanction to the use of methylated spirit in medicines for the army and navy. He himself strongly objected to the use. Methyl was a potent poison; and he believed that many of the deaths resulting from the administration of chloroform had been caused by chloroform made from methylated spirit. It was the duty of members of the Society to send out pure medicines, and adhere rigidly to the Pharmacopœia, and if they did this they would not use methylated spirit.

Professor Redwood said that the use of methylated spirit in liniments for the army and navy had received the authoritative sanction of the Army Medical Board. In many cases, too—as in hospitals and dispensaries—the use had been sanctioned by medical men; and when this was the case, the pharmaceutist had no choice but to do as he was ordered. He (Professor Redwood) set his face entirely against its use, and, with Professor Graham and Dr. Hoffman, had objected to its employment for sweet spirit of nitre. But the Excise authorities had over-ruled the objection, because so much spirit of nitre was made from illicit spirit, the manufacture of which it was their great object to destroy. He believed the use of methylated spirit in pharmacy had in some districts opened up an entirely new line of business. In North Wales, he was informed that tincture of assafœtida was now sold on a scale hitherto unknown. Assafœtida used to be taken solid, but now that a cheap tincture could be bought, it was preferred in that state, and large carboys of the tincture were sent to some small towns. Great difficulties, he thought, would be experienced in any attempt to stop the use of methylated spirit for medicines. With regard to the question of purification, the Professor said that methylated spirit had never been purified; that is to say, the methylic alcohol had never been removed from it. The empyreumatic oils, however, which accompany wood spirit could be oxidised by a little nitric acid added to the spirit, and the spirit could then be distilled tolerably free from flavour, and was better for the manufacture of spirit of nitre than the illicit spirit formerly used, which, in some cases, was loaded with aldehyde, a most noxious compound.

The Chairman remarked that, in his report to the Excise, Professor Redwood had opposed the use of methylated spirit for nitre, and now he said it was better for the purpose than pure alcohol.

Professor Redwood said that, when carefully made from methylated spirit, it was better.

A Member said that the question of the ill-effects of methylated spirit was one for medical men. He believed himself that no worse effects had ensued from chloroform made from methylated spirit than from that made with pure alcohol.

Mr Standring was altogether opposed to the use of methylated spirit, and he believed the strongly-expressed opinion of the Society would induce the Government to withdraw the permission for its use in army and navy medicines.

Mr. D. Hanbury, jun., said he also was opposed to the use for external as well as internal remedies. It was difficult to say what course should be adopted to stop the evil; but it was a matter which called for watchfulness on the part of the Council.

Mr. Deane remarked that Professor Redwood had admitted that methylated spirit could be purified, and it was folly to say it could not be. It might not be deprived of the methylic alcohol, but the disgusting flavour was got rid of by the use of nitric acid. He reiterated his protest against the Government sanction for the use of methylated spirit in medicines for the army and navy.

Mr. Hills spoke against the use of anything but pure spirit in pharmaceutical preparations. He thought methylated spirit advantageous for manufacturing purposes.

The Chairman said the Pharmacopœia Committee was opposed to the use of methylated spirit even in liniments, for fear it might stray into other preparations. The opinion of the entire Committee, and of the College of Physicians, was against its use.

Mr. Long was opposed to the employment of anything but pure spirit.

The Chairman said he hoped that some arrangement on the subject would soon be made; and the meeting adjourned.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, December 30, 1862.

E. W. Binney, *F.R.S., F.G.S., President, in the Chair.*

A paper was read by Edward Hull, B.A., F.G.S., on the "*New Red Sandstone and Permian Formations as Sources of Water Supply for Towns.*" The paper commenced by pointing to the advantage enjoyed by most of the large towns of the central counties from their geological position, when built on the new red sandstone. First, from being in proximity to coal; second, from having a dry foundation; third, from having easy access to building stone; and, fourth, from the fact of their resting upon natural reservoirs of water stored up in the sandstone itself. The author considered that many of these towns had not taken full advantage of this last-named source of water supply, partly from a distrust of the resources of the rock, and partly from the failure sometimes sustained in consequence of the positions for the wells having been selected without a proper regard to the geological structure of the country. The excellence of the new red sandstone and the lower permian sandstone as sources of water supply, was shown to depend upon three qualities. 1. Their porousness; 2. Homogeneity, or uniformity of structure and composition; 3. Filtering powers. Each of these were treated of in detail, and with examples from several wells in Lancashire and Cheshire. The author then referred to the failure of the attempts to obtain fresh water at Rugby, and a sufficient supply at Wolverhampton; and he maintained that an abundant supply might have been found at the latter town had the position for the well been selected with due regard to the geological structure of the country. The author then proceeded to lay down certain rules of general application for the selection of proper sites for wells, and illustrated the subject by reference to a well now being sunk under his direction at Whitmore, for the supply of the railway works and the town of Crewe. The position of the well is in a *trough,* both geographically and topographically, and on a four-inch bore hole being sunk to a depth of 148 feet the water ascended to the surface with a head of four feet, and has continued to flow without diminution for the last six months. The well, which is being made within

100 yards of the bore hole, has only reached a depth of 60 feet, but already yields 250,000 gallons per day. In conclusion, the author expressed his conviction that the question of water supply from the strata was becoming every day more a geological one, just as was the discovery of coal beneath the formations which overlie the coal-measures.

NOTICES OF BOOKS.

Chemistry. By W. T. Brande, D.C.L., F.R.S.L. and E.; and A. S. Taylor, M.D., F.R.S. London. 1863.

A most comprehensive and compact volume—indeed, a dictionary of chemical facts. The authors repudiating, with indignation and some harshness of expression, the more modern methods of treating the facts of the science, have constructed a text-book on the plan in vogue before the great discoveries of types and homologous series had been made. But notwithstanding that the relations of bodies to another are but occasionally traced in the present volume, yet it has one considerable merit—its information is recent, and is conveyed in clear language. Its index of some sixty closely-printed columns shows with what care new discoveries have been added to well-known facts. Cæsium, rubidium, and thallium are found in their appropriate places in the text; while of the processes of spectrum analysis and dialysis concise descriptions are given. As might have been expected from the special reputation as a toxicologist of one of the authors of this volume, great care has been devoted to all the paragraphs in which chemical facts bearing upon physiology are discussed. We have mentioned what many will consider a grave defect in the "Chemistry"—namely, the absence of system in the book. If, in justice to the authors, we have also pointed out some of its merits, we must also ask Drs. Brande and Taylor why they speak so contemptuously of the most illustrious chemists of the day? Not that they do so by name, but by implication only. They affirm, in the words which we quote below, that such men as Laurent, Gerhardt, Hofmann, Kekulé, Odling, and Brodie have been wasting their time over fond and foolish fancies. "Some chemical books," say Drs. Brande and Taylor, "are so filled with symbols, formulæ, and mystical language as to conceal the great facts of chemistry in a haze of technicality. . . The numerical value of atoms and volumes is not of so much importance to a student as a correct description of the properties and uses of the substances which they represent. On this part of the subject much labour appears to us to have been wasted by certain writers. They have apparently been engaged in working out an idea, and seeking for some Utopian standard of perfection in a new system of notation. . . . Thus, instead of pursuing the inductive method, and fitting hypotheses to facts, they have introduced a deductive system, by which facts are made to tend to hypotheses, and the elementary composition of bodies is altered, in order that they may correspond to certain artificial types." Who has altered "the elementary composition of bodies, in order that they may correspond to certain artificial types?" we ask. Is not the deductive method a most excellent and necessary one? Has it not led to some of the most brilliant discoveries of the present day? It is remarkable that Drs. Brande and Taylor almost completely omit to notice the successful application of hypothesis to the discovery of anhydrous acids; of the mode of artificially constructing many organic acids, and other complex natural bodies; and of the missing terms in many an homologous series. That the authors of the present volume have not sought an "Utopian standard of perfection" is evident from their resting content with the formulæ C_2H_2 ($C = 6$) for olefiant gas (p. 316), and C_2H_2Cl for its normal chloride (p. 318), in spite of the countless experiments which force us to double these formulæ. Consistently enough, marsh-gas is given as CH_2 (p. 309), not a hint being dropped as to its relations to the methyle series.

From what we have said, our readers will perceive that the book under review is rather a collection of descriptions of chemical phenomena, from various points of view, than a careful and consistent digest of the most important chemical facts. It is a dictionary, not a treatise; but its moderate cost, and the mass of information which it contains, entitle the volume to considerable praise.

NOTICES OF PATENTS.

2489. *Hardening Iron and Steel.* E. Partridge, Smethwick, Staffordshire. Dated October 5, 1861. (Not proceeded with.)

The inventor prepares a composition for hardening iron and steel, by mixing together the following ingredients: ferrocyanide of potassium, nitre, and common salt. These are finely powdered, intimately incorporated, and set fire to, for the purpose of producing an alkaline substance, fusible by heat, and suitable, when mixed with wood or animal charcoal, for the application mentioned in the title. The product, so prepared, may be used also in a liquid form, solution of ammonia being employed as the solvent.

In conducting the process of hardening, the inventor first heats the article, whether of steel or iron, to the temperature of melting lead—a bath of this molten metal may be employed with advantage; the heated iron is then removed, and dusted over with the powder, or immersed for a moment in the ammoniacal solution, and, according to the degree of temper required in the finished article, cooling at once in the air, or immersing again for a short time in the melted lead.

The efficacy of ferrocyanide of potassium for the purpose of hardening iron and steel is so well known, that there can only be a question as to the best method of applying it. The instructions given would entail the loss of a certain quantity of ferrocyanide in the production of a cheaper salt—the carbonate of potash. It would surely be more advantageous to employ direct the crude mixture of these ingredients as obtained in the manufacture of the yellow salt.

2605. *Smelting Copper, Gold, and other Ores.* H. McMeikam, Stratford, Essex. (A communication.) Dated October 18, 1861.

The patentee claims the use of carburetted gases having an affinity for oxygen for the purpose of effecting, at a high temperature, the reduction of metallic ores within the substance of the quartz or other matrix. Then, without fluxing the siliceous gangue, the ores so treated are submitted to the operations of crushing and washing, whereby the heavier metallic particles are separated and collected.

This process is applicable to the reduction of the red oxide of copper, the blue carbonate, and malachite ores, but cannot be followed with advantage in the treatment of copper pyrites and other sulphurets, since there would be a great difficulty in eliminating the sulphur, more especially when the ores are incrusted with a considerable proportion of stony matrix.

2607. *Manufacturing Oxygen Gas and Obtaining Certain other Products.* J. Webster, Birmingham. Dated October 19, 1861.

The details of this patent having so recently been discussed in the pages of this Journal,* it is only necessary to recapitulate the main features of the invention. Nitrate of soda is mixed with a certain proportion of oxide of zinc,

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* *Vide* Chemical News, vol. vi., pp. 218, 287, and 299.

or peroxide of iron, and subjected to a low red heat in close retorts. The gaseous products obtained are oxygen, contaminated with variable quantities of nitrogen, and nitrous acid vapour, the latter of which may be easily condensed by passing through water, whilst the residue in the retort consists of caustic soda and oxide of zinc. These two may be completely separated by the action of water, when the caustic soda dissolves, and the oxide of zinc serves again in a repetition of the process. The soda solution is applicable to the manufacture of soap, whilst the mixed oxygen and nitrogen gases answer excellently well for ordinary use in the oxyhydrogen blowpipe, for increasing the illuminating power of coal-gas, and for a variety of metallurgical and other purposes.

2609. *Manufacture of Titanic Pig Metal or Alloy of Titanium and Iron.* Robert Mushet, Coleford, Gloucestershire. Dated October 19, 1861.

This invention consists in smelting the ore of titanium, known as ilmenite, together with the red hæmatite iron ores of Cumberland and Lancashire, so as to produce in the blast-furnace an alloy of titanium, iron, and carbon, possessing properties which render it superior to ordinary pig iron.

2637. *Manufacture of a Certain Metallic Alloy.* Robert Mushet, Coleford. Dated October 22, 1861.

The principle of this invention is similar to that last described. Instead of using the hæmatite ores of iron, the patentee employs the native carbonates containing manganese (such as the spathic ores of Durham), mixing these in the blast-furnace with the ilmenite formerly specified, in order to produce an alloy of iron, manganese, titanium, and carbon. In lieu of the Durham ore, a certain proportion of black oxide of manganese may be added to the ingredients employed, for the production of titanic pig metal, according to the instructions conveyed in the specification of the patent No. 2609.

2618. *Carburetting Gases for the Purpose of Illumination.* F. J. Evans, Horseferry Road, London. Dated October 19, 1861.

It is proposed to augment the illuminating power of coal gas by passing a current of the gas through a system of hot pipes, and thence into a reservoir containing benzol or other volatile hydrocarbon.

Grants of Provisional Protection for Six Months.

2419. John Watt, Graham Street, Walworth, Surrey, and Thomas Snaith Haviside, Cornhill, London, "An improved method of treating flax and other similar vegetable fibrous substances to be used as substitutes for cotton."—Petition recorded September 1, 1862.

3253. Frederick Daniel Deef, Liverpool, and Thomas Cummings Gibson, Ramsey, Isle of Man, "Improved means and apparatus, whereby petroleum and other oils and hydrocarbons can be safely carried and stored."

3289. William Edward Newton, Chancery Lane, London, "Improvements in preserving animal substances."—A communication from Victor Pierre Celestin Baud, Rue St. Sebastien, Paris.

3297. Marshal Franklin Benton, Percy Street, Bedford Square, London, "Improvements in the manufacture of gunpowder."—A communication from Samuel Ricker, Frankfort-on-the-Maine.

3299. Richard Archibald Brooman, Fleet Street, London, "Improvements in treating liquorice root to obtain liquid and solid extracts therefrom."—A communication from Frumence Nicolas Frezen, Paris.

3317. Edward Toynbee, Lincolnshire, "Improvements in extracting oils and fatty matters from shoddy or refuse wool, skins, or skin pieces, glue pieces, cotton waste, and other animal or vegetable matter, and in producing an artificial manure."—Petitions recorded December 10, 1862.

3331. Charles Hancock, West Street, Smithfield, and Stephen William Silver, Bishopsgate Street, London, "Certain compounds and substances applicable for electric insulation and other purposes."

3334. Samuel Fox, Stockbridge Works, Deepcar, near Sheffield, Yorkshire, "Improvements in retorts and apparatus employed for the manufacture of gas, and also in purifying gas."—Petitions recorded December 12, 1862.

3353. John McInnes, Liverpool, and Elijah Freeman Prentiss, Birkenhead, Cheshire, "Improvements in the distillation and treatment of petroleum and other like oils to obtain products therefrom, and in the apparatus to be used therefor, parts of which can be applied for distilling other liquids."

3361. John Louis William Thudichum, Kensington, London, "Improvements in collecting human excreta, and in the apparatus and means employed therein."

3362. George Charles Wallich, Camden Hill Road, Kensington, London, "Improvements in apparatus to be used in deep sea sounding, for ascertaining the pressure and for raising specimens of the water."—Petitions recorded December 16, 1862.

3396. John Louis William Thudichum, Kensington, London, "Improvements in the preservation of beer and other fermented liquids, and in the apparatus and means to be employed therein."—Petitions recorded December 18, 1862.

Invention Protected for Six Months by the Deposit of a Complete Specification.

3383. Edmond Lepainteur, Paris, "Improvements in the fabrication of a salt for dyeing textile materials."—Deposited and recorded December 18, 1862.

Notices to Proceed.

2277. William Schnell, Charlotte Street, Fitzroy Square, London, "Improvements in extracting the sulphur and sulphurous acid from the oxy-sulphuret of calcium which is contained in the residues or waste material obtained in the manufacture of soda."—A communication from Ludwig Mond, Hesse Cassel, Germany.

2296. William Bird Herapath, Old Market Street, Bristol, "Improvements in treating crystallisable sugar, to render it more suitable for fermentation and conversion into alcohol and vinegar."—Petitions recorded August 14, 1862.

2326. John Garrett Tongue, Southampton Buildings, Chancery Lane, London, "Improvements in processes and apparatus for extracting the natural wax or fatty matters from wool, hair, or other animal or vegetable substances containing the same, and in the application thereof to various useful purposes."—A communication from Augustin Evrard, Paris.

2341. Sandiforth Featherstone Griffin, New Adelphi Chambers, London, "Improvements in apparatus to be used in the distillation of petroleum, or any oleaginous, resinous, or alcoholic bodies."

2446. William Clark, Chancery Lane, London, "Improvements in the manufacture of a blue colouring matter."—A communication from Charles Philippe Collin, Boulevart St. Martin, Paris.—Petition recorded September 4, 1862.

3196. James Adams, of Bridge House, Bow, and William Cornwell White, King Street, Regent Street, London, "Improvements in apparatus for boiling and evaporating."—Petition recorded November 28, 1862.

3331. Charles Hancock, West Street, Smithfield, and Stephen William Silver, Bishopsgate Street, London, "Certain compounds and substances applicable for electric insulation and other purposes."—Petition recorded December 12, 1862.

CORRESPONDENCE.

Preservation of Stone.

To the Editor of the Chemical News.

Sir,—In answer to the remarks contained in your last impression, emanating from Messrs. Bartlett Brothers and Co., if it can be shown that our so-called "secret process" interferes in the smallest degree with that employed by them, we should, without hesitation, discontinue any claim to novelty of idea, and further give up all thought of hardening and preserving stone by a process that appears to us to be the best yet introduced. Let us examine the facts of the case. As explained by Messrs. Bartlett, we took out a provisional patent for the introduction of fluo-silicate of baryta into the pores of the stone. This was thought at the time (June 6, 1861) to be good; but we, nevertheless, abandoned the idea for the following reasons: first, because there are many objections to the use of two solutions; and next, because we believe that chemical combinations, decomposition or double decomposition, to be effected beneath the surface of the stone, is uncertain in its action, because the quantities of each solution coming into contact cannot be determined with any degree of accuracy.

So much for the provisional patent. Now we come to our present mode of dealing with porous stone. We prepare a solution composed of silica, lime, alumina, and potash, and with this we saturate the stone until the pores are filled up. There is no precipitation, but the whole solidifies and becomes as hard as granite. There is no free potash; consequently, there is nothing to wash out. There is no silicate of potash to be mixed with aluminate of potash, as appears to be the case with the gentlemen who complain of our pirating their process. Our plan differs from all others in these important particulars,—that the one and only solution contains silica, lime, alumina, and potash; that these ingredients form one solid mass, there being no precipitation or decomposition, whereas others have no lime—or quantities of potash that are set free—or—but we need go no further; sufficient has probably been said to satisfy an unprejudiced mind, if not that of Messrs. Bartlett Brothers and Co.—We are, &c.

Jesse Rust and Co.

Lambeth Glass Works, Carlisle Street, S.

Boiler Incrustation.

To the Editor of the Chemical News.

Sir,—In the Chemical News of January 10, I have seen with pleasure a notice of a paper read at the Manchester Philosophical Society by Mr. Spence, on the means he adopts to prevent incrustation upon his steam-boilers, which is at once simple and effective if rightly managed. It is an observed fact, that where there is much free soda in solution in a boiler small portions of it are carried off with the steam, and is hurtful in practice to some of the stuffing in the engine. At the same time, it may be remarked, soda is of no use in some sorts of water for preventing incrustation. In the beginning of the year 1858, I read a paper before the Glasgow Philosophical Society, which was published in their *Transactions* for that year, upon "Incrustations on Steam-boilers." I send you the following short extract, which embraces the substance of the whole subject, and will show that Mr. Spence's experience completely verifies my proposition:—

"I have lately had my attention drawn to the whole subject, as a matter requiring a little investigation, and have got a few samples of crusts to analyse, some of which I have given. Where the crust is carbonate I have found little difficulty in preventing its formation—only a little care on the part of the engineer. As we have seen, this falls as a precipitate by the mere boiling of the water. If every night, after the fire is damped down, and a little time allowed for this precipitate to subside, it be then blown off from the bottom, little or no incrustation will ever form upon the boiler. Such crust as the sample shown, is always seen in nightly layers, the result of a deposit upon the boiler, which hardens and cakes by standing, and could all be removed by mechanical means and attention. I have been told by engineers who have adopted this system of blowing off, that they do not require to clean their boilers over once a-year; while other parties, using the same water, require to clean and chip off the crust every three months at least."

"The sulphate crust, as already stated, is a crystallisation upon the surface of the boiler, and cannot be removed by mechanical means. It is, however, easily and thoroughly decomposed by carbonated alkalies; the cheapest is soda. It has been already stated that the only defect in soda was its being added indiscriminately, and in too large quantities—a circumstance easily avoided. I make the matter a chemical question, analyse the feed water to ascertain the amount of sulphate of lime which is present in the gallon, take the size of the boiler and the quantity of feed water added per day. I then calculate the amount of carbonate of soda that will exactly neutralise the sulphate of lime in the quantity of feed water taken per day, which is dissolved in a small iron tank placed above the boiler. If the engine works twelve hours I form a syphon or tap that will run this soda liquor into the feed water in that time, so that at no time is there an excess of soda in the boiler, and thus the lime is converted into a carbonate which precipitates, and may be removed by mechanical means, as I have described. I have only had the opportunity of trying this in one boiler, using the filthy water of the Kelvin. The blowing-off was not carefully attended too: nevertheless, after six weeks, the usual time of cleaning and scaling, which generally gave a crust of $\frac{3}{16}$ths of an inch, there was nothing but a loose, sludgy deposit, which was brushed off with a hard hair brush."

There is no patent in the way of using this process; it is simple and effective with a little attention. It preserves, rather than otherwise, the boiler, and would, I think, make explosion less frequent; and I trust that the publication of Mr. Spence's experience will induce its adoption with water containing sulphate of lime.

I am, &c. James Napier.

Glasgow.

MISCELLANEOUS.

Royal Institution.—On Monday, January 19, a Special Meeting of Members will be held at 2 o'clock. The following lectures will be delivered:—On Tuesday, January 20, at 3 o'clock, Professor Marshall, F.R.S., "On Animal Mechanics;" Thursday, January 22, at 3 o'clock, Professor Frankland, F.R.S., "On Chemical Affinity;" Friday, January 23, at 8 o'clock, Professor Tyndall, F.R.S., "On Radiation through the Earth's Atmosphere;" Saturday, January 24, at 3 o'clock, W. S. Savory, Esq., F.R.S., "On Life and Death."

ANSWERS TO CORRESPONDENTS.

B. S. Proctor.—We shall be much obliged by an abstract.

R. Reynolds.—Received with thanks.

M. W.—Received. Answer next week.

C. E.—White gutta-percha of Dr. Cattell, Euston Road: we do not know the price. We have not the address of our correspondent "Electro-chemist." A reply by post as well.

J. W.—We can only refer our correspondent to the various papers on the subject published in this journal. There is at present no special work on the subject.

J. A. Davies.—Our correspondent is thanked for his communications, but his papers are of too abstract a nature to interest our readers. In reply to the last we may say that we have no doubt the speculations are perfectly novel and quite original.

A Chemist.—The best advice on the treatment of men who have come in contact with poisonous gases we can give our correspondent is to get the sufferers into the open air, and send for a medical man. If the patient is apparently dead, from sulphuretted hydrogen or carbonic acid, artificial respiration should be attempted and kept up for some time. The inhalation of sulphurous acid and chlorine is often followed by severe inflammation of the lungs and air passages.

THE CHEMICAL NEWS.

Vol. VII. No. 164.—January 24, 1863.

TECHNICAL CHEMISTRY.

On a Constant Aspirator and Blower, by M. Carey Lea, *Philadelphia.*

Various modes of producing a blast of air by means of the flow of water have been proposed for laboratory use. A somewhat complicated system, involving the use of two fluids, mercury and water, was published in the *Philosophical Magazine* some years ago, and more recently an application by Dr. Sprengel of the well-known principle of the Catalonian blast furnace was described.

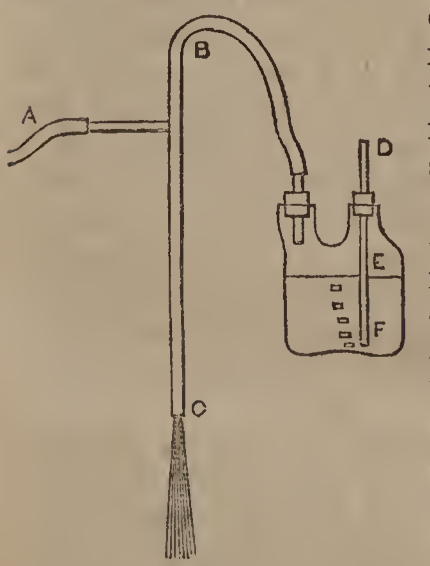
It occurred to me that this principle might be made use of for aspirating, as well as for driving a current of air, and experiment fully confirmed the idea. I have also modified the shape of the arrangement for catching the air described by Dr. Sprengel, introducing the current of water horizontally instead of vertically. I shall first describe the instrument intended for aspirating, and next, the complete apparatus for all purposes.

Fig. 1.

The aspirator is extremely simple. It is nothing more than a tin tube, represented by Fig. 1, about two feet long, and four-tenths of an inch internal diameter, with a branch three-tenths of an inch in diameter and four inches long, inserted horizontally at a distance of four inches from the upper end.

For use, the tube is supported vertically in any convenient manner over a sink. An India-rubber tube, communicating with a water faucet, is passed over the end of the small horizontal tube A. Another India-rubber tube connects the opening B with the apparatus through which air is to be drawn. As soon as the current of water is established, the air is aspirated. In the figure subjoined, the air enters at D, after being aspirated through the Wolfe's bottle, or other apparatus through which it is desired to pass a current of air, enters the tin tube at B, and is drawn through with the water supplied by the pipe A, and escaping at C. The power of this instrument is such that, with one no larger than here described, a column of water of fifteen inches was easily displaced. The end C may be inserted into the funnel of a Liebig's condenser, and the water employed for cooling may be made at the same time to keep the aspirator in action.*

Fig. 2.

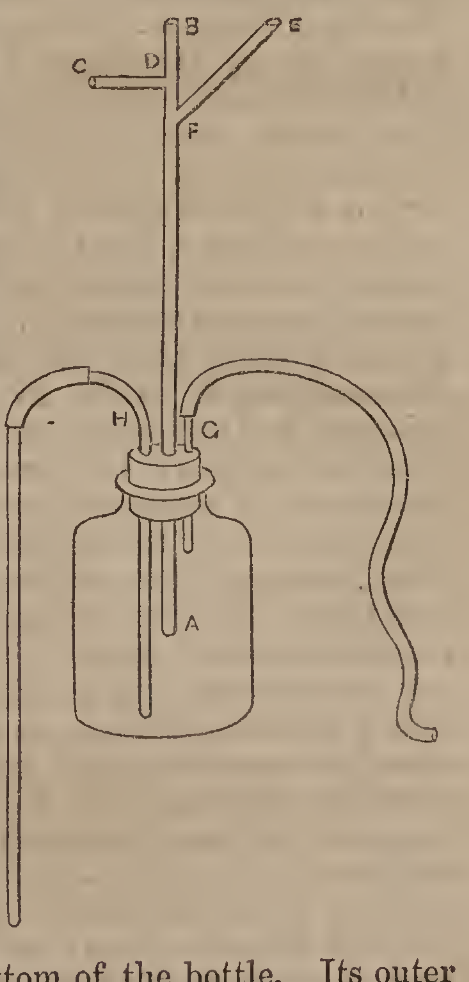


The aspirating tube (which may also at need replace the blowing tube subsequently to be described) may be easily made by the manipulator himself. A stout cork is bored parallel to its axis and to one side of the centre (not in the middle, as in the figure), and then a smaller hole is made at right angles to the first, communicating with it, but not passing further. Three pieces of tube are then fitted into the cork, not allowing either to extend as far in as the junction. The cork is then brushed over with sealing wax dissolved in alcohol.

Fig. 3.

It is, however, a more convenient plan to construct an apparatus capable of combining both the functions of blowing and aspirating. Such an arrangement is exceedingly useful, and can be made with very little trouble. For this purpose, a tin pipe, A B, about three feet in length and half an inch internal diameter, has two smaller pipes, four to six inches long, soldered into it. These are three-tenths of an inch in internal diameter. One, C D, is inserted at right angles, about four inches from the end; the other is inserted about an inch lower, and makes an angle of about 45°. The lower end of the tube passes through the cork of a tolerably wide-mouthed gallon bottle, extending rather more than half way down. The tubes may be made of smaller calibre and shorter, even 18 inches answers very well; but the sizes given are those which I have found to afford the best results. The tin pipes can be made by any tinsmith in a few minutes.

Fig. 4.



Two glass tubes also pass through the cork † of the bottle, a short small tube, G, over the outer end of which an India-rubber tube is passed, and a large tube, H, about half an inch in bore, extending to the bottom of the bottle. Its outer end bends over, and is connected by six inches of India-rubber tube with a straight tube of equal diameter. This last arrangement forms the siphon.

When the apparatus is to drive a blast, an India-rubber tube is connected with a hydrant and attached

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* This form of aspirator (the tube of glass) has been in use for some time at Sheffield Laboratory in New Haven.

† The tube G should extend six or eight inches above the cork, and should not pass below it, as represented in the figure.

to the open end of the short horizontal branch of the tin tube. When the water is turned on, the India-rubber tube, G I, is closed for a moment with the finger and thumb. This starts the water through the siphon, and then a continuous and powerful blast of air is driven through the tube, G I, which may be attached to a blow-pipe, a Herapath burner, or be used in any way desired. The main point is this, that the siphon must be capable of carrying off a larger stream of water than that which is allowed to enter. In this way there is never more than an inch or two of water in the bottle, and some air escapes through the siphon, but without stopping its play. Otherwise the bottle may fill up, and water be driven through G I. The proper balancing is easily attained, and then the apparatus may be put in motion or stopped in a moment, and when in motion goes on indefinitely.

When the apparatus is to be used as an aspirator, the tube I is closed by inserting a glass rod, or passing over it a Mohr's stop-cock. B is closed by a cork, and E is connected with the apparatus through which the current of air is to be drawn, or E may be closed and the connexion made with B.

The arrangement here described for introducing the air and the water at the upper part of the tube was ascertained, after a number of experiments, to be that which was most effective. When the water is thrown horizontally into the vertical tube, it appears to carry with it a larger volume of air than when introduced perpendicularly or in an inclined position. Although the force of the current of air may be regulated, to a considerable extent, by diminishing or increasing the stream of water, yet it is to be observed that there must be a certain proportion between the stream of water and the size of the tube. A large tube ceases almost abruptly to produce any air current when the stream of water is reduced below a certain point, and a small tube will not give an air current of more than a certain force, no matter how great the stream of water. If it is desired that a single apparatus shall produce all degrees of action, a tin tube, like that before described, but a little larger in all its parts, may be provided, with a cork fitting its upper extremity. Through a hole in this cork passes a thick glass rod, or tube sealed at one end, of the same length as the tin tube. The introduction of this cork and rod diminishes the effective calibre of the tube, and enables it to blow or aspirate a gentle current of air with a stream of water which would otherwise have failed to set the apparatus in motion; at the same time, it can easily be removed when a powerful air current is desired. The air in this case is derived wholly from the small inclined tube.

I have found this instrument to be of the greatest utility and convenience; so much so, that I have two of them permanently fitted up in my laboratory. One valuable application is for getting rid of poisonous vapours. In any distillation, for example, the recipient or Wolfe's bottle may be made to communicate with the open air or with a chimney, a cork with a tube may be inserted into the retort, or an extra tube through the cork of the flask in which the distillation is performed, and the flexible tube, G I, passed over it, and a current of air be driven through during the whole operation. Or, if the products of distillation are valuable, the tube may be closed at I by one of Mohr's stop-cocks during the distillation, and at the end a current of air may be passed through the apparatus, sweeping it perfectly clean. In this way I have been able to dissolve oxide of iridium containing osmic acid in *aqua regia*, and drive off the osmic acid without suffering any inconvenience from the latter. The chemist who has once used this arrangement will find it so simple and efficacious that he will be led to employ it when manipulating with substances much less deleterious than osmic acid. It is very convenient in operations where chlorine is to be passed over or through substances, especially where the operation requires to be occasionally interrupted to examine the result.

For driving a blow-pipe, an apparatus of this sort, put together with a bottle and a few pieces of tube, is infinitely more convenient than the most expensive and cumbrous table blow-pipe fed by a double bellows, especially when it is desired to ignite at high temperatures for a length of time, the automatic nature of the instrument removing the necessity for working the bellows. A considerable degree of heat is easily obtained. A silver piece was laid on a fragment of brick, and the flame of a Herapath burner, fed with a stream of air by this instrument, was turned down upon it: the coin withered up immediately. A similar coin was easily fused to a bright button in a porcelain crucible heated from below. Thick brass wire was melted off in drops, &c.

It is not necessary that the instrument should adjoin the blow-pipe or apparatus for which the air current is wanted. In one of mine, the current produced by the tube A B, two feet long, is carried through tubes about sixteen feet, and might easily be carried further without important loss of power.

The quantity of water required for keeping the apparatus in action depends, of course, upon the force of the current of air desired, but is never large. In an experimental trial, it was found that, with a consumption of water of 80 litres per hour, a stream of air amounting to 400 cubic centimètres per minute could be maintained; or, in other words, a cubic mètre (about a ton) of water would suffice to keep the apparatus in motion for twelve hours. This instrument may be appropriately called an *Æolus.—American Journal of Science.*

On Commercial Perchloride of Phosphorus,
by M. MULLER.

THIS compound has been for some time manufactured on the large scale, by saturating a solution of phosphorus in sulphide of carbon by dry chlorine. Some inconveniences attend this process, the worst being the liability of the product to become sulphurous,[*] and another, that it often contains free phosphorus, as is notably the case when the sulpho-carbonic solution is over concentrated, and cooled too suddenly at the beginning of the operation, before all the phosphorus has passed to the state of protochloride. In this case, a fawn-coloured and waxy product is obtained, clearly distinguished from pure pentachloride, which is yellow and crystalline.

If, after the operation, sulphide of carbon, forming the mother waters, is simply decanted, when cold the crystalline crust can be preserved intact for some time; but, after a certain time, it liquefies, and is reduced to protochloride of phosphorus. When much free phosphorus is present, this reduction can be effected suddenly, but then giving rise to dangerous projections.

To obviate these difficulties, M. Muller proposes, in

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[*] When cold, sulphide of carbon is only feebly attacked by chloride of phosphorus, and it is only in the end that the product becomes sulphurous, but, with heat, the reaction is very sensible.

place of the sulphide of carbon here serving as a vehicle, to substitute protochloride of phosphorus, which is also liquid. Phosphorus is then introduced, and the current of chlorine continued unto saturation. The operation can be repeated *ad infinitum;* the author proposes to conduct it in special wide-mouthed bottles, closed with India-rubber caps adjusted by rings, the stoppers being pierced to admit the pipe for the escape of the disengaged chlorine.—*Zeitschrift für Chem. und Pharm.*

PHARMACY, TOXICOLOGY, &c.

A Few Remarks about Extracts, by J. SCHWEITZER.

EXTRACTS contain the soluble active portions of plants, and, in a few instances, of animals, as cantharides, castoreum, &c. They are principally administered in the form of pills, for which purpose they require a certain consistence. Recently, a new sort of extracts have made their appearance, which are not evaporated to a pilular consistence, but are simply reduced to a semi-fluid, syrupy condition, and are called "fluid extracts"; they constitute a different and perfectly separate class.

The solutions which yield the extracts are obtained by means of water, alcohol, or ether. The mode of obtaining these solutions varies with the nature of the substance operated upon, and requires similar manipulations to those used for making tinctures, infusions, or decoctions. We prepare extracts accordingly by—

1. Crushing the fresh plant, and expressing the juice or sap;
2. Macerating the substance in cold water;
3. Making an infusion or decoction; or,
4. Percolating the substance with water, alcohol, or ether.

Extracts prepared by evaporation of the expressed juice of fresh plants, may be taken as types of what solid extracts should be. They possess the active principles of the plant unaltered and in a concentrated form; the chlorophyll, the gummy and albuminous matter, renders them plastic and admirably adapted for the making of pills, and, when properly prepared, they keep soft and good for many years. Albumen in a moist state is one of the most unstable compounds known, and often proves a source of annoyance and loss to the pharmaceutist, so that it has been proposed to remove it altogether from the extract. But this can only be effected by removing, at the same time, all the other coagulable matters. The generally-considered inert and objectionable chlorophyll and albumen, however, cannot be separated without injury to the appearance of the extracts, whose therapeutical effect may also, at the same time, become slightly modified. But it has been found that albumen, when preserved in a perfectly soluble condition, does not decompose and spoil an extract containing it, and that the intermediate state, partly coagulated and partly soluble, is most liable to subsequent decomposition; we may, therefore, with advantage, retain these substances in our extracts. It is hardly necessary to add, that extracts quickly prepared and evaporated at a low temperature are, in every respect, preferable to those obtained by long-continued boiling.

The roots, stalks, leaves, or whatever parts of the fresh plant have to serve for making an extract, must be fresh, in proper season, and sound; they should be well cleaned from all adhering sand before they are crushed, a process which is best effected in a mill. When the roots, &c., are of too dry a nature, the grinding or crushing can be accompanied by an addition of water sufficient to obtain from the pressed mass a volume of fluid equal to about three-quarters of the original pulp. This liquid has to be strained through cloth and quickly evaporated, to prevent incipient fermentation; the inspissation should proceed rapidly, but at a lower temperature than 212°. A long shallow pan, heated by warm water, may receive the strained juice, and the evaporation assisted by a current of air forced over the surface. When reduced to a pasty condition, it may be transferred to a water bath, and finished by constant stirring till it assumes the pilular consistence. In this way a firm plastic extract will be obtained which keeps well, and possesses the natural properties of the original plant in a concentrated and unchangeable condition.

In this manner almost all the extracts of fresh juicy plants may be prepared. The following are those more generally used:—Extracts of aconite, belladonna, colchicum, conium, cotyledon umbilicus, digitalis, hyoscyamus, lactuca, stramonium, taraxacum.

Extracts prepared by maceration in cold water are but few in number, extracts of aloes and liquorice juice being, perhaps, the only ones which require some comment; these are what may better be called purifications of a previously prepared crude extract. The aloes and liquorice are broken into pieces the size of a walnut, and placed with straw in alternate layers, till they fill the vessel, which, for greater convenience, should have a contrivance by means of which the saturated liquor can be withdrawn without much disturbing the vessel itself. The top layer should be covered by a weight, to prevent the water from lifting and disturbing the contents. After the vessel has been filled with cold water, and allowed to macerate for twenty-four hours, the liquor should be withdrawn and replaced with fresh water, repeating this operation two or three times, till the ingredients are exhausted. The clear liquors may each time be evaporated, either on a steam or water bath, the products mixed, and brought to the proper consistence.

The most complex and important of all our drugs is opium, the extract of which is also prepared by maceration in cold water. The nature of opium rendered it, no doubt, a difficult task to devise a process of extraction which should fulfil all requirements; but the perfect chemical knowledge of all its component parts has expunged a number of most unnecessary, and sometimes absurd processes, which were occasionally followed. Simple and effective as the treatment of opium with cold water is, I do not think that the result is so perfect as to defy all improvement. We possess private preparations of opium (*Liquor opii sedativus*, for example) which stand in higher repute than any of the Pharmacopœia opiates. The usual complaints against ordinary opium preparations are their variable strength and exciting properties, the first caused, no doubt, by differences in the drug itself, the other, perhaps, by a peculiar acrid, odorous principle.

The natural cause of the variable strength of opium are the different proportions of gummy, resinous, saccharine, and feculant matter which find their way into the watery solutions containing the active properties of the drug. We find that formerly many eminent pharmaceutists subjected the opium to fermentation, and used the fermented liquor for their preparations. Such processes were usually kept secret, and became only known after the lapse of years, and after the preparation had acquired some reputation. Fermentation undoubtedly removes many of the inert constituents of opium, destroys the objectionable smell, and tends to establish greater uniformity in different samples; and, when followed by a subsequent treatment with cold water, gives results which excel the ordinary Pharmacopœia preparations. To start and assist the fermentation, it is best to add a certain amount of yeast, and even some saccharine matter, to the opium suspended in cold water, and to allow the fermentation to take place briskly. When the fermentation has ceased, the liquor should be filtered and evaporated on a steam or water bath, and the extract allowed to stand for some time. It should then be extracted again with cold water, filtered, and again evaporated, and will then be ready for use. An extract so prepared keeps well, is necessarily free from most of the inert and objectionable ingredients in the drug, and, consequently, of a more uniform and reliable strength.

Besides extract of aloes, liquorice juice, and opium, the following extracts are prepared by means of percolation with cold water—a process which will be fully described hereafter:—Extract of cinchona bark, gentian root, hæmatoxylon, poppy heads, quassia, and rhatany. Extract of senna is best prepared by percolation with warm water, and occupies an intermediate place between extracts prepared by maceration in cold water and those made by infusion.

PHYSICAL SCIENCE.

On Three New Spectroscopes, by M. Janssen.

The first of these instruments is a spectroscope with direct vision; its description is as follows:—Behind the lunette, bearing the slit, and serving as a collimator, is, as usual, a flint prism; but this prism is succeeded by a reflecting crown prism, the faces being normal to the bundle of rays refracted at its entrance and exit; the face of this prism, whence the total refraction is effected, is so inclined on the entrance and exit faces, that the bundle passes out parallel with the axis of the collimator. A second system of two crown and flint prisms exactly similar to the first, and disposed symmetrically, has the effect of doubling the dispersion of the bundle, and of making it issue, not only parallel-wise, but also lengthwise with the axis of the collimator. Each flint prism is the complement of its crown prism, and a screw regulates the movement of the two systems of prisms, so that the position of the active prisms is presented always at the minimum of deviation. This movement serves, moreover, to pass each portion of the spectrum into the middle of the field of view. The rest of the instrument is made in the ordinary way. This spectroscope, being easily managed, and with great dispersive power uniting simplicity of construction, so that its price is very moderate, seems well adapted to be of great service in optico-chemical analysis, which is daily making rapid progress.

The second instrument is a pocket spectroscope, also adapted for direct vision, forming a small folding glass. The adjustment of the bundle is effected by means of a compound prism constructed on M. Amice's principle, formed, as we know, of a central and very dispersive flint prism, joined to two crown prisms with opposed apexes, which correct the bundle of rays. This ingenious disposition has the one drawback of not giving so energetic a dispersion as might be desired, because of the action of the crown prisms, which tend to achromatise the bundle. To remedy this defect, without immoderately increasing the length of the lunette, I used two extra-dispersive flint prisms at 90°, making one body with three cut crown prisms, cut with angles suitable for the correction of the bundle. This system possesses considerable dispersive power, and preserves to the bundle almost all its luminosity, by reason of the feebleness of the interior reflections. The lunette for examining the spectrum carries two objectives placed a short distance from each other. By enlarging the field of view, this arrangement allows the whole of the spectrum to be viewed at one glance. A scale, graven on glass, serves to measure the position of the spectrum rays.

With this little instrument, the solar spectrum can be seen at almost any time, as it is visible in the feeblest and most diffused light. It becomes very easy to follow, in the solar spectrum, the process of the dark bands induced by the terrestrial atmosphere, commensurate with the descent of this planet on the horizon. By substituting this spectroscope for the eye-piece of a lunette with an opening of several inches, and directing the instrument on the moon, a lunar spectrum is obtained in which Fraünhofer's rays can be recognised, and even some terrestrial atmospheric bands.

But this little instrument is more specially adapted for the analysis of flames. I will take, for example, the flame of a candle, in which can be seen consecutively the sodium ray, and those given by oxide of carbon gas burning in oxygen.

I have had constructed, also, a larger model of this instrument for experiments requiring a greater expansion of the spectrum. I have only to add, that all these instruments were made in M. Hoffmann's workshop, and that he paid particular attention to the optical part.—*Comptes-Rendus.*

COURT OF QUEEN'S BENCH.

December 10, 1862.

Simpson and Others v. Wilson and Another.

(*Continued from page* 30.)

Dr. W. A. Miller (examined by Mr. Day): I have given attention to arsenic acid, and its action on aniline for the production of colours. I have read Medlock's specification, and found the words "dry arsenic acid" used therein. The sense in which I, as a chemist, understand the word "dry" is, practically free from moisture. Arsenic acid is a very deliquescent body—less so in its anhydrous state, but it is deliquescent in that state. I took a portion of arsenic acid, which gave no colour to aniline, and exposed it to the air for nine hours. At the end of that time it had increased in weight between 15 and 16 per cent. I give the fact of its producing no colour with aniline as a test of its being anhydrous. I have mixed "dry arsenic acid," that is, arsenic acid practically free from moisture, with aniline, as described in Medlock's specification.

Lord Chief Justice Cockburn: Then you use the term "dry" in the sense of anhydrous?—Witness: I think "dry" is a term that is in common use. I think "anhydrous" means strictly and absolutely free from water. "Dry," I consider, means practically free from water. One is a term used in common life, and the other is a strictly scientific expression. If a thing were dry to the external senses, and yet contained a certain amount of water constitution, I should call it a "dry hydrate." I should think that a correct expression.

Lord Chief Justice Cockburn: Then dry, in your view of it, does not absolutely mean anhydrous?—Witness: Not necessarily. I speak in the abstract, because it depends on how it is applied. It often does mean "anhydrous."

Lord Chief Justice Cockburn: If you meant to express what we understand Dr. Medlock meant to express, namely, "dry," as contra-distinguished from acid in solution, you would call it "dry hydrate?"—Witness: I should call it "dry hydrate," certainly.

By Mr. Day: In my experiments I have used arsenic acid in all states, from the anhydrous to the hydrated acid, containing 66 per cent. of water. I do not mean to say I have made experiments in every degree of hydration, but ranging between those points. The anhydrous gave absolutely no colour.

Lord Chief Justice Cockburn: I take it that we may assume this—supposing "dry" must necessarily be taken to mean "anhydrous," there is an end to the patent.

Sir F. Kelly: We quite agree with that on our side. We say that the article of commerce, known as "dry arsenic acid," will do.

Examination continued: If you took arsenic acid containing one, two, or three equivalents of water, hydrated arsenic acid would be the proper expression to use. "Dry hydrated bichloride of tin" is a scientifically and strictly correct expression. The first experiment in which I got colour was made with 5 per cent. of water. I obtained but a small amount. Whether, commercially speaking, that would be a practical way of procuring the colour would depend entirely on the price given for it. There would be a waste of material. The greater part of the aniline would boil off, and most of what remained would be decomposed by the high temperature. I obtained the most successful result with 66 per cent. of water. The experiments were made in this way. I took equal weights of arsenic acid and aniline in each experiment. The arsenic acid was combined with a certain portion of water, and each was subjected to precisely the same process. The results increased in proportion up to 66 per cent. of water. One hundred parts of arsenic acid in the dry state, and eighty-one parts of aniline, were what I took. With 20 per cent. of water I obtained more colour than with 10 per cent., but less than with 66 per cent.

Lord Chief Justice Cockburn: Does that warrant the inference, that it would be better still if you increased the water further?—Witness: No: I take it that there are two objects in view in the use of water; one to effect the combination of the arsenic acid with the aniline, which would be impossible in the absence of water; and the next, to prevent the rising of the temperature. If you have aniline, you cannot boil it off below 360° F. The boiling point of the mixture with 66 per cent. of water would be much below that of aniline. Diminution of temperature prevents destruction of colour. I have bought arsenic acid of the gentlemen who are in the habit of supplying me with chemicals—Bolton and Barnitt. I ordered "dry arsenic acid," and got it practically anhydrous. I have only bought dry arsenic acid in small quantity for my laboratory experiments. I have never bought it as an article of commerce. I know it has been used by calico-printers, but I am not familiar with it. [The witness exhibited specimens of wool dyed with the colour produced by various proportions of water.]

Cross-examined by Mr. Grove: My experiments were made at the works of the defendants, with acid made by them. For the anhydrous experiment, I took acid which I ascertained to be anhydrous. I took another, which had been boiled down till it contained 19 per cent.; another, boiled down till it contained 5 per cent.: for the larger proportions I diluted the acid. I made my experiments independent of any patent. The mixture was heated gradually, by means of an oil-bath, until colour was developed—when there was any colour. It was heated to 360°, at which temperature the aniline not combined with the arsenic acid would boil away. The aniline is not wasted; it is distilled off, and used again. If I sent for arsenic acid, it would depend on what I wanted it for, whether I should order hydrated arsenic acid. If I wanted simply arsenic acid, I should simply order arsenic acid. Tartaric acid is a combination of two atoms of water and one of acid. It may be called a hydrate. In ordering tartaric acid, I should not order hydrated tartaric acid. In speaking of dry tartaric acid, I should not speak of the anhydrous acid; I should speak of the dry acid which contained water in combination. I do not know the exact date at which I ordered the arsenic acid of Bolton and Barnitt; it must have been twelve or fourteen months ago. I do not know in what sense the patentee may have used the word "dry," but I know I have always used it in the ordinary sense; that is, as practically free from moisture as you can get a thing. When you are told to use a dry thing, you make it as dry as you can.

By the Lord Chief Justice: Arsenic acid may combine with water without holding it. You may get crystals. There is no doubt there is a combination between arsenic acid and water.

The Lord Chief Justice: The question is, whether the term "dry" is satisfied by having water in combination, so long as it is not present in the shape of moisture appreciable to the senses?—Witness: I think you would have to strain a good deal for that. I do not think that is the fair and ordinary sense in which you would interpret it. With regard to metastannic acid, which I have said in my book, when dried in the air consists of $Sn_5O_{10} + 10\ HO$—I have already stated that the term "dry" must be taken correlatively. I do not use it in any strained sense—it depends on what it is associated with. Arsenic acid is a deliquescent substance, and if I am told to make it dry, I make it as dry as I can.

Lord Chief Justice Cockburn: When you speak of deliquescent, the water is visible, not in combination, but only mechanical?—Witness: It is mechanical; it takes up moisture from the atmosphere.

Lord Chief Justice Cockburn: Then, when you are speaking of a thing notoriously deliquescent, and you find that a person is speaking of it as dry, must not that be taken to mean the divesting it of water that would render it deliquescent?

Mr. Grove: It cannot possibly be divested of water so as to render it not deliquescent, because it would become deliquescent again.

Lord Chief Justice Cockburn: What I mean is, whether you may not take the term "dry" not as anhydrous, but as divested of all water except that in combination?—Witness: I can give a reason: I will simply state that this must be read in connexion with other processes, in a good many of which it must be absolutely dry.

Mr. Grove: In your description of metastannic acid you say that acid dried in the air consists of $Sn_5O_{10} + 10\ HO$; when dried at 212° it loses half its water; and by ignition becomes anhydrous. You use the terms "dry" and "anhydrous" in contra-distinction.—Witness: They are relative degrees of hydration.

Mr. Grove: I believe a thing may be anhydrous and still not dry.—Witness: It may be liquid: bichloride of tin is a liquid free from water.

Mr. Grove: It is anhydrous, not *dry*.—Witness: It is anhydrous, not *solid*; that is the proper way of putting it. Anhydrous means free from water: it can have no other meaning than that.

Mr. Grove: In your book you say oxide of copper is made anhydrous by boiling it in water?—Witness: Yes.

Mr. Grove: You do not say you make it dry?—Witness: No: oxide of copper is not in combination with water. Any substance may be mixed in water; that does not affect its anhydrous condition.

Re-examined by Mr. Hindmarsh: Anhydrous tartaric acid is not sold.

Mr. Hindmarsh: What meaning would you attach to the term "arsenic acid" simply?—Witness: I think I should consider it in the solid state, not necessarily so. If the term "arsenic acid" were used generally, it would present itself to my mind solid. If I were told to take "dry," I should make it as dry as possible. In arsenic acid, if you take pains to do so, you may drive off the whole of the water no doubt. Looking at Medlock's specification as a chemical description, it conveys to my mind, that I should obtain the arsenic acid as free from water as I could.

Lord Chief Justice Cockburn: Do I understand you to mean by that that you would make it anhydrous?—Witness: I think so, my lord. If it were immaterial whether it was used in a dry state or not, I should not have been told to take it dry.

By Mr. Hindmarsh: If I applied to a chemist for arsenic acid he would give it me solid. If I asked for dry arsenic acid, I should get it free from moisture, anhydrous. I should send it back if I found any moisture in it. If I were directed myself to make dry arsenic acid for manufacturing purposes, I should say that it did not answer my order, if it practically contained any water.

Dr. A. S. Taylor (examined by Mr. Hindmarsh): I have given attention to the action of arsenic acid on aniline. The term "arsenic acid" imports a solid, consisting of one atom of metallic arsenic, and five atoms of oxygen. It is deliquescent more slowly in the anhydrous than in the hydrated state. It requires about 20 per cent. of water to make it pasty. By a direction to use "dry arsenic acid," I should understand anhydrous acid, that is, arsenic acid without any water whatever. The expressions "dry" and "anhydrous" are often used in chemistry, particularly with reference to acids. They are generally used, I think, as convertible terms by the best authorities. I have found them used in books, and used them myself as synonymous terms. I have tried the action of arsenic acid on aniline, and read Delaire and Girard's patent. A colour may be produced with less water than the proportion stated there, but, according to my judgment, that proportion is necessary for the production of the most perfect colour, and especially for the conversion of the whole of the aniline into colour.

Cross-examined by Mr. Grove: The terms "dry" and "anhydrous" are not frequently used, as terms of contradistinction, by chemists. I know of no authority that so uses them.

Mr. Grove (after reading Miller's description of metastannic acid): The author of this book uses the term "dried" to signify driving off the water, and not making it anhydrous.—Witness; I do not agree with your inference at all. When subjected to a red heat it becomes anhydrous or dried acid.

Mr. Grove: Then no chemist would apply the term "dry" to a hydrate?—Witness: I should think not; anhydrous means dry.

Mr. Grove: "Dry tartaric acid has no action on dry carbonate of soda." Is either of the bodies there spoken of dry in the sense of anhydrous?—Witness: Certainly not. You will not find a grain of anhydrous tartaric acid in any shop in London. When you say "dry tartaric acid," and "dry carbonate of soda," it means the ordinary absence of moisture.

Re-examined by Mr. Hindmarsh: If I were told to produce dry arsenic acid, I should make it by Professor Graham's process, by which I should get anhydrous or dry acid. If I were directed to use dry arsenic acid, by a man conversant with chemistry, I should certainly not use a solution of the acid. A person conversant with chemistry would not use the word "dry" without some object.

Dr. Letheby (examined by Mr. Day): I have given my attention to arsenic acid and its action on aniline. Arsenic acid is deliquescent in the anhydrous state, but not so freely as in the crystallised or hydrated state. I have read Medlock's specification; and by the words "dry arsenic acid" I understand the anhydrous acid—arsenic acid as described in books with the water driven off. In that state, practically, there would be no water. I have made experiments by Medlock's process. I used what I understood to be dry arsenic acid, that is, the anhydrous; and I have used it with various quantities of water. When I mixed equal parts of the dry (anhydrous) acid, and let them stand together, there was no chemical action. They stood together nearly twelve months without the least evidence of change. When I applied heat to the mixture there was no change. The aniline passed off, but there was no production of colour. When 5, 6, or 10 per cent. of water was added I began to get colour. I do not think it a successful result with those proportions; it would be a question of price. In the first place, a great part of the aniline must be lost, or, if saved, there would be another process of acting on arsenic acid; and in the second place, the amount of colour obtained would be so small that the operation would be unprofitable. To obtain a good practical result I was obliged to mix very nearly equal weights of water, aniline, and arsenic acid in order to get arseniate of aniline. I found that practically no colour could be produced with the mixture until there had been a chemical combination of the arsenic acid with the aniline, that is, until you have arseniate of aniline. That is the first step in the process; you cannot get colour without that; and you cannot get the combination without 60 or 70 per cent. of water. The better quantity I find to be an equal weight of water.

By the Lord Chief Justice: I mean you cannot get the whole of the aniline converted into colour. I could get colour with 5 or 6 per cent. of water. To get the colour, there must be arseniate of aniline formed. With a little water, you get a little of the arseniate, but you do not get the whole of the aniline converted into the arseniate.

By Mr. Day: When a small quantity of water is used, the greater part of the aniline is either wasted, evaporated away, or distilled over. At the high temperature at which aniline boils, no doubt some is decomposed. I have read Delaire and Girard's specification, and have experimented with their process. I obtained a successful result: the proportions are those by which, I think, the most successful results are obtained. The proportions are about equal. I find you do not get a good proportion of arseniate of aniline until you come to 40 per cent. of water, and then, from that to 100, you get the best colour.

Mr. Day: Do you find the word "dry" used in contradistinction to "anhydrous," or synonymous with it?—Witness: That will depend very much on circumstances, and the substance spoken of. If there be a hydrated body, and the word "dry" is used, I take it to mean "anhydrous," unless the words "dry hydrate" are used. I use the word generically as synonymous with anhydrous; but it may be qualified in another way, as, for instance, by saying, "dry hydrated lime." That would mean, as dry as could be got with the due proportion of water to make the hydrate. That is the sense in which I find the word used in books on chemistry. Thirteen per cent. of water would give hydrated arsenic acid, or three equivalents of water with one of arsenic acid.

By Mr. Hindmarsh: In Phillips' translation of the Pharmacopœia, I find the word "dry" used in connection with nitric acid to mean "anhydrous" or "real" acid, as it is called there. In Graham's "Elements of Chemistry," anhydrous sulphuric acid is spoken of as "dry." The word "anhydrous" means "dry."

By the Lord Chief Justice: I could scarcely know what was meant by "arsenic acid" without some other term. If I sent to a chemist for arsenic acid, I think he would send the dry, because in most, if not all, chemical books, you are told to evaporate it to dryness, and expose it to a temperature little short of red heat; therefore, if any chemist made it according to the instructions of a book, he would make it dry or anhydrous.

Cross-examined by Mr. Grove: I have had arsenic acid in the amorphous or anhydrous form for years, I think.

I have not bought any. There is such a thing as anhydrous nitric acid. There was not, I believe, when Mr. Phillips' book was written. If any one were to use the term "dry nitric acid" now, I should understand the anhydrous. There are three hydrates of arsenic acid, containing respectively 7, 14, and 19 per cent. of water. When I used 60 or 70 per cent. of water, I heated the mixture in an oil bath. I did not use a thermometer, but I know the heat did not go beyond the decomposing temperature of oil, which is 600. I did not say that the aniline which was distilled over and collected is entirely useless. I said that, when distilled at a high temperature in the presence of atmospheric air, aniline did undergo a certain amount of oxidation and decomposition, and that chemically there is a loss. I know that in practice the aniline which distils over is collected, and used again.

By the Lord Chief Justice: It is not altogether decomposed, but there is a loss. Every time that aniline is subjected to heat in the presence of atmospheric air, a certain amount is changed.

By Mr. Grove: I do not know that, when operating in the large, rough, commercial way, the loss of aniline is only about one-half per cent. I was speaking of the loss which occurred when I carried on my experiments according to Dr. Medlock's specification.

Lord Chief Justice Cockburn: He says, when I did that with Medlock's process there was a waste of aniline; but not when I used the other process.

By Mr. Grove: No temperature is spoken of in Medlock's process. But let the temperature be what it may, so as to distil off the aniline, I say there is a loss by that process. Even when water is there, any distillation of aniline is so much loss. If there is any mention of a special temperature in Medlock's specification, I have forgotten it.

Mr. Grove: Medlock says, "I accelerate the operation by heating it to, or near to, its boiling point."—Witness: The boiling point of the aniline.

Lord Chief Justice Cockburn: Suppose you allow the mixture to stand for some time, then he says, "I accelerate the operation by heating *it*"—that is, the mixture, the last antecedent—"to, or near to, its boiling point until *it*"—that is, the mixture—"assumes a rich purple colour."

Mr. Grove: Dr. Letheby says it is the aniline.

Examination continued: The boiling point of aniline is 360°; that would be the maximum. [Specimen of dyed silk produced.] That could not have been produced by the process in the specification. I think that very likely it might have been done with arsenic acid containing not more than 20 per cent. of water; but I say that, with the same proportion of aniline necessary to produce the proportion of dye which would dye that amount of silk, you would get 20 or 30 per cent. more dye by having more water. Mine have been all laboratory experiments.

Re-examined by Mr. Hindmarsh: The waste in consequence of using too little water arises from the circumstance that all the aniline is not converted into arseniate. The arsenic acid must fix the aniline before it will produce colour, and it cannot be fixed without water.

(To be concluded in our next.)

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, January 15.

Colonel Philip Yorke, *F.R.S., Vice-President, in the Chair.*

The minutes of the previous meeting having been read and confirmed, the following gentlemen were balloted for, and unanimously elected Fellows of the Society:—Mr. William Spiller, Atlas Chemical Works, Walworth; Mr. John Noble, Lecturer on Chemistry and Physical Science at the Working Man's College, Halifax; Mr. James Liebig Muspratt, 31, Mornington Road, London.

Mr. John Hooker was admitted a Fellow of the Society, and signed the statute book.

A paper "*On the Atomic Weights of Nickel and Cobalt*," by Dr. W. J. Russell, was read by the Secretary. The author commenced by referring to the indefinite nature of the oxide of cobalt as obtained by the ignition of the carbonate or higher oxides of this metal in air. According to the degree of heat employed, the proto-sesquioxide (Co_3O_4) was formed, or, at higher temperatures, the oxide described by the anomalous formula, Co_6O_7. If, however, the ignition was conducted in the blast of a powerful blowpipe, and the product cooled out of contact with air, the true protoxide of cobalt was formed. To the varying composition of the oxide employed in previous experiments was to be ascribed, in a great measure, the discrepancy noticed between the results in former determinations of the atomic weight of cobalt; thus, Schneider deduced the number 30 as the equivalent of this metal, basing his experiments upon the reduction of the protoxide by hydrogen. Dumas corrected this to 29·5; and Rothoff arrived at the number 29·45. The pure protoxide had the power of slowly absorbing oxygen from the air, even at common temperatures, and, in consequence, a preparation made some eight or nine months ago was found to contain less metal by one or two-tenths per cent. than the freshly ignited product. On comparing the top and inferior layers of oxide of cobalt produced in a platinum crucible by exposure to intense heat, and cooled in air without adopting any precautions, the upper surface was found to be darker in colour—almost black,—and to contain much more oxygen than the brown protoxide beneath. If, however, the intensely-heated oxide was cooled in a current of carbonic acid gas, the substance remained uniform throughout, and contained the lower proportion of oxygen. In proof of this being the true protoxide, it might be stated, that it dissolved in hydrochloric acid without giving off any chlorine, and that a mixture of starch and iodide of potassium failed even to detect the liberation of the slightest trace of that gas. For the purification of the commercial oxide it was dissolved in hydrochloric acid, and the solution evaporated to dryness, taken up again with dilute acid, the silica filtered off, and the evaporation repeated. Being then dissolved in warm water, a current of sulphurous acid gas was passed through the solution to reduce the arsenic to arsenious acid, the excess boiled off, and sulphuretted hydrogen passed in order to precipitate the metals of this group. The iron in the filtrate was peroxidised and removed by ammonia, the cobalt itself being next converted into the red crystalline salt described by Claudet ($2CoCl,NH_4Cl,4NH_3$), which was further purified by re-crystallisation from hot dilute hydrochloric acid. This salt was heated first in air, and subsequently in hydrogen, and the reduced metal converted into protoxide. The arrangement of apparatus employed in the critical experiments which served for the determination of the atomic weight was briefly as follows:—A small capsule of platinum foil, containing the oxide of cobalt, was placed within a larger crucible of the same material, contact between the two platinum surfaces being prevented by the interposition of a thin slab of pipe-clay; the crucible lid was perforated for the reception of a tube which conducted the hydrogen or other gas into the closed chamber; and, in order to prevent the dispersion of the finely-divided metallic particles by the current of gas, a piece of platinum foil was loosely bound over the lower orifice of the tube. In connexion with this tube was a three-way junction-piece and stopcocks, which permitted of hydrogen and carbonic acid being alternately led in from their separate generators; these gases, as well as the air finally employed to displace the carbonic acid, were specially purified and thoroughly dried. The heat of a blast-lamp was employed, and every precaution taken to ensure a constant weight and absolute exclusion of hygroscopic water by the use of chloride of calcium trays in the balance case. The exact weight of the protoxide of cobalt being arrived at, the crucible was filled with hydrogen, and the reduction accomplished in a slow stream of this gas, and, whilst the heat was maintained, the carbonic acid was passed for the purpose of expelling the hydrogen; the crucible and its contents were then gradually cooled, and it was only necessary, before weighing, to replace the carbonic acid by dry air. 100 parts of protoxide of cobalt gave, as the mean of three experiments, 78·588 per cent. of metallic cobalt. In another set of experiments, the mean result was 78·595 per cent.; and in a third series, the amount was 78·59. Working in a precisely similar manner with a second sample of commercial cobalt, the amount of metal obtained was 78·597 per cent.; and with a third sample of cobalt, 78·593 per cent. The entire series of experiments furnished, as the ultimate mean, 78·5926 per cent., from which the atomic weight of the metal was deduced—

Equivalent of cobalt = 29·370.

The author had hoped to have been able to include in the present communication the results of his experiments upon nickel, but indisposition had prevented the fulfilment of this desire; and (through the Secretary) he begged the indulgence of the Society, under a promise of continuing the subject at a future meeting.

A Friend of Dr. Russell announced that the most interesting result arrived at in the case of nickel was the coincidence of its atomic weight with that of cobalt. This conclusion was founded upon a very complete series of experiments, parallel in every respect with those just now described.

The paper next read had for its title, "*On the Action of Sulphuric Acid upon Lead*," by Dr. F. Crace Calvert, F.R.S., in which communication were detailed the results of a series of experiments made by the author in conjunction with Mr. Richard Johnson, F.C.S., of Manchester. It had commonly been supposed that the rate of corrosion or solution of a metal by acids was dependant upon its purity, and that the presence of foreign metals hastened the oxidation by creating galvanic currents in various directions through the mass. Pure zinc was known to dissolve in dilute sulphuric acid much more slowly than ordinary spelter containing lead. The conclusion arrived at by the authors in the case of lead was directly opposed to this opinion, for they found, under all circumstances of temperature and strength of acid, that the purer the lead the more rapidly was it attacked by sulphuric acid. Three samples of sheet lead were made the subject of experiment; the first was common Welsh lead, of the following composition:—

Lead	98·81
Tin	·39
Iron	·36
Copper	·44
	100·00

The second was a good sample of "virgin lead" from Derbyshire, which did not contain an appreciable amount of tin; and the third sample was chemically pure lead. Thirty square centimètres of the sheet metal, digested in 50 cubic centimètres of sulphuric acid of specific gravity 1·84, experienced a loss of weight amounting in the case of pure lead to ·605 grains, whilst only 203 grains were dissolved in the same time from the plate of crude lead. With less concentrated acid, of specific gravities 1·7 and 1·6 respectively, the action was less energetic, but the quantities of lead dissolved were nearly in the same relative proportion. Some further results in corroboration are shown in the following table:—

Loss in Grains experienced by 1 Cubic Centimètre of Lead.

	10 days in cold acid, sp. gr. 1·84.	15 days in cold acid, sp. gr. 1·84.	15 days at 120° F. in acid, sp. gr. 1·84.
Crude lead . .	67	89	418
Virgin lead . .	134	194	458
Pure lead . .	201	279	507

The same was true also with acid having the specific gravity 1·59, at which it is drawn from the leaden chambers; thus, after 15 days, at temperatures varying between 113° and 122° F., the loss was as under:—

Crude lead	11·0 grains.
Virgin lead	15·8 ,,
Pure lead	19·5 ,,

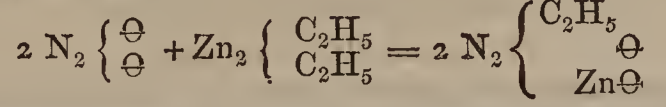
The circumstance which led to the investigation of this subject was gathered from the experience of a vitriol manufacturer, who erected in 1860 a supplementary lead chamber, which, at the expiration of a year's interval, was found to be very much corroded—in some places actually in holes—although his original chamber, constructed more than fifteen years since, remained unattacked. The analysis already given is that representing the composition of a piece of lead taken out of the side of the old chamber, whilst the virgin lead, not containing tin, was a portion of the metal used in the construction of the new chamber. It would appear that tin was the protective ingredient; but before this can be positively asserted, it will be necessary to study the influence of the other metals present, iron and copper, and to this point the authors are now directing their attention.

Dr. Frankland considered the electrical hypothesis quite inadequate to explain the results of Dr. Calvert, inasmuch as both tin and iron were electro-positive to lead. The nitrous gas had, probably, something to do with the corrosion of the leaden chambers, and he wished that fuller particulars had been given in respect to the appearances manifested by the corroded metal.

Dr. Odling felt a difficulty in understanding how it should happen that an electrical action should be set up in a homogeneous alloy. Brass was soluble as a whole in some acids; and if an alloy of copper and cadmium be boiled with hydrochloric acid, not only the latter metal, but with it the copper dissolves.

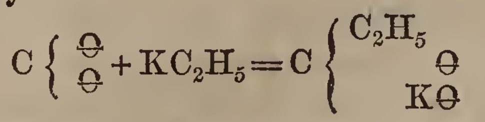
Dr. Normandy was surprised to hear that so small an amount of tin operated so powerfully in protecting the leaden chamber.

Dr. Frankland made a verbal communication to the Society on the subject of some "*Researches upon Leucic Acid*," lately presented by him to the Royal Society. Leucic acid was known to be produced by the action of fuming nitric acid upon leucine; and Mr. Versmann had succeeded in forming it also by the action of chlorine in presence of an alkali. The production of leucic acid by synthesis, which he had now to bring before the Society, bore some resemblance to the formation of dinitro-ethylic acid by the action of zinc ethyl upon the binoxide of nitrogen, thus:—



$$2\,N_2\left\{\begin{matrix}\Theta\\\Theta\end{matrix}\right. + Zn_2\left\{\begin{matrix}C_2H_5\\C_2H_5\end{matrix}\right. = 2\,N_2\left\{\begin{matrix}C_2H_5\\\Theta\\Zn\Theta\end{matrix}\right.$$

and likewise to the formation of propionate of potash by the action of carbonic acid upon potassium ethyl, as shown by Mr. Wanklyn—



$$C\left\{\begin{matrix}\Theta\\\Theta\end{matrix}\right. + KC_2H_5 = C\left\{\begin{matrix}C_2H_5\\\Theta\\K\Theta\end{matrix}\right.$$

The author then described the reaction by which he had succeeded in forming leucic acid synthetically. By operating upon zinc ethyl with oxalic ether he had obtained leucic ether, with the intermediate formation of a body not yet isolated, the bibasic character of which had been affirmed by Wurtz, but denied by Kolbe. This substance was zinco-leucic ether, the constitution of which might be represented by the following formula :—

$$\text{Zinco-leucic ether} = C_2 \begin{cases} C_2H_5 \\ C_2H_5 \\ \Theta \\ Zn\Theta \\ C_2H_5\Theta \end{cases}$$

By treating the ultimate product, the leucic ether itself, with an alkali, or better with baryta solution, it dissolved with liberation of ordinary alcohol, the leucate of baryta being at the same time formed. The acid prepared from this baryta salt was isomorphous with leucic acid.

Dr. Odling inquired, with reference to the formula written on the board, whether if, instead of two equivalents of ethyl, one atom each of methyl and propyl could be substituted? And, if so, would they give rise to the formation of a similar compound; the sum of the carbon and hydrogen atoms then remaining the same, although the constitution would certainly be affected? To cite another example, he would refer to the production of acetylene gas by passing the vapour of chloroform over red-hot copper.

$$2(CHCl_3) + Cu_{12} = C_2H_2 + 6Cu_2Cl.$$

In this case two equivalents each of carbon and hydrogen coalesced to form the new product, acetylene. Might not, then, $CH_3 + C_3H_7$, or, in short, C_4H_{10} in any other form, be taken into the compound described by Dr. Frankland?

Dr. Frankland replied that a more correct knowledge of the constitution of an organic compound was gained in the process of building up, than by the study of the products afterwards obtained on taking it to pieces. He was not yet in a position to answer Dr. Odling's inquiry; indeed, no compounds even of ethyl had yet been made from leucic acid.

A vote of thanks having been passed to the authors of the several communications, the meeting was adjourned until Thursday, February 5.

NEWCASTLE MICROSCOPICAL SOCIETY.

At the December meeting of this Society a Paper was read by Mr. B. S. Proctor, entitled "*Some Remarks upon Light.*"

As the paper was somewhat lengthy, and the points suggestive of discussion numerous, it was agreed to have the paper printed and distributed among the members previous to its discussion at the January meeting. The following is an abstract of its contents :—

The speaker commenced by remarking upon the transparency and brightness which are revealed by a microscopical examination of objects commonly said to be dull opaque white, and the translucency of so-called opaque bodies, of whatever colour, if reduced to thin films. Such common-place observations, he said, had suggested to his mind a variety of questions, the elaboration of which formed the material of his paper. The first point considered was, whether the transparency of white materials was universal, and, if so, what was the difference between the white powders, such as carbonate of lead, which are said to have a strong "body," * and those which, like carbonate of magnesia, are wanting in this property. From the number of white powders which had been examined, all proving to have considerable transparency, it was inferred as a rule that, in white powders, pretty free transmission of light takes place through the individual particles.

The phenomena of body were explained and illustrated by the action of three bundles of glass plates, one being immersed in oil of turpentine, another in water, and the third being dry. The latter was shown to have most body in consequence of the great difference between the refractive power of the glass and the films of air intervening; but where a medium of refractive power approaching that of glass existed between the plates, much less reflection and more transmission took place; thus, magnesia, which appears to have considerable body while dry, loses it when mixed with oil, from their refracting powers being nearly equal. White lead retains its body when ground in oil, because of its refracting power being much higher than the oil. The difference in the body of precipitated coil-lead, and that made by the old process, was next explained by showing that where there was apparently equal comminution in the two, the particles of the precipitated carbonate consisted of clear crystals; while in the other variety the particles consisted of aggregations of smaller particles, each of which was transparent in itself, but in the aggregate was scarcely translucent.

The white surface of dead silver was next noticed, and the microscope showed that the light reflected from it consisted of mixed, not of combined colours.

The attention of the meeting was then drawn to the bearing some of these facts have upon the mounting of microscopic objects; for example, Tous-les-mois was shown mounted dry, mounted in gelatine, and in balsam; its markings were much more clearly seen when the mounting medium was of a refracting power a little less than the starch.

The translucency of white materials having been acknowledged, the question, "Is anything opaque?" was next considered; and it was shown that no definite line could be drawn between transparent and opaque bodies; for, while nothing is found to transmit light without some loss, nothing is found absolutely impervious to light when reduced to a thin film. In most cases where much light is obstructed, some rays pass more freely than others, as indicated in the following table :—

Light Transmitted.

	Through	gold-leaf	is green.
	,,	,, tempered	,, brown.
†	,,	gold chemical films	,, grey violet.
†	,,	,, ,, powders	,, red, purple, or blue.
	,,	silver-leaf	,, grey violet.
	,,	,, chemical films	,, purple or brown.
	,,	copper	,, green.
	,,	antimony	,, grey.
	,,	arsenic	,, brown.
†	,,	platina	,, grey.
†	,,	palladium	,, grey.
†	,,	rhodium	,, brown or blue.
	,,	charcoal	,, grey.
	,,	iodine	,, red brown.

Taking the word "opaque" in its ordinary acceptation, the question, "Are all opaque bodies black when in a fine state of division?" was next discussed; and it was shown by diagrams how the breaking up of a smooth surface diminished the amount of light availably reflected from it. It was also shown that comminution produces subjacent surfaces which reflect some of the light transmitted by the first surface, and that with a certain degree of fineness the more transparent the material the more light will be reflected from the subjacent surfaces; and it was concluded that bodies become lighter coloured by powdering if their power of absorbing light is so small that the reflection from the subjacent surfaces more than compensates for the loss of reflection from the breaking up of the primary surface; and they become darker if the absorbing power is so great that the reflection from the subjacent surfaces does not compensate for the loss of reflection caused by the breaking up of the primary surface.

The doctrines of correlation and conservation were

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* "Body" is the power of hiding that which is beneath them when laid on as pigments.

† These are quoted from Faraday in *Phil. Mag.*; the others were exhibited to the meeting.

then briefly treated, and it was argued that light falling upon black bodies was neither annihilated nor absorbed, but converted into some invisible form of force; the power of the black body being, in some degree, the converse of that possessed by fluorescent and phosphorescent bodies, by which they convert invisible into luminous rays.

The fact, that reflection is not purely a superficial phenomenon, was then pointed out, as indicated by those parts of a soap-bubble which are too thin to reflect light being still possessed of two surfaces; and the questions were raised—If matter at some depth, however small, beneath the surface, continues to reflect light, at what depth does it cease to do so? Does it ever cease to do so? Or does the transmitted ray, as it speeds on its journey, always send back a beam in the opposite direction?

Different kinds of reflecting surfaces have different appearances; this is probably due, in some measure, to the effect produced upon the light by its passage into and out of that thickness of matter which is concerned in ordinary reflection.

Of homogeneous matter, we have opaque and transparent; the former giving metallic lustre, the latter vitreous. As a general rule, if not a universal, we find the more nearly a substance approaches the metals in opacity, the more it resembles them in the nature of its lustre. Thus sulphurets are in many cases very nearly opaque, and very like metals in the nature of their lustre. Carbon in its opaque form is a brilliant steel grey; while its transparent form has the vitreous lustre.

A micaceous or pearly lustre is the result of the superposition of a number of films of transparent material; the reflection from the first surface being added to by the reflections from the subjacent surfaces.

When light falls upon glass and is reflected, it is commonly said that the glass reflected. The speaker drew attention to the important part played by the air or other medium in contact with the reflecting surface, and illustrated it by obtaining total reflection from the surface of a prism when air was in contact with it, and only feeble reflections when the air was replaced by water or oil of turpentine; even a film of silver deposited on the prism was shown to reflect less light than a film of air. By a simple contrivance it was then shown that white paper reflects more light than a looking-glass, and that the appearance of lustre depends upon the reflection of shadows as well as of lights.

Mr. Proctor concluded his subject with the following words:—

"Sir D. Brewster, and other writers on optics, give the length of a wave of white light, the number of undulations in an inch, and the number in a second, calculating it as the *mean* of the number of undulations in the coloured rays; apparently forgetting that it is not the mean but the sum of the colours which forms white light—the mean being, according to Brewster's own table, yellow, with a tinge of green. Various writers have, probably, copied from the same source without investing thought upon the subject; one indication of which is, that several say so many millions of millions, whereas it would be more natural to say so many billions. I will just give you Brewster's figures, and then pass on:—

	Length in parts of an inch.	No. in an inch.	No. in a second. Millions of Millions.
White . .	0·0000225	44444	541
Yellow . .	0·0000227	44000	535
Yellow Green.	0·0000219	45600	555

"You observe the numbers given for white light are the same that would belong to a colour between yellow and yellow-green. White light, we may conclude, is not a definite undulation, nor a definite mixture of undulations, but a variety of mixtures of undulations, in any of which mixtures the average length of an undulation is that given by Brewster and others, but the number in an inch or a second is incalculable and indefinite. The length of the undulations in a pure unmixed colour is probably definite, and we have no reason to object to the measure and number usually adopted; we shall, therefore, accept them for further argument.

"The length of an undulation of violet light is seventeen-millionths of an inch; the red undulation twenty-six-millionths, or about one-half longer; undulations, longer or shorter than these, not being visible. The colours observed in soap bubbles and other thin films are produced by interference of the luminous waves. The colour produced depends upon the relation between the thickness of the film and the length of a wave of light. A film of air, four-millionths of an inch thick, produces the same colour as a film of water three-millionths, or of glass two-and-a-half-millionths of an inch thick. Therefore, we conclude that the length of the light wave varies with the medium. An undulation in air, measuring four, will measure only two-and-a-half when it enters glass, and will again elongate to its former measure on its exit. From these premises we may deduce various interesting conclusions. Faraday found that gold films were iridescent when they were only one-tenth the thickness at which air ceases to be iridescent. May we, then, conclude that light, while passing through gold, consists of undulations only one-tenth the length of those in air? Newton found that the thickness of films of a given colour was inversely proportionate to their indices of refraction. May we, then, conclude that gold has a refracting power in like proportion? If we say that luminous undulations, which in air measure twenty-two millionths of an inch, look yellow when they enter the eye, and in that organ measure one-third less, in consequence of its refracting power, then we come to the singular conclusion that the blue sky is yellow, sunshine is red, and the rosy tints of evening are not luminous at all until they enter the eye. If the colour depends upon the length of the light wave, and the length of the wave depends upon the refracting power of the medium through which it is passing, every beam of light changes colour; red it may be on its passing through the region of the stars, yellow or green it may be when it enters the air, blue or violet when it enters water, non-luminous as it passes through glass. But if light, which we perceive as violet while it exists in the aqueous humour of the eye, was red originally, what colour must that light be which we perceive as red? Its undulations in air must be too long to be luminous. This introduces us to the solemn thought, that all this vast universe is dark! Light exists only in the eye. It is only a sensation—a perception of that which in nature exists as a force capable of producing a sensation. We would feel grieved at the thought of light and sound having no tangible existence independent of ourselves, were it not for the glorious hope that all nature is full of forces equally grand—forces which we have not the power of perceiving, but which, with a higher development of our organism, may be sweet as music and genial as sunshine.

NOTICES OF PATENTS.

2643. *Extracting Silver from Lead.* G. H. Birkbeck, Southampton Buildings, Chancery Lane, London. (A communication). Dated October 23, 1861.

For the purpose of desilvering the pig lead a small quantity of zinc is added to the melted metal, intimately incorporated therewith, and then allowed to stand at rest until cold. The zinc will form the upper layer, which is said to contain the whole of the silver; and for the separation of this latter it is simply necessary to subject the alloy to distillation, or to dissolve it in diluted acids, when the silver, contaminated with a small quantity of lead, will be left as a residuum.

2661. *Improvements in Magnets, Induction Coils, and in Insulating Wire and Metal for Electric and other Purposes.* T. Morris, R. Weare, and E. H. Monckton, Trafalgar Square, London. Dated October 24, 1861.

For the manufacture of large permanent magnets, the patentees employ cast iron, afterwards decarbonising this metal by heating in hæmatite, or by any of the known methods. In this soft state they are filed and manipulated until their figure is tolerably perfect; then "case-hardened;" after which they are susceptible of becoming strongly magnetised, and will retain their power for a longer period than the ordinary steel magnets. The other subjects included in the specification are of minor interest.

2664. *Improvements in Heating Steel and Iron, and in Hardening and Tempering Steel, and in Apparatus employed therein.* J. Chesterman, Sheffield. Dated October 24, 1861.

Irrespective of improvements in the construction of apparatus, this invention consists in the use of a flux of readily fusible saline substances, such as bisulphate of potash, common salt, chloride of calcium, chloride of zinc, and borax; either of these, taken separately or mixed according to the application, is fused in a bath or suitable vessel through which the article of steel or iron is passed. When raised to the proper temperature the metal is withdrawn from the melted composition, and plunged into water, oil, or other fluid commonly used in the processes of hardening and tempering.

2665. *Preservation of Food.* J. McCall, Houndsditch, and B. G. Sloper, Walthamstow. Dated October 24, 1861.

In order to absorb the oxygen of the air from the tins in which provisions are ordinarily preserved, the patentees make use of the sulphite of soda, which is said to be in no way injurious to the food, or to the health of persons partaking of it.

2695. *Manufacture of Soda and Sulphuric Acid.* E. McClintock, New York, U.S. Dated October 28, 1861. (Not proceeded with.)

It is proposed to substitute oxide of copper for limestone in the ordinary process for manufacturing soda, this agent serving for the removal of sulphur from the "salt-cake"; and, by becoming itself converted into sulphide of copper, may be employed to furnish by roasting the sulphurous acid gas, from which oil of vitriol may then be prepared.

2707. *Manufacture of Spelter.* F. Bennett, Bagillt, Flintshire. Dated October 29, 1861.

In the process of manufacturing spelter from calcined zinc ores, whether calamine or blende, the inventor mixes a certain proportion of common salt (usually 4 per cent., by weight, of the calcined ore employed) with the coal and other ingredients, and subjects the mixture to distillation, the products being collected in the usual manner. Instead of common salt, hydrochloric acid or the chloride of calcium may be employed.

It is difficult to attribute to the chloride-flux any special efficacy in the retention of lead and other impurities; the only way in which it is likely to be serviceable is by fritting together the matters in the retort, and so contributing to their better incorporation or more intimate union, and, consequently, to a larger yield of sublimed zinc.

2702. *Soap.* J. Watt, Walworth; and T. S. Haviside, Cornhill. Dated October 29, 1861.

The patentees claim the use, in the manufacture of pale yellow soap, of the fatty acids obtained in the distillation of kitchen-stuff, ship-fat, and other dark-coloured refuse fats and grease.

2677. *Treating Bones and Gelatine.* T. Richardson, Newcastle-upon-Tyne; and R. Irvine, Harlet, Renfrewshire. Dated October 25, 1861. (Not proceeded with.)

The bones are first treated with muriatic acid, or dilute sulphuric acid, to dissolve out a portion of the mineral phosphates they contain, the residue being then acted upon with sulphurous acid; or, these chemical agents may be employed in the converse order. In either case, the gelatine obtained is treated with an alkaline sulphite or hyposulphite (to serve at once both as sizing and "antichlore" in paper-making?), or it may be treated with the phosphates or silicate of an alkali, or with the sulphurous acid solution of the diphosphates of lime and magnesia. By any of these processes, the hardness and sizing properties of gelatine from any source is stated to be very considerably improved.

Grants of Provisional Protection for Six Months.

3123. Johan Wilhelm Hjerpe, Wilhelm Holmgren, and Adolphe Victor Sunstedt, Stockholm, Sweden, "An improved composition or preparation of materials applicable for igniting matches."—Petition recorded November 20, 1862.

3389. Jules Pernod, Avignon, France, "A production derived from madders, called 'purpurine.'"

3390. John Savory, Bond Street, London, "A new or improved apparatus for the inhalation of medicinal powders or vapours for the treatment of diseases of the throat and lungs."—A communication from John Roberts, Paris.

3412. John McLean, Broxburn, Linlithgow, N.B., "Improved apparatus or arrangements for obtaining oil and other products from shale and the like bituminous minerals."

3423. Richard Archibald Brooman, Fleet Street, London, "A new or improved colouring matter or dye."—A communication from Pierre Chalamel, Puteaux, France.

3427. George Haseltine, Southampton Buildings, Chancery Lane, London, "Improvements in the mode of, and apparatus for, converting petroleum or coal oil into gas for lighting and heating, the said improvements being especially applicable to lamps and stoves."—A communication from James Rhodes, Cincinnati, Ohio, U.S.

3430. Thomas Callender Hinde, Cardiff, Glamorganshire, "Improvements in furnaces or apparatus for generating carbonic oxide."

3450. Charles Jones Denton, New Broad Street, London, "An improvement in the process of revivifying animal charcoal."—Petitions recorded December 24, 1862.

3266. Phineas Cowan, Hammersmith Bridge Soap Works, Barnes, Surrey, "An improved method of purifying gas."—Petition recorded December 5, 1862.

3280. John Joce, Colchester, Essex, "An improved composition or compositions for producing artificial sea water, or the odour or effects of sea water."—Petitions recorded December 6, 1862.

Notices to Proceed.

2348. Harper Twelvetrees, Bromley, Middlesex, "Improvements in the preparation of washing powders, soap powders, and cleansing crystals."

2410. John Henry Johnson, Lincoln's Inn Fields, London, "Improvements in coating or covering metallic surfaces with copper."—A communication from Charles François Leopold Oudry, Paris, France.

2419. John Watt, Graham Street, Walworth, Surrey, and Thomas Snaith Haviside, Cornhill, London, "An improved method of treating flax and other similar vegetable fibrous substances to be used as substitutes for cotton."—Petitions recorded September 1, 1862.

2434. Charles Garton, Bristol, "An improved method of applying heat in the manufacture of refining of sugar, and in malting, hop drying, brewing, distilling, and vinegar making."

2535. James Webster, Birmingham, "Improvements in the manufacture of nitric and nitrous acid, and other nitrogenous compounds."—Petition recorded September 15, 1862.

CORRESPONDENCE.

Pharmaceutical Morality.

To the Editor of the Chemical News.

Sir,—May I beg the opportunity of correcting a little misapprehension, on the part of your reporter, as to my communication to the Pharmaceutical Society upon "Methylated Spirit?"

I did not make any such personal statement as to say that "in Leeds there was hardly a druggist who did not use it." That its use throughout the drug trade of the country is enormous and increasing is well known, and this fact would render such a local allusion invidious and unfair.

Allow me to thank you for aiding in giving publicity to this question. We are fighting the battle of purity against a complication of personal interests. That we are serving the permanent interests of the Pharmaceutical Society is beyond question, for the lowering of the standard of Professional morality must cripple every effort to advance. It has been said that a Russian is merely a Cossack with a thin coat of French polish. We shall barely preserve a decent exterior, if we allow this impersonation of falsehood to penetrate all our business relations.

Some gentlemen affect to think that those who decry methylated spirit are influenced by a sort of hydrophobia as unreasoning as the manifestation of that malady. I claim a rational and consistent foundation for our objections. I regard methylated spirit only as one of the frauds which we must combat and expose.

The late Exhibition contained a collection intended to be illustrative of British pharmacy. Now, does any one believe that one-sixth of the drugs used in the kingdom come up to this standard of purity? I wish that I could do so. It really would be more to the purpose to have a collection of bad drugs, which would serve for warnings to us, such as the sailor lays down in his chart of shoals and wrecks. We have had too much optimism and the extreme fear of "treading upon other people's corns."

The introduction of French and German chemicals, consequent upon recent tariffs, has had a most material influence upon the supply of certain medicines. Direct responsibility of the maker has been almost lost, and purity has been also sacrificed. I call iodide of potassium to witness to this last fact. Then, again, the question of green extracts requires investigation, on account of the substitution of dried German herbs, as henbane, belladonna, &c., for those of native growth. The frequently bad condition of these herbs does not prevent their finding buyers amongst wholesale houses.

It appears to me that these questions of purity are, above all others, those which the Pharmaceutical Society should investigate; and that the organisation of committees, and its means of obtaining local information, should be alike concentrated upon such points.

I beg to enclose you a specimen of powdered opium lately supplied to a public Hospital. It contains 25 per cent. of actual earth and sand, and a quantity of ground poppy capsules in addition.

We have public duties in addition to those of conducting our own business upon honourable principles. One of these is to expose all frauds injurious to the public health.

I am, &c. R. Reynolds.

Leeds.

MISCELLANEOUS.

Royal Institution.—The following lectures will be delivered:—On Tuesday, January 27, at 3 o'clock, Professor Marshall, F.R.S., "On Animal Mechanics." Thursday, January 29, at 3 o'clock, Dr. E. Frankland, F.R.S., on "Chemical Affinity." Friday, January 30, at 8 o'clock, his Eminence Cardinal Wiseman "On the Points of Contact between Science and Art." Saturday, January 31, at 3 o'clock, W. S. Savory, Esq., F.R.S., "On Life and Death."

Improved Lucifer Matches.—So great is the demand for every description of safety matches that several of the most eminent metropolitan manufacturers have been induced to bestow much attention to improvements in this direction, and the result has been the production of good and cheap matches, offering security against accidental ignition, at the same time that they are equally certain and available for all the purposes of the common lucifer. We have already had occasion to describe the "Special Safety Match." * A patent for a somewhat similar invention has been granted to Mr. H. Mearing. Besides these, we find in commerce an excellent box of matches, manufactured by Mr. J. Hynam, of Prince's Square, Finsbury, and known as the "Imperial Safety Match." According to the announcement on the printed cover of the box, "this match is manufactured on the principle of the original inventor, introduced to the manufacturer about twelve years since;" and a guarantee is given by the maker to the effect that the match does not contain any preparation of phosphorus, that it is not liable to spontaneous ignition, and cannot be inflamed by friction upon any ordinary rough surface. A specially prepared friction surface, protected by a flap of chip, is provided on the top of the box; it consists of amorphous phosphorus, with a little pounded glass, and by rubbing upon this material alone the lighting composition on the head of the match can be fired. The latter is itself so little sensitive to heat and friction, that the wood can almost be charred, or the head of the match squeezed between pincers or struck with a hammer, without inflammation occurring. Chemical analysis discloses the presence of chlorate of potash, and sulphuret of antimony, among the ingredients of the lighting composition; but the detonating and combustible characters are so far moderated by the admixture of large proportions of red lead, powdered glass, and other neutral substances, that the safety match, apart from the box, can scarcely be deemed more dangerous than an ordinary waxed taper or splint. There is no sulphur used in the preparation of the match, the wood being dipped in stearin or paraffin to render it sufficiently inflammable at the tipped extremity. The introduction of these safety matches is calculated to benefit the insurance companies by diminishing the risk of fire, offering, as they do, great security against the accidental, and apparently spontaneous, ignition not unfrequently experienced with the lucifer matches in common use. The dangerous practice of carrying about loose matches is directly counteracted by the principle which lies at the basis of this invention.

ANSWERS TO CORRESPONDENTS.

Bile-Acids.—M. D.—The simplest way to test for bile-acids is to put about a couple of drachms of urine into a test-tube, drop in a fragment of white sugar of the size of a pea, and then add slowly, by pouring down the side of the tube, a drachm of strong sulphuric acid. If bile-acids be present, a fine violet or purple colour will be produced at the line of contact of the urine and acid; if absent, only a browning of the sugar will be observed.

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* *Vide* Chemical News, vol. v., p. 280.

THE CHEMICAL NEWS.

Vol. VII. No. 165.—January 31, 1863.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Separation of Copper from Nickel, by M. P. Dewilde, *Professor at the Institute of Gembloux.*

The process for separating these two metals, founded on the precipitation of copper by sulphuretted hydrogen, leaves much to be desired on account of the facility with which sulphide of copper, after washing, passes to the state of sulphate; and also because copper, during precipitation, always carries with it a considerable amount of nickel, which passes to the state of sulphide in the precipitate.

A good method of separation is an important desideratum where copper and nickel alloys are used for coinage, as is the case in Belgium, Switzerland, and elsewhere.

The following process has been studied at the desire of M. Melsens, who wished me to take as a starting point the property possessed by glucose of precipitating copper as a suboxide, when that metal exists in the form of tartrate dissolved by the aid of caustic potash. The operation is as follows:—

Dissolve about two grammes of alloy in hydrochloric acid, with the addition of nitric acid; evaporate the excess of acid, and dissolve the two chlorides in about fifty grammes of water. To the solution add of pure cream of tartar, double the weight of the alloy. Heat slightly, to favour the solution, and add, little by little, a solution of caustic potash in alcohol. The first additions of alkali precipitate the oxide of copper and nickel in the state of hydrates; but an excess of potash re-dissolves the whole, tartrates of copper and nickel being soluble in potash.

A blue liquid is thus obtained, which, after cooling, is treated by a solution of pure glucose or inverted sugar, and boiled for one or two minutes.

The copper is precipitated as a beautiful red suboxide, sinking quickly to the bottom of the vessel, but sometimes forming in flakes, which it is difficult to wash unless the solution is cooled before adding the glucose.

The alkalinity of the liquid and the complete precipitation of the copper is ascertained by adding a drop of the sugared liquid.

The precipitated suboxide of copper is washed, dried, and calcined. The suboxide is changed into nitrate, in which the copper can be estimated by M. Pelouze's volumetric method, or by transforming the nitrate into oxide by calcination.

The filtered liquid containing the nickel is evaporated to dryness, the residuum incinerated, and then washed to remove the carbonate of potash; and, as the incineration can never be complete, on account of the presence of this salt, it must be repeated.

The residuum, formed principally of oxide of nickel, is dissolved in aqua regia, from which the hydrated oxide of nickel is precipitated by potash. It is very difficult, if not impossible, to wash this very voluminous oxide, and we prefer to content ourselves with an incomplete washing, and to dry and slightly calcine the oxide. If it is now pounded in an agate mortar, it is easily freed from the last trace of potash by washing in warm water. The oxide thus obtained is reduced in a platinum crucible in a hydrogen atmosphere, and the metallic nickel is weighed.

The following, among other analyses, are the results obtained in separating alloys procured from the Brussels mint:—

Alloy No. 1.

	I.	II.	III.
Copper . .	74·40	74·32	74·38
Nickel. . .	25·55	25·58	25·57
	99·95	99·90	99·95

Alloy No. 2.

	I.	II.
Copper	74·33	74·30
Nickel	25·60	25·61
	99·93	99·91

These two alloys were free from iron and zinc, but contained traces of silicium. The legal standard of the binary alloy, copper and nickel, adopted by the Belgian mint is—copper, 75, nickel, 25.

We have ascertained (by operating on two grammes of alloy) that copper, during precipitation, takes with it barely two milligrammes of nickel, and that the nickel contains scarcely an appreciable trace of copper. —*Bulletin de la Société Chimique de Paris.*

TECHNICAL CHEMISTRY.

Browning Iron and Steel Objects, by M. Sauerwein.

Gun-barrels and other objects in iron and steel are browned, either to improve their appearance, or to preserve them from rust, by giving them at first a thin but entire coating of oxide of iron. The following process is successfully employed in Prussia for browning steel barrels:—

Dissolve two parts of crystallised ferric chloride, two parts of butter of antimony, and one part of gallic acid, in the smallest possible quantity of water (about four or five parts); with this moisten a sponge or cloth, and rub the object to be browned. Leave it to dry in the air, and repeat the operation several times. Then wash with water, dry, and rub with boiled linseed oil.

Objects browned in this way have a very agreeable dead grey appearance, and the shade deepens according to the number of times the operation is repeated. It is essential to the success of the operation, that solid butter of antimony should be used,—that is to say, a chloride of antimony containing as little free hydrochloric acid as possible.—*Dingler, Polyt. Journ.*, vol. clxv., p. 237.

General Considerations on the Constitution of Iron, Cast-Iron, and Steel, and their Application to the Manufacture of Gun Barrels, by M. LE GÉNÉRAL SOBRERO, *of Turin.*

IN the memoir bearing the above title, the author admits that nitrogenised compounds act as introducers of carbon, as will be explained further on; but he by no means admits as proved that nitrogen is an indispensable element in the constitution of steel.

M. Sobrero considers steel as always an intimate and homogeneous combination of a larger or smaller quantity of carbon and iron; but, to ensure the goodness and durability of steel, and its resistance to successive heatings, it must contain a metallic oxide difficult of reduction, and capable of solution in the steely mass, just as suboxide of copper dissolves in copper. The memoir attributes this property to oxides of manganese, titanium, and tungsten.

This idea dates, says the general, from the year 1829. Being at that time director of the chemical laboratory of the arsenal at Turin, he experimented on the cementing of the iron of the valley of Aoste. This iron is procured from ores identical with those of Dannémora, in Sweden, except that they contain no manganese. The cement used, in addition to pulverised coal, consists of soot, of leather and horn cuttings, and of sea salt. Steel obtained in this way is of unequal grain, and burns easily.

In 1834, M. Sobrero, after explaining his hypotheses to the Council of Mines, entered into an arrangement with a manufacturer for introducing oxide of manganese into the Aoste iron. At the first stage in the fining of the cast-iron, he wished to add a certain quantity of silicate of manganese as a substitute for the iron added to cast-iron in the fining process called *bergamasque.* M. Sobrero, being called to take the general direction of the artillery materials, could not carry out his projected experiments.

The Aoste iron is purer than that of Savoy, but the latter contains more manganese. Comparative experiments on the resistance of the wires of these two irons all resulted in favour of the Savoy iron.

Other experiments on the Aoste iron and that of Bienne, in Switzerland, prove the superiority of the latter, which, though less pure, contains manganese.

M. Sobrero reminds us that the most celebrated arms, as those of Milan and Toledo, were made from manganesian iron ores. In short, it is now a usual practice to add materials containing manganese during the refining, or to the puddling furnaces, or in Bessemer's cupola.

All these disputed points tended to strengthen the author's opinion that, to be good and durable, steel must contain oxide of manganese, titanium, or tungsten, or even aluminium, as the Wootz steel does.

M. Sobrero then adduces some more facts tending to elucidate the action of the above-mentioned oxides.

In 1839, the general, being charged by his government with the casting of some pieces of cast-iron ordnance in Sweden, had an opportunity of fully carrying out this method.

The furnaces and the fusing processes were the same as those of Aoste, so managed that the cast-iron was never allowed to remain perfectly quiescent in the crucible. If it became still, some pieces of rough ore were thrown in to agitate it.

The cast-iron obtained is mottled. The Aoste ore is the same, but it yields only grey cast-iron. By analysing the Swedish cast-iron used for ordnance, M. Sobrero ascertained the reason of this difference: they contained zinc. This gives the key to the nature of the cast-iron. Zinc has a tendency to volatilise, and thus causes a constant movement in the mass until the moment of its becoming solid. Thus, the cooling taking place very gradually, the carbon cannot separate itself from the iron, and white cast-iron is the result.

Arsenic produces the same effect as zinc.

The manganesian ores yielding white cast-iron, whilst those which do not contain it are grey, M. Sobrero draws the conclusion that, in the first case, there must have been a molecular movement, and that this movement is produced by the protoxide of manganese which is dissolved in the cast-iron in the presence of carbon. Oxide of manganese has become converted into protoxide, which has a strong affinity for oxygen—an affinity which is disputed by that of carbon; but, as one never dominates over the other, an uncertain state of equilibrium is produced. The cast-iron is agitated until the moment it solidifies. For this reason the carbon cannot crystallise separately, but remains in combination, and hence the cast-iron is white.

All that has been said about cast-iron applies equally well to steel.

The manganesian and non-manganesian cast-irons, which contain phosphorus, being sometimes white, M. Sobrero explains this fact by supposing that, when the ores contain earthy phosphates, these phosphates are not reduced, but are dissolved in the cast-iron. A state of uncertain equilibrium results, because of the affinity of earthy phosphates and carbon for oxygen, and the cast-iron obtained is white. If the cast-iron is completely freed from earthy metals, the phosphate can exist only as phosphide. It cannot whiten the cast-iron, as it produces no molecular movement.

Sulphur can exist in cast-irons only as a sulphide, the sulphates being easily reduced; and the result is, that sulphur exerts no blanching effect on cast-iron.

M. Sobrero has applied the preceding principles to steel, which is nothing more than refined cast-iron, containing a smaller proportion of carbon. He supposes a steel composed exclusively of iron and carbon. If heated and tempered, the carbon cannot separate from it; but if allowed to cool slowly, the separation is effected; but this will not be the case if the steel contains oxide of manganese. In fact, the greater part of the carbon will then remain in combination, and the steel will better preserve its characteristic properties, since it is combined carbon only which acts on the nature of the iron. All this applies to both natural and cast steel. The first always contains manganese, and oxide of manganese is usually placed in the crucibles in which steel is fused.

Cemented steel produced from Swedish iron presents an exception. It contains manganese no longer in the state of oxide, but in a metallic state. According to the author, the reducing, and at the same time cementing, agent is protocarburetted hydrogen proceeding from the action of water (contained in the carbon and in air) on the excess of carbon. Carburetted hydrogen, indecomposable by heat alone, is, on the contrary, easily decomposable by a body bearing an affinity for one of its elements. Here the affinity is double: hydrogen combines with oxygen, and carbon with iron. As the oxide of manganese is evenly distributed in the mass of iron, the carbon is deposited regularly, and the steel produced is homogeneous. With ordinary irons, on the contrary, the cementation is very irregular. Cemented steels produced from Swedish iron are the most homogeneous known. It is, consequently, impossible to obtain steels in Germany, Styria, or the Pyrenees so homogeneous as those of Sweden, because the manganesian irons are never homogeneous—homogeneousness being essential to the success of the cementation.

Cemented steels are inferior to natural or cast-steels; successive heatings transform them into hard iron, and then into soft. This fact, ascertained by Karstein and Valerius, is attributable to the absence of oxide of manganese in cemented steels.

When the iron contains no oxide of manganese, the author is of opinion that protocarburetted hydrogen would exert very little action, and that, where cementation by the flame of lighting gas is successful, the iron used most probably contained oxide of manganese.

When iron contains no oxide of manganese, cyanides take part in the cementation. According to M. Sobrero, dull red is the temperature most favourable to the formation of metallic nitrides, and of nitride of iron in particular, at this temperature the action of the carbon being null. By raising the temperature, the nitride is decomposed, and the carbon is at liberty to enter into combination with the iron. This granted, when the temperature of cementation cases reaches dull red, the cyanogen will decompose on the iron, and thus produce nitride; the temperature being further raised during the process, the nitride will become decomposed, and the carbon will take the place of the nitrogen, which will have acted as a preparing agent.

For the above reasons, it is probable that steel prepared in this way will never be so homogeneous as that furnished by an iron containing oxide of manganese heated in the ordinary cementing medium. Cementation of the manganesian irons by cyanides would succeed badly, or, in any case, not so well as cementation by protocarburetted hydrogen; it is best, then, in this case, to employ simple cement.

The hypothesis adduced in this paper seems adapted to account for the facts. To confirm it, it would be necessary to prove that all good steels contain manganese, and that natural and cast-steels contain it in a state of oxide. As to the first fact, a large number of steels do contain manganese, and researches on this point will probably show this to be general. As to the second fact, M. Sobrero believes that the constant losses undergone by steel and iron treated by hydrogen can only result from the presence of oxygen in the iron.

The conclusions derived from M. Sobrero's paper are—

That all cast-iron in which no molecular movement is produced, is grey or black after one cooling.

That cast-iron, in which there has been a molecular movement, is white when properly cooled. This applies when the cast-iron contains manganese, titanium, or tungsten. Cast-iron when refined will, in this case, give an excellent natural steel, or an iron which, by cementation, will produce the best obtainable cemented steel.

That, after slow cooling, cast-iron will be equally white if it contain zinc or arsenic.

The preceding conclusions, relative to oxides of manganese, titanium, and tungsten, are applicable to steels, and serve to explain how natural and cast-steels containing these oxides resist several heatings, the carbon being always in intimate combination with the iron. In cemented steels, on the contrary, where oxide of manganese is reduced, at least partially, to a metallic state, the stability of the steel decreases with each heating; the combined carbon diminishes, and the steel easily changes to hard, or even soft, iron.—*Répertoire de Chimie Pure et Appliquée.*

PHARMACY, TOXICOLOGY, &c.

On a Re-agent Suitable for Detecting Beetroot Alcohol, by M. Cabasse.

M. Cabasse proposes sulphuric acid as a precise re-agent for beetroot alcohol.

By mixing three parts, in weight, of this alcohol with one part of sulphuric acid, a red colour is immediately produced, which preserves its intensity for several months. No similar result is produced by operating with vinous alcohol. On operating with perfectly colourless alcohol, the liquid does not change directly, but after a shorter or longer time takes a slightly amber tint; this amber tint, which belongs to alcohols which have remained a certain time in cask, is heightened only by sulphuric acid, since it existed previously, but has no analogy with the red colour of beetroot alcohol.

The following are the results obtained by the action of sulphuric acid on mixtures of definite proportions:—

1. Vinous alcohol, three parts; beetroot alcohol, one part; red colour, apparent only with a white paper placed behind the flask.
2. Vinous alcohol, two parts; beetroot alcohol, two parts; decided red colour.
3. Vinous alcohol, one part; beetroot alcohol, three parts; red colour still more decided.

It is then easy, by means of pure sulphuric acid, to recognise immediately beetroot alcohol and its mixture in various proportions with vinous alcohol. The coloration produced by using the re-agent in the given proportions is due most probably to the action of the acid on the volatile oil, from which it has hitherto been found impossible to free the alcohol, to which it communicates its peculiar flavour and odour.—*Répertoire de Pharmacie.*

Quantitative Estimation of Starch, by H. Dragendorff.

In order to effect the quantitative estimation of starch in a given material, the author recommends that the dried substance should be digested in an alcoholic solution of potash, containing 5 or 6 per cent. of hydrate of potash in nearly absolute alcohol. The digestion should take place at 100°, and in a close vessel, and continue for from 18 to 30 hours. Two or three grammes of the dried substance require from 25 to 30 grammes of the alcoholic solution of potash. By this operation, all the proteine substances are rendered soluble in either alcohol or water, the fat is saponified, and the sugar and dextrine may be easily washed out. Some of the mineral acids are also taken up by the solution. The starch granules remain unaffected: the same is the case with the cellulose, mucilage, cortical matters and salts; but the whole is so disintegrated as to enable the other reagents to act quickly and perfectly. After the digestion is finished, the liquid is filtered. If an oily plant be experimented with, the mixture must be filtered hot, the residue on the filter washed with hot absolute alcohol, cold alcohol, and, lastly, with cold water, until exhausted. When mucilaginous seeds are operated on, the water used for washing must contain 8 or 10 per cent. of alcohol. The residue on the filter must now be dried, and either digested with dilute hydrochloric acid containing 5 per cent. of acid, or with a concentrated solution of malt at 56°, until the starch is converted into sugar. In either case, the residue is washed and dried, and the loss considered starch. When hydrochloric acid is employed, the slight error which arises from some mineral matters being dissolved, may be corrected by evaporating the saccharine solution, and calcining the residue. The solution of malt, of course, removes only the starch. If hydrochloric acid is used, the starch may also be estimated indirectly by determining the amount of sugar in solution, either by the fermentation process, or by the copper reduction test. In determining the amount of starch in mucilaginous substances, a strong solution of chloride of sodium acidulated with hydrochloric acid must be employed, and the subsequent washing of the residue must be performed with dilute alcohol. By these methods the author has determined the amount of starch and other matters in the following substances. The numbers in the different columns represent—A, the loss in drying; B, loss in treating by alcoholic solution of potash; C, starch; D, cellulose, corky matter, lignin, cuticle, mucilage, and insoluble mineral substances:—

	A	B	C	D
Wheat . . .	13·2	18·7	59·5	8·6
Wheaten flour . .	15·8	12·6	68·7	2·9
Rye . . .	11·0	23·2	59·7	6·1
Oats . . .	11·9	22·1	46·6	20·4
Barley . . .	11·5	23·5	57·5	7·5
Rice . . .	13·3	17·1	61·7	7·9
Peas . . .	5·0	34·2	37·3	23·5
Beans (white) . .	16·7	45·1	33·0	5·2
Clover seed . .	10·8	60·0	10·8	18·4
Linseed . . .	7·6	46·1	23·4	22·9
Mustard . . .	8·5	51·5	9·9	30·5
Rape . . .	5·8	63·5	8·6	21·1
Potatoes (dried) .	—	31·6	62·5	5·9

—*Pharm. Zeitschrift für Russland*, 1862, s. 41.

COURT OF QUEEN'S BENCH.

DECEMBER 10, 1862.

Simpson and Others v. Wilson and Another.

(Concluded from page 43.)

Mr. Spencer Dunn (examined by Mr. Hindmarsh): I am a member of the firm of Dunn and Heathfield, chemical manufacturers. We manufacture arsenic acid. We manufacture it dry, that is, anhydrous, free from water of every kind. If applied to for arsenic acid I should give dry.

By the Lord Chief Justice: I mean totally deprived of water, anhydrous. It depends somewhat on the customer. If he were a chemist, and he asked for "dry arsenic acid," I should take care that he had it anhydrous. If he were simply to ask for arsenic acid, I think it might contain a little water from having been kept, or from exposure to the air—accidental water.

Lord Chief Justice Cockburn: Would that be water in combination—water of constitution as it is called?—Witness: It would be so.

By Mr. Hindmarsh: Arsenic acid absorbs water: the longer it is kept the more water it contains. If a man applied to me for dry arsenic acid for a commercial manufacturing process, I should give him anhydrous acid.

By the Lord Chief Justice: If a manufacturer of colours who understood chemistry applied to me for dry arsenic acid, I should consider it my duty to supply him with anhydrous acid.

Lord Chief Justice Cockburn: And if you thought he was not sufficiently acquainted with the article, you would send him something not quite so good, would you?—Witness: If I thought he did not know what he wanted, and the other would be as good, I should send it.

By Mr. Hindmarsh: The amount of hydration depends on the length of time the acid is kept, and the method of keeping it. We make it free from water, and intend to send it out so, but we may not always send it out free from water. In our trade "dry arsenic acid" signifies the anhydrous acid.

Cross examined by Sir F. Kelly: I cannot say whether we supplied Mr. Warrington with acid containing 12 or 13 per cent. of water for dry arsenic acid. I will not say we did not. If Mr. Warrington said we did, I cannot say we did not. We have sold large quantities of arsenic acid to Mr. Hands, of Coventry. We did not specify it to be dry. I am not prepared to say that what we supplied him with did not contain 12 or 13 per cent. of water. I did not analyse it to ascertain the amount of water. My impression is that when it was sent out it was fairly free from water. What we supplied might contain more or less than 11 to 13 per cent. of water.

By the Lord Chief Justice: The acid supplied was probably packed in jars, and might have absorbed water from the atmosphere. It will absorb moisture unless hermetically sealed.

Mr. J. A. Phillips (examined by Mr. Day): I have read Medlock's specification, and noticed the term "dry arsenic acid." By "dry" I understand anhydrous acid. If I were to be asked for dry arsenic acid, without any qualification, I should give anhydrous.

By the Lord Chief Justice: If I were asked for dry hydrated acid I should consider that meant hydrated acid with accidental water got rid of.

By Mr. Day: I have always understood dry arsenic acid as the arsenic acid described in books, which description evidently refers, in most instances at least, to anhydrous acid. I have made experiments with Medlock's process, and also with various quantities of water and arsenic acid. I agree generally with what has been said by Mr. Campbell, Dr. Miller, and the other witnesses, as to the quantity of water necessary to produce a useful result.

Cross-examined by Mr. Grove: Dry tartaric acid is a trade term. It is used to mean dry crystals, and when it is powdered and stored dry, it is called dry tartaric acid, but it is not dry. Anhydrous tartaric acid is not a commercial commodity. A person asking for dry tartaric acid would have one equivalent of tartaric acid and two equivalents of water. I never heard it called a dry hydrate. I have experimented with arsenic acid and aniline. If aniline is distilled with an excess of arsenic acid without sufficient water to form arseniate of aniline, there would be a waste of aniline. I have not looked into the profit and loss of the matter, except at the defendants' works.

Re-examined by Mr. Hindmarsh: I think that water is necessary for the production of the colour. If I were told to furnish a person with dry arsenic acid, I should give him anhydrous.

Dr. F. Crace Calvert (examined by Mr. Hindmarsh): I am well-acquainted with arsenic acid. I manufacture some chemicals myself. In my judgment, the term "dry" arsenic acid signifies anhydrous arsenic acid. I have read Medlock's specification. There is nothing in it to induce me, as a chemist, to come to any other conclusion than that anhydrous acid is the material meant. Dr. Medlock is a chemist; he took out a chemical patent, and, therefore, knew the value of the term he used.

Mr. F. A. Manning (examined by Mr. Day): I am an analytical chemist, and have read Dr. Medlock's specification. By "dry" arsenic acid, I understand anhydrous arsenic acid, without water,—as dry as it can be made by any means. I have made a great many experiments on Medlock's process, and agree with the other witnesses as to the quantity of water necessary to produce a successful result.

Cross-examined by Mr. Grove: I have not taken the trouble to go to chemical establishments, and find what results they could obtain.

Mr. A. E. Fletcher (examined by Mr. Hindmarsh): I am one of the defendants. We have never made any other colour than the red, the magenta, by arsenic acid and aniline. I have tried to make the colour without water, but found it impossible to produce it. I have read Delaire and Girard's specification. We use about their proportions. The most convenient proportions I found to be equal weights of aniline, arsenic acid, and water.

Cross-examined by Mr. Grove: We apply heat to the mixture. The temperature varies throughout the process from about 250° to 350° F. I think it would be injurious to go above that. The way in which we apply the heat is a trade secret. We use an oil bath on a large scale. I decline to say what we do with our residue. We do not get any colour from it. When treated with spirit it gives a dingy brown colour with a trace of purple in it. I have never purified it.

Re-examined by Mr. Hindmarsh: After we have got the red colour, there is a residue. It consists of ingredients not mentioned in this patent. It would contain the blue colour mentioned in Medlock's specification if that description is correct; I only find a brown. When we have heated the aniline and arsenic acid together, we get a melted mass. We boil this in water along with other ingredients. It does not all dissolve. The residue is of a brownish colour; it is not of any use.

Mr. W. V. Wilson (examined by Mr. Hindmarsh): I am the other defendant. The process we use is substantially the same as Delaire and Girard's. When we have performed the process we put the material obtained into water and boil it, and so get the red dye. I have never observed any residue practically. It is very small in quantity. The red colour goes through several processes before it is finally purified.

Mr. Hands (recalled and examined by Sir F. Kelly): We have never used arsenic acid in solution. Certain quantities of the arsenic acid we used were purchased of Messrs. Dunn and Heathfield. All we purchased of them was dry arsenic acid The colour we made was invariably made without water. We have dyed upwards of 100,000 pounds' weight of silk with colour made under this patent. We made a fair profit in the way of trade by the manufacture. There is a certain waste of aniline in the process. I cannot tell the exact quantity; it is rather considerable. I should think it was four or five per cent. We made a satisfactory profit, nevertheless.

By the Lord Chief Justice: When we first began we were particular about the degree of heat. Since then it has become a rule of thumb almost. We can tell by the consistence of the product. It is heated up to 360° F., I think. It would not do without heating. I never tried it without heating. We add boiling water to the solid tarry mass when it is cold. The colour is produced gradually, and it intensifies until the finish of the operation.

Lord Chief Justice Cockburn: The patentee says, "I heat it to, or near to, its boiling point until it assumes a rich purple colour?"—Witness: That rich purple colour is contained in the dry matter which is formed. It is produced during the process of heating. We add no water to the arsenic acid, as we get it from Dunn and Heathfield. There is no water whatever put in.

Mr. Hindmarsh then summed up the evidence for the defendants; and Mr. Grove replied upon the whole case.

Lord Chief Justice Cockburn, in his summing up, said the matters in dispute divided themselves into two branches—the question with regard to the specification, and the question as to the infringement. The first question depended upon the construction to be put upon the word "dry." This must be looked at with reference to the manner in which those who would be likely to avail themselves of the invention would be likely to understand it. Manufacturers of colours did not probably manufacture the materials. They would go to a manufacturer for the articles specified by the patentee, and the question was, how would a manufacturer of chemicals understand the term? Scientific men have been called for the plaintiff, who say that the term "dry" is a flexible term, sometimes used in a stricter sense than it is in others—sometimes used in the sense of anhydrous, but generally meaning something not wet or moist in the common acceptation of the term. On the other side, scientific men have said directly the reverse. They have said that the term "dry acid," as used in chemistry, means anhydrous. If the latter is the true sense of the word, there was an end to the patent, because it is admitted on all hands that anhydrous arsenic acid will not answer the purpose. A conclusion must, therefore, be come to on the meaning of the word "dry" upon the scientific evidence of both sides, and also of Mr. Warrington, who said he sent to a manufacturer for dry acid, and got it with a certain per-centage of water; and further, from the evidence of Mr. Hands, who said he had manufactured large quantities of the colour from arsenic acid he bought of a London chemist. It might be, that, although the London chemist professed to sell anhydrous acid, the water might have been absorbed from the atmosphere, inasmuch as even the anhydrous arsenic acid was said to be so greedy of water as to absorb from the atmosphere, in the course of nine hours, as much as 15 or 16 per cent. of its own weight. The evidence on these points was so conflicting, not only on matters of fact, but also on scientific nomenclature, that laymen were bewildered, perplexed, and left in despair as to knowing how they were to decide. "It was very sad," said the Lord Chief Justice to the jury, "that you and I—or, rather, you (for it is you who will have to decide the question), who are laymen, will have to decide such a question without having for our benefit and assistance a unanimity of opinion among the scientific witnesses to whom we naturally resort for information." The Lord Chief Justice then alluded to the question of ambiguity or imperfection of description in the specification, which, also, he explained, would invalidate the patent. The jury would have to decide whether the term "dry" meant that for which the plaintiffs contend,—namely, arsenic acid in the form of a hydrate, or whether it would have been possible for the patentee to avoid and clear away the doubt which the use of this unfortunate phrase introduced. The question of the practical commercial availability of the process was then noticed. On this point a good deal of scientific evidence of a theoretic and, to a certain extent, a practical character had been heard to prove that, according to the process of the patentee, the colour could not be produced with a profitable result. But then came the evidence of the manufacturer, Mr. Hands, who said, that, although there had been considerable waste of material, yet, on the whole, the process was commercially profitable. It would be for the jury to say how far that practical evidence outweighed the theoretical evidence given by gentlemen who had not dealt with the matter commercially. The next question was that of infringement. The patentee did not intend to use arsenic acid in solution as the defendants admitted they use it; but, if the processes were substantially the same, then there would be an infringement. It depended upon how far the scientific witnesses were right in the explanation they gave of the stages through which the materials passed before they arrived at their final combination. If the jury were satisfied that, before the arsenic acid operated on the aniline to produce the colour, it had, by the expulsion of the water, arrived at the condition of dry acid, then the verdict would be for the plaintiffs. But if the jury thought that the two processes were distinct, and they could not sufficiently rely on the theoretical evidence of the scientific witnesses, then the verdict would be for the defendants. Dr. Medlock, the patentee, intended convey to the world that the dry, and not the dissolved acid, was to be used; yet, if the thing was substantially the same, the patentee secured to himself the precise form he specified, and any process which was, chemically speaking, an equivalent. It would be a striking instance of the operation of the patent law if, when a man has patented a thing in a particular form, it being his direct intention to exclude the use of that which was now said to be substantially the same, yet that the patent should prevent other people from using that which the patentee says he intended to exclude. Nevertheless, the plaintiffs, having bought the patent, were entitled to say they had a right to limit to themselves the use, not only of the precise form pointed out, but anything equivalent to it, and which was substantially the same. In concluding, the Lord Chief Justice remarked, that he was glad the jury, and not he, had to decide the case, and submitted to them the following questions:—First, to define the meaning of the term "dry," whether it meant the same as anhydrous. Second, whether the term is so ambiguous that a competent manufacturer *might* [Sir F. Kelly suggested, *would*] be deceived by it, and the patentee could have removed the ambiguity. Third, if the meaning of the term "dry" was that contended for by the plaintiffs, whether with such arsenic acid the process is commercially useful; and, lastly, with regard to the infringements, whether the processes—that is, the one specified, and that used by the defendants—were substantially the same.

After deliberating an hour and three-quarters the jury were not agreed, and, there appearing no probability of their agreeing, they were discharged; and so for the present ended this chemical *cause célèbre*.

PROCEEDINGS OF SOCIETIES.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, January 13.

E. W. Binney, *F.R.S., F.G.S., President, in the Chair.*

Magnesium.—Mr. Spence brought under the notice of the society a specimen of the metal magnesium. This metal has hitherto been produced only in small quantities, by means of sodium. The specimen shown derived its interest from the fact, that it is stated to have been produced by direct reduction, either from the oxide or some of its salts. If this information, which has been obtained through a friend of the inventor, proves correct, it is a matter of considerable importance, as the metal has most valuable qualities.

Rubidium.—Professor Roscoe read the following extract from a letter which he had just received from Professor Bunsen, respecting the properties of metallic rubidium:—"I have prepared metallic rubidium by reduction with carbon in an iron vessel; from 75 grammes of the bitartrate of rubidium I obtained 5 grammes of the metal in coherent masses, one of which weighed 3 grammes. Rubidium closely resembles potassium in its properties, its vapour possesses a greenish blue colour, the specific gravity of the metal is 1·65, it is more electropositive than potassium, and it melts at the very low temperature of 38·5° C."

Dr. Joule described a peculiar kind of mirage which he had witnessed from the northern extremity of Douglas Bay, Isle of Man, and at an elevation of 20 feet above the surface of the water. At about 9 a.m. a number of chimneys in the town were fired, and the products of combustion were driven out to sea by a very gentle breeze. Presently the wind changed, and drove the smoke at right angles to its former track. The steamer for Liverpool had in the meantime attained a distance of about three miles, and, although somewhat obscured, was distinctly visible through the smoke. Noticing something extraordinary in her appearance, he viewed her through a telescope, and then observed that nearly the whole of the hull was obscured by the horizon, the uppermost part of the paddle-boxes, the bowsprit, and tafferel being alone visible, whilst at the same time the masts and funnel appeared considerably elongated. A quarter of an hour afterwards, when the smoke had cleared away, and the steamer was about seven miles distant, the hull was seen as usual, quite unobscured by the horizon. Dr. Joule attributed the phenomenon to a stratum of highly refractive gases, lying about twenty feet above the level of the sea, and gradually diluted and diminished in extent on its upper and lower sides, so as to produce the effect of a convex lens.

Dr. Angus Smith said—"I saw once a very remarkable instance of the diversion of the rays of light from the straight line. Although fogs are common on our hills, I have met no instance of similar exaggeration of effect among us. I went with some friends up Skiddaw, and, near the top, entered a cloud, which prevented us from seeing many yards before us, although it was not extremely dark. When moving over those loose stones, which form a highway to the summit for a considerable distance, we observed a building, which appeared to us about fourteen feet high. The side towards us seemed to be a wall nearly square, and we took it for granted that the top of the hill was attained, and that the foremost of the party would be found there; but as we approached the building sank, and in the course of a few steps it went downwards until we found that there were only three layers of stones not very thick, and the whole under two feet high. The disappearance must have occurred within the space of twenty feet; my present impression is that it was less. Further on we attained the man at the summit, and the cloud became denser; we could not see many feet, and I did not think it safe to move from the spot for some time, not knowing the locality. Sitting there I saw four perpendicular lines on the cloud; I looked up, and found that they terminated in the body of an animal which was moving slowly towards us. The distance could not be measured, except by the number of steps taken by the animal, and from these I concluded that when first seen it was from twelve to twenty feet from us; I looked on twelve as being most probable. I do not exaggerate when I say that the height was in appearance thirty feet. Wondering what was to be the end of this strange vision, I called the attention of all, but the animal diminished in size so rapidly, that only one or two, who instantly attended to my call, could perceive the monstrousness of the exaggeration of form now presented by a moderately-sized pointer. On coming up to the more advanced portion of our party, they were enlarged and distorted, as we often hear described; but such effects are comparatively common, and they are not to be compared with the two instances mentioned. The first was seen with my face towards the sun nearly. It was about midday. The second phenomenon was seen twenty minutes later, when looking nearly north. Although I have lived much in sight of Morven, where one might expect to see the 'spirit of the mist on the hills,' I had not seen anything similar before, nor have I gained from the shepherds that such things are common. Perhaps, if they were common, Ossian's ghosts would be less imposing."

MICROSCOPICAL SECTION.

December 15, 1862.

Mr. J. G. Lynde, F.G.S., M. Inst. C.E., in the Chair.

Mr. Alfred Fryer was elected a member of the section.

The Chairman stated that he was led to pursue Dr. Roberts' suggestions on the use of magenta dye in examining tissues. From experiments made since the last meeting of the section, he finds that the dye has no power to colour living tissue, whether animal or vegetable, but that as soon as life is extinct the action of the dye commences. He is continuing the experiments, which are of a most interesting character, and he hopes to lay the result before the next meeting of the section.

Mr. LEIGH considered it probable that, so long as vital action continued, ordinary endosmosis could not take place.

Mr. MOSLEY said that Mr. Hepworth had frequently tried magenta for injections, and the results were not satisfactory, as the colouring matter diffuses itself through the whole of the tissues, giving an appearance of dyed flesh rather than that of injected preparations. This appears to confirm the preceding observations, and to account for the accumulation of colour where the integument is thickest, by means of which Dr. Roberts discovered the spot on the red blood discs, as announced at the previous meeting.

Mr. LEIGH drew the attention of the section to the adulteration of size as a cause of mildew in cotton goods.

Mr. WATSON named the investigations made by Mr. Thompson about twenty-five years ago, as to the cause of mildew in madder purple-printed cottons shipped to hot climates. It was attributed to the starch employed in finishing the goods, which, acted upon by moisture, heat, and pressure, had given rise to an organic acid which discharged the colour.

Mr. HURST described his experience of mildew on printed cottons and upon dyed fustians, at Gibraltar and Calcutta. In most cases it appeared in spots and round patches, which affected the colours. On the fustians, he had no doubt it was caused by the growth of a fungus, as the surface of the spots was sensibly raised.

Mr. MOSLEY considered there might be several kinds of mildew; that upon the fustians might be attributed to the bone size with which those goods were generally finished, and known by the characteristic smell. Mr. Mosley also exhibited a pattern of grey calico, which had become discoloured and quite rotten in irregular patches, from mildew; it had lain for some time in a damp place, under pressure; there was this peculiarity about it, that the coloured patches whilst damp were quite tender, but on exposure to the air and drying, the cloth had recovered its strength.

Mr. HEYS remarked that twenty years ago he was engaged in the manufacture of fine muslins; it was usual to soak the weft in soap-suds to facilitate the weaving; and it was found the cloth was most liable to mildew during hot, close, summer weather, and the greater the quantity of goods heaped together the more rapidly would mildew set in. The flour from which the size was made was always the best that could be purchased.

Mr. HEYS exhibited mounted specimens of the fibres of the *Zostera marina*, and stated that, as the fibre is considerably finer than the finest Sea Island cotton, it might, probably, be of use in the manufacture of fine muslins for ladies' dresses, if possible to obtain a supply, and separate the fibre from the plant by machinery. Mr. Heys also exhibited mounted specimens of Queensland cotton, lately sold at five shillings per pound; the fibre is very regular in size, and much more cylindrical than other cottons; also several specimens from ripe and unripe pods; and Mr. Heys expressed the opinion that great advantage would arise from a regular and careful examination of cotton fibre taken fresh from the plant through every stage of growth.

ROYAL DUBLIN SOCIETY.

Thursday, January 22.

Rev. Dr. HAUGHTON, *F.T.C.D., F.R.S., &c., in the Chair.*

MR. EMERSON J. REYNOLDS read a paper entitled "*Wood Spirit and its Detection.*" The products of the destructive distillation of various vegetable matters, together with those of primitive organic origin, have received more than ordinary attention and careful study at the hands of chemists within the last ten or twelve years. Numerous highly complex acids, neutral bodies, and bases have rewarded the time and labour expended in their investigation, by the acquirement of both fame and riches for the discoverer—fame, owing to the great scientific value of the results, and riches, in consequence of their practical application. When the ever-changing circumstances under which these bodies are produced are considered, it ceases to be a matter of surprise that the compounds formed should be both numerous and variable, or that the examination of these should open an almost inexhaustible field of research to the scientific explorer. The object of the author in laying the present communication before the society was to give a brief preliminary notice of some experimental results which he has arrived at in the course of an investigation undertaken with a view to the complete separation of the more volatile constituents of wood-naphtha; and likewise to bring forward a new and reliable test for the detection of pyroxilic spirit. After describing the manufacture of wood-naphtha, the author then went on to state that, when studying the deportment of various metallic salts with the purified wood-spirit, he observed the following reaction:—When some solution of chloride of mercury was mixed with a few drops of the spirit, the mixture warmed, and then excess of caustic potash added, the oxide of mercury first thrown down was re-dissolved, with the production of clear solution. When acetic acid was added in excess to the alkaline solution, a bulky, white, gelatinous precipitate was produced, but slightly soluble in dilute acetic, nitric, or sulphuric acids, though readily dissolved by hydrochloric acid, which, at the same time, appears to decompose it. This precipitate was found to be composed of mercury with an organic body. Subsequent experiments demonstrated that that *acetone* was principally concerned in the production of the compound in question. From this point, the author commenced the investigation of the mercurial compound, and has since succeeded in making out its history and relations; but, as this was not matter essentially connected with the practical application of the test, he reserved it to form the subject of a second communication, which he would shortly have the honour of laying before the society. Mr. Reynolds then gave the results of some experiments with the mercurial solution above referred to, in order to illustrate its more distinctive characters. These may be stated as follow:—1st. The compound existing in solution is resinoid in character, and destitute of crystalline structure. 2nd. A white precipitate is produced on boiling the liquid for some time, or on the addition of an acid. 3rd. It is decomposed by hydrochloric acid, the organic body passing off unchanged. 4th. It is always necessary to employ heat in the production of the solution, otherwise a precipitate will not be produced on the addition of an acid. Up to the present time but one test has been proposed for the detection of wood-spirit; this is generally known as "Ure's test," having been proposed by the eminent chemist of that name. This simply consists in adding to the suspected spirit powdered hydrate of potash. If wood-spirit be present, the mixture becomes brown in half an hour. That this is a simple, easy, and correct test, when the spirit is unmixed with vegetable principles, he (Mr. Reynolds) does not deny; but there is one serious source of error, which, he thinks, prevents it from being generally applicable to the detection of wood-spirit in alcoholic tinctures, and this is as follows:—In commencing tha examination of a sample of any tincture, it is, of course, necessary to distil it, and apply the test to the distillate. Most tinctures contain some volatile principles, extracted from the plants used in their preparation. When the sample is distilled, the volatile oil, though generally having a high boiling-point, is dissolved in the vapour of alcohol, and thus contaminates even the first portion of the distillate. If to this caustic potash be added, in many cases the mixture assumes a brown tint, even though wood-spirit is not present, owing to the well-known action of caustic potash on many essential oils. The method which Mr. Reynolds adopts is as follows:—A small quantity of the suspected liquid is distilled, and to the distillate a little dilute solution of chloride of mercury added, and, finally, excess of caustic potash. The whole is then warmed, and, if complete solution of the oxide of mercury has taken place, divided into two portions: in one acetic acid is cautiously added; this causes the formation of a yellowish, bulky precipitate. After a short time, the remaining portion is boiled strongly, and a similar precipitate is thrown down, thus proving with certainty that wood-spirit is present. In applying this test it is necessary to be careful not to add too much chloride of mercury, otherwise an insoluble compound would be formed, and, as a consequence, a negative result arrived at.* When this test is applied to the "cleaned" spirit, the same result is obtained as with that which has not undergone the purifying process. The author concluded by remarking, that numerous analyses have shown him that the adulteration of the officinal tinctures with methylated spirit is carried on to a considerable extent in Dublin.

Mr. Reynolds then read a "*Note on Pure Methylic Alcohol.*" Having to prepare some of this body in an absolutely pure state for some experiments, he thought a statement of his experience with it might be interesting, and serve to settle the question which has been recently raised as to its odour. The salt from which the pure alcohol was obtained was the oxalate of methyl, every precaution being taken to render the latter perfectly pure. It was then decomposed according to the following reactions:

$$\left.\begin{matrix}(C_4O_4)''\\(C_2H_3)_2\end{matrix}\right\}O_4+2\left.\begin{matrix}K\\H\end{matrix}\right\}O_2=\left.\begin{matrix}(C_4O_4)''\\K_2\end{matrix}\right\}O_4+2\left.\begin{matrix}C_2H_3\\H\end{matrix}\right\}O_2,$$

and

$$\left.\begin{matrix}(C_4O_4)''\\(C_2H_3)_2\end{matrix}\right\}O_4+2\left.\begin{matrix}H\\H\end{matrix}\right\}O_2=\left.\begin{matrix}(C_4O_4)''\\H_2\end{matrix}\right\}O_4+2\left.\begin{matrix}C_2H_3\\H\end{matrix}\right\}O_2.$$

The methylic alcohol prepared by both methods possesses the following properties:—It is a mobile, inflammable liquid, having an odour quite distinct from that of ordinary alcohol, though not disagreeable, and a burning taste. It does not cause the re-solution of oxide of mercury in caustic potash; and finally, when treated with powdered hydrate of potash, and allowed to stand in contact with it for four and twenty hours, it had not become coloured.

The Chairman said the question laid before the meeting was a very important one in theoretical chemistry; but there were one or two points of very great importance, and Mr. Reynolds would add greatly to the obligation under which the society were for his paper, if, at some future time, he would state the exact composition of the mercurial compound mentioned in the paper.

Mr. Robert H. Scott said that he was very glad Mr. Reynolds had undertaken the investigation of these compounds, as it was well known Sir R. Kane held views on the nature of the ketones totally at variance with those of other chemists.

Mr. H. N. Draper asked Mr. Reynolds whether wood spirit, when purified by Eschwege's process, gave the reaction as strongly as before?

Mr. Reynolds: I have already stated that the "cleaned" spirit gives the reaction, but whether it does so in as marked a manner as that which has not been so treated I cannot say, without resorting to quantitative determination. With regard to the other questions I may remark that I have analysed the mercurial compound, and could give a formula expressing its composition, but I have not done so, as I wished to separate the purely practical portion of the subject from that which is only of scientific interest.

Mr. W. Andrews, M.R.I.A., read a paper on the "*Salmon Fisheries of Ireland;*" after which the meeting adjourned.

NOTICES OF BOOKS.

Nets for the Construction of Models illustrating the Science of Crystallography. By J. B. Jordan, Museum of Geology. Stanford, Charing-cross.

We have much pleasure in calling the attention of our mineralogical friends to the above series of nets for the construction of the models of crystals. There are forty-one separate figures printed in six different colours, corresponding to the six systems into which crystalline forms are divided. The nets are beautifully and accurately drawn, and in making up require the smallest amount of ingenuity, accompanied by a sharp pen-knife and a gum-bottle. The plates are rendered still more valuable by a very concise and clear treatise on the science of crystallography according to the system of Naumann—a system that is every day gaining ground in this country.

Jurors' Reports: International Exhibition: Class III. *Sections* A *and* B. *Substances Used as Food.*

There is nothing to criticise in these reports, so our notice must be confined to a few extracts. Section A is confined to "Agricultural Produce," and offers nothing that will be of use to our readers; but section B, devoted to "Drysaltery, Grocery, and Preparations of Food as Sold for Consumption," affords some passages which will be of interest to the general reader, and those engaged in the manufacture of food preparations. The first relates to

"*Chocolate.*

"This alimentary substance was first brought from the American continent to Europe by Columbus, and for almost three centuries it merely served as a luxury for the rich; but as soon as the system of monopolies and fiscal restraints, so prejudicial to the free development of industry and commerce, were abandoned by the civilised states of Europe, it came into general use. In fact, until a very recent time, the use of cocoa and chocolate—as compared with that of coffee and tea—was very limited in England, as well as on the Continent; and the recent development of the chocolate trade must be ascribed, not only to the general extension of commerce and industry, but also and especially to many great improvements made in the preparation of the article. Attention has been directed to the quality of the berry, the purity of the materials with which it is ground, the quickness of manufacture—due to the use of steam-power and of improved machinery—and the cheapness of production, which latter makes the use of chocolate accessible even to the poorer classes of the community.

The principal species of cocoa from which chocolate is made, are thus classified by the French manufacturers, who are foremost in this industrial branch:—

"The Soconusco; the Caracas; the Maracaibo and Magdeleine; the Guayaquil; the Trinidad; the Marágnan and Para (from Brazil); the Cayenne (from Guiana); the cocoa of the islands Martinique, Hayti, Guadaloupe, Jamaica, St. Lucia, Bourbon, &c.

"But cocoa, even of the finest description, as supplied to us by nature, can by no means satisfy the refined taste of European palates; it must go through a number of processes before it can be converted into an agreeable beverage.

"In the first place, the nuts are winnowed, and those berries which have suffered from the sea-transport are, or ought to be, separated from the sound nuts, and not used as food, although they may be employed in perfumery.

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* It must not be forgotten that oxide of mercury is soluble to a very slight extent in potash; but such a solution would not yield a precipitate on boiling or on the addition of excess of acetic acid.

"The winnowing is followed by the roasting or torrefaction of the cocoa-nuts; an operation which requires much skill and experience, as the good or bad quality of the chocolate depends greatly on the care and attention bestowed upon it. The roasting must not go beyond the limits of a perfect drying, otherwise the cocoa-butter would be absorbed by the shell, and the chocolate lose a good part of its nourishing substance.

"From the roasting-cylinder the cocoa-nuts are taken and spread over a drying-board or hurdle until they get cold; they are then passed between two cylinders in order to have the shells broken. The kernels are now separated, first by means of a riddle, and then more carefully by hand, so that nothing but the kernel of the nut remains to enter into the mass of the chocolate.

"After this, the cocoa is ground and worked into a semi-fluid paste, with which the sugar and aromatic substances are incorporated. The trituration is then continued until the whole paste is converted into an entirely homogeneous mass, which is finally shaped into various forms, as blocks, loaves, tablets, lozenges, &c. Some art and ability are required to make a good composition of the ingredients by employing the right proportion of sugar and spices for each sort of cocoa.

"Sometimes the fat substance is altogether separated from the cocoa, and the butter thus extracted is employed separately as a kind of delicacy.

"The inferior kinds of chocolate made in large manufactories are generally mixed with starch, while the medicinal chocolates are combined with arrowroot and sometimes with Iceland moss.

"It would seem that *France* is foremost among all nations in the manufacture of chocolate. In England, however, attention is being paid by several manufacturers to the specialities of the French productions. They are, for example, importing from France the berries which give to the French chocolate its peculiar coolness and softness to the mouth. These are blended in their proper proportions, with due regard to the characteristic flavours of each; and they are ground to paste without allowing the cylinder to become heated unnecessarily; after which they are mixed with sugar and flavoured. Among the varieties of cocoas which are thus selected are the cocoa of Puerto Cabello d'Estrella, Maracaibo, and others before mentioned."

Our next extract relates to

"*Confectionery and Sweetmeats.*

"In a very conspicuous place in the Eastern Annex there is a case of Dr. Hassall's, which represents, as far as confectionery is concerned, the various kinds of adulteration which were practised about the time of the first Exhibition. Every comfit and bonbon contains an amount of impurity which would shut them out from the market at the present time—chalk, plaster of Paris, flour, and poisonous pigments abound in the composition of most of them. A glance, however, at the confectionery which is exhibited in the neighbourhood of this case, will show how great has been the improvement of its manufacture. And not only has there been a large improvement in the quality of the goods, but there has also been a large increase in the quantity of them produced. No longer ago than the year 1855, the total amount of confectionery made in this country did not exceed 8000 tons in the year, whereas at the present time the quantity is not less than 25,000 tons per annum. One house alone, as we are informed, is making about two and a-half tons of lozenges and comfits per day, besides about fifty tons of marmalade and sixty of jams in the course of the year.

"Looking at the kind of confectionery which was in common use in this country at the time of the first Exhibition of 1851, it is manifest that a great improvement has been effected in every stage of its manufacture; no longer are the poisonous compounds of lead, mercury, and copper employed as colouring ingredients; and rarely do we find that any notable proportion of starch or flour has been introduced into the composition. Manufacturers are still of opinion that a small quantity of flour is necessary in some cases to bring up the colour, and to make the comfits freely soluble in the mouth. In the manufacture of almonds, also, a certain proportion of gum and flour must be used as a ground or coating over the almond for the adhesion of the sugar. That this, however, is not essential, is proved by the many varieties of French and German comfits which are entirely without this material. And then with regard to the use of dangerous pigments: although the higher class of confectioners have long since abolished them, and have resorted to the most inert preparations of the vegetable kingdom, yet a few of the inferior manufacturers still employ the yellow and red compounds of lead for colouring materials. This is actually the case with some of the confectionery exhibited on the present occasion, and, therefore, it has been passed over without award or commendation. The practice is now so utterly unnecessary, and withal is so dangerous, that it ought to be entirely abolished. It is scarcely necessary to refer to the records of toxicology to furnish instances of the mischief which has been occasioned by this kind of adulteration; but it may be stated, on the authority of Dr. Guy and Dr. Letheby, that many most serious consequences have resulted from it. Nor, on the other hand, is it necessary to refer to the confectionery of the French and German departments, as well as that of Schooling and Co., Keiller and Son, and Messrs. Wotherspoon, in the English Department, as illustrative of the non-necessity for the employment of dangerous pigments. All these specimens are remarkable for their beauty and purity; and, as the public have long since condemned the use of such dangerous materials, it is high time that they should be wholly abandoned. A simple test will always discover the presence of any serious mineral impurity: burn a comfit completely away, or as far as it will burn away, in an iron spoon, and, if there is any residue in notable quantity, the comfit is adulterated. The presence of lead may always be discovered by exposing the comfit to an atmosphere of sulphuretted hydrogen, and, if it turns black, it is a dangerous commodity."

It is satisfactory to find that pickles have improved as much as confectionery, and that copper is now a rare adulteration.

"*Pickles.*

"Since the Exhibition of 1851 a very great improvement has been effected in this branch of manufacture. Public attention has been drawn to the fact, that most of the green pickles of English commerce were tinted, or rather mordanted with copper, the metal having been put into the vinegar purposely, in the form of a salt, or else communicated to the pickles by boiling them in copper pans. In some cases the proportion of copper was considerable; and instances are on record where emetic properties have been communicated to the pickles by the large quantity of copper contained in them. Little by little, however, this practice has been discontinued, or rather diminished, until the proportion of copper in a bottle of pickles hardly reached the hundredth part of a grain. Manufacturers were anxious to exclude even this small proportion; but they were met by the difficulty that, when the pickles were prepared without a minute quantity of copper, they were very unsightly, and in many cases were wholly unsaleable. The pickles of France and Belgium, however, demonstrated the fact, that very bright colour might be communicated or retained without the admixture of a particle of copper; and very recently some of the manufacturers of this country have accomplished the object without the aid of this metal. The finest specimens of this class of goods were, beyond all doubt, those very recently produced by Messrs. Batty and Co., who use a colouring material from vegetables and green foliage. All the West Indian pickles of every exhibitor are largely contaminated with copper, and ought not, therefore, to be eaten."

It would be interesting to know how the "colouring material from vegetables and green foliage" is procured and used by Messrs. Batty and Co. Our pickle manufacturers may take a hint from the French, who, says the Report, "have a habit of setting off the colour of the pickles by the aid of bottles of a brilliant grass green. It is a matter for consideration whether this practice is or is not deserving of imitation in this country."

Our concluding extract gives a description of two new processes, of which we gave some account in our notices of the Exhibition, but which is here given in greater detail:—

"*Preserved Meats.*

"Very little improvement has been made in this branch of industry since the Exhibition of 1851, notwithstanding the importance of it, and the large increase of its trade. Most of the meat is overdone, and is loaded with fat, and in many cases the food had acquired a metallic flavour from the vessel in which it was preserved.

"As in 1851, the principal mode of preserving the meat is by the exclusion of atmospheric air. This is accomplished by first filling the case with the meat or soup, then soldering down the cover, which has a small aperture in it for the exit of steam. The cases are then stood in a saline bath, or upon a hot plate, until the liquid within them boils freely and expels the contained air. While thus boiling, and steam is freely issuing from the minute orifice in the cover, the hole is stopped with solder, and so the case is hermetically sealed. By this process the meat is generally overdone; it loses its fresh flavour, and becomes sodden and broken up; besides which, on the cooling of the case, the steam within it condenses, and there is a constant vacuum and liability to indent and buckle from atmospheric pressure. A trace, also, of air frequently remains within the case, and thus the oxygen slowly but surely sets in putrefactive decomposition. To remedy these defects two improvements have been suggested. One of these was patented by Messrs. John McCall and Co., of Houndsditch, in 1861. It consists in the introduction into the tin of a small quantity of the sulphite of soda. The proportion used is about twelve grains to each pound of meat. The effect of this salt is to absorb from the meat and the air of the case the small proportion of free oxygen which, under the best of circumstances, often remains within it. In order that the sulphite of soda shall be isolated until the air is expelled and the case soldered down, the salt is enclosed in a tin capsule soldered to the inside of the top of the canister. The capsule has two holes in it, plugged with fusible metal, which melts at a few degrees above the temperature of boiling water (about 218°). As soon as the steaming hole is closed, and the temperature raised by the steam pressure within, the fusible metal melts and releases the sulphite of soda within the capsule. It is presumed that the effect of this improvement will be the doing away with the necessity for the prolonged steaming of the food before the cases are soldered down, and thus the prevention of that overcooking which has hitherto been so objectionable. In the case of soups, the salt is not always enclosed in a cap, but is put into the soup.

"The other improvement is a process of Messrs. Jones and Trevethick, of Botolph Lane. It provides for the preservation of food in the raw state, without the disadvantage which all preceding processes have of maintaining a vacuum within the case. The process is conducted in the following way:—The meat is wrapped up in a piece of cloth, and is enclosed in a tin case; the cover of the case is then soldered down. From the top of the case there projects a short tube which is brought into connection with the exhausting power of an air-pump—a very ingenious contrivance is adopted whereby the tin cases are protected from external pressure during exhaustion. This is accomplished by placing the tins in an air-tight vessel filled with water, the inelastic property of which supports the sides of the case, and prevents the slightest collapse. When the air has been exhausted from the tins, a charge of pure nitrogen is let into them. This also is exhausted, and then a minute dose of gaseous sulphurous acid is introduced. Finally, another charge of nitrogen is let in, and the vessels are permanently soldered down. The effect of the sulphurous acid is to combine with the minute trace of oxygen which remains in the case, and the nitrogen, by filling the case, prevents collapse from atmospheric pressure. The nitrogen is obtained pure from atmospheric air by a simple chemical process which absorbs the oxygen. Mutton, beef, hams, fish, poultry, and game were exhibited preserved in this way, and the results were remarkably successful and satisfactory. The poultry and game had the appearance of having been but an hour or two from the poulterer's. The ham and bacon were as good after many years' keeping as if they had been just cured; and, although the fresh meat had acquired a rather bright red tint from the action of the gases upon the flesh, yet the flavour was decidedly superior to that of any other kind of preserved meat. The process offers a promise of great economic value."

NOTICES OF PATENTS.

2694. *Preservation of Stone, Brick, &c.* W. Smith, Stafford, Dated October 26, 1861.

The patentee takes flint or other siliceous mineral, and reduces it to a fine state of division by calcination and grinding. With this he mixes powdered alum, or dry sulphate of alumina, and forms the composition into a kind of paint by the addition of bituminous substances, glue, linseed oil, and turpentine. This material is applied as a coating to brick walls, stone, or stucco, and is said to afford a protection against decay and damp.

The advantage of employing soluble substances like glue and alum, or the deliquescent sulphate of alumina, to keep out the damp may well be questioned. For common purposes a useful paint might be prepared from the remaining ingredients.

2716. *Treatment of Skins and Hides.* J. H. Johnson, Lincoln's Inn Fields, London. (A communication.) Dated October 29, 1861.

In the course of kneading and fulling the skins the patentee introduces compounds of the fatty acids with metallic oxides, particularly those containing the oxides of iron, manganese, or chromium, baryta or lime. The silicates of these bases may likewise be employed either alone or in combination with the fatty substances already mentioned.

2718. *Composition for Igniting Lucifer Matches.* M. A. Mennons, Paris. A communication. Dated October 30, 1861. (Not proceeded with.)

This specification describes a mode of preparing a friction surface suitable for the ignition of non-phosphoric, or safety matches. Shellac, or a similar resin, is heated nearly to the melting point, and twice its weight of alcohol added, and well stirred, for the purpose of producing a homogeneous paste, with which one-tenth part of amorphous phosphorus in fine powder is then intimately incorporated. Whilst warm, this preparation is applied as a coating to the side of the match box, or is laid upon a suitable tablet either of pasteboard, wood, or earthenware.

This resinous varnish would constitute an excellent protection and means of attachment, since it is repellant of moisture, besides being very adhesive.

2726. *Manufacture of Stearin.* E. De Bassano and A. Brudenne, Brussels. Dated October 30, 1861.

This claim refers to the employment of sulphurous acid for effecting the conversion of stearin and oleic acid into descriptions of fatty matter having higher melting points, and more suitable for the manufacture of candles.

2744. *Manufacture of Cast-Steel or of Homogeneous Iron.* Robert Mushet, Coleford. Dated November 1, 1861.

For the preparation of superior qualities of cast-steel and homogeneous iron, the inventor mixes with these metals respectively a certain proportion of the alloy of iron, manganese, and titanium, with or without carbon, obtained by processes described in the specification of a former patent, No. 2637.*

2750. *Preventing the Injurious Effects occasioned by Smoke, Sulphur, and the Deleterious Gases which escape from Chimney Stacks, Calcining Houses, Chemical and Other Furnaces.* W. B. Smith and W. Bennett, Cornwall. Dated November 1, 1861. (Not proceeded with.)

In this scheme for the suppression of the smoke nuisance, the inventors maintain a draught of air through the furnace by the rotation of a fan placed in the flue, and which is intended to drive forward the smoke and products of combustion; but these are not allowed to escape by the chimney until they have been passed through a series of intermediate chambers, and a diaphragm either of canvas or wire gauze.

Grants of Provisional Protection for Six Months.

3308. Louis Amand Lesage, Rue St. Appoline, Paris, "An improved method of preparing jellies and jams."—Petition recorded December 10, 1862.

3375. Fedor de Wylde, Great College Street, Camden Town, London, "Improved means for the protection and preservation of lead surfaces exposed to the action of water, and for the protection of such surfaces from decomposition by atmospheric action."—A communication from Professor Dr. Henry Schwarz, Breslau, Prussia.

3468. William Edward Newton, Chancery Lane, London, "Improvements in preserving organic substances from decay."—A communication from Leonard Laureau, Rue St. Sébastien, Paris.—Petitions recorded December 29, 1862.

3312. Astley Paston Price, Lincoln's Inn Fields, London, "Improvements in the manufacture or production of blue colours."—A communication from Augustus Eisenlohr, Heidelberg, Baden.

3443. Ebenezer Stevens, Cheapside, London, "Improvements in machinery for preparing dough and paste suitable for making bread, biscuits, pastry, cakes, and similar articles."

3465. Frederick Tolhausen, Faubourg Montmartre, Paris, "The use of petroleum or coal-oil as fuel, and also for the machinery and apparatus to be employed for this purpose."—A communication from Edward John Biddle, Faubourg Montmartre, Paris.

Invention Protected for Six Months by the Deposit of a Complete Specification.

67. Liveras Hull, Massachusetts, U.S., "Improvements in the treatment of ground caoutchouc, and for the purpose of rendering it elastic, or improving its elasticity, as well as imparting to such caoutchouc other useful properties."—Deposited and recorded January 8, 1863.

Notice to Proceed.

2462. Samuel Pudney, Manor Street, Clapham, Surrey, "Improvements in apparatus to be used in the manufacture of sulphuric acid."

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* Chemical News, vol. vii, p. 35.

CORRESPONDENCE.

Separation of Silver from Lead.

To the Editor of the Chemical News.

Sir,—A new process for extracting silver from lead would be, indeed, successful, to compete with that of Pattinson, now universally employed in this country. On perusing your "Notices of Patents," in the last Number of the Chemical News, I was surprised to observe that the re-patenting of an old invention had not called forth some of the sensible remarks which usually indicate your opinion of the various novelties proposed and registered in the Patent Office.

Mr. Birkbeck must be ill-informed of metallurgical progress not to have heard of Mr. Parkes' ingenious process for extracting silver from lead by means of zinc, patented about twelve years ago, and which, though not at present in uae, has been tried on a large scale in South Wales. It is identical with the re-discovery patented again by Mr. Birkbeck.—I am, &c. William Baker.

Lead Works, Sheffield.

To the Editor of the Chemical News.

Sir,—In last week's Number of the Chemical News I find notice of a patent in the name of G. H. Birkbeck—No. 2643, October 23, 1861—"for extracting silver from lead," by the addition of a small quantity of zinc to the melted lead, agitating, allowing to cool, when the silver will be found combined with the zinc floating on the surface of the lead; the silver is then separated either by distillation or by solution in dilute acids, when the silver is left behind as a residuum.

Now, as it is but fair that the merit of any new discovery should be given to the real inventor, I beg to call your attention to the following facts:—

I have no less than three patents, dated June 11, 1850, June 24, 1851, and May 1, 1852, all for the use of zinc in separating silver from lead by methods identically the same as given in Mr. Birkbeck's specification. My patents have been extensively carried out in one of the largest smelting works in Wales.

Under these circumstances, I should feel greatly obliged if you will insert this in your next Number, being anxious that my prior right to the discovery should be made known.

I am, &c.
Pro Alexander Parkes,
H. Parkes.

Birmingham.

International Medal and the Pharmaceutical Society.

To the Editor of the Chemical News.

Sir,—In your report of the last Pharmaceutical meeting you do not mention that the chairman exhibited the medal which had been awarded for the display of drugs shown by the society. It was taken but little notice of by the members present, I am told. Probably, each one thought he had but little individual interest in the matter. I wish, however, by means of the Chemical News, to call the attention of the members of the society to the circular of Mr. Wyon, in which he says he has been granted the privilege of supplying copies of the medal, identical with, and indistinguishable from, the originals, to all members of a company or firm to which a medal has been granted by the Commissioners. It would seem from this, that each member of the society can have a medal on the payment of one guinea to the Messrs. De la Rue, and I dare say there are a good many who would like to possess one. The object of the display by the society would then appear to be gained; for certainly the award of a medal to the society in its corporate capacity looks rather absurd.—I am, &c.

M. P. S.

MISCELLANEOUS.

PURE BREAD.

In these camel-swallowing times, if a waiter at an eating-room were to hand a customer a piece of bread with his bare fingers, the chances are that, if the diner were an irascible man, the unfortunate wight would soon find himself in the street. If a guest at your dinner-table were to commit the same heinous offence, no doubt you would consider the crime against good breeding and cleanliness so great that it would be his last appearance at your board. And yet the hands of the two culprits were undoubtedly scrupulously clean. The bread they had touched, however, and about which we feel no disgust when handed with a fork, was prepared in a loathsome, underground den, reeking with filth and vermin; the hands with which the dough was mixed were dirty; and the exertions of handling heavy masses of leavened flour in a close, unventilated bakehouse caused the perspiration to roll off the arms and face of the workmen in streams into the trough. The board on which the dough was laid preparatory to its being fashioned into loaves, had served, but a few moments before, with a filthy sack thrown over it, as the temporary couch of a journeyman baker, to whom personal ablution was a stranger. To fill up this picture of horrors, there is one bakehouse at the West-end in which the space between the kneading-trough and the wall was used for the same purpose as those cast-iron erections which are now beginning to form inseparable adjuncts to our cab-ranks. Our readers, we are sure, are in possession of the strongest stomachs that long courses of sulphuretted hydrogen, kakodyl selenethyl, and other odoriferous compounds, can bestow on the human subject; but we think we have said enough to raise a slight disgust to ordinary shop bread. If we have not said sufficient, let them read a copy of Mr. Tremenheere's report on the condition of the journeymen bakers of the metropolis, and they will find horrors piled on horrors to such a height, that they will never eat shop-bread again. Several readers of that fatal report have abjured the baker, and now subsist entirely on home-made bread, kneaded and shaped into loaves by the fair hands of some female member of their family, instead of by the filthy paws of a journeyman baker.

But it is not every one who possesses in his family a good bread-maker, for bread-making is a talent, and cannot be acquired without a long apprenticeship. Hence, the differing qualities of home-made bread. The poor bachelor, too, who is obliged to gulp down the unsavory penny-loaves from round the corner—our pity is great for him. We were much pleased, therefore, to receive an invitation from Stevens' Bread Machinery Company, to inspect their bakehouses, lately established at Islington and Brompton. It does one's heart and stomach good to see the cleanliness with which all the operations are carried on, from the flour to the finished loaf. The establishment at Islington consists of a spacious shop, at the back of which is the machinery with which the bread is kneaded, worked by steam. This we will not describe, as Stevens' Bread-Making Machine has already been before the public for some years. Suffice it to say, that the laborious operation of kneading is accomplished by a series of claws, revolving in the trough in which the mixture of yeast, flour, and water, is contained. This portion of the building is divided from the shop by a glass partition, so that the public may see for themselves the cleanliness with which all the processes are conducted. Behind the machinery are two tiers of ovens, approached by light, iron staircases, and, above all, is the flour store. The bakery at Brompton is conducted on a precisely similar plan, and the inhabitants of these favoured localities are enabled to procure a constant supply of pure bread. Although we do not generally approve of joint-stock companies entering into trade, we must confess that, in this instance, an inestimable advantage will be conferred on the public at large by the establishment of a few large concerns of this sort in different parts of London. The private baker who persists in employing the dirty hand-and-arm-kneading process will soon find his customers forsaking him, and will seek a remedy in employing one of the company's machines. Of course the prejudice in the trade will have to be broken down, and this will take time. In the meantime, such of our readers as do not inhabit the localities we have named, will do well to inspect the company's family dough-making machines and fireclay ovens, which will bake bread in front of a good kitchen fire. There is one part of the company's method of bread-making that we must, in parting, take exception to, and we do so without hesitation, inasmuch as the remedy can be found by the exercise of a little of the ingenuity which has been brought to bear on the other portions of their machinery. A theoretically pure loaf ought not to be touched by human fingers until it reaches the home of the consumer. In the company's works, as soon as the dough leaves the fermenting trough, it is fashioned into loaves by the assistants. The public are so accustomed to the shape of the ordinary household loaf, that no other would be acceptable. Square bricks serve very well for sandwiches, and "cottages" for people possessed with perfect teeth, but will not do for general purposes. In these days, when tiles and drain-pipes of the most complicated forms are made by machinery, we can see no reason why the ordinary household loaf should not be made by mechanical means.

In conclusion, let us advise the company most strongly to have nothing to do with testimonials from the numerous adulteration-mongers, from whom, we have no doubt, they have long since received applications to analyse their bread. If their bread is good—and the samples we have tasted were excellent—the public will soon find it out, and bestow on them the best of testimonials—a large trade; while fulsome letters from half-a-dozen "chemical and microscopical analysts," with half the alphabet added to their names, can only bring suspicion on their wares, and make sensible people chary of dealing with them. If "good wine needs no bush," what shall we say of that still greater rarity—good bread? The company has commenced in a spirited and dignified manner, without puff or flaming advertisements. Let them go on so, and we venture to prophesy that their household loaves will soon become "household words."

Royal Institution.—On Monday, February 2, a General Monthly Meeting will be held at 2 o'clock. The following lectures will be delivered:—On Tuesday, February 3, at 3 o'clock, Professor Marshall, F.R.S., "On Animal Mechanics." Thursday, February 5, at 3 o'clock, Dr. E. Frankland, F.R.S., on "Chemical Affinity." Friday, February 6, at 8 o'clock, James Glaisher, Esq., "On Aërial Scientific Research." Saturday, February 7, at 3 o'clock, W. S. Savory, Esq., F.R.S., "On Life and Death."

ANSWERS TO CORRESPONDENTS.

⁂ All *Editorial Communications* are to be addressed to the Editor; and *Advertisements* and *Business Communications* to the Publisher, at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

⁂ In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

Vol. VI. of the Chemical News, containing a copious Index, is now ready, price 10s. 8d., by post, 11s. 2d., handsomely bound in cloth, gold-lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. if sent to our Office, or, if accompanied by a cloth case, for 6d. Vols. I. and II. are out of print. All the others are kept in stock. Vol. VII. commenced on January 3, 1863, and will be complete in 26 numbers.

THE CHEMICAL NEWS.

Vol. VII. No. 166.—February 7, 1863.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Researches on the Platinum Metals,* *by* Wolcott Gibbs, *M.D.*

(*Continued from vol.* iii., *page* 149.)

The mass of soluble chlorides obtained by the before-mentioned method contains all the platinum metals, although only traces of osmium and palladium are present; in addition, there is usually more or less insoluble matter, consisting partly of the impurities of the ore itself, and partly of undissolved oxides. A certain portion of iron also remains with the mass even after careful washing.

The washings contain a very large quantity of iron, a little ruthenium, in the form of bichloride of ruthenium and potassium, and possibly a trace of palladium. When the washing with chloride of potassium has been carefully executed with a cold and saturated solution, the quantity of ruthenium dissolved is too small to be worth separating. It only remains, therefore, to separate the metals in the mass of mixed double chlorides. Platinum and iridium exist in this mass in the form of bichlorides, as $PtCl_2,RCl$ and $IrCl_2,KCl$; ruthenium is present partly as sesquichloride, and partly as bichloride, $Ru_2,Cl_3,2\,KCl$ and $RuCl_2,KCl$; rhodium appears to be present only as sesquichloride, $Rh_2Cl_3,3\,KCl$, so far, at least, as it is possible to determine. The separation of these metals from each other is a problem of no ordinary difficulty.

Of the methods which have been proposed for this purpose, I have no hesitation in saying that that of Claus is the only one which yields the different metals in a state of purity. In fact, after a long and laborious study of the subject, I believe that no other chemist has worked with pure preparations of iridium and rhodium, the descriptions even of Berzelius applying only to mixtures of isomorphous salts in various proportions. In addition, the discovery of ruthenium by Claus rendered a complete revision of the subject necessary. Such a revision Claus has himself given in his elaborate and most valuable memoir already cited.† For the details of Claus's process I must refer to his paper. In principle his method consists in separating osmium and ruthenium by fusion with a mixture of caustic potash and saltpetre; cold water then dissolves out osmate and ruthenate of potash. The residual mass consists chiefly of the oxides of iridium, rhodium, and platinum. These, after distillation with aqua regia, are brought into the form of double chlorides with ammonium, the iridium reduced to sesquioxide by means of sulphydric acid, and the platinum separated from the other two metals by washing with a strong solution of sal-ammoniac. The filtrate from the chlorplatinate of ammonium contains iridium and rhodium as sesquichlorides; the iridium is converted by chlorine and nitric acid into bichloride, and the insoluble chloro-iridate of ammonium separated by filtration from the soluble rhodium salts. This method has given excellent results in the hands of its author, but is not free from objection, the chief difficulty lying, in my opinion, in the separation of ruthenium from iridium. The ruthenium salt, $Ru_2Cl_3,2KCl$, is scarcely to be distinguished in appearance from the iridium salt $IrCl_2$, KCl; it possesses about the same degree of solubility in water and in solutions of chloride of potassium and ammonium. By the action of reducing agents, the sesquichloride of ruthenium is reduced, apparently, to a protochloride, the double salts of which, like $Ir_2Cl_3,3KCl$ and $Ir_2Cl_3,3NH_4Cl$, are quite soluble in water. Oxidising agents readily convert the protochloride of ruthenium into sesquichloride, which is again precipitated, in Claus's process, with $IrCl_2,KCl$, in the form of $Ru_2Cl_3,2KCl$. The portion of ruthenium which exists in the mass of double chlorides, in the form of $RuCl_2,KCl$, may be easily and almost completely removed by repeated and careful washings with a cold and strong—but not saturated—solution of chloride of potassium, in which the salt, $RuCl_2,KCl$, is soluble, while the other double chlorides remain undissolved. The small quantity of ruthenium dissolved in washing out the sesquichloride of iron may be recovered by precipitating the iron carefully with a solution of carbonate of potash, adding a slight excess of chlorhydric acid to the filtrate, and evaporating to dryness, when the ruthenium salt remains mixed with a great excess of chloride of potassium. In Claus's process, however, this method is of little use, since the greater part of the ruthenium is removed in the form of ruthenate of potash, while another portion remains as $Ru_2,Cl_3,2KCl$, insoluble in chloride of potassium.

Another difficulty in Claus's process arises from the fact, that the rhodium salt, $Rh_2Cl_3,3NH_4Cl$, is quite insoluble in a strong solution of chloride of ammonium, while a weak solution of the same salt dissolves a considerable portion of the iridium and ruthenium salts, $IrCl_2,KCl$ and $Ru_2Cl_3,2KCl$.

Claus's method of separating ruthenium—in the form of Ru_2Cl_3—from iridium by adding a few drops of ammonia to the neutral solution, and boiling so as to precipitate $Ru_2O_3 + xHO$, gives good results when the quantity of ruthenium is large in proportion to the iridium present, but not otherwise. Small quantities of ruthenium cannot be separated at all by this process, nor have I in any case been able to obtain iridium absolutely free from ruthenium by boiling. For these reasons, while doing full justice to the extraordinary skill and success of the Russian chemist, I have still thought the problem of the complete separation of the metals of the platinum group worthy of a new investigation.

The method which I now use consists essentially in

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* From the *American Journal of Science*, vol. xxxiv., page 341.

† "Beiträge zur Chemie der Platin-metalle." Dorpat, 1854.

the employment of the alkaline nitrites as separating agents; in addition, however, I avail myself, as Claus has so skilfully done, of the different degrees of solubility of the double chlorides of the platinum and alkaline metals.

The relations of the alkaline nitrites to the platinum metals have not hitherto been studied, and require special attention in this place. The remarkable double salts which these metals form when treated with the nitrites of potash and soda, will be fully described hereafter; but the general character of the salts themselves, upon which my methods of separation are based, may advantageously precede the details of their practical application.

Osmium.—A solution of osmic acid is reduced by addition of nitrite of potash to osmious acid, which unites with the alkali, forming the well-known beautiful red salt discovered by Frémy. The solution may be evaporated to dryness without decomposition. The nitrite may, therefore, be added with great advantage when solutions containing free osmic acid are to be evaporated, or even transferred from one vessel to another. No other reducing agent yet applied answers the same purpose, as the osmium is obtained at once in a very convenient form for preservation. When a solution of osmic acid, to which nitrite of potash has been added, is evaporated sufficiently, and then allowed to cool, beautiful garnet-red octahedral crystals of the osmite of potash separate. These should be dried *in pleno*, over sulphuric acid, and not in contact with paper or organic matter, which partly reduces the osmious acid to the brown sesquioxide of osmium. Nitrite of potash exerts no sensible action when boiled with a solution of chloro-osmate of potassium. Any salt which may be formed is very soluble in water.

Ruthenium.—When a solution of nitrite of potash is added in excess to the sesquichloride of ruthenium, either free or in combination with chloride of potassium or ammonium, a yellow or orange-yellow colour is produced, but no precipitate is formed. A precisely similar change occurs when the ruthenium is in the form of bichloride; but in this case the change of colour is produced more slowly, and usually requires heating or even boiling. The change of colour produced is in both cases owing to the formation of an orange-yellow double salt of ruthenium and potassium, which is very soluble in water and alcohol; its relations to alcohol, in particular, enable us to distinguish ruthenium from the other platinum metals more perfectly than has hitherto been possible. Nitrite of soda forms a similar salt with solutions of the bichloride of ruthenium.

When a few drops of sulphide of ammonium are added to a solution of the ruthenium double salt above mentioned, a magnificent crimson colour is produced. This reaction furnishes a characteristic test for ruthenium of the greatest value, since it is not, like Claus's beautiful reactions with sulphocyanide of potassium or sulphuretted hydrogen, materially affected by the presence of the other metals of the same group. The test may be most advantageously applied as follows:—The liquid supposed to contain ruthenium is first to be rendered neutral or alkaline by addition of carbonate of soda or potash. I prefer to use an excess of alkali. Nitrite of potash in solution is then to be added, the liquid boiled for an instant, allowed to become perfectly cold, and a drop or two of colourless sulphide of ammonium added. On shaking, the colour appears and rapidly deepens to the finest red. When the quantity of ruthenium present is very small, or when large quantities of the other platinum metals are also present, it is better, after adding the alkaline carbonate and nitrite, to evaporate the whole to perfect dryness on a water bath, and treat the dry and powdered mass with a small quantity of absolute alcohol. The alcoholic solution is then to be filtered off, and tested directly with sulphide of ammonium. In this manner, the smallest trace of ruthenium may be detected, even in the presence of very large quantities of the other platinum metals. A solution of the double nitrite of ruthenium and potassium is completely precipitated by a long-continued current of sulphydric acid gas. Sulphide of ammonium also precipitates the solution after a short time, but when added in excess re-dissolves the dark chocolate-brown precipitate. The addition of a slight excess of dilute chlorhydric acid then completely precipitates the sulphide of ruthenium.

Iridium.—When a solution of nitrite of potash or soda is added to one of chloro-iridate of potassium or ammonium, the colour of the solution instantly changes to olive-green, the iridium being reduced from bichloride to sesquichloride. The reduction takes place most rapidly in a hot solution, in which it is almost instantaneous. When the solution cools, the new double chloride usually crystallises. An alkaline nitrite is a far more elegant and convenient reducing agent for the separation of iridium from platinum than either sulphuretted hydrogen, sulphurous acid, or cyanide of potassium. The reduction in question is expressed by the equation,

$$2(IrCl_2,KCl)+KO,NO_3=Ir_2Cl_3,3KCl+NO_4.$$

A very different result is, however produced, when an excess of nitrite of potash is added to a solution containing either of the chlorides of iridium, and the solution is boiled for a few minutes, or even allowed to evaporate spontaneously. The olive-green liquid becomes yellow, and contains the whole of the iridium in the form of a double salt, which is soluble in water, but insoluble in alcohol.

When, however, the solution is boiled, an excess of the alkaline nitrite being present, part of the iridium is thrown down as a heavy snow-white powder, which is insoluble in cold water; hot water dissolves it in small proportion; the solution, however, speedily becomes milky, and remains so for a long time. Chlorhydric acid, even on boiling, exerts but little action upon it. Nitro-muriatic acid, with the aid of heat, gradually yields a solution containing bichloride of iridium. The insolubility of this compound in water and acids is very remarkable, the similar salts of the other platinum metals being nearly all readily soluble either in water or in dilute acids. Nitrite of soda, under the same circumstances, forms with iridium a soluble orange-yellow salt. The two soluble double nitrites of iridium and potassium or sodium give no precipitate with alkaline sulphides, even on boiling. The soda salt is easily decomposed by boiling with chlorhydric acid, giving a solution of chloro-iridate; the potash salt is decomposed with great difficulty.

Platinum.—Solutions of the alkaline nitrites exert a scarcely sensible reducing action upon chloroplatinate of potassium or ammonium, even after long boiling. The salt retains its colour, and crystallises unchanged from the solution on cooling. When other metals belonging to the same group are present, and the solution, after adding the nitrite, is boiled for some time, a small quantity of platinum is dissolved, giving a yellow salt very soluble in water and alcohol. When nitrite of potash is added to a solution of platinum, sulphide of ammonium immediately throws down a brown precipitate of sulphide of platinum. The same effect is produced in a solution of the yellow salt above mentioned.

Palladium.—A solution of either protochloride or bichloride of palladium immediately becomes yellow or orange colour, when an excess of nitrite of potash is added to it. Two different double nitrites of palladium and potassium are usually formed in this reaction. Both are soluble, and are precipitated in a crystalline form by alcohol from concentrated solutions. One of these salts has a deep orange-red colour, the other is lemon-yellow; both are readily soluble in water, and alkaline sulphides precipitate palladium completely from the solutions.

These two salts were first described by Fischer.[‡]

(To be continued.)

On Arithmetical Relations between Chemical Equivalents,[*] *by M.* Carey Lea.

In previous numbers of this Journal I have published a series of papers on this subject, one point of which has been subjected to criticism; namely, that I have introduced, in order to complete certain series, negative equivalents, as is alleged, without giving any explanation of such a conception.

In answer, I may observe, that I have not used the expression attributed to me. I have never once in the whole series of papers referred to, spoken of negative equivalents. Such an expression would, I conceive, be an absurdity. Any negative quantity taken in an isolated sense is, as Carnot has proved, an absurdity. But I have pointed out that, by carrying certain decreasing arithmetical series to successive terms, the equivalents of well marked series of chemical bodies were obtained, and that, finally, when we reached a point at which the last term was less than the common difference, the series might yet be continued, and the equivalents of other elements, admitted by all chemists to belong to the same natural group, might be obtained, although with negative signs affixed. For example, if commencing with antimony, we subtract a number corresponding nearly with 45, we obtain in succession the equivalents of arsenic and phosphorus. With phosphorus we reach a term, the numerical value of which is less than the common difference. We cannot, therefore, carry the series forward another term without encountering negative numbers. Nevertheless, there remains another member of the group, nitrogen, intimately bound to it. Is it not, then, a matter of great interest to observe that this last member of the group, apparently cut off from it, is reached, although with a negative sign, by simply carrying the series another step?

There next arises the question, "What signification is to be attached to a quantity presenting itself under these circumstances with a negative sign?" I need not remark that the theory of negative signs has been a stumbling-block in the way of mathematicians, and has given rise to long disputes between men of the most exalted intelligence; such as those which occurred between Leibnitz and Bernouilli, and between Euler and D'Alembert.

But the ideas which prevail at the present day are sufficiently clear to throw a light on this subject, and I cannot do better than quote *verbatim* the following passage, which I translate from Carnot:—

"The true sense which is to be attached to this expression (that of a negative quantity) is, that this absolute quantity does not belong to the system on which the reasonings have been established, but to another which stands with it in a certain relation; such that, in order to render applicable to it the formulas found for this first system, it is necessary to change from + to — the sign which precedes it.

"But from the necessity of placing for example $-y$ in the place of $+y$ it does not follow that the quantity represented by y has become negative; but only that, as has just been proved, it is the difference between two other quantities, a, z, of which that which was the greater in the system on which the reasoning was established and the formulas found, has become the least in the system to which it is desired to apply these formulas. For the quantity represented by y being constantly, by hypothesis, the difference between the two quantities, a, z, will be now $a-z$, now $z-a$, according as z is less or greater than a; but in all cases it will be the greater of these two quantities less the lesser, and, consequently, always positive; and the expression $-y$ will never be anything more than a simple, algebraical expression, without signification in itself, but having the property, that by being substituted in the formulas found, in place of $+y$, it will render them applicable to cases not previously foreseen, or which, at least, were not included in those on which the reasoning was primarily established."[†]

The above is precisely the case in the question of equivalents before us. The above quantity, a, may be taken as the common difference in a series of equivalents, and z any term in that series, y the following term. Now the sign of y depends upon the relative greatness of the numbers a and z; but in the words just quoted, "from the necessity of placing $-y$ in the place of $+y$ it does not follow that the quantity represented by y has become negative."

The criticism to which I am now referring has, probably, been founded upon the very general assumption, that a negative quantity is less than nothing. This assumption is so plausible as to have deceived many eminent mathematicians, and even Newton himself, and Euler. But D'Alembert and Carnot have both pointed out its erroneous nature. D'Alembert states the proportion $1 : -1 :: -1 : 1$, and observes that if -1 be less than nothing, then it must be less than $+1$, and we should have a greater number standing to a less in the same relation as a less to a greater, which would be an absurdity. Therefore -1 is not a less number than $+1$. Carnot considers this reasoning of D'Alembert unanswerable, and has himself proved the same in an equally ingenious manner. If -3 be less than nothing it must be less than $+2$. But $(-3)^2=9$ and $(+2)^2=4$. Therefore the square of the less number would be greater than the square of the greater, which is absurd.[‡]

Clearly, therefore, it cannot be maintained that negative numbers are less than nothing. Taken in an isolated sense they are mere mathematical abstractions,

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[‡] *Pogg. Ann.*, vol. lxxiv., page 115

[*] *American Journal of Science.*

[†] Carnot, "Géometrie de Position." Paris, An. xi. (1803.) Dissert. Prél. xix.

[‡] Another argument advanced by Carnot, and which is equally cogent, is the following, which I quote in the original: "Je dis d'abord que la première de ces notions est absurde, et pour la détruire, il suffit de remarquer qu' étant en droit de négliger dans un calcul les quantités nulles, par comparaison à celles qui ne le sont pas, à plus forte raison devrait-on être en droit de négliger celles qui se trouveraient moindres que 0, c'est à dire les quantités negatives; ce qui est certainement faux: donc les quantités negatives ne sont pas moindres que 0." *Op. Cit.* ix. See also Maseres, "On the Use of the Negative Sign." The position assumed by D'Alembert and Carnot appears to be received by modern mathematicians as correct. See remarks of Mr. Galloway in Brande's Dictionary, Art. "Negative Numbers."

but, considered in connection with the operations by which they were produced, they are full of significance.

Philadelphia.

TECHNICAL CHEMISTRY.

On a New Chrome Green, by M. Mathieu Plessy.

The following is my method of operating:—

In 10 parts of boiling water I dissolve 1 part of bichromate of potash; to this I add 3 litres of biphosphate of lime, then 1·250 kilogrammes of brown sugar.

After a little time a tumultuous disengagement of gas takes place, which must be moderated by sprinkling over the froth.

After calcination the whole is left to stand, and by the following day the green is deposited. The supernatant liquid, of the colour of salts of chromium, decant, and wash the precipitate with cold water until the acid reaction ceases; it is then placed on a cloth *essoré*, and taken to the stove.

The above quantities give 2·500 of product.

This green, containing, as we have shown, no poisonous substance, is unalterable in the sun; sulphuretted hydrogen has no effect on it; acids, even though concentrated, do not destroy it, or, at least, act very slowly as solvents. In fixing it by albumen, and printing with it, there is no inconvenience, except a slight paleness of tint.

By the firm of Betremieux it has been used in printing on a plain paper ground, producing an agreeable water green colour. As a smooth ground it has also been employed as an oil colour at the Louvre, and the tint has remained unaltered since its application a year ago.—*Repertoire de Chimie Pure et Appliquée.*

On a Method of Decomposing Rock Salt, by J. Nicklés.

Sea salt in the state of rock salt, and sulphate of lime in the state of anhydrite, gypsum or plaster stone, are always found in proximity in the mineral kingdom, particularly in variegated marls. Of this we have a striking example in the lower strata of the department of the Meurthe. Sulphuric acid, lime, chlorine, and sodium, when associated, are arranged so as to form on one side sulphate of lime, and on the other chloride of sodium. It will be admitted that it is under these forms that the above substances manifest the greatest degree of stability, and not as sulphate of soda and chloride of calcium.

An inverse arrangement has been in vain attempted. *A priori*, the thing is not feasible when the operation is confined exclusively to these substances. A different result altogether is obtained by calcining the mixture of these two salts, after adding to them a certain amount of peroxide of manganese. In this case sulphate of soda is always produced.

In this operation the theory which guided M. Nicklés is exceedingly simple: he counted on the possibility of displacing the chlorine of chloride of sodium by the oxygen of a peroxide; for instance, peroxide of manganese, to obtain the soda necessary for the production of sulphate of soda. This, in fact, takes place; the displaced chlorine is disengaged, the crucible retains sulphate of soda, lime, and any excess of the manganese and sulphate of lime employed. The maximum of sulphate of soda which M. Nicklés has been able to obtain, under these conditions, is 15 per cent. It would, no doubt, be difficult to exceed this quantity, on account of the volatilisation of the chloride of sodium,—volatilisation which takes place just about the temperature at which the above decomposition is effected.—*Mémoires de l'Académie*, vol. i., p. 460.

PHARMACY, TOXICOLOGY, &c.

A Few Remarks on Extracts, by Julius Schweitzer.

(*Concluded from page* 40.)

The dry and woody parts of plants often require the action of boiling water before they part with their extractive matter, and are, therefore, treated by decoction or infusion. Every article, however, cannot with full advantage be boiled over the naked fire, and where steam is available it may be resorted to with far better success. Sarsaparilla, for instance, should be placed in a deep vessel, and covered with cold water. Steam should then be passed directly into the water, and the steaming continued for an entire day. The liquor should then be withdrawn, and replaced by another quantity of cold water, and the steaming repeated. The liquors should be concentrated at once, and the concentrated fluid, after defecation and straining, evaporated to the proper consistence. As the extractive matter is only contained in the fibrils and outer bark of the larger roots, it is unnecessary to split the roots.

The above process is but a modification of decoction, and answers, in many instances, far better than boiling the ingredients in water over the fire. Some of the extracts which are usually prepared by maceration in cold water, may also be produced by infusion and decoction, as dulcamara, gentian, hæmatoxylum, krameria, pareira, quassia, senna, uva ursi, &c.

Alcoholic and ethereal extracts are usually of a much more powerful or poisonous nature, and, in many respects, more resemble impure alkaloids than ordinary extracts. They are ordered by preference in Continental Pharmacopœias, but are not always useful preparations. Some of them are unreliable, variable in strength, and objectionable in every respect where we possess corresponding good watery extracts and pure and crystalline alkaloids, as, for instance, alcoholic extract of aconite, belladonna, conium, &c. These alcoholic extracts of fresh plants are prepared by evaporating their expressed juice to a syrupy consistence, and diluting and mixing the fluid extract with a large quantity of alcohol. After allowing it to stand and subside, the alcoholic liquid is filtered, the alcohol distilled off, and the residue evaporated to a proper consistence. Such extracts readily absorb moisture, and are almost always in a semi-fluid condition.

A number of alcoholic extracts of dry parts of plants are of far greater necessity and importance. Their active principles are neither soluble in water nor convertible by chemical manipulation into crystalline forms, but consists for the most part of resinous matters, soluble in alcohol or ether, as extract of Indian hemp, colocynth, cubebs, ergot, ginger, jalap, male fern, pepper, scammony, sumbul, valerian, &c. These are prepared by percolation, with the exception, perhaps, of scammony, which, like aloe and liquorice, undergoes, for further purification, maceration in alcohol. The quantity of scammony used is but small, the drug itself very expensive, and so justifies a more abundant use of alcohol to obtain its active resinous matter. It is converted into a tincture, and the alcoholic solution evaporated to dryness on a water bath.

Percolation or displacement, notwithstanding its greater practical difficulty, is a process which gains yearly in general application and favour. It consists in extracting and filtering the extracted matter at the same operation. The ingredients subjected to this process must be uniform, finely divided, carefully packed in the tapering conical percolator, so as to be close enough to prevent the formation of channels, without altogether choking and preventing the flow of the liquid. To obtain these ends with greater certainty, it is, perhaps, best to soak the dry finely or coarsely powdered species first of all in the fluid used for extraction, taking care not to render them more than just damp enough to allow of their being easily handled. When sufficiently soaked and swollen, they may be packed in the percolator more or less firmly, and by small quantities. The tightness of the packing must be regulated according (1) to the quantities operated upon, (2) to their more or less mucilaginous nature, (3) and according to the density of the menstruum which has to percolate. The greater the quantities used the higher will be the column or filtering bed which the fluid has to penetrate, and the greater the obstacle to its easy flow; consequently, the single particles may be slightly coarser and the packing less close.

Mucilaginous and starchy substances, which, in contact with water or weak alcohol, are liable to swell and form a pasty, impermeable magma, as colombo, rhubarb, buchu, &c., offer great difficulties for percolation by watery fluids, and require, like gummy and purely resinous substances for alcohol, the addition of an inert medium for admixture to counteract and overcome this obstructing tendency. Powdered glass, clean sand, chopped straw, or similar insoluble materials, answer this purpose very well. The more limited or confined the solvent power of the menstruum, and the greater its state of fluidity and power of penetration, the finer may be the state of division of the species, and *vice versâ*. It is very difficult, if not impossible, to give one rule which could invariably be applied to every case; nay, it is often necessary slightly to vary the process according to circumstances; but, when bearing in mind the above general directions, it will soon be apparent that with a little discretion, and a few trials, percolation will lead to most satisfactory results. In every instance the aim is to produce a slow, regular, and gradual flow of the fluid through the mass to be extracted, to be sure that each particle of liquid travels downwards, in a perpendicular line, so as touch and penetrate every particle of material just below it, and to give it time to exert its solvent power to its fullest extent, before it reaches the side or bottom of the percolator and escapes. It is, perhaps, better to pack the different materials rather too loose than too tight, and to have a contrivance attached to every displacement apparatus, by means of which the too ready flow of the menstruum may be checked. A single hole or outlet in the bottom of the percolator, to allow the saturated liquor to flow out, gives us full control over the process. This outlet may be fitted with a cork or tap, and withdrawn or opened to regulate the discharge. A layer of tow or cotton wool should be placed at the bottom, and over the top of the powdered species; the first to prevent any solid particles from escaping, the second to place some weights upon it to keep the mass from being lifted up by the liquid. Odd glass stoppers, of which every chemist and druggist has an abundance, answer very well the purpose of weights.

Fluid, or Liquid Extracts.

The preparation of this class of extracts requires far greater skill and nicety than the preparation of solid extracts. The evaporation must be carried on at a temperature lower than the boiling point, because a liquid boiled down to a syrupy consistence is very apt to separate into two layers a much thinner one, which rises to the top, and an insoluble thick sediment.

Watery extracts, it may be inferred, will not keep by themselves in a fluid condition, and require an addition of spirit, sugar, or glycerine. For internal use they should be made as palatable as possible, for which purpose sugar answers well; in some cases, however, as in rhubarb and other substances, rich in tannin, where the astringent properties are neither wanted nor desirable, these may be removed by means of gelatine, or isinglass, which, in rhubarb, for instance, may be done without injury to the laxative properties. Glycerine has, for some years, been used in the preparation of fluid extracts for external application, under the name of glyceroles. It seems to be admirably adapted for such purpose, being a ready solvent for most vegetable extracts, and a number of organic and inorganic salts a good antiseptic; and in the pure state may, with great propriety, be also applied for the preservation of fluid extracts for internal use. Alcohol, besides its own powerful influence on the human frame, sometimes materially alters the nature of watery extracts, by producing precipitates, and giving them frequently a very unpleasant and disagreeable taste. It need hardly be mentioned that any of the previously mentioned solid extracts may also be prepared in the fluid state, but care must be taken to attend to all precautions which have been pointed out. Long and energetic boiling has to be particularly avoided, without, however, prolonging the evaporation so as to produce fermentation or acidity in the fluid.

The following list contains the fluid extracts in general use, with concise directions for their preparation.

Fluid Extracts or Glyceroles.

For external use:—Conium, hyoscyamus, opium, &c. Equal parts of solid extract and glycerine.

For internal use:—Rhubarb, senna, taraxacum. By preparing an extract as directed, evaporating, however, only to a syrupy consistence, and adding 15 per cent. of glycerine.

Fluid extract containing 15 per cent. of alcohol:—Rhubarb: prepared by percolation with cold water, and the syrupy extract mixed with 15 per cent. of alcohol. Senna: by percolation with warm water, the syrupy extract mixed with 15 per cent. of alcohol. Taraxacum: from juice of fresh root, evaporated and mixed with 15 per cent. of alcohol. Sarsaparilla, as indicated by steam, and mixed with 15 per cent. of alcohol.

Fluid extracts prepared by percolation with alcohol or ether:—

Cubebs, male fern, secale cornutem, sumbul, valerian. The powdered articles are percolated either with alcohol, but most with ether, and the ether allowed almost spontaneously to evaporate.

*Notes on the Extraction and Estimation of Some of the Crystalline Principles of Opium,** *by* R. Haines, *M.B., Professor of Materia Medica, Grant College, Bombay.*

In the year 1856, learning that there lay in the medical stores some 300 or 400 lbs. of Khandeish opium, unsaleable on account of adulteration, and for the same

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* From the *Transactions of the Medical and Physical Society of Bombay.*

cause not considered suitable for issue for medical purposes, I got permission to make a preliminary trial upon it, and, having found that it would yield five or six per cent. of muriate of morphia, I made an offer to the medical board to extract all the morphia from it in the form of muriate. The offer was accepted, and I commenced operations, in the first place, upon 200 lbs.

In choosing the process to be adopted, I was obliged to have regard to the nature of the appliances at my disposal. My vessels and furnaces being but small, I preferred to avoid, if possible, great boilings and filtrations. The extraction, therefore, with a sufficient quantity of cold water, and the filtration and expression, were effected at the medical stores, though not, I fear, very completely.† The filtered liquors being collected, solution of ammonia was poured in with vigorous stirring, until a distinct ammoniacal odour was perceptible. About 1 lb. was required for every 10 lbs. of opium. The first effect of the ammonia was to precipitate a vast quantity of resin, which, by stirring, soon balled together into a tough, pitchy mass. This could be at once removed. After some hours the morphia was deposited at the bottom and sides of the vessel in distinct granular crystals of a pale brown colour. The crystals, after being washed, were dissolved in hydrochloric acid, and the hydrochlorate of morphia purified in the usual way. The resinous mass I found, rather to my surprise, to contain, besides the greater part of the narcotine of the liquid, a large quantity of morphia enclosed as crystals within it. In order to utilise this morphia, I first attempted to treat the mass like the first crystals, by solution in hydrochloric acid and crystallisation; but I found that so viscid and pitchy a mass resulted that the crystals would hardly form in it. I was, therefore, driven to endeavour to get rid of the resin before operating upon the enclosed morphia. Treated with twice or thrice its weight of boiling alcohol, the resin softened and dissolved, but, as the temperature fell, the greater part separated. The spirit was poured off, and the resinous mass, while still warm and soft, was folded in strong calico, and subjected to very gradual but ultimately powerful pressure. An exceedingly tough black mass was very slowly forced out, and at last a light brown cake of morphia and narcotine was left in a state fit for solution in acid. Although I had carefully studied the accounts given of the various processes for extracting morphia, I had not been led to expect exactly this behaviour of the aqueous solution when treated with ammonia.

In a subsequent operation, the precipitant used was carbonate of soda. Less resin was thrown down, and the morphia was lighter in colour, but not quite so abundant, and the precipitation did not appear to be so complete. The watery solutions from which the morphia had been precipitated were not thrown away, but were preserved for the operations presently to be noticed.

One reason why I adopted the above process in preference to that of Gregory was, that the results of a preliminary trial on a few pounds of opium of what I believed to be that process, had been unfavourable, for, after the addition of chloride of calcium and evaporation to a syrup, the hydrochlorate of morphia crystallised with extreme slowness and very imperfectly, so that, after standing a month, more than half the salt remained in solution, and it was at last necessary to separate the morphia by diluting the expressed fluid with water and precipitating with ammonia. In this case I followed the process as detailed in the Edinburgh Pharmacopœia of 1841, which, as I afterwards found, omitted one most essential step, without which, in fact, complete success is impossible. In the process as originally devised by Gregory in 1831,‡ the directions are to add the chloride of calcium to the concentrated liquid, and then to pour the whole into a large quantity of cold water, which causes the separation of abundance of resinous flocculi, after straining off which the liquid may be again evaporated to crystallisation. Not only is this step omitted in the Edinburgh Pharmacopœia of 1841, but it is omitted by Gregory himself in his "Handbook of Organic Chemistry." The London College, however, in the Pharmacopœia of 1836, substituting chloride of lead for chloride of calcium, does not neglect this part of the process. It is, in fact, a most essential one, for the quantity of resinoid extractive matter got rid of by this simple means is something enormous, and the liquid now freed from it leaves on evaporation a thin, aqueous syrup, which scarcely interferes with the crystallisation, instead of the tough, viscid, black mass of the former case.

The last portion of the opium, consisting of 71 lbs., was treated in this way by the chloride of calcium process, and with perfect success. The total yield of pure hydrochlorate of morphia from the first 294½ lbs. of opium by the precipitation method was 14 lbs. 5 oz. 6 dr., or 4·87 per cent.; that of the last 71 lbs. 11 oz. of opium, 4 lbs. 1½ oz., equal to 5·71, or nearly 5¾ per cent., after the extraction of the codeia; the increased quantity in the latter case being, perhaps, partly due to the greater care taken in exhausting the opium, which was done by myself in the College laboratory. In this way a total of 18 lbs. 6 oz. 14 dr. of hydrochlorate of morphia of the best quality has been supplied to the stores at the mere cost of a few pounds of solution of ammonia and of hydrochloric acid, and of a hundredweight of coal and charcoal.

I have tried at various times most of the processes that have been recommended for the extraction of morphia, with the exception of that of Merck,§ and I have certainly found no reason to dissent from the preference universally shown by English manufacturers for Gregory's method, taken as a whole, and especially with the proviso above noticed. The substitution, by the London College, of chloride of lead for chloride of calcium has met with disfavour, partly by reason of the expense of the lead salt, and partly from the large quantity of water required to dissolve it. But I am not sure that a little modification would not remove these objections, and ensure a more abundant yield of morphia. I have been astonished to observe what an exceedingly copious dark precipitate is formed in the opium residues by solution of sugar of lead in Anderson's method, and the great clearing and thinning in the subsequent extract which results; a thinning which, in fact, has the effect of allowing the almost immediate and copious crystallisation of one of the constituents, narcein, which, otherwise, may remain for months in solution. I would propose, then, that after the first evaporation of the watery solution, and the separation of resinous matter by dilution with water, the liquid be precipitated with a saturated solution of acetate of lead, added as long as a deposit is formed. The meconic and sulphuric acids are

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† The marc of this portion, and of the succeeding 100 lbs., was unfortunately not preserved, as it might have been, for the extraction of the narcotine.

‡ I cannot find the original paper, and, therefore, I have drawn my account from Geiger und Liebig's "Handbuch der Pharmacie," 1843, Band i., s. 1191—a work which for fulness and accuracy cannot be too much commended.

§ Geiger und Liebig's "Handbuch der Pharmacie," Band i., s. 1188.

completely removed, with a great abundance of resinoid substances, but none of the crystalline bases. The liquid is filtered through cloth. To save trouble in washing out the bulky precipitate, the best method would be, after one or two affusions of water, to fold it in calico and squeeze out the liquid in the press, and thoroughly dry the precipitate; it may then be powdered, and the soluble matter may be readily extracted by digestion in water. The liquid being now brought to the boiling-point, a solution of chloride of calcium, equivalent in quantity to the acetate of lead used, is to be added. On evaporating the filtered liquid to a small bulk, almost the whole of the remaining lead will crystallise out as chloride, after separating which by filtration, the evaporation may be continued until the muriate of morphia crystallises. The small remaining portion of lead will be effectually got rid of in the final purification with animal charcoal. In this way all the advantages of the use of the lead salt are obtained without the expense and trouble of preparing and dissolving the chloride of lead.

In all the forms of Gregory's process a small portion of the morphia of course remains in solution in the black syrupy residue; it must be eventually extracted by diluting with water and precipitating with ammonia. Mohr's process, which consists in pouring the watery solution of opium into boiling milk of lime (in which the morphia, at first precipitated, speedily re-dissolves), filtration, evaporation to a small bulk, and precipitation with muriate of ammonia, is admirably adapted to experiments on a small scale, if sufficient time can be allowed for the thorough washing of the lime residue, and for the evaporations. I have generally found that it afforded a better yield of morphia, and in a purer form, than the other methods; but on a large scale it is not suited, I should think, to the appliances of a general laboratory, from the difficulty in boiling such large quantities at once, and in the filtration of the bulky mass of lime and resin.

The mother-liquors from the first 300 lbs. of opium, after precipitation by ammonia, were operated on with the view of obtaining the narcein and meconin. Not being at the time acquainted with the recent researches of Anderson,‖ I could only consult for this purpose the old papers of Pelletier and Couerbe, their respective discoverers.¶ The process described was simple in the extreme. Pelletier, in obtaining narcein, treated the mother-liquors with baryta water to separate meconic acid, and then with carbonate of ammonia to separate baryta; after the requisite filtrations, the liquid was evaporated to a syrup, when narcein crystallised after a time, and was obtained by pressing out the liquid through linen. Couerbe, to obtain meconin, omitted the treatment with baryta, and merely evaporated to a syrup, after precipitating the morphia and narcotine with ammonia: after fifteen or twenty days meconin and narcein crystallised together. Since Couerbe obtained both principles by the simpler process, and Pelletier only one by the more complex, I of course adopted the former plan. The liquids were evaporated to a syrup, and set by for a month; nothing, however, could be found—neither narcein nor meconin. But after standing for months some ounces of small hard brown crystals had formed at the bottom, which turned out to be narcein. Meconin could not be discovered. Acting on the known solubility of meconin in ether, I attempted to obtain it by shaking up the syrupy liquid with ether, and separating and distilling off the ether. There remained a small quantity of what appeared to me to be a resin, and I threw it away. I have no doubt now that it was chiefly meconin. From repeated losses of this kind one becomes at last extremely cautious in throwing away even worthless-looking dregs and residues; and the eloquent words of Sir John Herschel on the use of the residues, ever and anon recur to the mind with a full feeling of their force and truth.**

In treating the mother-liquors of the last portion of opium, 71 lbs., the method adopted by Anderson was put in practice. The liquid was first diluted with water, and the copious deposit strained off; it was then precipitated with ammonia, which threw down, with much resin, morphia, narcotine, thebaia, and a portion of the papaverine. The filtered liquor was neutralised and mixed with solution of sugar of lead in slight excess; after filtering off the very copious dark precipitate, the excess of lead was removed by sulphuric acid, the sulphate of lead separated, and the acid neutralised by ammonia. The liquid was now evaporated to a thin syrup. In a few days it appeared semi-gelatinous, owing to the presence of a congeries of very fine silky crystals of narcein. These were separated by filtration and pressure, and re-crystallised. The liquid now contained only meconin and papaverine. It was shaken several times with ether, the ether removed and distilled off, and the resinous-looking residue boiled with water, in which it partly dissolved; meconin crystallised on cooling. I could find no papaverine in this substance.

(To be continued.)

PHYSICAL SCIENCE.

*On the Violet Flame of Many Chlorides,** by J. H. Gladstone, *Ph.D., F.R.S.*

In a recent number of Poggendorff's *Annalen*, there is an important paper by Alexander Mitscherlich, in which he shows that the prismatic spectra of the flames of certain compounds of the metals are different from those of the metals themselves. He gives drawings of the spectra obtained from four chlorides and one iodide, and shows the bearing of his observations on solar chemistry and other questions.

Now, his experiments, though very valuable and suggestive, are far from giving a complete story; and they recalled to my recollection some experiments of my own, the record of which had never proceeded further than my note-book, but which evidently were elucidated by, and were capable of elucidating, those of the German physicist. I have repeated and extended these during the last few days, and I send you the results, hoping to lead other experimenters into the same field, and, perhaps, to return to it myself when I have greater leisure.

The fact is, that the majority of the lines represented in Alexander Mitscherlich's diagram of the flame of chloride of copper, and which, by the way, are represented in Professor W. A. Miller's diagram of the same in the *Philosophical Magazine* for August, 1845, are common to a large number of chlorides if they are sufficiently heated. In the whole range of spectrum analysis, there is no fact better known than that common salt gives rise to the yellow double line D; yet it is popu-

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‖ *Trans. Royal Soc. Edin.*, vol. xx., part iii., p. 347. Liebig und Kopp's "Jahresbericht," 1852, s. 537.

¶ *Ann. Chim. Phys.*, tome l., pp. 240 and 337.

** "Discourse on the Study of Natural Philosophy," § 161, *et ante*: "It was a happy thought of Glauber to examine what everybody else threw away."

* From the *Philosophical Magazine.*

larly known that, if salt be thrown on to the red-hot coals of an ordinary fire, it produces violet flames. This is beautifully seen when old ship timber is burnt. These flames, when analysed, are found to consist, not of the yellow light, but of three groups of lines: the first green, and extending to the fixed line b; the second bluish-green and blue, lying on either side of F; and the third violet, stretching from midway between F and G to a little beyond G. When examined by a spectroscope of no great power, these three groups have a close resemblance to one another, each consisting of four lines about equidistant, of which the two middle ones are brighter than the outer ones; but when they are more carefully examined with a narrow slit, the lines of the second and third groups at least appear as bands of a certain width, and are even resolvable into double bands, of which the more refrangible are the narrower and fainter. That they are identical with the lines of chloride of copper was proved, not only by the identity of their appearance, but by angular measurement of the more prominent ones. No more refrangible rays are seen when this light is examined by a spectroscope the lenses and prism of which are made of quartz.

Many other chlorides also give the same light when sufficiently heated. The flame of a spirit lamp is enough to produce it with the chloride of copper; and the chloride of platinum or of gold, if put into such a flame, gives a bright momentary flash of the violet light, which, when analysed by the prism, is resolved into the same bands, those from the platinum salt exactly coinciding with those from the copper salt when the two are seen side by side. Chloride of mercury in the gas flame from a Bunsen's burner gives this light also. A hydrogen flame is capable of producing it from the chlorides of nickel and cobalt; but it requires the heat of red-hot coals to obtain it from chemically pure chloride of sodium, potassium, or barium. Indeed, in the latter case the fire must be very intense, but there is then no mistaking the characteristic violet. Chloride of zinc also exhibited it at this high temperature, and so did chloride of iron, though less distinctly; but a doubt must rest on such observations made with a coal fire, as it is quite conceivable that these chlorides may give up their chlorine to the alkalies or earths of the ash. Chloride of silver was doubtful, and I did not succeed in obtaining the violet colour from either chloride of calcium, lead, or manganese.

That this light should be emitted from chemically pure chloride of barium at a very high temperature has a peculiar interest, since it was this chloride which first attracted Alexander Mitscherlich's notice by green lines not belonging to barium, that make their appearance when it is ignited in association with chloride of ammonium. These green lines do not coincide with those of the violet flame.

The question naturally arises, to what is this violet flame due? Is it the chloride itself in the gaseous form that emits these rays when heated up to a certain temperature, which differs according to the metal with which the chlorine is combined? Or is the peculiar flame produced by chlorine when the chloride is decomposed by heat? Or does it depend on the combination of the chlorine with the carbon or hydrogen of the combustible? The latter supposition is negatived by the fact that anhydrous chloride of copper emits these rays equally, whether it be placed in a flame of hydrogen or of pure bisulphide of carbon. It is difficult to accept the second supposition, for, though chloride of copper or gold is certainly decomposed in the spirit lamp flame, chloride of nickel or iron is so likewise, and chloride of mercury is reduced to the subchloride, and yet these last do not exhibit the coloured flame at that temperature. Besides, a stream of chlorine or of hydrochloric acid passed into a flame never gives the violet light; nor does Dutch liquid, muriatic ether, or chloroform mixed with alcohol and burnt in a spirit lamp. The rays in question also appear to bear no relation to those emitted in Plücker's experiments. The origin of this violet light evidently, therefore, requires further elucidation.

NOTICES OF BOOKS.

A Dictionary of Calico-Printing and Dyeing; containing a Brief Account of all the Substances and Processes in Use in the Arts of Printing and Dyeing Textile Fabrics, with Practical Receipts and Scientific Information. By Charles O'Neill, F.C.S., &c., &c. London: Simpkin, Marshall, and Co. 1862.

Any one casting about for an example of the "connexion between science and art" could hardly find a better illustration than is afforded in calico-printing. Here the artist and the scientific man go hand-in-hand, the one comparatively helpless without the other. The artist may design, and the engraver may cut his patterns, but without the assistance of the chemist (who, however, is not always aware that he is a chemist) he can only loosely apply the roughest and simplest colours. The chemist, without the aid of the artist, can dye a fabric of one uniform and beautiful shade; but when the two combine, the fancies of the artist, with their sparkling contrasts or graceful blendings of colour, are laid down and permanently fixed by the science of the chemist, and we see produced those things of beauty which, to the wearers, at all events, are sometimes joys—as long as they last.

We have already noticed the first part of this work (Chemical News, vol. vi., p. 68); and also two years ago Mr. O'Neill's "Chemistry of Calico-printing," of which this Dictionary is a reconstruction. This latter work was, as it deserved to be, very successful, and we hope the author will some day extend and republish it, or complete the encyclopædia on which he is engaged. The present book is of a more practical character, and designed for the use of those who have not the advantage of considerable scientific knowledge.

There are many "practical men," we know, who will be found to dispute the advantage of scientific knowledge in print works. It is quite true that some happy accidents have occurred, and some fortunate guesses have been made in dye and print works; but although accidents and guesses sometimes advance our knowledge a long distance in a single bound, yet progress in that way is always slow compared with the results obtained by patient research and intelligent experiment. Certainly, of late years, the greatest advances in the art, and the largest fortunes in the trade, have been made by those who have either possessed or employed the highest scientific knowledge.

We can safely recommend this book to the working man who is anxious to get practical information on the matters of which it treats, and also to the chemist, who is often in want of a book of reference on such matters. The latter will find in it many suggestions which may direct him to a profitable series of experiments. For instance, speaking of "pigment colours," Mr. O'Neill says, that "the application of pigment colours in a perfect manner to calico printing is one of the most important objects which can be aimed at by an inventor. The methods at present are so defective, the vehicles are so expensive, and even so uncertain in the degree of fastness they communicate to the colours, that nearly everything remains to be done in this direction. It is, perhaps, too much to expect that any powder or substance applied merely upon the fibre should have the same degree of fastness as colouring matters which appear to be seated in the very interior of the fibre; and it is to be feared that any species of protecting varnish would have an elasticity less than that of the fibre, and would consequently crack by the ordinary wear of the material. But the really surprising manner in which albumen fastens a harsh, gritty powder like ultramarine, gives encouragement to hope that even more suitable vehicles will be procured. The advantages which pigment colours have in bloom and freshness, and the opportunities they present to an extended scope of design, are so considerable, that I have no doubt they will before long receive the attention they deserve."

Another want is a thickening agent for liquid colours. The agents of this kind in use at present are either gums, which are rather expensive, or substances such as starch and dextrine ("calcined farina"), which are obtained from materials used for food. Now, there are persons who think that anything which can be used for food, and is converted to any other purpose, is so much taken out of people's mouths. There are more of this way of thinking amongst the intelligent classes in France than amongst the corresponding classes in this country, and "the Industrial Soctety of Mulhouse have offered a prize to any one introducing into the market thickening matters capable of replacing those now in use, and not made from articles used as human food."

We quote these passages to show the scientific reader the hints he may get, besides the practical information he may be in search of. Our quotations must be limited to one more, a ready and practical process for the analysis of soap, to which a correspondent not long since drew attention:—

"In all genuine samples of soap there will be nothing but water, fatty matters, and alkali. The water can be determined by taking one hundred grains of the soap and heating it in a porcelain dish, with a gentle heat, until all the water is dissipated; the heat may be pushed as high as 260° F., or until the dry soap begins to exhale the odour of fatty matter; this will take place in about fifteen minutes, and give more exact results than a water or oil-bath, which would require as many hours. For the determination of the alkali, another hundred grains may be dissolved in about three ounces of water, and an excess of dilute sulphuric acid of known strength added to it. There should be about twice as much acid as would be necessary to wholly decompose the soap. The solution containing the fatty matter should be kept hot until all milkiness has disappeared, and the oil floats clearly on the top; then a weighed quantity of pure bees'-wax (about fifty grains) is added to the solution, and all kept hot until the wax is completely incorporated with the fatty matter; the whole is then allowed to cool. Upon cooling, the wax solidifies along with the fatty matter, forming a well-cohering cake, which may be removed from the liquid, washed with a little water, and the washings added to the original solution, then carefully dried and weighed. The excess of weight above that of the bees'-wax employed is the quantity of fatty matter present. A graduated alkaline solution is then added to the first liquid until it is neutral. It is found how much of the acid first used was neutralised, and the amount of alkali calculated from that."

The above is a rough-and-ready process, which will be found useful, but which will not often tell us all we want to know.

NOTICES OF PATENTS.

2783. *Soap.* H. Orth, Wissenborg, France. Dated November 5, 1861.

The inventor claims a mode of preparing a crude kind of soap by boiling together twenty parts of common resin with an equal weight of alkali (either caustic potash, soda, or ammonia, or one of the carbonates of these bases), the latter being dissolved in fifty parts by weight of water, and the ebullition continued until perfect combination is effected, when 100 parts of finely levigated clay (kaolin or other aluminous earth) are to be added, and the whole intimately incorporated to form a homogeneous paste, which is afterwards dried in moulds or otherwise to give the cakes of soap a suitable form.

The ingredients mentioned in the specification are well known as being precisely those which are commonly employed in the adulteration of ordinary yellow soap. After the discussion which has recently taken place in our columns,* it is not, perhaps, wise to insist upon the admixture of resin being in all cases considered an adulteration; but the introduction of China clay, now so commonly practised in the manufacture of cheap washing materials, can only be regarded as a "make-weight," or serve at best a mechanical purpose. The article described above is soap only in name, and would be suited only for the roughest applications.

2784. *Electro-plating or Depositing Metals.* G. T. Bousfield, Loughborough Park, Brixton. A communication. Dated November 5, 1861.

In the process of depositing silver, copper, and other metals upon iron by galvanic agency, the inventor claims the employment of highly concentrated cyanide solutions of the coating metal, and the use of a battery current of great intensity, whereby the electro-deposit is formed more quickly and regularly than by the ordinary process. The purest description of fused cyanide of potassium is recommended for the preparation of the electro-plating solution.

2787. *Furnaces for Reducing Zinc Ores.* A. Prince, Trafalgar Square, London. A communication. Dated November 6, 1861.

This invention refers to an improved construction of furnaces for the distillation of zinc, whereby twice the usual number of muffles or retorts are heated by fires placed between an upper and lower series. A great saving of fuel is said to be effected in this way, since a charge of 100 lbs. of roasted zinc ore requires no more than 150 lbs. of coal for its perfect reduction.

Grants of Provisional Protection for Six Months.

26. Stephen White, Suffolk Grove, Southwark, Surrey, "Improvements in the method of, and apparatus for, purifying, bleaching, and refining oils and oily and fatty matters."

42. Charles Tiot Judkins, Ludgate Street, London, "New alloys."—A communication from Moses Gerrish Farmer, Salem, Massachusetts, U.S.

48. Edward Vincent Gardner, Berners Street, London, "Improvements in the treatment of petroleum and mineral oils, and in apparatus employed therein."

Notices to Proceed.

2542. William Clark, Chancery Lane, London, "Improvements in the treatment of peat and peat tar for the production or manufacture of various products, and in apparatus for the same."—A communication from Leopold Eugene Lavigne, Boulevart St. Martin, Paris.—Petition recorded September 16, 1862.

3427. George Haseltine, Southampton Buildings, Chancery Lane, London, "Improvements in the mode of, and apparatus for, converting petroleum or coal oil into gas for lighting and heating, the said improvements being especially applicable to lamps and stoves."—A communication from James Rhodes, Cincinnati, Ohio, U.S.—Petition recorded December 23, 1862.

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* Chemical News, vol. vii. p. 12.

2483. Theodor Fleitmann, Iserlohn, Prussia, "Improvements in the manufacture of copper from copper ores."—Petitions recorded September 9, 1862.

2496. Thomas Steel, Bradford, Yorkshire, "Improvements in treating soap-suds or other saponaceous or oily matters."

2508. Peter Ward, Clouds Hill Villas, St. George's, Bristol, "Improvements in the manufacture of a double sulphide of calcium and sodium."—Petitions recorded September 11, 1862.

2556. Ludwig Mond, Sydney Street, Brompton, Middlesex, "An improved method of obtaining hyponitric acid and nitric acid from nitrate of soda."

2604. Richard Archibald Brooman, Fleet Street, London, "An improved composition for painting."—A communication from Paul Caubet, Paris.—Petition recorded September 24, 1862.

2645. Henry Ellis, Bangor, North Wales, "Improvements in the manufacture of compounds of silica, and in the application of certain compounds of silica to mineralise woven fabrics, paper, and paper pulp, to harden and preserve stone and cement, in the production of artificial stone and paint, and in the production and glazing of porcelain and such like manufactures."—Petitions recorded September 29, 1862.

CORRESPONDENCE.

Boiler Incrustation.

To the Editor of the Chemical News.

Sir,—I feel no jealousy from your correspondent, Mr. Napier, setting forth his prior suggestion as to a mode of preventing the great evil of incrustation in steam boilers. Alkalies have been used for this purpose for at least a quarter of a century; in fact, most of the nostrums, patented or otherwise puffed, have soda or soda-ash for their basis. The small merit that belongs to my suggestion arises from its simplicity, and the minimum of attention it requires from the workmen—a single act daily, and that done in two minutes—insures perfect immunity from incrustation. I have now had a satisfactory experience of more than twelve months; and my only object in thus referring to your correspondent's letter is in order, by the further publicity thus given to the matter, to induce some of your numerous readers who may be subject to the annoyance to give my plan a trial, and they will at once cure the evil.

I am, &c. Peter Spence.

Pendleton Alum Works, Newton Heath, Manchester.

Preservation of Stone.

To the Editor of the Chemical News.

Sir,—If none but professional chemists peruse the pages of your Journal we should not have troubled you further in the matter of preservation of stone; but knowing full well that your columns stand high in estimation as affording valuable data for the non-professional world, we would beg the privilege of a reply to the very specious explanation of Messrs. Rust on the matter of the silicate of alumina.

Messrs. Rust acknowledge the abandonment of their provisional patent for the adoption of our idea of using in one solution the agent and re-agent, so that the grave objections to the use of two solutions were thus seen, and the discovery appropriated by them forthwith, with this unimportant variation in its application, that they profess to keep their materials in stock enclosed in one jar, until required for use, instead of two; and, further, that being doubtful of finding lime or magnesia enough in a lime or magnesian limestone, they add to their solution this very scarce and essential agent, after an assertion that their solution has no free potash, and implying that ours has a detrimental quantity. They inform the readers of a technical work on an abstruse science that they "need go no further, for sufficient has probably been said to satisfy an unprejudiced mind," &c., &c.

If all conclusions were thus to be arrived at, and rights thus to be set at nought, it would certainly create an unwelcome revolution in that science whose existence almost depends on the laborious and persevering researches of truthful and right-minded men, whose theories and deductions are sought to be balanced by the infinitesimal portion of a grain. I am, &c.

Sanders Trotman,
Pro Bartlett Bros. and Co.

Devonshire Wharf, Camden-town, N.W.

Electrical Decomposition.

To the Editor of the Chemical News.

Sir,—I will, with your kind permission, ask a question or two, on which I am anxious for exact information. In books on the subject, 33 parts of zinc, dissolved *in* a battery, are represented as precipitating 31 parts of copper, 108 parts of silver, &c., *out* of the battery.

1. Now, does this law hold good in all circumstances, or for composite solutions in which a minute quantity of some good conductor is diffused, and combined with other substances less conducting? 2. Or, for simple, but very weak, solutions of salts of a good conductor, as chlorides of platinum or silver?

3. In these cases, would not a weak battery be best, so that the current may be sufficient only to carry off the trace or traces of the best conductor first?

4. But would not several cells or pairs of elements be necessary in order to overcome resistance, or push the one-cell electricity through the solution, and thus a consumption of zinc be occasioned merely to generate intensity or pushing force? I am, &c. C. R.

The Cavendish Society.

To the Editor of the Chemical News.

Sir,—As the time for the annual meeting of the Cavendish Society is drawing near, I wish to throw out a suggestion to the subscribers and council of that society. It seems to be useless to wait for the completion of Gmelin's Handbook before the society is reconstructed. That book may perhaps be finished some day, but, instead of in two years, as the council expected, we shall, at the present rate of publication, have to wait at least six years. During all this time, the present society will stand in the way of a more useful and energetic association.

I propose, then, that a new society be formed immediately, the present to hand over the balance of funds, which are to be entirely devoted to the completion of Gmelin; that those who have paid their subscriptions to the present time receive the books due, but after these have been issued, the remaining volumes and the index be issued by the new society to those who wish them, at cost price, allowing for balance of funds handed over by the old society.

I do not think there would be much difficulty in starting a new society, as, no doubt, all who subscribe to the old would continue their subscription. All that is needed is a little more activity and enterprise in the management, and some care in the selection of books. There will never again be occasion to enter on such an engagement as the publication of a book on the same scale as Gmelin—for which, after all, the subscribers must be grateful; and, unsaddled by such a weight, there is no reason why a regenerated Cavendish Society should not be active and prosperous. I am, &c.

A Subscriber to the Cavendish Society.

On Relations among the Equivalents.

To the Editor of the Chemical News.

Sir,—Many chemists, and M. Dumas in particular, have, on several occasions, pointed out some very interesting relations between the equivalents of bodies belonging to the same natural family or group; and my present purpose is simply to endeavour to proceed a little further in the same direction. I must, however, premise that many of the observations here collected together are well known already, and are only embodied in my communication for the purpose of rendering it more complete.

Before proceeding any further, I may also remark, that in the difficult task of grouping the elementary bodies, I have been guided more by chemical characteristics than by physical appearances, and have, therefore, taken no notice of the ordinary distinction between metals and non-metallics. The numbers which I have attached to the various groups are merely for the purpose of reference, and have no further significance whatever. For the sake of perspicuity, I have employed the old equivalent numbers, these atomic weights being, with one or two exceptions, taken from the 8th edition of "Fownes' Manual."

The following are among the most striking relations observed on comparing the equivalents of analogous elements. (In order to avoid the frequent repetition of the word "equivalent," I have generally used the names of the different elements as representing their equivalent numbers—thus, when I say that zinc is the mean of magnesium and cadmium, I intend to imply that the equivalent of zinc is the mean of those of magnesium and cadmium, and so on, throughout the paper):—

Group I. Metals of the alkalies:—Lithium, 7; sodium, 23; potassium, 39; rubidium, 85; cæsium, 123; thallium, 204.

The relation among the equivalents of this group (see Chemical News, January 10, 1863) may, perhaps, be most simply stated as follows:—

1 of lithium + 1 of potassium = 2 of sodium.
1 ,, + 2 ,, = 1 of rubidium.
1 ,, + 3 ,, = 1 of cæsium.
1 ,, + 4 ,, = 163, the equivalent of a metal not yet discovered.
1 ,, + 5 ,, = 1 of thallium.

Group II. Metals of the alkaline earths:—Magnesium, 12; calcium, 20; strontium, 43·8; barium, 68·5.

In this group, strontium is the mean of calcium and barium.

Group III. Metals of the earths:—Beryllium, 6·9; aluminium, 13·7; zirconium, 33·6; cerium, 47; lanthanium, 47; didymium, 48; thorium, 59·6.

Aluminium equals two of beryllium, or one-third of the sum of beryllium and zirconium. (Aluminium also is one-half of manganese, which, with iron and chromium, forms sesquioxides, isomorphous, with alumina.)

1 of zirconium + 1 of aluminium = 1 of cerium.
1 ,, + 2 ,, = 1 of thorium.

Lanthanium and didymium are identical with cerium, or nearly so.

Group IV. Metals whose protoxides are isomorphous with magnesia:—Magnesium, 12; chromium, 26·7; manganese, 27·6; iron, 28; cobalt, 29·5; nickel, 29·5; copper, 31·7; zinc, 32·6; cadmium, 56.

Between magnesium and cadmium, the extremities of this group, zinc is the mean. Cobalt and nickel are identical. Between cobalt and zinc, copper is the mean. Iron is one-half of cadmium. Between iron and chromium, manganese is the mean.

Group V.—Fluorine, 19; chlorine, 35·5; bromine, 80; iodine, 127.

In this group bromine is the mean between chlorine and iodine.

Group VI.—Oxygen, 8; sulphur, 16; selenium, 39·5; tellurium, 64·2.

In this group selenium is the mean between sulphur and tellurium.

Group VII.—Nitrogen, 14; phosphorus, 31; arsenic, 75; osmium, 99·6; antimony, 120·3; bismuth, 213.

In this group arsenic is the mean between phosphorus and antimony.

Osmium approaches the mean of arsenic and antimony, and is also almost exactly half the difference between nitrogen and bismuth, the two extremities of this group; thus, $\frac{213-14}{2}=99\cdot5$.

Bismuth equals 1 of antimony + 3 of phosphorus; thus, $120\cdot3+93=213\cdot3$.

Group VIII.—Carbon, 6; silicon, 14·20; titanium, 25; tin, 58.

In this group the difference between tin and titanium is nearly three times as great as that between titanium and silicon.

Group IX.—Molybdenum, 46; vanadium, 68·6; tungsten, 92; tantalium, 184.

In this group vanadium is the mean between molybdenum and tungsten.

Tungsten equals 2 of molybdenum, and tantalium equals 4 of molybdenum.

Group X.—Rhodium, 52·2; ruthenium, 52·2; palladium, 53·3; platinum, 98·7; iridium, 99.

In this group the first three are identical, or nearly so, and are rather more than half of the other two. (I may mention, by the way, that platinum is rather more than the half of gold; thus, 98·7 × 2 = 197·4, gold being 197.)

Group XI.—Mercury, 100; lead, 103·7; silver, 108.

Lead is here the mean of the other two.

If we deduct the member of a group having the lowest equivalent from that immediately above it, we frequently observe that the numbers thus obtained bear a simple relation to each other, as in the following examples:—

Member of group having lowest equivalent.		One immediately above the preceding.		Difference.
Magnesium	12	Calcium	20	8
Oxygen	8	Sulphur	16	8
Carbon	6	Silicon	14·2	8·2
Lithium	7	Sodium	23	16
Fluorine	19	Chlorine	35·5	16·5
Nitrogen	14	Phosphorus	31	17

A similar relation, though not quite so obvious as the above, may be shown by deducting the lowest member of a triad from the highest. The numbers thus obtained in the different triads correspond to a great extent. (By a triad I understand a group of three analogous elements, the equivalent of one of which is the mean of the other two.) Of this relation I append a few examples:—

Lowest term of triad.		Highest term of triad.		Difference.
Lithium	7	Potassium	39	32
Magnesium	12	Cadmium	56	44
Molybdenum	46	Tungsten	92	46
Sulphur	16	Tellurium	64·2	48·2
Calcium	20	Barium	68·5	48·5
Phosphorus	31	Antimony	120·3	89·3
Chlorine	35·5	Iodine	127	91·5

In the relation previously pointed out, the difference between the lowest member of a group, and the next above it, was either 8, or 8 × 2 = 16; and in the first of these triads the difference is 8 × 4 = 32; in the next four it approaches 8 + 6 = 48; and in the two last triads it is nearly twice as great.

The difference between the highest member of the platinum group, viz., iridium 99, and the lowest, rhodium 52·2, is 46·8, a number which approximates very closely to those obtained in some of the above triads; and it, therefore, appears possible that the platinum metals are the extremities of a triad, the central term or mean of which is at present unknown.

I am, &c.
J. A. R. N.

P.S. With the view of economising space I have omitted most of the calculations, which, however, are very simple, and can be verified in a moment by the reader. The equivalents thus obtained by calculation will be found to approximate to those procured by experiment, as closely as can be expected in such cases.

I also freely admit that some of the relations above pointed out are more apparent than real; others, I trust, will prove of a more durable and satisfactory description.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Action of Chloride of Phosphorus on some Metallic Sulphides.—M. Baudrimont sums up the results of his experiments as follows (*Comptes-Rendus*, t. lv., p. 378):—1. Protochloride of phosphorus easily attacks metallic sulphides at a red-heat. With those of barium, calcium, &c., it produces a splendid incandescence, and gives immediately the phosphorous sulphide PS_3, and the corresponding metallic chlorides. With the sulphides of antimony, lead, mercury, &c., it furnishes at first sulphophosphides by the union of the sulphide of phosphorus with the metals; these are afterwards destroyed by an excess of PCl_3, and are completely changed into PS_3. 2. Perchloride of phosphorus reacts twice on sulphide of hydrogen, producing at first chloro-sulphide of phosphorus, and afterwards, at a higher temperature, phosphoric sulphide, PCl_5. 3. It behaves in the same way towards earthy and alkaline sulphides. 4. With the sulphides of antimony, tin, lead, mercury, &c., it produces the same results in the end, but forms intermediary sulphophosphides corresponding to PS_3 rather than PS_5. 5. The action of perchloride of phosphorus on sulphide of antimony is the basis of an easy method for the preparation of chloro-sulphide of phosphorus—(see Chemical News, vol. v., p. 41). 6. Chloro-sulphide of phosphorus attacks metallic sulphides exactly as it does PCl_5. 7. The sulphophosphide of mercury obtained either by the reaction PCl_3 or PCl_5 on cinnabar, has for its formula $PS_3,3(HgS)$, and appears to be the type of a group of sulpho-salts different from those described by Berzelius.

Compounds of Perchloride of Phosphorus with other Chlorides—M. Baudrimont has also devoted a long series of experiments to the study of these compounds (*Comptes-Rendus*, t. lv., p. 361). He finds that PCl_5 combines with the chlorides of selenium ($SeCl_2$), of iodine (ICl), of aluminium (Al_2Cl_3), of iron (Fe_2Cl_3), of tin ($SnCl_2$), of mercury ($HgCl$), and of platinum ($PtCl_2$). All these compounds may be obtained either by acting on the simple bodies with PCl_5, or by combining the latter directly with the chlorides. The excess of PCl_5 can always be driven off by careful and prolonged heating to 160° or 180° C. At a higher temperature the double chloride sublimes. All these compounds are solid, volatile, and in some cases partly decomposed by heat. They fume in the air, are changed by moisture, and decomposed by water. The author describes the compounds formed with the chlorides named above, noticing particularly the platinum compound as being the only volatile salt of that metal known, and the iodine compound as being unequalled in causticity.

II. ORGANIC CHEMISTRY.

An Isomer of Amylic Alcohol.—Würtz (*Comptes-Rendus*, t. lv., p. 370) has compared the compound formed by the direct combination of hydriodic acid with amylene, and the iodide of amyl formed with amylic alcohol, and has been led to regard the two compounds, not as identical, but as isomeric. The bodies act very differently on moist oxide of silver, iodide of amyl having no action at the common temperature, while hydriodate of amylene acts energetically even at 0°, the principal product being an organic hydrate, which Würtz regards as an isomer of amyl alcohol. With iodide of amyl and acetate of silver the author obtained an acetate of amyl, with its characteristic odour; but with the hydriodate of amylene a body having very nearly the composition of acetate of amyl, but possessed of a very different odour. The author points out the relations which exist between the compounds of amyl and of amylene; and expresses his belief that hydrates, homologous with the hydrate of amylene, and isomeric with ordinary alcohols, may be obtained in a similar way from hydrocarbons near to amylene, such as caproylene, œnanthylene, and caprylene.

Transformation of Urea into Sulpho-cyanide of Ammonium.—M. Fleury effected this change (*Comptes-Rendus*, t. lv., p. 519) by heating urea with an excess of bisulphide of carbon according to the equation $C_2H_4N_2O_2 + CS_2 = CO_2 + NH_4.C_2NS_2$. He has not perfectly succeeded in changing sulpho-cyanide of ammonium into urea.

MISCELLANEOUS.

Simpson and Others *v.* Wilson and Another.—This cause is set down for trial on Monday, the 9th. If any new feature be introduced in the course of the trial we shall report it.

Royal Institution.—On Monday, February 2, a General Monthly Meeting was held, William Pole, Esq., M.A., F.R.S., Treasurer and Vice-President, in the Chair. The Earl of Clanwilliam, Edward W. Cox, Esq., Sir William Augustus Fraser, Bart., General Charles H. Hamilton, C.B., and Peter Vanderbyl, Esq., were elected Members of the Royal Institution. The Secretary reported that the executors of the late James Walker, Esq., F.R.S., M.R.I., had bequeathed to the Institution a marble bust of Professor Faraday, by Mr. Matthew Noble, M.R.I. The thanks of the members were returned to Professor Tyndall, and to his Eminence Cardinal Wiseman, for their discourses on the evening meetings on Fridays, January 23 and 30. The presents received since the last meeting were laid on the table, and the thanks of the members returned for the same. The following lectures will be delivered:—Tuesday, February 10, at 3 o'clock, Professor Marshall, "On Animal Mechanics." Thursday, Feburary 12, at 3 o'clock, Dr. E. Frankland, "On Chemical Affinity." Friday, February 13, at 8 o'clock, Dr. E. Frankland, "On Artificial Illumination." Saturday, February 14, at 3 o'clock, W. S. Savory, Esq., "On Life and Death."

Carbonic Acid as an Anæsthetic.—**M. Ozanam** has given a mixture of three parts carbonic acid with one part of atmospheric air with success as an anæsthetic. After breathing it for ten minutes the patient became insensible, and an operation was performed without his evincing any sign of pain.

ANSWERS TO CORRESPONDENTS.

⁂ All *Editorial Communications* are to be addressed to the Editor; and *Advertisements* and *Business Communications* to the Publisher, at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

Vol. VI. of the Chemical News, containing a copious Index, is now ready, price 10s. 8d., by post, 11s. 2d., handsomely bound in cloth, gold-lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. if sent to our Office, or, if accompanied by a cloth case, for 6d. Vols. I. and II. are out of print. All the others are kept in stock. Vol. VII. commenced on January 3, 1863, and will be complete in 26 numbers.

R. H.—We regret the change as much as our correspondent, but circumstances compelled the alteration.

Juvenis.—Noad's lectures, or the volume of Lardner's Handbook of Natural Philosophy containing "Electricity."

Dialyser.—We do not know where the substance can be procured. It is obtained from a sea-weed.

J. A. M. and Co.—Apply to Mr. Wilson, of Price's Patent Candle Company.

A Subscriber to the Cavendish Society should address a letter to the council or secretary of the society.

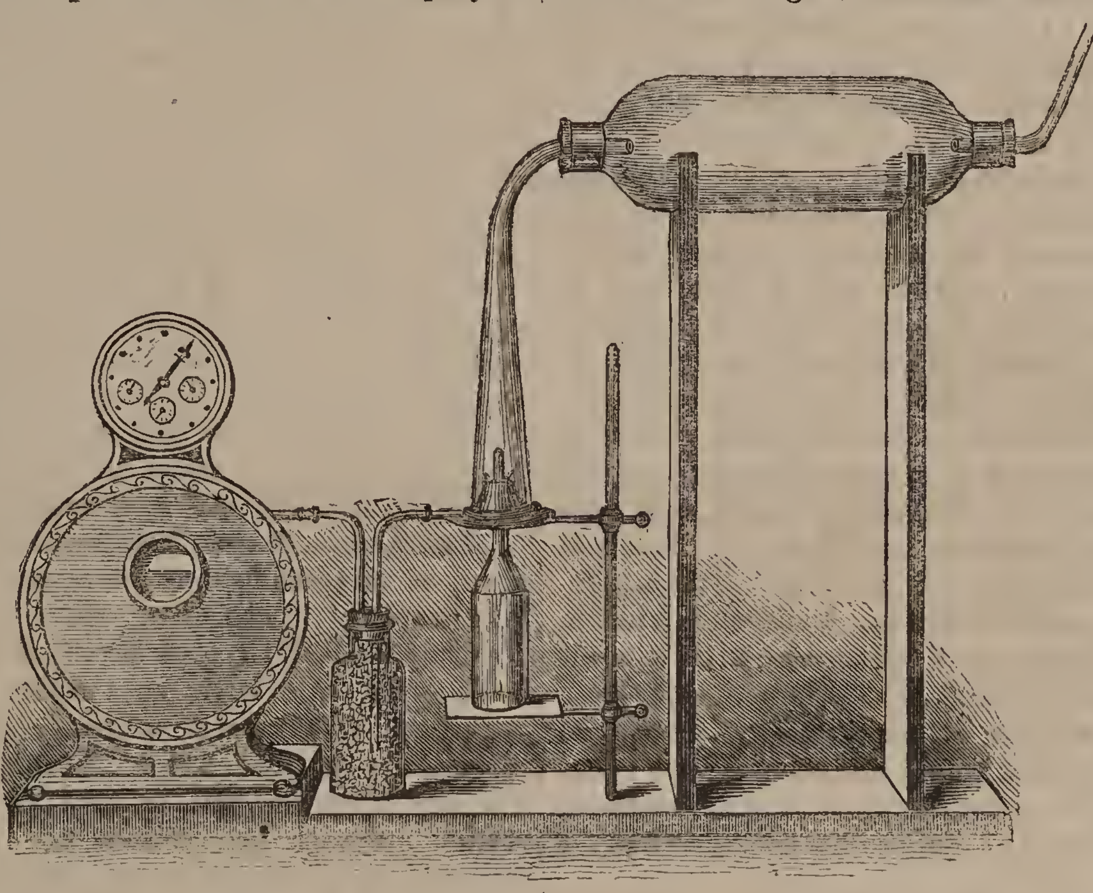
THE CHEMICAL NEWS.

Vol. VII. No. 167.—February 14, 1863.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Dr. Letheby's Apparatus for the Estimation of Sulphur in Coal Gas.

In a former Number of the Chemical News (vol. iv., p. 39) we gave incidentally a description of the method employed by Dr. Letheby for estimating sulphur in coal gas, by burning the gas in an atmosphere of ammonia. We are now, by the kindness of Mr. Sugg, enabled to give a cut, representing the apparatus in use, in illustration of which we again briefly describe the process. After passing the meter, the gas goes through a bottle filled with pebbles moistened with dilute sulphuric acid, for the purpose of estimating any ammonia the gas may contain. The inlet pipe to this bottle is allowed to pass only one inch through the cork; the outlet pipe passes to the bottom of the bottle. The gas is thus forced through the pebbles from the top, which prevents any accumulation of salt about the inlet pipe. In place of this bottle, a tube, about six inches long, an inch and a-half in diameter, and drawn out at each end, may be used. It is filled, of course, with pebbles, moistened with sulphuric acid like the bottle, and may be placed horizontally. The gas thus purified from ammonia is consumed at the rate of about half a foot per hour in a Leslie's burner, which is placed under the long funnel-shaped tube, as represented in the engraving. This tube is connected to one end of a large glass condenser, the other end of which is furnished with a piece of glass tube, about four feet long, joined in such a manner that any products of combustion condensed in it will flow back into the large receiver. In our former notice of this process we described two cylindrical receivers as being used, but experience has shown that when a large one is employed a second is unnecessary.



The means by which ammonia is supplied to the burner are the following:—A wide-mouthed bottle, filled with the strongest liquor ammonia, is placed immediately beneath the burner, and a funnel, with a short tin tube attached to it, is placed over the bottle in an inverted position. This short tube passes through the centre of the burner, so that the end of it is about two inches above the top of the flame. The draught produced through the whole apparatus, when the burner is alight, is sufficient to evaporate the ammonia in the bottle, which is discharged through the tube into the centre of the receiving tube over the burner, and about two inches above the flame. Immediate combination then ensues between the ammonia and the sulphur products of the consumed gas, and the resulting sulphite of ammonia is condensed, along with the water formed, in the large receiver. The sulphur may then be estimated as sulphate of baryta in the usual way, taking care to acidulate well with nitric acid.

Researches on the Platinum Metals,[*]

by Wolcott Gibbs, *M.D.*

(Continued from page 63.)

Rhodium.—When nitrite of potash is added to a solution containing the sesquichloride of rhodium, no change is at first produced, but, on heating, the solution becomes yellow, and on boiling or evaporation to dryness, part of the rhodium is precipitated in the form of a bright yellow or orange-yellow crystalline powder, which is extremely insoluble in hot or cold water, but which dissolves readily in hot chlorhydric acid. Another portion of the rhodium usually remains in the form of a yellow salt soluble in water, but insoluble in alcohol. The solutions of these two salts are decomposed with great difficulty by boiling with strong acids: alkaline sulphides give a dark brown precipitate of sulphide of rhodium soluble in a large excess of the precipitant, and completely precipitated from the solutions by the addition

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[*] From the *American Journal of Science*, vol. xxxiv., page 341.

of an excess of chlorhydric acid. Nitrite of soda also gives a soluble and an insoluble salt with solutions of rhodium, but only the soluble salt is formed when the rhodium solution is boiled for a short time with an excess of the alkaline nitrite.

The application of these facts to the separation of the several metals of the group is as follows:—

Platinum from Iridium.—The separation of platinum from iridium, for the purpose of obtaining the two metals in a state of chemical purity, may be effected by either of the following processes:—The iridium is, in the first place, to be brought into the form of bichloride by means of a current of chlorine or by nitric acid, and the two metals are then to be precipitated together as $PtCl_2,KCl$ and $IrCl_2,KCl$, by the addition of a concentrated solution of chloride of potassium. The colour of the mixed salts varies from orange to almost black, according to the quantity of iridium present. The mass of crystals is to be rubbed fine in an unglazed porcelain mortar, and boiling water added in the proportion of three volumes of water to one of salt. A dilute solution of nitrite of potash is then to be added, until the liquid becomes deep olive-green, carbonate of potash being thrown in from time to time in quantity sufficient to prevent the solution from becoming strongly acid. The iridium is instantly reduced to sesquichloride, while the platinum salt remains as a reddish orange powder. The deep olive-green solution is to be poured off, and the undissolved mass treated a second time with hot water and nitrite. This process must be repeated as long as the liquid remains olive-green. The mixed solutions on cooling, or after evaporation, deposit a beautiful mass of crystals of the double chloride of potassium and iridium, $Ir_2Cl_3,3KCl+6HO$. By re-solution and repeated crystallisation, the iridium salt may be obtained perfectly free from platinum. Instead of nitrite of potash, nitrite of soda may be employed in the above-mentioned process; the iridium and sodium salt has the formula $Ir_2Cl_3,3NaCl+24HO$, and crystallises well. The undissolved mass and the mother-liquors from the iridium salt contain a large quantity of platinum, with a comparatively small quantity of iridium. When the absolute quantity of platinum salt is not very large, it may be dissolved in boiling water, a small quantity of an alkaline nitrite added, and the solution allowed to crystallise; the resulting chlorplatinate of potassium contains only a trace of iridium.

The process just mentioned gives satisfactory results when carefully executed, but requires attention to two points. In the first place, the alkaline nitrite must be added in quantity just sufficient to reduce the iridium from bichloride to sesquichloride, but not so as to produce further chemical changes by the formation of the double nitrites of iridium and potassium or sodium. With a very little experience this is easily managed. In consequence of the facility with which the double nitrite of iridium and sodium is decomposed by boiling with chlorhydric acid into the double chloride $IrCl_2,NaCl$, it is better to use nitrite of soda in the above process, because, in case an excess of nitrite is used, the mixed solution of double chloride and double nitrite can easily be brought to the form of double chloride, $Ir_2Cl_3,3NaCl$, by boiling with chlorhydric acid, neutralising with carbonate of soda, and then reducing the iridium to sesquichloride by cautiously adding a very dilute solution of nitrite of soda.

In the second place, it may happen, as in working with crude platinum solutions obtained, not from osmiridium, but from platinum ores, that the quantity of platinum is very large when compared with that of iridium. The process applies equally well to this case so far as the iridium is concerned, but it is difficult and troublesome to re-crystallise large quantities of a salt so insoluble as the chlorplatinate of potassium, $PtCl_2,KCl$, and small quantities of the corresponding iridium salt are difficult to remove. A method of obtaining platinum in a state of chemical purity will be given further on.

The above process is capable of giving chemically pure iridium when platinum is the only other metal present. This is rarely the case, and the following method is usually more advantageous:—The greater part of the platinum is first to be separated in the manner above pointed out. The solution of double chloride of iridium and sodium, $Ir_2Cl_3,3NaCl$, is then to be filtered, an excess of nitrite of soda added, and the solution boiled until it assumes a clear orange-yellow colour. To the boiling solution sulphide of sodium is to be added, drop by drop, as long as this produces a cloudiness, and until a small quantity of the precipitated sulphide of platinum, PtS_2, is re-dissolved. Dilute chlorhydric acid is then to be added cautiously until the liquid, previously allowed to become cold, is distinctly, though faintly, acid, when it is to be filtered, and the sulphide of platinum on the filter washed continuously with hot water. The filtrate is then to be boiled with chlorhydric acid in excess, and the resulting chloro-iridate of sodium evaporated, precipitated by a cold and strong solution of chloride of ammonium, and washed with the same. This salt, on ignition, yields pure iridium, if the operation has been well conducted. It is in all cases, however, well, after separating the sulphide of platinum by filtration, to neutralise the filtrate with carbonate of soda, boil a second time with a little additional nitrite of soda, and then add sulphide of sodium, and proceed as before. In this manner every trace of platinum is removed, and the resulting iridium salt is chemically pure.

Platinum from Ruthenium.—Ruthenium in the form of bichloride may be approximately separated from platinum by precipitating the two metals together in the form of $PtCl_2,KCl$, and $RuCl_2,KCl$, and washing out the ruthenium salt with cold water, in which it is readily soluble. The mixed solutions should be evaporated to dryness with an excess of the alkaline chloride, and the dry mass rubbed to fine powder in a mortar, after which, almost the whole of the ruthenium may be washed out with water or with a cold and moderately strong solution of chloride of potassium. The undissolved platinum salt may then be purified by crystallisation, but usually retains traces of ruthenium. The rose-red solution of the ruthenium salt contains a small quantity of platinum, from which it cannot be wholly freed by the difference in solubility of the two salts. Chloride of ammonium may be employed in this process in place of chloride of potassium.

To obtain a complete separation, the following process may be followed with advantage:—The chloro-ruthenate of potassium, separated as far as possible from the platinum salt, is to be heated with a solution of nitrite of potash in quantity sufficient to convert the whole of the ruthenium into the soluble yellow double nitrite of ruthenium and potassium, carbonate of potash being added in small quantities so as to keep the solution neutral or alkaline. The yellow or orange solution is to be evaporated to dryness in a water bath, the dry mass reduced to powder and boiled with absolute alcohol until the ruthenium salt is completely dissolved. This is best effected in a flask furnished with a condensing tube bent upwards, so that the alcohol vapours may be condensed and flow back into the flask. The boiling need not be continued for a very long time, as the ruthenium salt is readily soluble in alcohol. The solution is then to be filtered off from the undissolved salts, and these are to be washed with absolute alcohol until the washings are colourless, or until they no longer give the characteristic ruthenium reaction with sulphide of ammonium. The filtrate and washings may then be distilled, to separate and save the alcohol, water being first added in small quantity. The residue in the retort or flask is then to be evaporated with chlorhydric acid, which readily decomposes the double nitrite, and yields a fine deep rose-red solution of the chloro-ruthenate of potassium, containing at most only a trace of platinum. The mass of salts undissolved by the alcohol contains nearly all the platinum in the form of chlorplatinate of potassium, which is easily separated. The solution of chloro-ruthenate of potassium is now so pure that it gives the reactions of a chemically pure salt. To obtain the ruthenium in a state of absolute purity, the solution is to be evaporated to dryness with a saturated solution of sal-ammoniac in excess, re-dissolved, again evaporated, and the dry mass washed with a little cold water to remove the alkaline chlorides. The chloro-ruthenate of potassium is in this manner, for the most part at least, converted into chloro-ruthenate of ammonium. This salt is then to be dissolved in hot water, a solution of ammonia added, and the liquid boiled until it assumes a clear yellow or orange-yellow colour, after which it is to be evaporated to dryness upon a water bath. In this manner the ruthenium is converted into the chloride of ruthen-diamin, $2NH_3,RuCl+3HO$, discovered by Claus. The yellow mass is to be dissolved in boiling water, and a solution of chloride of mercury added. A beautiful yellow crystalline double salt is precipitated, and the mother-liquor, when cold, contains only traces of ruthenium and platinum. The double chloride of mercury and ruthen-diamin has the formula $2NH_3,RuCl+HgCl$; it is almost insoluble in cold water, but is soluble in boiling water, and is easily rendered absolutely pure by re-crystallisation. On ignition, this salt yields chemically pure metallic ruthenium as a silver-white porous mass. When, in a mixture of solutions of ruthenium and platinum, the ruthenium is present either partly or wholly as sesquichloride, the liquid is to be boiled with nitrite and carbonate of potash as above, evaporated to dryness, boiled with excess of chlorhydric acid to convert the double nitrite of ruthenium and potassium into chloro-ruthenate of potassium, $RuCl_2,KCl$, and the resulting solution treated by the process already described.

Platinum from Rhodium.—The separation of these metals may be approximately effected by bringing the platinum into the form of $PtCl_2,KCl$, or $PtCl_2,NH_4Cl$, and the rhodium into that of $Rh_2Cl_3,3KCl$, or $Rh_2Cl_3,3NH_4Cl$, and then carefully washing out the rhodium salt by small successive portions of cold water, or, better, of a moderately concentrated solution of chloride of potassium or ammonium. This is the method usually employed. By re-crystallising the platinum and rhodium salts respectively, they may be obtained in a state of purity, since they are not isomorphous. To obtain rhodium absolutely free from platinum, it is best to convert the two metals into the ammonium double salts, separate the rhodium salt as completely as possible by washing with a solution of sal-ammoniac, and then evaporate the double chloride of rhodium and ammonium with a solution of ammonia. In this manner the rhodium is converted into the chloride of the ammonia-rhodium base discovered by Claus, $5NH_3,Rh_2Cl_3$, while the platinum forms no well defined or crystallisable compound. The chloride of Claus's base may then be purified by repeated crystallisation.

Iridium from Ruthenium.—The separation of these metals cannot be effected by igniting them with a mixture of saltpetre and caustic potash. Under these circumstances, the ruthenium is oxidised to ruthenate of potash, but a portion of the iridium also becomes soluble in the alkali, though in what state of oxydation it is difficult to ascertain. Moreover, the complete oxydation of a mixture of the two metals, even when in a finely divided state, can hardly be effected by a single fusion. Claus has given no general method for the separation of iridium and ruthenium in the wet way. In his method of treating the Siberian ores, the greater part of the ruthenium is separated as ruthenate of potash by the primary fusion with saltpetre and caustic potash, but a portion always remains with the iridium, and is difficult to remove, especially as, after solution in chlorhydric acid, salts of both sesquichloride and bichloride of ruthenium are invariably present. No part of the present investigation has cost more labour than the complete separation of iridium and ruthenium, the properties of mixtures of the salts of these metals having sometimes almost led to a conviction of the existence of new metallic elements in the osmiridium.

A perfect separation of ruthenium from iridium may be easily effected by the following process, which is applicable to all cases, without reference to the state of oxydation in which either metal may exist. To the solution containing the two metals nitrite of soda is to be added in excess, together with a sufficient quantity of carbonate of soda to keep the liquid neutral or alkaline. The whole is to be boiled until the solution assumes a clear orange-yellow or orange colour. If a green tint should be perceptible, more nitrite of soda must be added, and the solution again boiled. Both ruthenium and iridium are converted into soluble double nitrites. A solution of sulphide of sodium is then to be added, in small quantities at a time, until a little of the precipitated sulphide of ruthenium is dissolved in the excess of alkaline sulphide. The first addition of the sulphide gives the characteristic crimson tint due to the presence of ruthenium, but this quickly disappears and gives place to a bright chocolate-coloured precipitate. The solution is then to be boiled for a few minutes, allowed to become perfectly cold, and then dilute chlorhydric acid added cautiously until the dissolved sulphide of ruthenium is precipitated, and the reaction is just perceptibly acid. The solution is then to be filtered through a double filter, and the sulphide of ruthenium washed continuously and thoroughly with boiling water. The filtrate is perfectly free from ruthenium: it is to be evaporated with chlorhydric acid, and treated with sal-ammoniac in the manner already pointed out in speaking of the separation of iridium from platinum. The washed sulphide of ruthenium is to be treated, together with the filter, with strong chlorhydric acid and chloride of ammonia added in quantity sufficient to form chloro-ruthenate of ammonium. Nitric acid is to be added from time to time, in small quantities, until, with the aid of heat, the whole of the sulphide of ruthenium is oxidised and dissolved. The liquid is then to be filtered, the filter well washed, and the filtrate and washings evaporated to dryness on a water bath, when, after washing out the soluble salt with a strong solution of chloride of ammonium, the salt, $RuCl_2,NH_4Cl$, remains almost chemically pure. It is to be dissolved and converted into the compound of chloride of mercury and ruthen diamin, $2NH_3RuCl+HgCl$, by the process already described. From this salt chemically pure ruthenium may be obtained by ignition, which is best effected in an atmosphere of hydrogen, as the reduced metal is easily oxidised in the air.

It may happen that the precipitated sulphide of ruthenium contains traces of iridium. This can only arise from imperfect washing or want of proper care in precipitating with sulphide of sodium. In this case, the washings from the chloro-ruthenate of ammonium are yellow, and contain sulphate of iridium, probably $Ir_2O_3,3SO_3$. The quantity of iridium in such cases is too small to be worth the trouble of separate treatment.

When a solution contains iridium and ruthenium in the form of bichlorides, the ruthenium may be easily and completely separated by boiling the solution with nitrite of potash in excess, adding, at the same time, enough carbonate of potash to give an alkaline reaction, evaporating to dryness, and dissolving out the double nitrite of ruthenium and potassium by means of absolute alcohol, in the manner recommended for the separation of ruthenium from platinum. The undissolved mass in this case contains the two double nitrites of iridium and potassium. By adding a strong solution of chloride of ammonium, evaporating to dryness, igniting the dry mass in a porcelain crucible, and dissolving out the soluble salts, metallic iridium remains in a state of purity. This method may be used for the quantitative separation of iridium from ruthenium, but when the object is simply to prepare both metals in a state of chemical purity, I prefer the separation by means of sulphide of sodium.

(To be continued.)

On the Presence of Arsenic in the So-called Pure Commercial Hydrochloric Acid.—Preparation of Pure Hydrochloric Acid, by M. Glénard.

So-called pure hydrochloric acid, sold by the manufacturers of chemical products, is often as arseniferous as the crude acids. Twice has M. Glénard, at a year's interval, examined numerous specimens of hydrochloric acid considered as pure. In two specimens very recently examined he found 2·5 grammes of arsenious acid per kilogramme.

According to M. Glénard, the presence of arsenic in hydrochloric acid may prove very injurious in the preparation of certain pharmaceutical products. It is, then, important that pharmaceutists should be aware of this fact, and abstain from employing hydrochloric acid which they have not previously tested carefully. The examination is attended, fortunately, with no difficulty. It is only necessary to mix the acid to be tested with its volume of a solution of hydrosulphuric acid, or to pass into it, during a few minutes, a current of this gas; or, better still, to throw into it a few morsels of artificial sulphide of iron. Under these three conditions, the acid, if it contains arsenic, will become troubled, owing to the separation of the yellow sulphide of arsenic.

But how to obtain pure hydrochloric acid? M. Glénard proposes—

1. The direct preparation of this acid.
2. The purification of commercial acid.

In the direct preparation, the first step is to ascertain that the sulphuric acid is free from arsenic; otherwise, the arsenic, transformed during the preparation of the hydrochloric acid into very volatile chloride of arsenic, will escape with the acid, and with it become condensed in the water. The first thing to do is to eliminate the arsenic from the sulphuric acid. To effect this, the acid is diluted with half its weight of water, and to it is added a little hydrochloric acid, and then a current of hydrosulphuric acid gas is passed into it. The arsenic soon separates in the form of sulphide. It is next filtered through a funnel packed with amianthus, then heated in a capsule, to expel the excess of sulphuretted hydrogen, and to bring it to 60° of Baumé's areometer. The sulphuric acid thus purified is employed in the ordinary way, and furnishes perfectly pure hydrochloric acid.

After describing the simple and ingenious process which consists in disengaging, by means of concentrated sulphuric acid, the hydrochloric gas contained in commercial acid, and in condensing this gas in distilled water, M. Glénard proposes the following method:—

Into the crude acid to be purified a current of hydrosulphuric acid is passed until all the arsenic is precipitated.

The sulphide of arsenic is separated either by letting the acid stand or by filtering it through a funnel packed with amianthus. Should the filtered liquid contain excess of sulphuretted hydrogen, some grammes of a concentrated solution of perchloride of iron are added, which destroys the hydrosulphuric acid, becoming reduced to protochloride.

As the hydrochloric acid would then contain only fixed matters, it could then be rectified. Hydrochloric acid gas is displaced by means of sulphuric acid freed from arsenic.—*Journal de Pharmacie et de Chimie.*

TECHNICAL CHEMISTRY.

The Indian Madar Plant, and its Useful Applications.

Numerous applications having been made for further information on the above subject, in consequence of the article given in vol. vi., p. 59, we are induced to print the following extract from Major Drury's "Useful Plants of India," a work which is little known in England:—

Calotrophis gigantea. Natural order, *Asclepiadacæ.* English name, "Gigantic Swallow-wort." Hindostanee name, "Muddar," "Ack." Tamil name, "Yercum."

Description: Shrub, 6 to 10 feet; leaves stem-clasping, decussate, oblong-ovate, wedge-shaped, bearded on the upper side at the base, smooth on the upper surface, clothed with woolly down on the under side; segments of corolla reflexed with revolute edges, stamineous corona five leaved, shorter than the gynostegium; leaflets keel-formed, circinnately recurved at the base; incurved and subtridendate at the apex; umbels sometimes compound, surrounded by involucral scales; follicles ventricose, smooth; seeds comose; flowers rose colour and purple mixed.

Uses: Of late years the plant has attracted much notice from the many and important uses to which its several properties can be applied. The silky floss which surrounds the seeds has been woven into shawls and handkerchiefs, and even paper, besides a soft kind of thread, by the natives. In addition to its medicinal qualities, this plant is valuable from the fine strong fibres with which it abounds. To procure them, the straightest branches are cut, and exposed for at least twenty-four hours to wither. On the second and third day they are slightly beaten. The skin is then peeled, and the stringy substance between the bark and the wood taken out. They are then dried in the sun. This slow process is necessarily expensive; but if the bark is steeped in water, it becomes discoloured, and cutting will destroy it. Still the fibre is strong, and possessed of many of the properties of European flax. It can be spun into the finest thread for sewing or weaving cloth. It resists moisture for a long time. From recent experiments made by Dr. Wight, its tenacity, compared with other Indian fibres, is as follows:—

	Breaking weight. lbs.
Yercum, or Madar (*Calotrophis gigantea*)	552
Janapum, or Lun (*Crotalaria pinæa*)	407
Kattalay (*Agave Americana*)	360
Cotton (*Gossypium herbacium*)	346
Marool (*Sanseviera zeylanica*)	316
Poolay munja (*Hibiscus cannabinus*)	290
Coir (*Cocus nucifera*)	224

This fibre, however, is too valuable for ordinary cordage, and might fetch a high price in Europe. It is said by good judges to be better for cloth than cordage. It is much used in this country for bow-strings, ropes, bird nets, and tiger-traps. It has never been cultivated as a cordage plant. It is widely diffused through the southern provinces of the peninsula; while in the Bellary district and to the north it is replaced by the *Calotrophis Hamiltonii*, which is equally abundant. In the *Journal of the Society of Arts*, it is stated that yercum, which much resembles Belgian flax, is well calculated for prime warp yarns, and worth 100*l.* per ton. It has been tried to employ the viscid juice as a caoutchouc, and a great quantity was collected for that purpose. To prepare it, the juice was evaporated in a shallow dish, either in the sun or the shade. When dry, it may be worked up in hot water with a wooden kneader, as this process removes the acridity of the gum. It becomes immediately flexible in hot water, but is said to become hard in cold. It is soluble in oil of turpentine, takes impressions, and will, no doubt, prove a valuable product, either alone or mixed with other substances. In experiments made in London, Petersburg hemp bore 160 lbs., brown hemp of Bombay and Jubbulpore hemp 190 lbs., which latter was also the strength of the yercum. Its value in England might probably be reckoned at from 30*l.* to 40*l.* the ton.

Calotrophis Hamiltonii. English and native name the same as above. Shrub, 6 to 10 feet; lobes of the corolla patulous, revolute on the margin; leaflets of the crown of the stamens equalling the short, depressed gynostegium; the circinnate portion short, often acuminate.

Uses: This species differs from the former in not having the segments of the corolla reflexed. It is a widely-distributed plant, very abundant in the Bellary district, but quite unknown in the southern provinces. In uses the two specimens are probably similar in every respect.

The *Calotrophis procera* is a Persian plant, totally different from either of the preceding ones.

On a Process for Extracting Soda from Cryolite, *by* M. J. Bing.

Take eight tons of cryolite to eleven tons of chalk or calcareous stone, and pulverise separately, as finely as possible, by vertical mill-stones, and sift, still separately, through movable tammies; then mix, and re-grind by vertical mill-stones. (The portions of cryolite and chalk remaining in the tammies are again ground and sifted separately, then mixed and re-ground as in the first operation.)

The mealy powder produced in this way is placed in furnaces constructed for the purpose, wherein it is baked and constantly stirred with iron instruments. During baking, the material must be carefully watched, to prevent it from fusing. When at white heat, it is taken from the furnace, and, when sufficiently cooled, to be sifted through an iron sifter; the lumps which may have agglutinated during the baking are separated. (After eight days, when, from the influence of the air, these morsels have lost some of their coherence, they are ground before being re-baked.)

The calcined product is sifted and put into washing-tubs, and boiling water poured upon it, until no more soda remains to be extracted. The lixivium is conveyed into reservoirs intended to receive the carbonic acid destined for the saturation. The carbonated lixivium is directed into receivers, at the bottom of which clay is deposited. After clarification, the supernatant liquid is pumped, either into evaporating pans, to be reduced to the point necessary for crystallisation, or into calcining vessels, if salt of soda is to be produced.

Carbonic acid is obtained from the furnaces used to calcine the mixture, whence it is conducted by canals into a purifying apparatus, and thence into its reservoirs (iron cylinders; the boilers of a steam-vessel will answer the purpose).

This operation, during which three tons of coal are burnt, produces 175 kilog. of crystals of soda and 16 kilog. of pure alumina per 100 kilog. of cryolite. Theoretically, the 100 kilog. of cryolite ought to yield 204 kilog. of crystals of soda, and 24 kilog. of alumina. The alumina is used in manufacturing alum, sulphate of alumina, aluminate of soda, or aluminium.—*Répertoire de Chimie Pure et Appliquée.*

PHARMACY, TOXICOLOGY, &c.

*On the Presence of Arsenic and Thallium in the Medicinal Preparations of Bismuth, and on a Means for Purification,** *by* W. Bird Herapath, *M.D., F.R.S.L. & E.*

Some preliminary experiments have shown me that it is possible to remove the arsenical impurities by a very simple process, and one which would not add much to the cost of the material. On boiling either of the insoluble salts of bismuth with a sufficient quantity of solution of caustic soda or potassa, the arsenic is quickly removed in the soluble form, and the residue, on treatment a second time with the same re-agent, and, subsequently, well washing it by means of large quantities of water, and decantation, is rendered perfectly pure and fit for medical purposes. I am not aware whether this process has been hitherto proposed, but it has succeeded perfectly in purifying some samples of nitrate and carbonate upon which I have tried it in my own laboratory.

The greatest objection to this mode of purification is, of course, the change which the caustic alkali exerts upon the preparation employed, as the nitrate will be wholly converted into the yellow oxide, whilst the carbonate will be also partially so changed. This process, therefore, entails the resolution of the oxide in nitric acid, and its renewed precipitation as "trisnitrate" by the addition of water in the usual manner; whilst the waste acid obtained from this precipitated and purified nitrate may be employed for the resolution of the changed and purified carbonate, whence the bismuth may be

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* Extracted from the *Pharmaceutical Journal;* being an addition to a paper published by the author in the Chemical News, vol. vi., p. 304.

again thrown down in the pure form by the addition of carbonate of soda to that solution. Thus, by carefully economising the nitric acid used in the process of manufacture, very little additional expense would be entailed by following this method of purification. A few words may be advisable upon the best means of detecting and estimating the arsenic in preparations of bismuth, as it is not an easy matter to do so by the plan recommended by Rose, and many chemists may readily overlook small quantities like these by depending upon such a method of proceeding. But reduction by hydrogen gas furnishes us with both a ready method of detection and even of quantitative estimation, and, in fact, was the plan adopted by me in all these analyses. One precaution is necessary, however, in the quantitative analysis, in addition to all those which ensure the purity of the acids, zinc, and apparatus employed, which is to select a tube of hard green glass, about a quarter of an inch in diameter; then, by means of the blowpipe, draw it out in successive portions, to narrow its calibre and reduce its weight, and, for convenience in heating and weighing, to subsequently bend it into a triangular form; and having carefully counterpoised it, now attach it to the hydrogen gas apparatus (furnished, of course, with an exit tube, filled with chloride of calcium) by means of a little tube of India-rubber, and, having arranged proper supports, it only remains to apply heat at three or four portions of the tube simultaneously by separate spirit-lamp flames properly arranged, and, of course, having previously allowed the air to escape from the apparatus, and ensured the purity of the gas evolved by heating one of the bends of the tube for some time previous to the introduction of the weighed portion of the bismuth to be tested into the tubular funnel of the apparatus, whence it should be carefully washed down into the gas-bottle by directing a stream of distilled water by a pipette, or the ordinary wash-bottle. In this way twenty grains have sufficed for very accurate analysis, as the hydrogen escapes free from arsenic, as may be proved by lighting it at the extremity of the tube, and testing it by Marsh's method, from time to time, in the ordinary way. In the calculation of the per-centage it is necessary to remember that the arsenic is obtained as metal, whereas it existed in the sample as an arsenite, either of lime or bismuth; consequently, it must be converted into the theoretical quantity of arsenious acid.

It is generally the case that the evolution of gas stops during some minutes after the introduction of the nitrate of bismuth, probably from the production of ammonia by the reduction of the nitric acid, but on the further addition of hydrochloric acid it goes on as before. In this way three or four films of arsenical deposit are obtained, and in great beauty, from the quantity of bismuth I have already named.

I need scarcely say that the hydrochloric acid employed was proved to be chemically pure and free from arsenic, not only by Reinsch's method, employing electrotypic copper for the purpose, but by the more rigorous method of passing the hydrogen gas evolved by it during one hour through a red-hot glass tube; this latter method detects a quantity of arsenic perfectly inappreciable by Marsh's usually trusty method of testing for arsenic. The alkaline liquid obtained after thus boiling one sample of carbonate of bismuth, about 1 lb. in weight, has furnished me with evidence of the existence of another metallic impurity in bismuth preparations. This will very probably turn out to be thallium, which I have nearly identified, but I am now engaged in the elucidation of this matter.

The hydrogen gas-flame, when a sample of bismuth containing thallium is tested by Marsh's method in a darkened room, by means of a white porcelain plate, gives a brilliant green light where the flame impinges on the plate; the arsenical flame is violet, and when sulphur is present we have a beautiful blue. I have found that thallium may be obtained as a deposit in the red-hot tube like sulphur, and of a reddish-brown colour. On heating this whilst the current of hydrogen is still passing through the tube, the green flame is again rendered evident, if it had previously disappeared.

P.S.—Since the above was written I have found three samples of bismuth tolerably free from arsenic—only the most minute traces to be obtained in the tube. These samples were, two prepared by the Messrs. Howard and Sons, and a specimen of carbonate made by Mr. Schacht, of Clifton; all other samples, more than twenty in number, were decidedly very arsenical.

On the Detection of Cantharidine,

by C. R. C. Tichborne, *Esq.*

Some time since I was called upon to examine some brandy, which was suspected to have been tampered with for aphrodisiac purposes, the symptoms indicating cantharides. The suspected fluid was the remains of a glass of brandy-punch, sweetened with sugar. It was quite bright, and free from any *débris* of the flies. Now, if the flies themselves had been used, it is next to impossible that every particle of the elytræ would escape detection by the aid of the microscope; but in such a case as that given above, this conclusive test would, of course, be wanting. Desirous of investigating the subject, I performed some experiments, which proved satisfactorily to myself the ease with which small quantities of cantharidine in solution can be detected, even when mixed with large quantities of foreign extractive matter. The only process given by Taylor for the detection of cantharides is to evaporate to dryness, extract with ether, and to try the vesicating properties of the ethereal extract after evaporation. Taylor then goes on to say, that "this mode of testing is somewhat uncertain, unless the quantity be large . . . there is, however, no other mode of discovering cantharides in solution, whether as tincture or infusion, than this."* I also found this process very unsatisfactory, which is, probably, due to the loss of the cantharidine when occurring in small quantities from the employment of heat. The chemical tests for cantharidine are too vague to admit of application, therefore we would naturally look to its vesicating properties as the best indication of its presence. The *modus operandi* that I pursued I now give, and it is extraordinary how small a quantity of the poison can be detected with certainty if the details are strictly complied with.

Chloroform is the best solvent of cantharidine that we have, and I should recommend its employment for the extraction. In the experiments above referred to, a tincture was used that represented three grains of cantharides. This was added to half a pint of wine,† and to this, which represented a suspected fluid, was added one ounce of chloroform. The whole was repeatedly shaken during the day, and left to subside until next morning. The chloroform was then carefully separated with a funnel, and passed through bibulous paper. The chloroformic solution was then allowed to evaporate

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* "Taylor on Poisons," page 546.

† In a second experiment it was added to half a pint of porter.

spontaneously to dryness in a watch-glass. A small pellet of lint (which had previously been teased out), about half the size of a pea, was moistened with a drop of olive oil, and with this little pellet the whole of the film of extractive matter was mopped off the watch-glass. The lint was then placed upon the arm, and covered with a piece of gold-beater's skin. When taken off in three or fours considerable rubefaction had taken place, and after wiping it off with chloroform a large vesicle was formed. As small a quantity as one grain of flies (= to about 4-1000ths of a grain of cantharidine) was detected in solution by this means. To look for less than this in a medico-legal investigation would be useless, as this is much under a medicinal dose. The smallest dose of the tincture which has been known to destroy life was an ounce = 6 grains of powdered cantharides. In an investigation of the kind our object is to concentrate the active principle, which is diffused through an extensive medium, and to bring it to bear upon a limited surface; or, in other words, to bring it to a focus. Of course, in such a manipulation, where there is so small a quantity of the principle, and which principle is in so diluted a form, it requires to be done with the greatest care, in order to avoid loss, and to get it as free as possible from foreign matter, which might seriously diminish its activity.

Dublin, January 17.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, February 5.

Professor A. W. Hofmann, *LLD., F.R.S., President, in the Chair.*

The minutes of the previous meeting having been read and confirmed, the following gentlemen were duly elected Fellows of the Society:—R. R. Carew, Esq.; Dr. Reginald Thompson, M.B.; Alfred H. Smee, Esq.; and H. P. Meaden, Esq.

Mr. William Spiller, Atlas Chemical Works, Walworth, was admitted a Fellow of the Society, and signed the statute book.

Mr. A. H. Church read a paper, in which he detailed his experiments "*On the Transformation of Benzol into Phenic and Benzoic Acids.*" The author accomplishes these changes by first forming chloride of phenyle by the action of nascent chlorine on benzol; the chloride of phenyle thus made is purified, and then warmed with alcoholic potash. The change proceeds according to the equation, $\text{Ꞓ}_6H_5Cl+KH\Theta=KCl+\text{Ꞓ}_6H_5H\Theta$. Mr. Church showed that the product of this reaction is identical in every chemical and physical character with that ordinarily known as phenic acid. Treated with oil of vitriol it yields phenyl-sulphuric acid, $\text{Ꞓ}_6H_5HSO_4$. The phenyl sulphate of barium, dried at 115°, and mixed intimately with half its weight of cyanide of barium, yields, when submitted to dry distillation, large quantities of phenic acid and benzol, with minute traces of cyanide of phenyle; the formation of the latter substance being explicable according to this scheme,—

$$\left.\begin{matrix}\text{Ꞓ}_6H_5\\ Ba\end{matrix}\right\} SO_4+BaCy=Ba_2SO_4+\text{Ꞓ}_6H_5Cy.$$

To ensure a successful result, large quanties of material must be used. From the cyanide of phenyle, or benzonitrile, thus made, benzoic acid, with all its characteristics, was obtained by the action of boiling potash.

Mr. Church also read an interesting paper entitled "*Analysis of Red Chalk.*" The late Mr. H. Styleman Le Strange placed some specimens of red chalk in the author's hands for analysis. Some of these were found to contain as much as 36 per cent. of sesquioxide of iron, but they varied much in hardness and composition. Both the red chalk itself and the water percolating through are almost entirely free from sulphuric acid in any form, a fact which precludes the idea of a pyritic origin for the iron oxide. Several other facts of interest are noted in the paper: among others may be instanced the change which the red chalk undergoes when strongly heated. If the iron oxide and the carbonate of lime be in a proportion approaching 4 : 9, the ignited residue presents a dull, olive-green colour, scarcely re-absorbs carbonic acid when exposed to that gas, and seems, from certain synthetical experiments of the author's, to be a ferrite of lime, having the formula, $3Ca_2\Theta,Fe_4\Theta_3$.

A paper, by Dr. W. G. Russell, "*On the Atomic Weight of Nickel,*" was read by the Secretary. It will be remembered that the results obtained in the determination of the atomic weight of cobalt were communicated to the Society at its last sitting, and were duly duly reported in this Journal;* the continuation of these researches, so far as they related to nickel, remained to be described. After making a series of experiments upon several different samples of the purified metal, the author arrived at the conclusion, that the atomic weights of nickel and cobalt were practically identical. The mean result in the case of nickel furnished 29·369, as the numerical expression of its atomic weight.

The President, in proposing a vote of thanks to Dr. Russell, took occasion to remark that, however small appeared the final result of these investigations—the conclusion being generally expressed by a few figures, or written in a single line—there was the greater credit due to the patient investigator who took them in hand, and, by his fundamental data, prepared the way for chemical researches of a more agreeable character.

The next communication was a "*Note on the Bicarbonate of Ammonia,*" by Dr. T. L. Phipson. The author had, through the kindness of Captain Marcus Lowther, R.N., been furnished with specimens of a white substance which was discovered underlying, to the extent of a few inches, the beds of guano in several islands off the coasts of Peru and Chili. This substance was found on analysis to consist of the bicarbonate of ammonia, with a small proportion only of calcareous impurities in admixture; it was perfectly inodorous, nearly all soluble in water, and furnished results in accordance with the formula—

$$NH_4O,CO_2,HO,CO_2.$$

A reference to Dana's "Mineralogy" indicated the existence of natural deposits of a similar kind both in Africa and Patagonia. It was interesting to remark the separation from guano of a highly nitrogenous compound such as that described, whilst, on the other hand, the upper beds in this and several other localities furnished the article known in commerce by the name of "West Indian guano," and which, although rich in phosphates, scarcely contained any nitrogen—none, indeed, in the form of uric acid, and only a small proportion, seldom exceeding ·5 per cent., in the form of xanthic oxide. The mineral "sombrerite" contains an increased amount of earthy phosphates, and is quite destitute of nitrogen.

(Some interesting particulars relating to this class of manures were, on a former occasion, communicated to the Society by Dr. Phipson, These, with the accompanying analyses, will be found described in the Chemical News, vol. vi., pages 16 and 48.)

Dr. B. H. Paul wished to offer a few remarks on the subject of "*Water Analysis.*" On a recent occasion a paper was read before the Society, and afterwards printed in the Journal, which professed to describe a method of analysis so rapid and simple, that the quantitative exami-

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* Chemical News, vol. vii., p. 43.

nation of a sample of water could be undertaken and completed within a period of two or three hours. On reading the instructions given in the paper by Dr. Edward Nicholson, he found many sources of error, some of which had already been pointed out (by Dr. Hofmann and Mr. Dugald Campbell);† and inasmuch as the object of its author was to furnish a ready means of analysis, and one that should serve as a guide for comparatively inexperienced operators, he felt it a duty to point out some grave errors which had not yet been noticed or corrected, for he believed that as the matter stood it was calculated to bring chemistry and its professors into disrepute. On referring to the analysis of the water supplied by the Chatham Company to Fort Pitt,‡ it would be seen that Dr. Nicholson found 16·9 grains per gallon of carbonate of lime, with no more than 2·52 cubic inches of carbonic acid gas to keep it in solution; there is said to be no sulphuric acid, and the table does not give any account of nitric acid nor of organic matter. How, then, is the carbonate of lime dissolved? Not, surely, in pure water; for Fresenius and his own experiments prove that about two grains per gallon is the utmost limit of its solubility. By way of trial, Dr. Paul had made comparative experiments upon New River water by this process against the ordinary mode of analysis. Its "hardness," according to Dr. Clark's scale, is, at the present time, about 15°, eleven of which are due to carbonate of lime; the sulphuric acid in combination amounts to about three grains in the gallon, and yet by this method it becomes *nil.* In searching for the cause of this anomaly, it was observed that nitrate of baryta solution, of the strength prescribed (1·3 gramme per litre), gave no precipitate in a pint of the water even on standing for twenty-four hours, whilst chloride of barium gave almost immediately a precipitate. Then, with regard to the mode of determining free carbonic acid, by taking the difference between the original hardness and that exhibited after evaporation of the water with excess of sulphuric acid, the results were altogether fallacious, for he had satisfied himself that the calcium hardness was invariable, whether it be present in the form of sulphate or bicarbonate of lime. These instances were sufficient to show that full examination was necessary before placing reliance upon, and adopting the recommendations contained in the analytical report recently presented to the Society.

The President enquired whether any of the members were prepared to bring forward facts bearing upon the extent of solubility of carbonate of lime in water containing carbonic acid? In his opinion there was truth in a statement which had been made, to the effect that the amount of carbonic acid required was considerably less than that demanded for the formation of bicarbonate of lime; the proportion did not, however, appear to be always definite.

Mr. Church mentioned an instance in which he had collected a sample of water for analysis, and, for the better preservation of the carbonic acid, had hermetically sealed it within a glass tube on the spot. A few days later, on examining the contents of the tube, a portion of the carbonate of lime was found to have separated in minute crystals against the glass. This result could not be accounted for by the escape of carbonic acid, but tended to show that slight mechanical causes operated in disturbing the proportion which could be retained in solution.

Mr. Riley referred to the marked interference of nitric acid in preventing the precipitation of sulphate of baryta. On this account the employment of nitrate of baryta as a re-agent was seldom to be recommended.

A conversation ensued on the disadvantages attending the use of the ordinary soap-test from its liability to crystallise in cold weather; when Professor Redwood described a mode of preparing a perfectly neutral potash-soap by decomposing the common *Emplastrum plumbi* (lead plaister) of the Pharmacopœia with an equivalent amount, or rather less, of dry carbonate of potash. On triturating these materials together in a mortar they were soon decomposed, carbonate of lead and pure oleate of potash being formed; on the addition of rectified alcohol the latter could easily be dissolved, and this solution, which was neutral to test paper, and gave no deposit on keeping, appeared to answer particularly well for the soap-test in Dr. Clark's process.

Before adjourning the meeting until the 19th inst., the President announced that there remained no papers on hand, and he trusted the members present would assist in supplying a want which would be felt on the next occasion.

PHARMACEUTICAL SOCIETY.

Wednesday Evening, February 4.

M. P. Squire, *President, in the Chair.*

A paper, by C. H. Wood, was read "*On the Production of a Neutral Potash Soap, Applicable to the Preparation of Clarke's Soap Test.*" The author stated that soap test, prepared according to Dr. Clarke's directions with hard soap, was constantly liable on keeping, especially in cold weather, to deposit a portion of the soap from solution, causing delay and inconvenience when it was required for use. To avoid this objection, Mr. Nicholson had recently suggested that the soft soap of the Pharmacopœia should be substituted for the soda soap. Mr. Wood pointed out that this preparation is necessarily too impure and uncertain in its composition to be well suited for employment as a delicate re-agent, and stated that, although it is impossible to obtain as pure potash soap by direct saponification, yet it is very easy to produce it, by indirect means, from the previously formed soap of another base. If a pure lead soap, such as the emplastrum plumbi of the Pharmacopœia (consisting of olive oil saponified with oxide of lead), be treated with carbonate of potash in the presence of rectified spirit, a double decomposition occurs, yielding at once insoluble carbonate of lead and a spirituous solution of a pure and perfectly neutral potash soap. Both the plaster and the alkaline carbonate being insoluble in spirit, any excess of either remains insoluble. The easiest method of proceeding consists in taking 150 parts of lead plaster and 40 parts of dried carbonate of potash, rubbing these in a mortar until they form a homogeneous mass, and then adding some rectified spirit, which dissolves the potash soap formed; the liquid is filtered to separate the carbonate of lead, and the precipitate washed with more spirit. By evaporating the solution a pure soap may be obtained, which analysis has indicated to be quite neutral. To prepare soap test it is only necessary to dilute the alcoholic solution with proof spirit until the exact strength is attained, which is learnt in the usual manner.

Professor Redwood called attention to the extended use of Clarke's test, recently suggested by Dr. Nicholson (Chemical News, vol. vi., p. 248), and remarked that the great value of Mr. Wood's process was that it furnished a test which would keep any length of time without change—which was, in fact, a permanent soap-test.

The next paper was by Dr. Attfield, on a "*Method of Dissolving Alkaloids in Oils.*" By the ordinary process of dissolving quinine in alcohol, adding the solution to cod-liver oil, and then heating the mixture till the spirit was evaporated, only about two grains of the alkaloid is taken up by an ounce of the oil; and, moreover, in the process the oil acquires a strong tendency to become dark and rancid. When thinking over this fact it occurred to Dr. Attfield that an oleate of the alkaloid, if it could be made, would be easily soluble in the oil, and obviate the other objections to the old process. He found that an oleate was readily prepared by digesting for a short time the perfectly dried alkaloid in oleic acid, at a temperature under 212°. The resulting oleate is an oily fluid, miscible

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† Chemical News, vol. vi., p. 248.

‡ *Journal of the Chemical Society,* vol. xv., p. 475.

with oil in all proportions. It is necessary to employ about twice the weight of oleic acid to the quinine. The oleate is a stable compound, and may be preserved to dilute with oil as the mixture is wanted. The solution of the oleate in oil, Dr. Attfield said, was not more prone to rancidity than the oil itself, and the alkaloid is never deposited.

Other oleates besides that of quinine may be prepared; *e. g.*, quinidine, cinchonine, morphine, strychnine, veratrine, atropine; and of some metallic oxides, as those of mercury, lead, zinc, and iron, which are also soluble to any extent in oil. Dr. Attfield regards the oleates as definite compounds; he could not crystallise any of them. An experiment proved that quinine has not the power of taking oleic acid from oil, and leaving glycerine, like the artificial alkaloids, which produce true saponification. No directions for the preparation of any oleate but that of quinine was given, the process being altogether so simple. In concluding, Dr. Attfield remarked that, when Medical men desired new compounds of remedial agents, it was the duty of the pharmaceutist to apply his chemical knowledge in the preparation of them; but when the supply preceded the demand, monopoly is often fostered, and the materia medica overloaded.

The Chairman said the process just described was an exceedingly convenient and ready one for obtaining a solution of quinine in cod-liver oil, and was one, he thought, quite unobjectionable.

Mr. Deane said it was very undesirable to be constantly ringing the changes on medicines. He did not see any advantage in a solution of quinine in cod-liver oil over the two taken separately. Druggists' shelves were now overloaded with solutions of this sort, which were not at all necessary. He thought Dr. Attfield's process, however—if people must have the solution—a great improvement on the old method of obtaining it;—it was, in fact, a true philosophical method, and a great boon to the pharmaceutist.

Professor Redwood considered the process a great improvement, since the product was not so liable to deterioration as that made in the old way, which gave an abominable compound of two good medicines, spoiled by being put together. The mixture rapidly absorbed oxygen, and as quinine was very susceptible to oxidation, and oxygen was present in the form of ozone, the alkaloid was favourably situated for destruction. One striking advantage in Dr. Attfield's process was that the solution of the oleate of quinine in oil could be made in any quantity when wanted, and there was, therefore, no necessity to keep it for any length of time.

Mr. D. Hanbury, Jun., remarked that the mixture of cod-liver oil and iodide of iron was another example of two useful and not disagreeable medicines being converted into a nauseous and unsightly compound by the mixture of the two. There was no reason in administering compounds like these. Such solutions were very prone to change; all those in the International Exhibition were altered by the time the Exhibition closed.

The Chairman said that Mr. Hanbury's observation would apply to all the solutions of the iodides and bromides in oil, all of which were very liable to change.

The next paper was by Mr. E. C. C. Stanford, F.C.S., on "*Oxide of Copper.*" Oxide of copper, so largely used in organic analysis, is usually made by calcining the nitrate. This method, besides being expensive, leaves a suspicion that all the nitrogen may not have been got rid of, and so leaves a doubt as to the accuracy of the analytical results. Mr. Stanford's object, therefore, was to find a good laboratory method of making the re-agent free from objection. Various plans were accordingly tried. Copper turnings were heated to bright redness in a muffle for twelve hours, by which an oxide was obtained that contained 40 per cent. of metallic copper. Tennant's oxide, made on the large scale in a similar way, Mr. Stanford found to be a mixture of sub- and protoxide containing 30 per cent. of metallic copper. The diacetate yielded a partially reduced oxide, suitable for combustion, but the source is expensive. Brunswick green, said to be an oxychloride of copper, was tried, but the sample the author procured, contained no copper. The best blue verditer, a carbonate of copper, yielded a remarkably good oxide, but a cheaper article Mr. Stanford found to contain a large proportion of sulphate of baryta. Simply heating sulphate of copper was then tried, but no crucible was found which would withstand the heat necessary to get rid of all the sulphuric acid. Hessian and Cornish crucibles were quickly destroyed, and a cast-iron one melted readily. In an experiment with a good hammered iron crucible, the author found what looked like a fine dense oxide of copper, but which proved to be protoxide of iron, the whole of the copper being found in the metallic state at the bottom. With some oxide prepared in a Hessian crucible some silicate of copper was found. Some experiments were then made with the idea of splitting up the sulphuric acid into sulphurous acid and oxygen, and so to effect the reduction at a lower temperature. Sulphur was mixed with the sulphate, but it burnt off, and there was also a difficulty in getting rid of the last traces of sulphurous acid. Coke answered no better, and some metal was reduced in the experiment. Charcoal and coal gave no better results. Oxalic acid did not answer. Ammonio-sulphate of copper gave no better results than the simple sulphate. A mixture of one part diacetate and three parts sulphate of copper answered well; but the mixture aggregates into lumps, which require to be removed, powdered, and re-heated. The best method, however, of obtaining the oxide, Mr. Stanford found to be fusing together equivalent proportions of dry carbonate of soda and dry sulphate of copper. The fused mass is poured into water, and the oxide filtered off and well washed. It is not necessary to dry the salts; they may be heated together in a porcelain dish, when they liquefy in the water of crystallisation, and on continuing the heat nearly all the carbonate is reduced to oxide; but to complete the process the mixture must be heated to redness in a crucible. The equivalent proportions of the crystallised salts are about two parts of sulphate of copper and three parts of carbonate of soda. In conclusion, Mr. Stanford called attention to a mistake contained in all books on chemistry, and first pointed out by Mr. Braithwaite, namely, the assertion that chloride of copper is a highly deliquescent salt. The green chloride (Mr. Stanford said) contained uncombined water, and when exposed to moist air absorbed more; but the blue chloride was not at at all deliquescent. The author analysed the blue salt, and obtained results which accorded with the formula,—$Cu\,Cl + 2HO$.

Dr. Squire remarked that sulphate of copper was very liable to contain iron, and a pure nitrate was easily prepared by the double decomposition of nitrate of baryta and sulphate of copper. He thought, too, that in Mr. Stanford's process there would be a difficulty in washing out all the alkali.

Mr. Stanford said the black oxide obtained by his method was very dense, and he had found no difficulty in washing out the sulphate of soda; nor had he found any carbonate of iron mixed with the oxide.

(To be continued.)

ROYAL INSTITUTION OF GREAT BRITAIN.

On Friday, the 6th inst., Mr. Glaisher gave a lecture on "*Aërial Scientific Research.*"

The lecturer commenced with a short history of aërostation from the time of Montgolfier, relating an interesting anecdote of Dr. Black, who, soon after the discovery of hydrogen by Cavendish, invited a party of friends to witness the ascent of a small balloon in a room, which they believed was effected by means of a fine thread passing through the ceiling. The scientific ascents of Gay-Lussac, Bixio and Barral, and of Mr. Welch, under the auspices of the British Association, were then noticed. The only result of the last-named gentleman's voyages was the supposed discovery that the temperature of the air decreased one degree for every three hundred feet of elevation. Since that time, until Mr. Glaisher resumed the experiments, the balloon has only been used as a toy to please a crowd. A committee of the British Association, considering that experiments made on the earth were necessarily subject to the earth's influence, was anxious that further experiments should be made in the air to test the laws of refraction, to determine the pressure and temperature of the atmosphere, to compare the reading of the aneroid and mercurial barometers, to test the presence of ozone, to observe the vibrations of the magnet, to determine the dew point up to five miles of elevation, the greatest height at which man may be located, and also to observe the phenomena of sound, and make any general observations which might present themselves. Mr. Glaisher accordingly undertook these investigations, availing himself of the services of that experienced and scientific aeronaut, Mr. Coxwell. The balloon made use of had a capacity of 90,000 cubic feet. It was, on leaving the earth, only half filled with gas, as at the height of three miles and three-quarters the volume of gas is doubled. The lecturer gave an eloquent description of the sensations experienced in a balloon ascent—the rapid rise in the air without any sense of motion, the partial darkness in the clouds, and the bright sunshine behind them—the solemn stillness which reigns above the reach of earthly noises—the extensive views of landscape obtained at great elevations, and the extreme personal discomfort felt above an altitude of four miles. The lecturer then gave a very short and rapid account of the several ascents made, and came to the memorable voyage on September 5, 1862. On this occasion the balloon is supposed to have reached the height of 29,000 feet, but, on account of Mr. Glaisher's temporary insensibility, no observations were made above 26,000 feet. At this height his arms became powerless, his head dropped on his shoulder, he was unable to speak, he lost his eyesight, and finally sank down in the car of the balloon insensible. On the descent of the balloon, Mr. Glaisher soon recovered his strength and consciousness. The scientific results of these ascents have been but small; the lowest temperature recorded in the ascent of September 5, was —5°, at a height of five miles. The rate of decrease was found to vary with the condition of the sky, being much more rapid in a cloudy than a clear one, and by no means uniform in either; so the idea that temperature decreased 1° for every 300 feet of elevation must be abandoned. The humidity of the atmosphere was also found to vary according as the sky was clear or cloudy, and in a clear sky, at a height of five miles, there was found to be scarcely any moisture at all. Sound passed very readily to a great height, the bark of a dog and the note of a railway whistle being heard clearly at an elevation of two miles. The aneroid barometer read correctly with the mercurial to seven inches. Mr. Glaisher passed very rapidly over these results, announcing that the tables of temperature and other things were in the hands of the printer, and would soon be distributed. In conclusion, the lecturer paid a well-merited compliment to Mr. Coxwell, and expressed his opinion that a balloon ascent was not safe beyond three miles, stating that above this height the personal distress experienced was so great that it became difficult to make accurate observations.

NOTICES OF BOOKS.

On Chronic Alcoholic Intoxication. By W. Marcet, M.D., F.R.S. Second Edition. John Churchill and Sons. 1862.

This work, which has reached a second edition, describes, in the first place, the symptoms and effects of the long-continued use of alcoholic stimulants in the human subject. The author distinguishes between delirium tremens, which is an acute and violent disturbance of the nervous system, and what he terms *chronic alcoholism*, which is a more or less disordered state of the brain, nerves, muscles, and stomach, brought on by the continual use of alcohol, even without intoxication being once produced. Having fully described the symptoms of the disease, Dr. Marcet gives an interesting account of the therapeutical effect of oxide of zinc in allaying and removing them. From his experiments on a large number of patients, oxide of zinc appears to be the true antidote in cases of alcoholic poisoning. It seems to act as a strong tonic on the nerves, being, at the same time, a powerful sedative and anti-spasmodic. In doses varying from two to ten grains per diem, it is also a valuable remedy in epilepsy, chorea, hysteria, paralysis, and lead palsy. The peculiar sleep-producing properties of zinc compounds cause this metal to stand apart from all others with whose therapeutical qualities we are acquainted. In recommending this book to our medical friends, let us also call their attention to the fact that, although chemists have discovered and described more than half a hundred metals, hardly more than half a dozen have been used for internal exhibition in human disorders. The fields and forests of the world are being ransacked by numberless intelligent pharmaceutists in search of new remedies. Let some of our readers follow their good example with regard to the mineral kingdom, and endeavour to add new remedies to our very small array of metallic curative agents.

NOTICES OF PATENTS.

2761. *Treating Peat to Render it Useful as Fuel, and for Illuminating and Metallurgical Purposes.* G. Evans, Gloucester Terrace, Portman Square, London. Dated November 2, 1861.

This invention consists, in the first place, in depriving raw peat of the greater part of its moisture, so as to render it fit for a variety of practical purposes. The method of preparation for fuel is carried out by mixing with the saturated peat a quantity of wood charcoal, or other matters of a dry and absorbent nature, which by quickly abstracting its water will permit of the mass being moulded into any required shape, while the materials so added will not impair, but rather improve, the qualities of the peat. If required to be used for metallurgical purposes, the patentee adds ground quicklime or earthy minerals, which may afterwards serve as fluxing agents; or, if it is intended to be used in the smelting furnace, the dressed metallic ores may themselves be incorporated with the moist peat, and the fluxes being added will supply at once all that is necessary for charging the furnace and effecting the reduction of the ore.

For the manufacture of illuminating gas the inventor mixes the peat with coal tar, bituminous shale or the oil extracted from the same, petroleum oils, or any other cheap hydro-carbon, adding, if needful, a small proportion of quicklime to aid in the removal of water. Such materials are suitable for the production of lighting gas in retorts, and apparatus of the ordinary construction.

2800. *Preparing and Treating India-rubber and Gutta-percha.* W. A. Shepard, Pall Mall, London. Dated November 7, 1861.

In the preparation of gutta-percha or India-rubber by this process, the natural gum is purified in the first instance from gritty or extraneous particles by washing in the usual manner. The material is then thoroughly masticated and afterwards introduced into an iron receiver, in which a vacuum is created, and the air thus extracted from the pores of the gum. Sulphur vapour is now admitted to the chamber, or a proportion of solid sulphur, never exceeding an ounce to a pound, may be incorporated by grinding with the gum, previously to its exposure in the exhausted receiver. In order to effect partial vulcanisation the well-kneaded material is exposed for three or four hours to a degree of heat which may vary between 300° and 450° Fahrenheit, according to the quality of the gutta-percha or India-rubber. In some cases the employment of steam in a super-heated condition will prove advantageous. The gum is now once more kneaded or masticated to bring it into a suitable condition for mixing with pigments such as one of the following:—Prepared chalk or gypsum, white lead or zinc, magnesia or steatite, graphite, emery, pitch, boiled oils of various degrees of consistence, or colouring materials according to the class of goods to be manufactured therefrom. Along with one or more of the before-mentioned pigments, must always be added an additional proportion of sulphur, hyposulphite of lead, or of an equivalent compound of sulphur.

The patentee claims also the mode of effecting the vulcanisation of gutta-percha by heating in a vacuum, by which means a more compact and durable material is said to be produced.

CORRESPONDENCE.

The Cavendish Society.

To the Editor of the Chemical News.

Sir,—I am glad to see that some notice is being taken of the condition of the Cavendish Society. I hate, on any occasion, calling attention to accounts, but there is something so odd in those of the Society, that I shall be glad if you can explain it. In 1855, two volumes were supplied to the subscribers, and I see, in the balance-sheet for 1855-6, the following items:—

Printing and engraving	£264
Paper (with duty, remember) . .	74

For the last three years we have had but one volume a-year, and, in the meantime, the paper-duty has been removed. I see, however, in the balance-sheet for 1860-1, the following charges:—

Printing	£319	0	0
Paper	117	6	6

I do not understand how the paper for one volume can cost nearly twice as much as for two, nor the printing for one a good deal more than two. You probably know something about the cost of paper and printing, and will, perhaps, explain this.—I am, &c. A Subscriber.

[Our correspondent had better attend the annual meeting of the Society when it is held.—Ed. C. N.]

MISCELLANEOUS.

THE NEW GUN METAL.

A letter in the *Times* from one of our most distinguished metallurgists, signed with the well-known "Y," gives some interesting particulars respecting the new gun metal lately invented in Austria by Baron von Rosthorn. Before giving any account of this new alloy, the writer states his opinion that the days of wrought iron are numbered, and that its place will be soon supplied by steel in some form or other. The new alloy, which has received the name of "sterrometal," from a Greek word signifying tough or firm, is composed of copper, spelter, iron, and tin, in proportions that may be slightly varied without much affecting the result. In colour it resembles brass rather than gun metal; it is very close in its grain, and free from porosity. It is possessed of considerable hardness, and will take a very fine polish. Several eminent Vienna engineers have tried it for the cylinders of hydraulic presses with great success. Two specimens of the alloy have been submitted to rigorous tests by the Polytechnic Institute of Vienna and the Imperial Arsenal. The proportions used in each case were the following:—

	Polytechnic Institution.	Imperial Arsenal.
Copper . . .	55·04	57·63
Spelter . . .	42·36	40·22
Iron . . .	1·77	1·86
Tin	0·83	0·15
	100.000	99.86

The specimen tested at the Polytechnic Institute gave the following results per sectional inch (English):—A bar, prepared by simple fusion, bore a weight of twenty-seven tons. Forged red-hot it broke at thirty-four tons. Drawn cold, at thirty-eight tons: the figures in the case of the specimen tried at the Imperial Arsenal being twenty-eight, thirty-two, and thirty-seven tons respectively; while the best English gun metal, containing ten per cent. of tin and ninety per cent. of copper, broke at eighteen tons under similar circumstances. The specific gravity of the metal is about 8·37 when forged hot. These results, which are official, are truly astounding when we consider that the average breaking strain of wrought iron, as given by Mr. Anderson, of Woolwich Arsenal, is only twenty-six tons, whilst that of the best steel is only thirty-five tons per sectional inch. The elasticity of the sterrometal is also very great. It may be stretched $\frac{1}{600}$th of its length without undergoing permanent elongation; gun-metal giving only $\frac{1}{1590}$th, and wrought-iron $\frac{1}{1500}$th. No surprise is, therefore, felt when we are told that a tube of sterrometal is capable of resisting a pressure of 763 atmospheres, a tube of wrought-iron of similar size and form giving way under 267 atmospheres.

Quoting Mr. Anderson, the writer concludes by saying that the best alloy for guns is yet to be discovered. It seems to us, however, that sterrometal is very near perfection. The subject of alloys is one that, with constant and persevering experiment, must yield most valuable results, and we strongly advise any young chemist desirous of laurels and fortune to take up the matter. It seems singular that, with all our boasted knowledge of chemistry and metallurgy, there are but half-a-dozen alloys that may be turned to economic uses.

THE OIL SPRINGS OF AMERICA AND CANADA.

The transactions during the past month have been very large, though prices of refined in particular have, as will be read below, been very capricious. The accounts from all directions continue to show extraordinary development, and petroleum in every form attracts universal attention.

The large number of refiners that are commencing in this country suggest the query as to whether they will be able to distil to compete with the Americans or not. The scope is so great, the demand so general, and public criticism becoming so correct, that it must be admitted that, where there is real merit, appreciation will be found for both. At present, however, the American refining is so incomparably superior to the British, as to leave the former in absolute possession of every market in Europe.

Crude.—Early January found American nominal at 21*l.* to 22*l.*, since which gradually lowering prices were accepted until we reached 17*l.* 10s., under which there are good buyers now. The same date found Canadian nominal at 13*l.* 10s., but it also gave way, and even 10*l.* was accepted. The nearest quotable price to-day, 10*l.* 10s. The sales of both kinds for the month about 4000 casks.

Refined.—The new year found the best quality current at 2s. 3d. Advices of large shipments being made dulled the market, however, to 2s., which dullness was further accelerated by some holders determining to sell, with a prospect of a spring or summer replacement. Large transactions took place, even at 1s. 9d., when buyers coming in very spiritedly, a reaction again to 2s. resulted. There was little permanency in the advance, and the market became unsettled. Pending the arrival of the *Cheshire* and the *Adelaide*, 1s. 11d. to 1s. 9d. were again accepted; but on these vessels turning up, and their respective cargoes coming to the two ablest holders, prices at once advanced to 2s. and 2s. 3d., with large sales at the former, retail sales at the latter. The end of the month sees prices once more at 2s., and at this date there is a strong drooping tendency, with great cessation of demand.

Benzine is 1s. 9d. to 2s., according to quality, with a good inquiry, and every symptom of a large spring business.

Wax.—Two or three parcels of pressed white are for sale this week, the value of which is 9d. to 10d. per lb.

Lubricating Oils.—Some consignments to hand, of which more again.—*Alex. S. Macrae, Liverpool.*

A Martyr to Science.—It is with real pain that we have to announce to our readers the death of Mr. Lucas Barrett, the distinguished naturalist, who was accidentally drowned whilst investigating the structure of some coral reefs at Port Royal, Jamaica. All who visited the Jamaica Court at the International Exhibition will remember the enthusiasm and painstaking kindness with which this gentleman was ever ready to show and explain the various mineral and geological specimens collected and exhibited by him. Although one of the most active of the Jamaica Commissioners, he still found time to officiate as one of the local secretaries of the British Association, besides keeping a term at Cambridge. Before returning to Jamaica to renew his researches as one of the chief members of the West Indian Geological Survey, he ordered a diving dress and pumping apparatus of the latest and most scientific construction, for the purpose of personally examining the rocks and coral reefs lying in the neighbourhood of most of the West India islands. He first tried this dress at Port Royal, on December 17, in shallow water, and was so well pleased with the result that he determined to give it a trial in deeper water. Two days afterwards he took with him his servants and boat's crew, all of whom were negroes, and descended into the deep water between the reefs. The men in the boat continued to pump without intermission as on the former occasion, but they noticed that he remained longer in the water than usual. Suddenly, to their horror, they saw him floating on the surface at a little distance from the boat. They got to him as quickly as possible, but all was over. The cause of his death will remain a mystery. He was not drowned by the influx of the water, as the diving-dress contained only air. The only explanation to be given is, that the air exit valves became permanently closed in some mysterious manner; but even this seems open to doubt, as the men continued to pump without interruption. Mr. Barrett was only twenty-five years of age when he died; and the enormous amount of valuable work done by him during his brief career, gave promise of his speedily becoming one of the chief ornaments of the science he so ardently loved, and to the too enthusiastic pursuit of which he fell a victim. For three years before his engagement on the West Indian survey, he delivered most of the geological lectures for Professor Sedgwick, and was made by him curator of the Woodwardian museum at that university. His collection of *Radiata* in that Museum is one of the finest in the world. His loss to science will be felt severely, not merely on account of his own personal exertions in the cause of truth, but from the enthusiasm he communicated to those who had the privilege of his acquaintance.

Spontaneous Decomposition of Chloride of Lime.—About three years ago Dr. Hoffman mentioned the case of a choice specimen of chloride of lime in his laboratory undergoing spontaneous decomposition, and bursting the bottle to pieces. (Chemical News, vol. ii., p. 243). Another instance of this decomposition occurred in Dr. Letheby's laboratory. The specimen of chloride was perfectly dry, and the bottle had not been opened since June last. While making an attempt to loosen the stopper it was projected with great violence. The residual gas was instantly secured by covering the bottle, which was not broken. It was colourless, there was no odour of chlorine, but when tested by a match with spark it was re-lighted several times—an experimental proof that the result of the decomposition was oxygen.

SCIENTIFIC SOCIETIES.

MEETINGS FOR THE ENSUING WEEK.

February 16. *Monday.*

Asiatic—5, New Burlington Street. 3 p.m.
Medical—32a, George Street, Hanover Square. 8.30 p.m.

17. *Tuesday.*

Royal Institution—Albemarle Street. 3 p.m. Professor Marshall, "*On Animal Mechanics.*"
Civil Engineers—25, Great George Street, Westminster. 8.30 p.m.
Statistical—12, St. James's Square. 8 p.m.
Pathological—55, Berners Street. 8 p.m.
Ethnological—4, St. Martin's Place, Trafalgar Square. 8 p.m. Mr. Crawfurd, "*On the History of the Gypsies;*" by Dr. Shortt, "*A Brief Account of the Yenadies of the Chingleput District.*"

18. *Wednesday.*

Society of Arts—John Street, Adelphi. 8 p.m. A. N. Shaw, "*On the Best Means of Promoting the Growth and Improving the Quality of Cotton in India.*"
Geological—Somerset House. 8 p.m.

19. *Thursday.*

Royal Institution—Albemarle Street. 3 p.m. Dr. Frankland, "*On Chemical Affinity.*"
Royal—Burlington House. 8.30 p.m.
Chemical—Burlington House. 8 p.m.
Linnæan—Burlington House. 8 p.m.
Antiquaries—Somerset House. 8.30 p.m.
Royal Society Club—St. James's Hall. 6 p.m.
Zoological—11, Hanover Square. 4 p.m.
Numismatic—13, Gate Street, Lincoln's Inn Fields. 7 p.m.

20. *Friday.*

Royal Institution—Albemarle Street. 8 p.m. Rev. G. Williams, "*On Recent Discoveries at Jerusalem.*"
Geological—Somerset House. 1 p.m. Anniversary.
Philological—Somerset House. 8 p.m.

21. *Saturday.*

Royal Institution—Albemarle Street. 3 p.m. Prof. Max Müller, "*On Language.*"

ANSWERS TO CORRESPONDENTS.

⁂ All *Editorial Communications* are to be addressed to the Editor; and *Advertisements* and *Business Communications* to the Publisher, at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

Received.—M. P. S., next week; Quero; A Subscriber, &c.

W. W.—It is a solution of permanganate of lime.

G. H. F.—1. Answer next week. 2. Bisulphide of carbon will dissolve sulphur. Thanks for suggestions.

A. Z. F.—We are obliged to our correspondent. As far as possible we report all that we think will interest our readers.

G. C. C.—We are not at present in possession of the information. None but those practically engaged in the operation could give it.

E. P.—Evaporate the solution to dryness, powder the residue finely, mix with an equal weight of litharge, and fuse at a strong red heat. A button of lead and gold remains, from which the lead may be dissolved by warm nitric acid. The gold then remains as a loose, spongy mass.

THE CHEMICAL NEWS.

Vol. VII. No. 168.—February 21, 1863.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Researches upon Artificial Colouring Matters.—On the Composition of the Blue Colours Derived from the Tertiary Monamines of the Chinoline Series, by A. W. Hofmann, *F.R.S., Ph.D.*

The chemists who visited the International Exhibition will not easily forget the magnificent collection of products exhibited in the French Court by M. Menier, of Paris. Among those compounds, as remarkable for variety as for beauty, some superb crystals of cyanine, rivalling in brilliancy the acetate of rosaniline of Mr. Nicholson, especially attracted attention. M. Menier, who has produced this colouring matter upon a very large scale, has kindly put at my disposal some of these most beautiful crystals, in the hope that a detailed examination might lead to a method which would give stability to this new colour, unequalled for brilliancy and purity of tint by any other blue recently derived from coal-tar. The composition and mode of formation of the cyanine being unknown, I have seized with avidity the opportunity of examining this interesting substance. I ought to mention that, in a practical point of view, my experiments have failed; nevertheless, during the course of my studies of the nature of this until now obscure substance, I have been led to the observation of some facts which may be useful in its history, and which I take the liberty of submitting to the Academy.

The discovery of blue compounds derived from chinoline and its homologues dates as far back as 1856. At that time, M. Greville Williams renewed the examination of the bases extracted by M. Runge from coal-tar, and obtained by Gerhardt from the alkaloids of the cinchona barks, the identity of composition of which I recognised in one of my first researches. Amongst the numerous compounds of these bases so carefully examined by M. Williams,* were also found the methylic and ethylic derivatives, especially the iodide of methylleucolyl-ammonium, which I obtained in my research on the action of iodide of methyl on ammonia and its analogues. It was in this compound of chinoline, obtained by distillation from cinchonine, and in separating the corresponding oxide to this iodide by means of oxide of silver, that M. Williams first observed the magnificent coloration which has led to the discovery of this new tinctorial matter, now known under the name of "cyanine."

Phenomena exactly similar were observed later, by M. Von Babo, in treating chinoline with the sulphates of methyl and ethyl, and the colouring substances thus produced were named by this chemist, "methyl" and "ethyl-irisines." M. Williams was inclined to attribute the formation of this new substance, in which he recognised distinctly basic properties, to a process of oxidation, M. Von Babo, although with much reserve, represents the substances which he obtained by the improbable formulæ, $C_{22}H_{14}N_2O_4$ and $C_{26}H_{18}N_2O_4$. Since this time, strange to say, no effort has been made to establish, by further investigation, the composition of these singular bodies. Indeed, several years slipped by without these remarkable phenomena again attracting the attention of chemists, until the development of the manufacture of aniline and its products drew from oblivion reactions which have since awakened general interest among dyers. M. Williams has shown, that among the colouring compounds obtained by the action of the iodides of the alcohol radicals upon bases of the chinoline series, that obtained by the action of iodide of amyl is particularly remarkable for its tinctorial power. He gave a very interesting description of it,† as well as precise details as to the manufacture of this body, and it was soon after known in commerce as cyanine. Unfortunately, the dye produced by cyanine is even more fugitive than brilliant, and the hopes then entertained as to its commercial future have not yet been realised. Nevertheless, the importance attached by dyers to M. Williams' discovery is sufficiently shown by the offer of a prize of 10,000 fr., made by the Industrial Society of Mulhouse, for the discovery of a means of fixing the colour produced by cyanine.

The crystals given me by M. Menier were prisms sufficiently well formed for a crystallographical examination. They are at the present moment in the hands of M. Quintino Sella. Their faces glisten with a metallic green lustre with a golden reflection, which, along with their form, distinguishes them from the acetate of rosaniline, which they resemble in all other points. The crystals are nearly insoluble in ether, with difficulty soluble in water, but they easily dissolve in alcohol. The solution is of a deep blue colour, with a coppery lustre on the surface. The addition of an acid destroys this colour; ammonia and the fixed alkalies seem not to alter it, but give place to a deep blue precipitate, the filtrate from which is colourless. The green crystals were recognised as the iodide of a particular base; the iodine is retained with tenacity, but it may be precipitated from the alcoholic solution by oxide of silver, which sets free the base. In like manner, the iodine may be replaced by bromine and chlorine, when the solution is treated by the bromide or chloride of silver. The analysis gave results leading to the formula $C_{60}H_{39}N_2I$, which was further confirmed by the examination of a platinum salt crystallising in small rhomboidal tables. They were obtained by precipitation with bichloride of platinum of the chloride corresponding to the iodide in presence of an excess of hydrochloric acid. Nevertheless, a slight difference between the theoretical values of the formula and the results of the experiment induced me to admit in the crystals the presence

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* *Trans. Royal Society Edinburgh*, vol. xxi., part 3.

† See Chemical News, October 20, 1860, vol. ii., p. 219.

of an homologous compound containing less carbon and hydrogen, $C_{56}H_{35}N_2I$. This hypothesis, little encouraged at first by the remarkable stability in composition of the iodide, even after three or four crystallisations, was entirely confirmed when the chloride was submitted to a methodical fractional precipitation by bichloride of platinum. After several repetitions of this process, there was obtained two platinum salts, of which the one, the least soluble, was recognised as the pure platinum salt corresponding to the iodide with sixty equivalents of carbon, whilst the other was sufficiently pure to be recognised as, in fact, belonging to the homologous iodide with four equivalents of carbon less. The quantity of iodide, $C_{56}H_{35}N_2I$, which soils—if one may apply such a term to so beautiful a substance—the iodide, $C_{60}H_{39}N_2I$, is so small in quantity, that its presence does not materially influence the results obtained in the ultimate analysis of the compound. There is no difficulty in explaining the formation of this iodide. The substance is evidently derived from lepidine, $C_{20}H_9N$, so that the extremely small proportion of the other iodide is due to the presence of a small quantity of chinoline, $C_{18}H_7N$, in the volatile bases operated upon. Indeed, M. Williams, in describing the preparation of cyanine, distinctly observes, that the chinoline does not require to be absolutely pure for the success of the operation. Besides, M. Menier wished, also, to furnish me with a considerable quantity of the crude product of the distillation of cinchonine, and from which the green crystals were obtained at his manufactory. The latter (the crude product) was recognised as a mixture of several bases, in which the presence of lepidine and chinoline was ascertained, without the slightest difficulty, by the analysis of the platinum salts. It is necessary to distinguish two different phases in the formation of the new iodide. First, the transformation of the lepidine into iodide of amyl-lepidyl-ammonium,—

$$\underbrace{C_{20}H_9N}_{\text{Lepidine.}} + \underbrace{C_{10}H_{11}I}_{\text{Iodide of amyle.}} = \underbrace{C_{30}H_{20}NI}_{\text{Iodide of amyl-lepidine.}}$$

Secondly, the condensation, by potash, of two molecules of this compound into one molecule of a higher order—

$$2C_{30}H_{20}NI + KO,HO = C_{60}H_{39}N_2I + 2HO.$$

It was indispensable to verify these reactions by the analysis of additional compounds. The green crystals dissolve easily in boiling dilute hydriodic acid. The colourless solution deposits on cooling yellow crystals of remarkable beauty, the analysis of which furnished numbers corresponding to the formula—

$$C_{60}H_{40}N_2I_2 = C_{60}H_{39}N_2I,HI.$$

These crystals are isomeric with iodide of amyl-lepidyl-ammonium, from which they, nevertheless, differ in all their properties. They dissolve in cold water without decomposition, but in treating them with hot water or alcohol the blue colour immediately re-appears, the mono-acid compound being re-produced. The same change takes place at 100°, so that, in preparing the substance for analysis, it must be dried *in vacuo*.

By the facility with which its di-acid compounds change into mono-acid salts, this substance resembles rosaniline, which, as I before remarked, also forms unstable, colourless salts. The green iodide dissolves easily also in hydrochloric and hydrobromic acids, yielding perfectly colourless solutions, which deposit well crystallised salts containing respectively, besides iodine, chlorine and bromine. In submitting the green iodide, dissolved in alcohol or aqueous hydrochloric acid, to the action of chloride of silver, all the iodine separates in the form of iodide. There is produced a blue solution which, when slowly evaporated, deposits the mono-acid chloride in green prisms, with a metallic lustre and of perfect beauty, containing $C_{60}H_{39}N_2Cl$. Dissolved in hydrochloric acid, these prisms furnish a di-acid compound, which, after a long evaporation *in vacuo*, separates in straw-yellow needles. The very deliquescent character of this substance hindered me from analysing it; but if there existed any doubt that this compound contained

$$C_{60}H_{40}N_2Cl = C_{60}H_{39}N_2Cl, HCl,$$

it would be removed by the analysis of a beautiful platinum salt, difficultly soluble, crystallising from an alcoholic solution containing much hydrochloric acid in small rhomboidal tables, which are deposited whenever bichloride of platinum is added to the solution of the di-acid chloride, and which were ascertained by analysis to contain $C_{60}H_{40}N_2Cl_2,2PtCl_2$. I prepared likewise a beautiful gold salt. The mono-acid bromide deposited in fine needles, easily crystallisable, and the di-acid sulphate, a magnificent colourless salt, was obtained without difficulty in perfectly developed rhombic tables. I did not multiply the analyses, because I was fortunate enough to observe a reaction which thoroughly corroborated the anylytical results. Remembering the simple breaking up which I had noticed in exposing the iodide of tetrethyl-ammonium, which changes this compound into iodide of ethyl and tri-ethylamine, I was induced to submit to distillation the green iodide which forms the starting-point in these investigations. These crystals fuse easily into a blue liquid with a shining coppery surface. If the temperature be raised, decomposition takes place, and a mixture of lepidine and iodide of amyle condenses in the receiver, the return of which into the state of iodide of amyl-lepidine ammonium may be prevented by receiving them in hydrochloric acid. At the same time, there is disengaged a gas which burns with a brilliant white flame, condensable by bromine, and which, by means of a worm surrounded with ice, was easily condensed into a very volatile liquid. There was a sufficient quantity of this hydrocarbon obtained to determine its boiling point, which corresponded with that of pure amyline. If the heat is carefully applied to the retort, the quantity of carbon remaining is comparatively small. The observed phenomena are explained by the following equations:—

$$C_{60}H_{39}N_2I = 2C_{30}H_9N + C_{10}H_{11}I + C_{10}H_{10}.$$

At this point I proved also in the green crystals the presence of the homologous chinoline compound. Indeed, after having separated from the iodide of amyle the hydrochlorate of the volatile base, and distilling with potash, and receiving aside the first portion of the base which comes over with the vapour of water, I ascertained, by aid of the platinum salts, that that substance consisted of chinoline; whilst the portion distilling towards the end was recognised by analysis to be pure lepidine.

The results obtained in these experiments furnish a new illustration of the tendency to molecular accumularion which distinguishes the ammonias and their derivatives. A few weeks ago I had the honour to present to the Academy a note on a compound of this class obtained as a secondary product in the manufacture of aniline. The coloured derivatives of the bases of the chinoline series present striking analogies in composition to paraniline.

Aniline Series.

$$\underbrace{C_{12}H_7N}_{\text{Aniline.}} \qquad \underbrace{C_{12}H_7,HCl}_{\text{Chloride.}}$$

$$\underbrace{\left.\begin{matrix}C_{12}H_7N\\C_{12}H_7N\end{matrix}\right\}}_{\text{Paraniline.}} \qquad \underbrace{\left.\begin{matrix}C_{12}H_7N\\C_{12}H_7N\end{matrix}\right\}HCl}_{\text{Mono-acid chloride.}} \qquad \underbrace{\left.\begin{matrix}C_{12}H_7N\\C_{12}H_7N\end{matrix}\right\}\begin{matrix}HCl\\HCl\end{matrix}}_{\text{Di-acid chloride.}}$$

Amyl-lepidine Series.

$$\underbrace{C_{30}H_{19}N,2HO}_{\substack{\text{Hydrate of amyl-lepidine}\\\text{ammonium.}}} \qquad \underbrace{C_{30}H_{19}N,HCl}_{\text{Chloride.}}$$

$$\underbrace{\left.\begin{matrix}C_{30}H_{19}N,2HO\\C_{30}H_{19}N,2HO\end{matrix}\right\}?}_{\substack{\text{Blue base in free}\\\text{state.}}} \qquad \underbrace{\left.\begin{matrix}C_{30}H_{19}N\\C_{30}H_{19}N\end{matrix}\right\}HCl}_{\substack{\text{Mono-acid green}\\\text{chloride.}}} \qquad \underbrace{\left.\begin{matrix}C_{30}H_{19}N\\C_{30}H_{19}N\end{matrix}\right\}\begin{matrix}HCl\\HCl\end{matrix}}_{\substack{\text{Di-acid yellow}\\\text{chloride.}}}$$

I have written the formulæ of the coloured derivatives so as to fully explain their analogy with those of the paranilic series,—in one word, to characterise them as salts of the para-amyl-lepidine-ammonium. But I am far from attributing to these formulæ any other value. Indeed, the molecular construction of this new class of compounds remains to be established by other experiments. It is evident that the theory which, in 1852, represented in a very satisfactory manner the constitution of the nitrogenised bases then known, requires to be extended, in order to embrace the tinctorial ammonias which have come to our knowledge during the last ten years. The time is not yet arrived to announce the amplification of these generalisations. I wish also to mention here, that I have not omitted to examine the corresponding oxide to these salts. In treating an alcoholic solution of the iodide by oxide of silver, the base is set at liberty, and separates by evaporation as an indistinctly crystalline mass of a deep blue, soluble in pure water, more easily soluble in alcohol, insoluble in anhydrous ether. The base is precipitated by means of ether from the alcoholic solution. I have not yet analysed it. Submitted to distillation, the free oxide gives birth to an oily base, which I expected to be lepidine, but it appears to be a different compound, in the examination of which I am engaged. It forms a series of crystalline compounds. Having gone so far, I wished to examine for myself the formation of the remarkable substance the nature of which I have tried to investigate. With this end, I prepared the compounds obtained by the action of the iodides of methyle and amyle upon chinoline and lepidine bases, put at my disposal in sufficient quantity by my friend, Mr. David Howard. I have not examined in detail the substances thus produced. I was satisfied by recognising that the general nature of these reactions was sufficiently illustrated by the study of the amyl-lepidine compounds. As for the complicated secondary phenomena which take place in this process, and specially the simultaneous formation of a red colouring matter, I have nothing to add to the perfect description which has been given by the distinguished chemist to whom we owe the discovery of these reactions.

Allow me, in conclusion, to express my sincere thanks to M. Menier. Without the magnificent products obtained in his manufactory, I could never have been able to investigate this question. Science, although proud to guide industry past obstacles which arrest it, always acknowledges, without blushing, that there are times when she cannot advance without the support offered to her by her vigorous companion. The many researches of this kind cannot but fail to seal the union between industry and science.—*Comptes-Rendus*, December 15, 1862.

[Translated expressly for the CHEMICAL NEWS by Mr. John Binning, member of the Chemical Society of Paris.]

Analysis of Cast-Iron and Steel—Detection of Sulphur and Phosphorus in these Metals, by M. J. NICKLES.

THE influence of the smallest quantities of sulphur and phosphorus on the quality of iron, which they render harsh and brittle, is well known. Their detection and estimation in iron is a problem often mooted, but hitherto unresolved, if we may judge from the numerous attempts still making to arrive at a simple, practical, yet exact, method of analysis.

The difficulties do not, however, inhere solely in the estimation of the sulphur and phosphorus, but arise from the slowness with which the iron dissolves in the various acid vehicles employed, and from the losses entailed by the operation, sulphur and phosphorus having a strong tendency to combine with hydrogen, and so form gaseous compounds.

The usual way to dissolve the iron, cast-iron or steel, is to reduce it to a fine powder, either by filing or by grinding in a steel mortar; a very long and fatiguing operation, but which is essential, in order to avoid the risk of introducing foreign matters into the powder, especially morsels detached from the instrument used to divide it, whether file or mortar. Moreover, a few decigrammes only of the substance are generally operated upon, a quantity, in most cases, insufficient.

I was much struck with these drawbacks on the occasion of an experiment I had to make on the composition of several specimens of cast-iron from a large factory in Lorraine—cast-iron which I found to be quite free from sulphur, though rich in phosphorus.

To obviate the causes of error I have enumerated, I endeavoured to find some vehicle energetic enough to dissolve iron, even in morsels of several grammes weight, without causing disengagement of gas, and, at the same time, to change the sulphur and phosphorus to their most stable states,—namely, to sulphuric and phosphoric acids.

These desiderata are attained by means of pure bromine, mixed with distilled water. At first—especially if the metal to be analysed is in presence of very little water—the liquid becomes heated to the point of emitting abundant bromine vapours. This reaction is to be avoided. It is prevented by the very gradual addition of the bromine, or, if this precaution has been neglected, by placing the flask in cold water. Towards the close of the operation, it must, on the contrary, be heated, which is easily done with a lamp or water-bath. This is nearly the same method which I indicated for the preparation of bromide of antimony and bromide of arsenic by the wet way (*Comptes Rendus de l'Academie des Sciences*, vol. xlviii., p. 837; and *Journal de Pharmacie et de Chimie*, vol. xxxvi., p. 161).

It should be stirred from time to time, in order to detach the surrounding layer of graphite from the metallic core, which, by its interposition between the iron and the solvent, slackens the action.

The intervention of heat is necessary only when time is an object; otherwise the reaction may be left to itself. If there is sufficient bromine present, the solution goes on without need of any supervision.

A piece of cast-iron, made with coke, of 15 grammes weight, containing 6 per cent. of graphite, has been dissolved in less than forty hours, without requiring more attention than five or six stirrings.*

In estimating the iron at the same time as the sulphur and phosphorus, it must be taken into consideration whether the estimation is by standard solutions, or by weight. In the first case, the iron must be in the proto state; then excess of bromine must be avoided; and before all the iron is dissolved, it is digested hot, in order to reduce as much as possible the persalt formed in the first instance; nothing then prevents finishing the reduction by sulphurous acid.

If, on the contrary, the metal is to be estimated by precipitation, &c., excess of bromine is indispensable to change all the iron to the state of sesquibromide; and this may be ascertained by means of red prussiate, which ought to give no precipitate of Prussian blue.

The degree of oxidation of the iron is of small importance in estimating the sulphuric acid produced by the oxidation of the sulphur contained in the metal experimented upon; but, on the contrary, it is of great importance in estimating phosphorus in the state of phosphate, on account of the protoxide of iron, which in this case is always precipitated. Sesquioxide is, no doubt, quite as easily precipitable by alkalies; but a property possessed by it to the exclusion of protoxide, and which is of great service in this instance, is the power of resisting the displacing action of alkaline oxides when in presence of a sufficient quantity of tartaric acid.

To a solution of sesquibromide tartaric acid or tartrate of ammonia is added, until a small quantity of this liquid can bear without result the addition of excess of ammonia.

When the experiment has reached this point nothing more is required than to supersaturate the liquid with ammonia,† add sulphate of magnesia, then a certain quantity of alcohol, and then stirred and left at rest during the night; the double phosphate is then deposited in the form of adherent microscopic crystals, generally clinging closely to the sides of the vessel; for this reason the operation should be conducted, not in a flask, but in a beaker, so as to get the precipitate, and to detach it from the sides.

The addition of alcohol is for the purpose of favouring the precipitation of ammonio-magnesian phosphate; this is because this double salt is soluble in the complex mother-waters in which it ought to form, since with bromine and iron they contain ammonia, tartaric acid, and, consequently, various ammoniacal salts.

A cloudiness, dispersed by shaking, appears on the addition of alcohol; this must be disregarded, and alcohol added till the cloudiness begins to become permanent, taking care at this point to discontinue adding it, so as not to induce the precipitation of substances foreign to the double phosphate we desire to obtain.

As to the sulphur, it is estimated in the state of sulphate of baryta, by means of chloride of barium, added after the bromoferric solution has been freed from graphite. The water in which the sulphate of baryta has been washed is added to the filtered liquid; then, by means of a few drops of sulphuric acid, or sulphate of ammonia, it is freed from any excess of barium it may contain. This solution is then treated with tartaric acid, in order to protect the sesquibromide of iron from the action of the ammonia; then it is supersaturated with carbonate of ammonia, and sulphate of magnesia is added, so as to obtain the ammonio-magnesian phosphate, which, treated by the usual methods, indicates easily the quantity of phosphorus to be abstracted—*Journal de Pharmacie et de Chimie.*

TECHNICAL CHEMISTRY.

On the Purification of Coke, by M. E. Kopp.

Though in very many cases coal and coke are already substituted for wood and charcoal, there are, nevertheless, certain metallurgic operations for which charcoal is still used, through fear of the impurities of the coke injuring the quality of the product. We need only cite as instances the blast furnaces, furnishing cast-iron with wood or charcoal, the fining of iron by charcoal, the preparation of what is called German shear-steel, by fining steely cast-iron, &c., with wood-charcoal. If, on the one hand, coke could be deprived of all its injurious components, such as sulphur, phosphorus, and the large proportion of earthy matters, and, on the other hand, if all the useful matters in which it is deficient could be added to it, such as carbonates, and alkaline chlorides and silicates, there can be no doubt that, thus purified and modified, it would in nearly every instance economically replace charcoal.

Innumerable methods of purifying and ameliorating coke have already been proposed; but some are almost inoperative, others too expensive, and others again would rapidly destroy the coke furnace. M. Scheerer (*Berg-w. Hüttenm.*, Zeit., 1854, No. 29) passes a current of water vapour through the furnace of red-hot coke, thus reducing the proportion of sulphur from 1·1 per cent. to 0·4 per cent. M. Bleibken damps the coal previously with milk of chalk or lime. (*Dingl. Journ.*, vol. cxlvii., p. 295), and so produces limed coke. MM. Claridge and Roper (*Rep. of Patent Inv.*, November, 1858, p. 375) also use water vapour, superheated (which seems useless in presence of coke, at almost white heat); others mix sea salt with the coal, or add it to the coke when incandescent in the furnace; and others advise the employment of fluoride of calcium, &c.

It is now some years since, in 1856 (*On the Continuous Preparation of Lighting and Heating Gas*), that we proposed to purify coke by very weak hydrochloric acid, and the addition of a little carbonate of soda to the coke, purified, washed, and dried.

In many places, and especially in Lancashire and Yorkshire, coke furnaces are actually in the immediate neighbourhood of manufactories of chemical products, or could without any difficulty be there established. These factories, by converting sea salt into salt of soda, produce an enormous quantity of hydrochloric acid, which is absorbed by and dissolved in water in condensing towers. There are usually two towers, one furnishing commercial concentrated hydrochloric acid, used in the preparation of chloride of lime, chlorate of potash, chloride of tin, &c.; the second draining off only very weak, watery, hydrochloric acid, because the condensation of the last traces of hydrochloric gas necessitates the use of a very great excess of water.

This weak acid is generally rejected, and turned off as waste, but it is just this waste which could be admirably utilised for the purification of coke.

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* This occurred in summer, at a temperature between 20° and 30° C.; the bromine being sufficient in quantity to form sesquibromide.

† To avoid obtaining too much liquid, I neutralise with solid carbonate of ammonia. A large crystalline precipitate is then formed. This precipitate is a slightly soluble bitartrate of ammonia, which disappears as the neutralisation advances, and the acid salt passes to the state of neutral tartrate, which we know is very soluble.

The process is simple enough. After the conversion of coal into coke in the usual way, the coke is taken red-hot from the furnace. In well constructed coke furnaces this is effected mechanically; the furnace is open at each extremity, and a very thick, shield-shaped plate of cast-iron, adjusted to a strong toothed rod of iron, which is propelled with great force by gears, gradually pushes before it, and finally expels from the furnace, in a single block, the whole of the prepared coke. The incandescent coke is generally extinguished by throwing a large quantity of water over it. This water we propose to acidulate with hydrochloric acid; or, simpler and better still, to let the incandescent coke fall into a reservoir, containing the slightly acid water flowing from the second condensing tower.

The incandescent coke may contain protosulphide of iron, proceeding from the pyrites, which at red heat have lost half their sulphur, sulphide of calcium proceeding from the reduction of sulphate of lime, phosphates, silicates, &c. By contact with the diluted hydrochloric acid, the sulphides of iron and calcium are transformed into chlorides of iron and calcium, with disengagement of sulphuretted hydrogen, which is carried away by the water vapours, escaping abundantly when the coke is extinguished. The phosphates are converted into soluble biphosphates, and even the silicates may be attacked; the bases dissolve as chlorides, a portion of the silica at the same time becoming soluble, which happens when a silicate is digested, hot, in a very diluted acid. Were the coke merely sprinkled with hydrochloric water, and allowed to dry immediately from the heat retained in it, even when the burning had ceased the chlorides, biphosphates, and silica would remain; but certainly the sulphuretted hydrogen, and with it nearly all the sulphur, would have been eliminated. But, on the contrary, if the coke were impregnated with acid solution, and were then washed with water, a large portion of the impurities specified would be expelled by the washing. The washed coke can then be dried, either in the air, or by the unutilised heat of the coke kilns. To impart to it still more the qualities of charcoal, it may be sprinkled with a weak solution, or powdered over with dry carbonate of soda, as free as possible from sulphate, about a few millièmes the weight of dry coke being sufficient.

The carbonate of soda, prepared by means of kryolite (double fluoride of aluminium and sodium), naturally containing no trace of sulphate, answers best for this purpose.

In most instances finely powdered kryolite may be used with advantage, and $\frac{1}{3}$ to $\frac{1}{2}$ per cent. added to the coke. Kryolite supplies economically the alkali which the coke lacks; and though the soda exists in this mineral in the state of fluoride, there seems no doubt as to its beneficial influence.

We have had occasion to make several trials in fusing cast-iron and steel with coke purified by weak hydrochloric acid, with addition of a little carbonate of soda, and the results were decidedly favourable, as compared with those obtained with ordinary unprepared coke.

Finally, we think that a series of experiments should be undertaken, to ascertain what effect kryolite used in blast furnaces would have on the yield and the quality of cast-iron, and in puddling furnaces on the properties of puddled steel. Kryolite is found in very large masses in Greenland, and the price will decrease as the applications of it become more numerous.—*Repertoire de Chimie Pure et Appliquée.*

PHARMACY, TOXICOLOGY, &c.

*Notes on the Extraction and Estimation of Some of the Crystalline Principles of Opium,** *by* R. Haines, *M.B., Professor of Materia Medica, Grant College, Bombay.*

(*Continued from page 67.*)

The precipitate by ammonia was boiled with rectified spirit. After cooling, the spirituous liquid, which contained the thebaia with a little narcotine, was separated from the undissolved portion, the spirit distilled off, the residue mixed with a slight excess of acetic acid, and then thrown into a large quantity of cold water. A copious deposit of resin was formed. After filtration, basic acetate of lead was added until the liquid had a faint alkaline reaction. A copious precipitate fell, containing, with oxide of lead, resin, narcotine, and papaverine. From the liquid the excess of lead was removed as sulphate by sulphuric acid; after filtration it was nearly neutralised with carbonate of soda, and then treated with solution of ammonia in slight excess. Thebaia, morphia, and resin were thrown down as a light brown precipitate. This was collected, dried, and powdered, boiled with spirit, filtered, the spirit distilled off, and the residue set aside in a capsule for some days. It became filled with crystals of thebaia and morphia. After pressing and re-crystallisation, the morphia was removed by digestion in solution of caustic soda, and the thebaia purified by solution in acetic acid, digestion with animal charcoal, precipitation with caustic soda, and repeated crystallisation out of alcohol.

The precipitate with basic acetate of lead was dried and boiled with spirit, the spirit distilled off, and the cooled residue, after separating the crystals of narcotine, mixed with dilute hydrochloric acid. After some days, the hydrochlorate of papaverine ought to have crystallised, but although a few crystals were observed at one time, when I was too busy to attend to them, they disappeared again, and could not be reproduced. I, therefore, varied the proceeding. The liquid was mixed with twice its volume of spirit, then with excess of caustic soda; ether equal in bulk to the alcohol was now added, and then with agitation water, until the ether separated, carrying with it papaverine and narcotine. The ether was evaporated, and the residue boiled with spirit; crystals of narcotine separated on cooling. The papaverine should now have been in the mother-liquor. This was evaporated, and a further crop of small crystals obtained, which were supposed to be the looked-for base. After they had been purified with a great deal of trouble, they weighed 20 grains; they were found, however, to be nothing but narcotine. I am inclined to think that the failure was owing rather to bad management in some stage of these complicated operations, than to the absence of papaverine in the opium.

The narcein obtained by me corresponded more closely with the description of this substance given by Pelletier than with that by Anderson. It is coloured of a beautiful azure blue by moderately strong sulphuric and hydrochloric acids, as described by the former, though the latter denies the coloration. Strong sulphuric acid dissolves it to a dark brown, almost black solution. I could not obtain the red colour with sulphuric acid, described by Anderson.

Estimation of Morphia.—On this subject an interesting paper was read by Dr. Giraud, in December,

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* From the *Transactions of the Medical and Physical Society of Bombay.*

1850, in which the advantages of O'Shaughnessy's process, as giving at one and the same operation the proportion both of morphia and of narcotine, were pointed out. I can, upon the whole, fully join in the praises bestowed upon this method; but, at the same time, I have found that, in the form practised by the author of that paper, it did not quite meet all requirements, especially in regard to the purity of the bases obtained by it. I have been, therefore, induced to make some little modifications, which, I think, leave it in a state as nearly perfect as possible. It will be recollected that the principles upon which it proceeds are—1st, the solubility of narcotine in a sufficient quantity of cold rectified spirit; and 2nd, the strong basic property of morphia, whereby it is capable of decomposing a salt of ammonia when boiled with it, setting the ammonia free, and combining with its acid. The method of proceeding which I have found most advantageous is the following (the quantity of opium to be used is immaterial; if sufficient is at hand, 1000 grains, as recommended by O'Shaughnessy, is a convenient quantity):—The opium is broken up into pieces as small as possible, and digested in five times its weight of rectified spirit in a stoppered bottle, with occasional agitation, for a couple of days. The tincture is strained through calico and pressed, and the marc transferred to a flask, which is to be closed with a stopper or glass plate. The calico is washed with half as much spirit as that first used, which is then digested with the opium for twenty-four hours more, after which the flask is placed in a water bath, the cork having been removed, until the spirit begins to boil. It is then removed from the bath, and when cool the liquid is strained through calico and thoroughly pressed out, being washed at last with a few drachms of spirit. In this way a complete exhaustion is effected, any remnants of narcotine being brought into solution at the boiling temperature. The liquid, if turbid, is filtered through paper, poured into a retort placed in a water bath, and solution of ammonia, in quantity sufficient to destroy completely the acid reaction, is added. Two drachms will be ample for this purpose; the use of so much as one ounce, formerly recommended, is quite unnecessary. If the spirit is very strong, a few drachms of water may be added, and the distillation proceeded with. When four-fifths of the spirit have been distilled off, the retort is removed,* its contents poured into a porcelain capsule, and the retort washed with a small quantity of rectified spirit, which is added to the rest. The capsule is covered and left for twenty-four hours. During the distillation, most of the alcohol is driven over, and a weak spirit, much under proof strength, remains, in which, when cold, narcotine is scarcely at all soluble. The mass is squeezed through linen, which is better than calico in this case, the crystals of narcotine washed several times with a little rectified spirit, and again pressed.

The liquors are united, and mixed with water equal to four times the weight of the opium employed. After twenty-four hours the clear liquid is strained off from the deposited resin, gently evaporated to one-fourth of its bulk, the remainder brought nearly to boiling, and mixed in a beaked tumbler with half its bulk of rectified spirit. Solution of ammonia is then added, until, after blowing away the air in the vessel, the smell of ammonia remains permanently perceptible, or until the air retains an alkaline reaction. In a few minutes flaky crystals of morphia make their appearance, and rapidly increase as the liquid cools. They adhere but little, and sometimes not at all, to the walls of the vessel. It is well, at the expiration of twelve hours, to stir the liquid smartly, which expedites the complete precipitation of the morphia. After twenty-four hours, the crystals are filtered off, washed, and dried. If they should be very much coloured, which is not often the case, they may be dissolved by warming with a little caustic soda or potash, the liquid diluted, filtered, heated, mixed with alcohol, slightly supersaturated with acid, and precipitated as before by ammonia.

The crystals of narcotine are treated with cold caustic soda or potash of the usual strength. This dissolves the adhering resin, but leaves the narcotine unaffected. After some hours, if the crystals are not quite colourless, the liquid may be heated to 180° for a short time, poured off from the crystals, and the operation repeated with fresh solution of soda. The liquid is then diluted with much water, and the narcotine allowed thoroughly to subside. The clear liquid is then poured off, water added, and the narcotine collected upon a paper filter, washed, and dried. The strong alkaline liquid clogs the paper too much to render immediate filtration practicable; hence the necessity of separating it, in the first place, by the subsidence of the narcotine. The crystals are thus obtained at once and without the trouble and loss of re-crystallisation, snow-white, notwithstanding the dark colour of the liquid in which they are formed, for the colouring matter is merely superficially adherent; they are also perfectly pure, every trace of morphia being removed by the alkali. The decolorisation of morphia is much more difficult, the colouring matter being incorporated with the substance of the crystals. It can only be removed entirely by the use of animal charcoal.

The process, it will be observed, differs from that employed by Sir W. O'Shaughnessy and by Dr. Giraud in the following points:—The extraction of the opium is effected in two macerations instead of one, the last time at a boiling temperature. The quantity of ammonia used is only a third or a fourth of that originally employed. The narcotine is washed with spirit instead of water, the immediate effect of the latter being to throw down quantities of resin upon the crystals in an insoluble form. The narcotine is not dissolved in acid, and re-precipitated by ammonia, which, in reality, does very little towards purifying it. I have repeatedly obtained the narcotine by O'Shaughnessy's process as an almost black resinous mass, adherent to the paper, and totally devoid of crystallisation. It is evident, therefore, that the results are not fully to be depended on. By the present method the trouble is less, and the result, as may be seen by the specimens on the table, very satisfactory. As to the morphia, the syrupy liquid which contains it is diluted with less water, but I find that the quantity I have mentioned is more than sufficient. Instead of adding the ammonia at once to the cold clear liquid, by which a highly impure and resinous morphia is thrown down, the solution is concentrated, mixed with alcohol, and precipitated hot with ammonia. The spirit keeps the resin in solution, and, aided by the effect of heat, retards the separation of the morphia, so that it has time to form distinct crystals. This has been previously pointed out by M. Fordos. The loss of morphia by solution in the spirit is perfectly inappreciable, and is more than compensated by the reduction in the bulk

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* It is essential that all the ammonia be driven off, otherwise, on the subsequent dilution of the liquid with water, some of the morphia will be precipitated with the resin and lost. The last portions of spirit should, therefore, be tested, and, if found alkaline, an additional quantity of pure spirit should be poured into the retort, and the distillation renewed until no more ammonia appears in the distillate.

of the liquid by previous evaporation. It is, I believe, an essential step in all precipitations of morphia.

(To be continued.)

PHYSICAL SCIENCE.

On a Particular Mode of Forming Bubbles of Liquid, *by* MM. Minary *and* Sire (*of Besancon*).

M. Felix Plateau, son of the celebrated Belgian physicist, has recently described a new way of forming soap bubbles, namely, by throwing in a layer, at an angle of about 45°, a solution composed of about one proportion of Marseilles soap and forty of water. Part of the layer thus formed, irregularly jagged at the edges, resolves itself into full drops, more or less voluminous, while another portion produces air bubbles, the diameter of which, though very variable, is sometimes as great as 7 centimetres.

The purpose of this notice is to call the attention of the Academy to an equally remarkable method of producing liquid bubbles, by pouring into a certain quantity of olive oil, once and a-half, or twice its volume of concentrated sulphuric acid, and stirring the mixture energetically with a glass rod.

The mixture being stirred in a conveniently large beaker, numerous little hollow bubbles immediately make their appearance, and flutter about in all directions. The largest (sometimes 1 to 2 centimetres in diameter) usually sink again into the mixture, after a feeble ascent; but the smaller ones exhibit their commotion by leaping into the air. Bubbles of great tenuity are thus produced, numerous according to the violence of stirring, and in the way which seems most favourable to their formation.

When, for a certain time, the bubbles have been suspended in the air, they assume the colours of thin plates, like soap bubbles. From the odour they emit on bursting they would seem to be filled with a mixture of air and sulphurous acid. It has, in fact, been proved that a certain quantity of the latter gas is disengaged from the mixture, the temperature of which is raised considerably at the commencement of the stirring, and so might give rise to the supposition that the disengagement of heat makes the bubbles ascend; but we have ascertained that the production of bubbles is quite as abundant, if not more so, when the mixture is stirred twenty-four, or even forty-eight hours after its composition. At the end of this time, and according to the quality of the olive oil, the mixture becomes so viscid that stirring becomes almost impossible, and the result negative; but the addition of a little sulphuric acid restores all its properties to the mixture.

The experiment succeeds with most liquid oils at the ordinary temperature; however, pure olive oil seems to yield most certain results.

On examining with a lens the bubbles which have floated some time, their surface will be found covered with little blisters, owing to the separation of the liquids constituting the mixture. When the large bubbles burst, a slight mist arises for a few seconds, formed of particles, which appeared spherical, and which we believed to be hollow, but without any proof on this head. On intercepting the passage of a few bubbles with litmus paper, they left a deep red impression, showing the presence of sulphuric acid in the envelope.

The mixture of oil and acid cannot be easily blown into bubbles at the extremity of a bell-mouthed tube; they can be blown scarcely more than 1 or 2 centimetres in diameter, and persist for a very short time; while M. Plateau's soap water and glyceric liquid, so favourable to the production of bubbles by insufflation, imprison in liquid films each gaseous bubble as it tends to escape, forming a froth, but does not abundantly disengage bubbles by stirring, like the acid mixture above described.

The distinguished Professor of the Ghent University, who in his son's experiment sees evidence of the vesicular nature of the vapour of clouds, does not depreciate the difficulty of admitting the previous formation of a liquid sheet. Moreover, the formation of vesicles rising above our acid mixture, and determined by the gaseous bubbles arriving at its surface, is not easy of explanation. We now confine ourselves to a simple statement of the fact, and of the principal circumstances accompanying it, reserving the arguments which seem to us to elucidate this obscure point in molecular physics, with the intention of submitting them, at some future time, to the consideration of the Academy.—*Comptes-Rendus.*

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

Friday Evening, February 13.

Dr. Frankland delivered a lecture on "*Artificial Illumination.*" He commenced by stating that it was ten years since he delivered a lecture on the same subject at the Royal Institution. In the interval very little improvement had taken place in the means of producing artificial light; but recently a new illuminating agent had been introduced, and the magneto-electric light had received an important application. The lecturer exhibited and described the electric light; and went on to speak of Dr. Faraday's discovery of the magneto-electric spark, and its application to lighthouse illumination. Professor Holmes' machine, he said, had been in operation at the South Foreland lighthouse for twelve months without break or failure. The electric light, however, was of no use for domestic purposes; its cost was too great, and it was only available where light of the greatest intensity was required. Some improvements, in the form of mechanical contrivances for keeping the carbon poles at proper distances, have been introduced, as instanced in the clockwork arrangement in Dubosc's lamp; and the electric light had received a modification in the substitution by Mr. Way of mercury for the carbon poles. The light emitted from the mercury poles Dr. Frankland showed to be far inferior to that emitted from the carbon points. He stated that it only amounted to one-twentieth; but the battery used was not the best adapted to procure a good light, as it required quantity rather than intensity.

In gas manufacture very little improvement had been made in these ten years. Sulphur was still found in it in considerable and, perhaps, undiminished quantity. It was present in the form of bisulphide of carbon, which was irremovable by the ordinary modes of purification. A method, however, had been devised by the Rev. — Bowditch, the Vicar of Wakefield, by which almost all could be easily got rid of. It consisted in passing the gas over hydrate of lime heated to 400°: by this means the sulphur of the bisulphide of carbon was brought into the form of sulphuretted hydrogen, which was removed by the ordinary oxide of iron purifier. On the small scale, this process was found to take away nearly all the sulphur, and it might, perhaps, answer as well on the large. The lecturer stated that he had never found more than nine or ten grains of sulphur in 100 cubic feet of gas, but he was aware that others had found as much as 40 or 50, and he believed the quantity commonly present was about 20 grains in the hundred cubic feet. Recently, a new illuminating constituent, acetylene, had been discovered in coal gas, and the discovery may, perhaps, entirely revolutionise the manufacture. At present, a comparatively low temperature is employed in the manufacture of gas; but an intense heat is favourable to the production of acetylene. It is produced when carbonic oxide and carburetted hydrogen are strongly heated together. It would be necessary, the lecturer said, to investigate how this body could be produced on a large scale to increase the illuminating power of gas; but the subject was still *in embryo.* Acetylene may be obtained from gas by passing it through a solution of subchloride of copper, by which means, what might be called an acetylide of copper was produced, in the form of a brick-red precipitate. The lecturer showed that this acetylide of copper was decomposed on the addition of dilute hydrochloric acid, and that the acetylene evolved burnt with a brilliant flame. The acetylide of copper is an explosive compound, which has been the cause of several accidents where gas has been passed continuously through copper tubes. It is exploded by friction, percussion, and by heat.

The use of animal and vegetable oils for illuminating purposes had received no new development in the past ten years; but a new source of light of the greatest importance has been discovered in the oils obtained by the distillation of coals and shales at low temperatures. This oil, however, has recently found a formidable rival in the oil distilled by nature herself. The native oil of the United States and Canada is obtained in immense quantities: from the latter country alone as much as 20,000,000 of gallons have been procured, which, it has been calculated, would give as much light as 180,000,000 of pounds of sperm candles. The importance of these oils could not be overrated. Some accidents had resulted from their use, apparently from careless manufacture, it being necessary to remove the lighter constituent oils before they could be used with perfect safety. The lecturer explained that it was necessary to burn these oils, as well as Young's paraffin oil, in lamps made of some badly-conducting material like glass, so that the oil in the reservoir might not become heated; and he showed the explosiveness of some oils and non-explosiveness of others when heated to 120°.

The following diagram exhibits the illuminating equivalents of various materials, showing the quantities of other substances required to give the same amount of light as would be obtained from one gallon of Young's paraffin oil:—

Young's paraffin oil	1·00	gallon.
American rock oil (1)	1·26	,,
,, ,, (2)	1.30	,,
Paraffin candles	18·6	pounds.
Sperm ,,	22·9	,,
Wax ,,	26·4	,,
Stearic ,,	27·6	,,
Composite ,,	29·5	,,
Tallow ,,	39·0	,,

The comparative cost of light was shown in a diagram exhibiting the comparative cost of the light of twenty sperm candles, each burning ten hours, at the rate of 120 grains per hour:—

	s.	d.
Wax	7	2½
Spermaceti	6	8
Tallow	2	8
Sperm oil	1	10
Coal gas	0	4¼
Cannel gas	0	3
Paraffin candles	3	10
,, oil	0	6
Rock oil	0	7 3/6

It was thus shown that paraffin and rock oils are the best sources of light for domestic purposes, inasmuch as they give the largest amount of light with the least development of heat.

Amount of carbonic acid generated, and heat evolved, per hour, in obtaining a light equal to twenty sperm candles, each burning 120 grains an hour:—

	Carbonic acid in cubic feet.	Units of heat.
Tallow	10·1	100
Wax Spermaceti	8·3	82
Paraffin	6·7	66
Coal gas	5·0	47
Cannel gas	4.0	32
Paraffin oil Rock ,,	3·0	29

The lecturer then entered upon the chemical and physical principles concerned in the production of light, explaining that it was produced by the incandescence of certain solids or vapours. The incandescence of liquids is never used, and in one case only—the mercurial light—is a vapour employed. In all other cases it is the incandescence of solid carbon. He explained, too, how the light was affected by the pressure of the atmosphere and temperature; a greater pressure of the atmosphere brought more solid particles to incandescence, and a fall of one inch in the barometer involved a reduction in illuminating power of 5 per cent. When the air supplied to the burner, or the gas itself is heated, an increased illumination is obtained with the same consumption of gas amounting to 62 per cent.; or, for an equal amount of light, the saving of gas would amount to 33 per cent.

Dr. Frankland then explained the conditions of a good light, and showed that it was necessary that light for ordinary purposes should contain all the colours of the spectrum, as the light obtained in all common modes does. He showed the effects of the monochromatic light of sodium on colours. Solar light, he stated, was defective in showing colour, as it was incapable of showing tints brought out by sodium, and that, consequently, a pigment containing these tints would be invisible by daylight. Sunlight passed through a variety of vapours, of which sodium was one, and the lecturer showed how the sodium band in the spectrum was obscured when the light passed through the incandescent vapour of sodium. Solar light, however, had one great advantage over that from all other sources, inasmuch as it was attended with less heat, and, consequently, ordinary daylight produced less discomfort in the eye than any artificial light.

The disadvantage of having heat associated with light was, that the greater part of it was absorbed by the humours of the eye, there causing pain and discomfort. The behaviour of the eye towards the heat of a moderator lamp had been examined, and the following diagram represented the amount reflected and absorbed by the various media:—

	Eye of ox.	Eye of sheep.	Eye of pig.
Rays reflected at surface of cornea	4	4	4
Rays absorbed by cornea	59·8	56·9	57·5
,, ,, aqueous humour	19·2	—	20·6
,, ,, crystalline lens	6·8	30·7	7·2
,, ,, vitreous lens	2·5	—	1·6
Rays which penetrate to retina	7·7	8·4	9·1
	100·0	100·0	100·0

In conclusion, Dr. Frankland alluded very briefly to the difficulties in the way of applying the discoveries of science to every-day purposes. It was thirty years ago that Reichenbach first made paraffin and paraffin oil in the laboratory, and twenty years elapsed before any practical use was made of them. It was thirty years since Dr. Faraday showed the magneto-electric spark. How long shall we have to wait for any development of thermo-electricity, or the direct transformation of heat into light by electricity? In the magneto-electric machine the transformation was accomplished by the intermediate transformation of heat into mechanical force, by which there was experienced a loss of nine-tenths of the heat force. The man of science was rewarded by the truths which he discovered: it was not his function to apply these truths to useful purposes. That required quite different powers of mind.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, January 27.

E. W. BINNEY, *F.R.S., F.G.S., President, in the Chair.*

THE PRESIDENT said that during the last few years the MS. papers of the late Mr. Sturgeon, the electrician, and Professor Eaton Hodgkinson, F.R.S., had been presented to the Society, and were now deposited in its archives. The Council had resolved to collect, as far as possible, all MSS. by the late illustrious President of the Society, Dr. Dalton. He thought that this Society was the most proper depository for the scientific papers or correspondence of any of its deceased members. As it was probable that members and other parties might possess papers and correspondence of the late Dr. Dalton, he hoped that they would be induced to present them to the Society. He was sure that their excellent librarian would devote great care in cataloguing and arranging the papers, and that they could not be in better custody than in the Society with which that illustrious philosopher had been connected for so long a period of his life.

Mr. BAXENDELL, referring to the nebula discovered by Mr. Hind, in 1852, in the constellation Taurus, but which has since disappeared, and to the variable star near its north following edge, stated that he had lately found that another star, distant only about nineteen minutes of arc from the place of the nebula, was also variable. His observations of the brightness of this star were as follow:—

1862.—March 19. = 10·4 magnitude.
October 18. = 10·1 „
November 15. = 9·9 „
December 26. = 9·6 „
1863.—January 23. = 9·2 „

These numbers show an apparently progressive increase of brightness; but it is not improbable that a minimum occurred during the summer months, at which time no observations could be made, owing to the star's proximity to the sun; and, therefore, the actual range of variation may have been greater than that indicated by these observations. This star is No. 705, Zone + 19°, of Professor Argelander's *Bonner Sternverzeichniss*, where its magnitude is stated to be 9·4. Its approximate mean place for 1860·0 is

R.A. 4h. 13m. 40s. Declination north, 19° 28′·9.

According to Professor Argelander's system of nomenclature this variable will be called U *Tauri*.

Mr. Baxendell also communicated the following observations of the variable star near the edge of the nebula (T *Tauri*):—

1862.—March 19. T = 10·3 magnitude.
October 18. below 12·5 „
November 15. = 12·6 „
„ 16. = 12·6 „
December 26. = 12·7 „
1863.—January 23. = 12·1 „

From the last observation it would appear that T *Tauri*, after being at a minimum for some time, is now brightening up again; and, judging from the course of its changes in the early part of last year, a maximum may be expected to occur within the next two months.

A paper, by Mr. THOS. HEELIS, on "*Swell Observed at Sea, particularly in the Regions of the South-east Trade Wind*," was then read.

NOTICES OF BOOKS.

Board of Works for the Fulham District: Copy of a Memorial to the Metropolitan Board of Works against the Scheme for Deodorising the Sewage of the Western District at Sand's End, Fulham; also Copy of the Report of the Medical Officer.

Report on the Sanitary Condition of the City of London for the Year 1861—1862. By HENRY LETHEBY, M.B., &c., Medical Officer of Health for the City of London.

THE metropolis is exceedingly fortunate in having its sanitary affairs looked after by a very active and vigilant staff of medical officers. It was decided in 1856 to carry the sewage of the western suburbs down somewhere by the Kensington canal, and, after subsidence and deodorisation, to empty it into the River Thames there. In September, 1858, the Metropolitan Board of Works received tenders for this deodorisation, which were duly published in the newspapers along with other matters relating to the scheme. Recently, the Board began to take steps to carry out the plan, and so brought upon itself a deputation of medical officers of health protesting against the scheme, and requesting that the sewage of the western district should be taken along with the rest to the general outfall east of the metropolis. The Board yielded: the whole of the drainage north of the river will be discharged at Barking Creek, and most people fancy that then London will be fairly quit of its sewage.

How far these fancies or hopes will be realised, we are enabled to form some idea of from the annual report of Dr. Letheby, just issued. It appears from this report that the water of the Thames is submitted to a regular periodical examination, the results of which for the past two years are included in this report. The water is taken at high tides in the middle of the stream, and six feet from the shore, about every ten days. In the early months of the year, and generally during continued rainy weather, the water has somewhere about what we may call its normal proportion of dissolved constituents, showing that there is but little upward flow of sea-water. In the hot months, however, it is otherwise. Sea-water comes up in large proportion, and, of course, will bring with it whatever filth may have been discharged into the river below London. And, says Dr. Letheby,—

> "One thing is manifest, that as during three (July, August, September) months of the year there is little or no water flowing downwards in the river, but, on the contrary, a rather large supply of sea water upwards, to compensate for the enormous evaporation which goes on in the summer and autumn months, that some process of complete and safe deodorisation of the sewage must be effected before it is discharged into the river; for, though it may be poured in at high tide, and at a point low down in the course of the river, it will return during those months to the higher bed of the stream, and be carried upward beyond the Houses of Parliament. At that time, if the sewage is not carefully treated, and its solid constituents removed, the offensive decomposition of it and the sea-water with which it is mixed will be unbearable. I have had sewage under such circumstances fermenting continuously in the laboratory for more than two months, and the volumes of foul gas evolved from it have been enormous. This must be provided for, or the mischief of the present system will not only be not abated, but will be seriously aggravated."

What plan of deodorisation may be decided on, we of course do not know, but we have no hesitation in asserting that not one hitherto proposed is of the smallest value; and to spend a farthing on any one would be simply throwing money away.

As we shall, probably, have occasion to return to this subject soon, we now only append a table of the analyses of the river water for the first and third quarters of the year. The normal amount of dissolved impurities in the water, beyond the influence of the tide, is about 28 grains in the gallon.

Mean Composition of Thames Water at High Tide at London Bridge during each Quarter of the Years 1861 and 1862.

Properties of the water and constituents per imperial gallon (grains).	1st Quarter January to April		3rd Quarter July to October	
	1861.	1862.	1861.	1862.
Appearance on standing . .	Clear	Clear	Turbid	Turbid
Odour of the water . . .	None	None	Faint	Faint
Colour of the deposit . . .	Brown	Brown	Blackish	Blackish
Dissolved Constituents . .	25·7	26·8	141·5	114·2
Organic	3·4	4·1	8·4	9·5
Mineral	22·3	22·7	133·1	104·7
Suspended Matter . . .	6·4	5·1	9·5	2·6
Organic	1·3	1·2	1·8	0·6
Mineral	5·1	3·9	7·7	2·0
Total per gallon (grains) . .	32·1	31·9	151·0	116·8
Ammonia per gallon (grains) .	0·5	0·5	1.1	0·6
Combined sulphuric acid (grains)	1·8	2·3	8·6	8·6
Alkaline chlorides (grains) . .	3·3	4·9	105·2	79·1
Mean temperature of the river .	40·4	42·1	63·8	61·9
Highest temperature . . .	47·0	51·6	67·5	67·1
Total rain-fall (inches) . . .	4·4	5·8	4·3	6·4
Number of wet days . . .	40·0	43·0	44·0	36·0

NOTICES OF PATENTS.

2801. *Manufacture of Benzole, Naphtha, Naphthaline, Aniline, and Carbolic Acid.* J. Barrow, West Gorton, near Manchester. Dated November 7, 1861.

In the manufacture of the substances named in the heading it has hitherto been customary to employ as the raw material the waste coal-tar of gasworks; but, owing to the increased value and scarcity of these products, it has lately become advantageous to treat coal directly with a view to their manufacture, avoiding as far as possible the production of illuminating gas. The patentee employs by preference a kind of coal known in the neighbourhood of Wigan as "Cannel slack," although any coal suitable for the manufacture of gas will answer equally this purpose. This coal is charged into the ordinary iron gas-retorts, set horizontally, but in such a manner that, whilst the bottom and sides are capable of being heated to bright redness, the upper portion of the retort above the charge is carefully protected by brickwork from the heat of the fire. The retorts are of a size to hold from 8 to 12 cwt., and the ondensing pipes of the series are made to terminate in one general main, from which the crude coal-oils may be drawn off. The process is continuous; and in order to sweep out the inflammable vapours before opening the retorts to introduce a fresh charge, the patentee prefers to pass into and through them a current of steam. The crude oils first obtained are partially purified (from tar and pitch) by a rough distillation; the rectified portion sometimes deposits crystals of naphthaline on cooling, which may be collected, dried by pressure, and again sublimed. In order to separate aniline and carbolic acid from the distillate, he treats the crude oil with hydrochloric acid, which unites with the aniline, and subjects the remaining oil to the action of caustic soda or other alkali in solution, when the carbolic acid alone dissolves; the residual oils require then to be rectified in order to furnish benzole and the other constituents of coal-naphtha.

The patent-right of Mr. James Young appears to be very similar in principle to that now described, the chief object being, however, the preparation of paraffin.

2815. *Generating Hydrogen Gas.* F. H. M. C. de Lacombe, Paris. Dated November 9, 1861. (Not proceeded with.)

For the preparation of hydrogen or of a mixed gas suitable for heating and illuminating purposes, the inventor generates superheated steam by dropping water into a close iron retort, raised to the temperature of melted lead, and then passes the steam over iron or carbon heated to redness to effect its decomposition.

The principle of this invention has been so often described in the "Manuals of Chemistry," and been already the subject of so many patent claims, that it will be sufficient merely to refer to former specifications of a similar character.*

2819. *Obtaining Alkaline Phosphates.* R. A. Brooman, Fleet Street, London. A communication. Dated November 9, 1861.

The mode of treatment described is applicable to all substances containing phosphate of lime, whether of animal or mineral origin, such as bones, animal black, coprolites, apatite, and phosphorite. These are digested with sulphuric acid for the purpose of obtaining soluble superphosphates, which are afterwards dissolved out by washing with water. In the event of the acid liquor containing a small proportion of iron, a quantity of yellow prussiate of potash is added in order to precipitate the same in the form of Prussian blue. The purified superphosphate is then concentrated until the free acid (phosphoric) acquires sufficient energy to disengage the volatile acid contained in one of the saline compounds at this stage to be added. This salt may be either the hydrochlorate, carbonate, sulphate, or nitrate of potash or soda, according to the nature of the alkaline phosphate which it is desired to produce. The evaporation is carried to dryness and heat continued as long as any volatile acid makes its escape, when the residual product will consist of phosphate of potash or soda, which only requires to be dissolved in water and crystallised.

In an earlier patent, that of M. Gerard de Meley, No 1200,† is described a mode of preparing phosphate of soda which is based upon the decomposition of common salt by superphosphate of lime at a high temperature; the use of ferrocyanide of potassium in the removal of iron also being specified.

2844. *A New Process of Tanning.* L. F. Davil, and L. A. Beaudet, Paris. Dated November 12, 1861.

The essence of this invention consists in the employment of closed tanks from which the air is altogether excluded, or its place substituted by hydrogen or nitrogen gas, the primary object being to prevent by either of these means the oxidation of the tannin.

2846. *Making Paper from Cochlearia, Armoracia, or Horse Radish.* T. L. Holt, Brentford. Dated November 13, 1861. (Not proceeded with.)

The inventor takes the midrib or stem of the leaf of the horse-radish, and treats it in a similar manner to hemp or flax, reducing it to fibre by steeping and boiling. From the pulp so obtained the paper is made in the usual manner.

2855. *Manufacture of Flowers of Sulphur, and Roll and other Forms of Sulphur.* W. H. Balmain and J. Hean, St. Helen's, Lancashire. Dated November 13, 1861.

This invention consists in a means of charging the retorts with melted brimstone, without admitting air or interrupting the process.

2863. *Soap.* G. T. Bousfield, Loughborough-park, Brixton. A communication. Dated November 13, 1861.

The patentee employs soda crystals in the state of fusion in their own water of crystallisation for effecting the saponification of fatty matters generally, or as a means of

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* *Vide* Chemical News, vol. iv., pp. 267-289, and vol. v., p. 251.

† *Vide* Chemical News, vol. v., p. 153.

dissolving resin or resin oil, in the process of converting these materials into soap. Claims also for the employment of effloresced carbonate of soda, soda ash, and "soluble glass" in combination with the soda crystals, are inserted in the specification.

2864. *Manufacture of Gas.* J. Leslie, Conduit Street, Hanover Square, London. Dated November 13, 1861. (Not proceeded with.)

This specification refers to a mechanical contrivance for aiding in the diffusion of hydrocarbon vapours throughout a larger volume of illuminating gas; and to the direct production of such gas by the action of heat upon liquid hydrocarbons, which is effected in retorts provided with a series of dropping tubes.

2871. *Improvements in Treating Certain Natural Saline Compounds to fit them for Agricultural Use, and in order to Obtain Potash and Other Salts.* F. R. Hughes, Burrowstounness, and T. Richardson, Newcastle-upon-Tyne. Dated November 14, 1861. (Not proceeded with.)

This invention consists in a mode of treating certain saline products which are found at Strassfurth, in the Duchy of Magdeburg, for the purpose of separating the potash and of decomposing the deliquescent chloride of magnesium. The ingredient last mentioned hinders their use for agricultural purposes; it is decomposed and rendered nearly insoluble by the action of heat alone, or preferably in a current of steam.

Grants of Provisional Protection for Six Months.

13. Frederick Collier Bakewell, Haverstock-terrace, Hampstead, Middlesex, "Improvements in apparatus for burning oils and other inflammable fluids as fuel."—A communication from Augustus Theodore Schmidt, Pittsburgh, Pennsylvania, U.S.

44. John Leigh, Manchester, "Improvements in the treatment of gas produced by the distillation of coal, cannel, bituminous shale, boghead, mineral oils, petroleum, or other combustible substances, and for the obtaining of certain products therefrom."

70. Robert Thomson Monteith, St. Malo, France, and Robert Monteith, Crystal-terrace, Cecil-street, Greenheys, Manchester, "Improvements in the manufacture of dyes from aniline and its analogues."—A communication from Georges Auguste Jules Delvaux, Rue Corneille, Paris.—Petition recorded January 8, 1863.

92. Dan Dawson, Milnsbridge Chemical Works, Huddersfield, Yorkshire, "Improvements in manufacturing 'magenta' colour or dye."

2583. James Wilson, North Brixton, Surrey, "An improved composition for preventing and removing incrustation in boilers."

2605. William Maddick, jun., Liverpool, "An improved process or method of treating and preparing madder for dyeing purposes."

2634. Michael Henry, Fleet Street, London, "Certain new and improved applications of petroleum and its products, certain agents produced by combining the same with other substances, and certain modes of treating caoutchouc, gutta-percha, and their compounds, and substances similar thereto."—A communication from the Société Chartier et Compiègne, Boulevart St. Martin, Paris.—Petition recorded September 27, 1862.

3117. George William Oldham, Moll Spring, Honley, near Huddersfield, Yorkshire, "Improvements in preparing and dyeing silk, waste flax, hemp, Indian or China grass, or other similar fibrous substances."

3123. Johan Wilhelm Hjerpe, Wilhelm Holmgren, and Adolphe Victor Sundstedt, Stockholm, Sweden, "An improved composition or preparation of materials applicable for igniting matches."—Petitions recorded November 20, 1862.

67. Liveras Hull, Massachusetts, U.S., "Having reference to the treatment of ground caoutchouc, and for the purpose of rendering it elastic, or improving its elasticity, as well as imparting to such caoutchouc other useful properties."—Petition recorded January 8, 1863.

3418. Matthew Clark, Glasgow, Lanarkshire, N.B., "Improvements in treating waste liquors obtained when dyeing Turkey-red colours."—Petition recorded December 22, 1862.

Notices to Proceed.

2880. Thomas Goulston Ghislin, Hatton Garden, London, "Improvements in the treatment and utilisation of certain foreign plants for the obtaining of useful fibres therefrom."—Petition recorded October 25, 1862.

3423. Richard Archibald Brooman, Fleet Street, London, "A new or improved colouring matter or dye."—A communication from Pierre Chalamel, Puteaux, France.—Petition recorded December 23, 1862.

92, Dan Dawson, Milnsbridge, Huddersfield, Yorkshire, "Improvements in manufacturing magenta colour or dye."—Petition recorded January 12, 1863.

2654. Alexander Prince, Trafalgar Square, Charing Cross, London, "Improvements in the manufacture of varnish, printing-ink, paint, and printing colours."—A communication from David Steinert, Hamburgh.—Petition recorded September 30, 1862.

2675. Alexander Dalrymple, Eyre Street, Sheffield, Yorkshire, "Improvements in the processes of depositing metals by galvanic action, either with or without the aid of galvanic batteries, and in the ornamentation of metal surfaces thereby."—Petitions recorded October 3, 1862.

2688. William Clark, Chancery Lane, London, "Improvements in the means of preserving goods from fire, and in apparatus for the same."—A communication from Paulin Gay, Boulevart St. Martin, Paris.

2726. John Henry Johnson, Lincoln's Inn Fields, London, "Improvements in the manufacture of paints or pigments."—A communication from Charles François Leopold Oudry, Paris.—Petition recorded October 9, 1862.

2815. John Fuller, Bishopsgate Street, London, "An improvement in treating India-rubber used on a wire or wires for insulating the same."—Petition recorded October 18, 1862.

CORRESPONDENCE.

Spontaneous Decomposition of Chloride of Soda.

To the Editor of the Chemical News.

Sir,—Seeing in your last Number a memorandum of another instance of spontaneous decomposition of chloride of lime, I would call attention to a similar decomposition of a solution of "chloride of soda," of which I had a rather forcible demonstration some few months since. A bottle of the above solution, prepared by passing chlorine into a solution of carbonate of soda, had been lying by untouched for several months, on a shelf where it was exposed to the light. Requiring to use some of it, I proceeded to warm the neck of the bottle in order to loosen the stopper which had become fixed, when the bottle exploded in my hands with a loud report, and the pieces (few larger than a shilling) were scattered all over the room. Although I had the contents sent over my face, I escaped with a few slight cuts. I am, &c.

Alfred Noble.

Preservation of Stone.

To the Editor of the Chemical News.

Sir,—On the 10th of January last a letter was addressed to you upon this subject by Messrs. Bartlett Brothers and

Co., in which those gentlemen referred to an explanation of our process that appeared in your journal on the 6th of December last, which process they attacked in a way that called for remarks from us in the following week, and which we consider a full answer to Messrs. Bartlett. Here the matter rested, until last week these gentlemen appear again in print. But we have failed to discover their motive. What we do know is, that they are misrepresenting our *modus operandi*, and over-estimating the value of their own. They evidently do not understand anything at all of the plan adopted by us, and refuse to see the defects in their own system, although they are as plain as the sun at noon. Perhaps it may be an assistance, in their further research, to have those defects pointed out. This can be done, on application to us; but we cannot spare time for newspaper correspondence.—We are, &c.

Jesse Rust and Co.

Lambeth Glass Works, Carlisle Street.

To the Editor of the Chemical News.

Sir,—In reference to the discussion which has lately taken place in your columns, between Messrs. Bartlett and Mr. Jesse Rust, I have one remark to make: I have carefully examined specimens of the dried residues of the stone-preserving solutions, made by these two firms, and I cannot discover any essential difference between them. Mr. Jesse Rust affirms his preparation to be new, and says that lime is present in his compound; I have been unable to detect anything more than a minute trace of it, present probably as an impurity, certainly not as a constituent; while silica, alumina, and potash are the common ingredients of both products.

I am, &c.

Je Veux de Bonne Guerre.

MISCELLANEOUS.

PHOTOGENIC GAS COMPANY.

A French gentleman, of the name of Mongruel, is about to establish a company for the working of a patent, which, at first sight, appears to be similar in all its details to one taken out by the late lamented Charles Mansfield, for burning the vapour of benzole and other light hydrocarbon oils. Since 1847, some thousands of pounds have been expended by different individuals in endeavouring to work Mansfield's patent successfully, but without arriving at any practical result. The plan proposed by Mansfield was the carrying of a current of atmospheric air through a vessel containing a mass of sponges or wicks saturated with light coal oil. The air thus became impregnated with the vapour of the hydrocarbon, which burnt with a very brilliant light. If we recollect rightly, one of the great obstacles in the proper working of the apparatus was the continual freezing of the hydrocarbon used, from the intense cold produced by the evaporation; an objection which Mansfield proposed to obviate by burning a jet of the vapour underneath the vessel—a plan as dangerous as drying damp gunpowder in the oven. The use, too, of a hydrocarbon of extreme volatility would be attended with great danger to careless people; and the sapient municipal bodies who procured the passing of the Petroleum Bill would be frightened out of their wits at the idea of storing such dangerous material in or near their towns and cities. The journals that have already noticed this apparatus speak of it as a new invention, being apparently unaware or forgetful of the existence of Mansfield's prior patent.

New Scientific Entertainment.—Mr. S. Highley gave the first of a series of exhibitions of his "Science and Art Photographs" at the Burlington Gallery, on Tuesday evening last. He proved, conclusively, that the days of magic-lantern-slide-painting are at an end for all scientific and artistic purposes, and that transparent photographs must eventually supersede the creations of the brush, no matter how beautiful they may be, even for the production of purely amusing subjects. Mr. Highley exhibited, by means of the oxyhydrogen light, an immense number of slides of great beauty and interest, illustrative of every conceivable branch of science and art. Some instantaneous views of Paris, the Bible pictures of Schnorr, Kaulbach's illustrations to Reynard the Fox, besides numerous microscopic, astronomical, pathological, and ethnological subjects, elicited loud applause from a large and attentive audience.

SCIENTIFIC SOCIETIES.

MEETINGS FOR THE ENSUING WEEK.

February 23. *Monday.*

Geographical—15, Whitehall Place. 8.30 p.m.
British Architects—9, Conduit Street. 8 p.m.
Institute of Actuaries—12, St. James's Square. 7 p.m.
Medical—32a, George Street, Hanover Square. 8.30 p.m.

24. *Tuesday.*

Royal Institution—Albemarle Street. 3 p.m. Professor Marshall, "*On Animal Mechanics.*"
Medical and Chirurgical—53, Berners Street. 8.30 p.m.
Civil Engineers—25, Great George Street, Westminster. 8 p.m.
Zoological—11, Hanover Square. 9 p.m.

25. *Wednesday.*

Society of Arts—John Street, Adelphi. 8 p.m. J. Cheetham, "*On the Best Means of Promoting the Supply of Cotton.*"
Royal Society of Literature—4, St. Martin's Place. 4.30 p.m.
Archæological Association—32, Sackville Street. 8.30 p.m.

26. *Thursday.*

Royal Institution—Albemarle Street. 3 p.m. Dr. Frankland, "*On Chemical Affinity.*"
Royal—Burlington House. 8.30 p.m.
Antiquaries—Somerset House. 8.30 p.m.
Philosophical Club—6 p.m.
Artists and Amateurs—Willis's Rooms, St. James'. 8 p.m.

27. *Friday.*

Royal Institution—Albemarle Street. 8 p.m. J. Lubbock, Esq., "*On the Ancient Lake Habitations of Switzerland.*"

28. *Saturday.*

Royal Institution—Albemarle Street. 3 p.m. Prof. Max Müller, "*On the Science of Language.*"
Royal Botanic—Inner Circle, Regent's Park. 3.45 p.m.

ANSWERS TO CORRESPONDENTS.

⁂ All *Editorial Communications* are to be addressed to the Editor, and *Advertisements* and *Business Communications* to the Publisher, at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

Pyrotechnist.—Next week.

A Reader.—Calcined mandioca is mandioca starch heated to form dextrine. For Mandioca, see Chemical News, vol. vi., p. 155.

J. N.—No, it is usually expressed as follows:—"When the quantity of heat developed by the chemical combination of two bodies renders the compound luminous, the bodies are said to burn, and the phenomenon is called 'combustion.'" It is quite true that one of the combining bodies is almost invariably oxygen, but the other instances are cases of true combustion.

A. Ashby.—Our correspondent's letter was unfortunately mislaid, or it would have been answered before. 1. Bunsen and Kirchoff on "Spectrum Analysis," translated by Dr. Roscoe. 2. All the conditions for a perfect balance are described by Dr. Noad, "Chemical Manipulations and Analysis," p. 117; or in most books on Mechanics. See Lardner's "Handbook of Natural Philosophy,"—volume relating to Mechanics.

THE CHEMICAL NEWS.

Vol. VII. No. 169.—February 28, 1863.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

*Researches on the Platinum Metals,** *by* Wolcott Gibbs, *M.D.*

(Continued from page 76.)

Iridium from Rhodium.—Iridium may be approximately separated from rhodium by the process recommended by Claus, which consists in taking advantage of the solubility of the double chloride of rhodium and ammonium, $Rh_2Cl_3,3NH_4Cl$, in moderately strong solutions of chloride of ammonium in which chloro-iridate of ammonium is nearly insoluble. This method is difficult of application when the quantity of rhodium is small, and is, at best, tedious and unsatisfactory. A better method is that given above for the separation of iridium from ruthenium. The mixed solutions of iridium and rhodium are to be treated as above mentioned with nitrite of soda, the rhodium precipitated by sulphide of sodium in slight excess, the liquid rendered slightly acid, filtered, and the dark brown sulphide of rhodium thoroughly washed. The filtrate is perfectly free from rhodium, and is to be treated in the manner already described. The sulphide of rhodium is to be oxidised in the same manner as the sulphide of ruthenium, and converted into the double chloride of rhodium and ammonium, $Rh_2Cl_3,3NH_4Cl$, which is insoluble in a cold saturated solution of sal-ammoniac, in which it may be washed once or twice to remove alkaline salts and any traces of iridium which may be present as sulphate. The rhodium salt is then to be purified by crystallisation, or converted into the chloride of Claus's rhodium-ammonia base by evaporation on a water bath with a solution of ammonia. The sulphate of iridium, $Ir_2O_3,3SO_3$, does not give a basic compound under these circumstances. The chloride $5NH_3,Rh_2Cl_3$, is then to be further purified by crystallisation.

Rhodium from Ruthenium.—The separation of rhodium from ruthenium is best effected by means of nitrite of potash. The mixed solution of the two metals is to be boiled for a short time with an excess of the nitrite, together with a little carbonate of potash to keep the solution neutral or slightly alkaline. The yellow or orange-yellow solution is then to be evaporated to dryness upon a water bath, the dry mass rubbed to fine powder and then treated in a flask with absolute alcohol in the manner pointed out for the separation of platinum from ruthenium. After filtration and washing with absolute alcohol, the rhodium remains undissolved in the form of a mixture of the two double nitrites of rhodium and potassium. These may be ignited with a large excess of sal-ammoniac, so as to yield, after washing, metallic rhodium, or the nitrites may be dissolved in hot chlorhydric acid, ammonia added, and the rhodium precipitated as sulphide, which is then treated in the manner already pointed out, so as to convert the rhodium into the double chloride of rhodium and ammonium. To remove the last traces of ruthenium, the rhodium salt may be a second time treated with nitrite of potash, as above, and again washed with alcohol. The presence of the least trace of ruthenium is easily detected by adding a drop of colourless sulphide of ammonium to the alcoholic solution. The method of obtaining pure ruthenium from the double nitrite of ruthenium and potassium has already been given.

For the separation of osmium from the other metals of the group I have no better method to offer than that which is universally employed, namely, the volatilisation of the osmium in the form of osmic acid. The separation of palladium is in all cases also best effected by the processes commonly employed, especially by taking advantage of the solubility of the double salts of protochloride of palladium in solutions of the alkaline chlorides.

With these preliminary and general statements, the method which I employ in separating the different metals of the platinum group from each other will be intelligible. The mass of double chlorides, obtained as already mentioned, is to be rubbed to a fine powder, introduced into a deep porcelain evaporating dish, and mixed with four or five times its volume of boiling water. A solution of nitrite of soda is then to be added in small quantities at a time, the solution being continually stirred and occasionally neutralised by addition of carbonate of soda. The liquid soon becomes olive-green, and the greater part of the mass dissolves; it is advantageous, when the quantity of the mixed chlorides is large, to pour off the liquid as soon as it appears saturated, and to repeat the operation with a fresh quantity of water. The undissolved mass, which consists chiefly of the impurities of the ore, when these have not been removed before the process of oxidation, is then to be thrown upon a filter, and washed with boiling water until the washings are colourless. By keeping the solution somewhat alkaline, the whole of the iron remains upon the filter as sesquioxide, with the other impurities. The filtrate contains iridium and rhodium as sesquichlorides, ruthenium partly as bichloride and partly as protochloride, platinum as bichloride. When the operations already mentioned have been well performed, no determinable quantities of osmium and palladium are present. On cooling, the greater part of the platinum is deposited as $PtCl_2,KCl$, mixed with a little of the corresponding iridium salt, and is to be separated by pouring off the olive-green supernatant liquid. The quantity of the alkaline nitrite to be added in this process need not exceed half of the weight of the mass of double chlorides, but with a little experience it will be found unnecessary to weigh the nitrite added, the process of the reduction of the iridium salt, $IrCl_2KCl$, being evident to the eye.

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* From the *American Journal of Science*, vol. xxxiv., page 341.

To the filtrate a solution of nitrite of soda is to be added, and the whole boiled until the liquid assumes a clear orange colour. Nitrite of soda should be used in this process because the resulting double nitrite of iridium and soda is easily decomposed by boiling with chlorhydric acid, which is not the case with the potash salt. When nitrite of potash is used, a small quantity of the white insoluble double salt already mentioned is usually formed and renders the solution turbid.

To the clear yellow or orange-yellow boiling solution sulphide of sodium is to be added until a portion of the dark brown precipitate of the sulphides of ruthenium, rhodium, and platinum is dissolved with a brown-yellow colour, and an excess of the alkaline sulphide is, consequently, present. The liquid is then to be allowed to cool, and treated with dilute chlorhydric acid until a distinctly acid reaction is produced. In this manner the whole of the platinum, ruthenium, and rhodium present in the solution are thrown down as insoluble sulphides. After complete subsidence, the sulphides are to be thrown on a double filter and thoroughly and continuously washed with boiling water. When the operation is carefully performed, the filtrate and washings contain only iridium. It is best to neutralise this solution with carbonate of soda, boil a second time with a little additional nitrite of soda, and treat as before with sulphide of sodium and chlorhydric acid. In this manner very small additional quantities of the sulphides of platinum, ruthenium, and rhodium may sometimes be separated.

The filtrate is to be evaporated and boiled with an excess of strong chlorhydric acid, which completely decomposes the double nitrite of iridium and soda, yielding the salt, $IrCl_2,NaCl$, which is very soluble in water. An excess of a pure and strong solution of chloride of ammonium is then to be added, the whole evaporated to dryness, and the dry mass washed with cold water and then with a cold and strong solution of the ammonium salt. There remains a mass of pure chloro-iridate of ammonium, which may be advantageously rubbed to a fine powder, dissolved in boiling water, and allowed to crystallise. The resulting salt is chemically pure, and the crystals possess an extraordinary beauty and lustre. The mass of mixed sulphides, together with the filter, are to be treated with strong chlorhydric acid, and nitric acid added in small portions at a time. By the aid of a gentle heat the sulphides are readily oxidised and dissolved. After sufficient dilution, the liquid is to be filtered, the pulp of undestroyed filter-paper washed, the filtrate evaporated to dryness, the dry mass digested with concentrated chlorhydric acid, and again evaporated to dryness. The dry mass of chlorides and sulphates is to be re-dissolved in water, and the platinum, ruthenium, and rhodium precipitated by metallic zinc, after addition of chlorhydric acid. The finely divided metals, after filtration, washing, and drying, are then to be mixed with chloride of potassium, and treated with dry chlorine at a low red heat. In this manner the metals are again brought into the form of double chlorides, and the difficulties which arise from the presence of the sulphates are avoided.

The mixed double chlorides are to be boiled with nitrite of potash, evaporated to dryness, and the soluble nitrite of ruthenium and potassium dissolved out with absolute alcohol in the manner described in speaking of the separation of platinum from ruthenium. The ruthenium may then be obtained pure by converting it into the double chloride of mercury and ruthen-diamin already mentioned.

The mass undissolved by alcohol consists of chlorplatinate of potassium mixed with both the soluble and the insoluble double nitrites of rhodium and potassium. It is to be boiled with dilute chlorhydric acid, neutralised with carbonate of potash, again evaporated to dryness, after the addition of nitrite of potash, and again boiled with absolute alcohol, which sometimes dissolves a trace of ruthenium. The undissolved mass is then to be treated with hot water, and again evaporated to dryness, and this process repeated two or three times, so as to convert the whole of the soluble rhodium salt into the insoluble salt. The chlorplatinate of potassium may then, after reducing the mass to fine powder, be dissolved out by boiling water, when the rhodium salt remains pure as a fine orange-yellow crystalline powder. This may be dissolved in hot chlorhydric acid, evaporated to dryness with an excess of pure chloride of ammonium, and ignited in a clean porcelain crucible, when pure metallic rhodium remains as a porous mass mixed with chloride of potassium.

When the process above described has been carefully conducted, and especially when the quantity of nitrite of soda added is sufficient, the mixed sulphides will be found to contain only platinum, rhodium, and ruthenium, and to be free from iridium. If, however, after converting the sulphides into double chlorides in the manner pointed out, iridium is found to be present, the process to be pursued is still the same so far as regards the separation of the ruthenium; the remaining mass is then to be dissolved in water with addition of chlorhydric acid, the solution nearly neutralised with ammonia, the platinum and rhodium separated as sulphides in the manner already pointed out, brought into the form of double chlorides, and then separated by nitrite of potash as before.

For the complete success of this method it is absolutely necessary that the mass of mixed double chlorides be freed from osmium as completely as possible. This is to be done in the usual manner by repeated evaporation with nitro-muriatic acid.

PHARMACY, TOXICOLOGY, &c.

Notes on the Extraction and Estimation of Some of the Crystalline Principles of Opium,[*] *by* R. Haines, *M.B., Professor of Materia Medica, Grant College, Bombay.*

(*Concluded from page* 91.)

All the processes hitherto recommended had in view merely the estimation of morphia. In addition to the method of Couerbe,[†] commonly known as Mohr's, and the manufacturing process of Gregory,[‡] adopted, but not advantageously, as an analytical one by Berthemot,[§] may be mentioned that of Guillermond,[||] who precipitates a spirituous solution at once and without concentration by ammonia, and separates the morphia and narcotine mechanically; or the modification of Riegel,[¶] who separates the narcotine from the morphia by treatment with ether; the original one of Robiquet,[**] who precipitates the aqueous solution by boiling with magnesia, dissolves the precipitate in boiling alcohol, and treats the crystals

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[*] From the *Transactions of the Medical and Physical Society of Bombay.*

[†] Pereira's "Materia Medica," 3rd ed., vol. ii., p. 2107.

[‡] Geiger und Liebig's "Handbuch der Pharmacie," band i., s. 1191.

[§] Pereira's "Mat. Med.," *loco cit.*

[||] *Journal de Pharmacie*, 3me sie., tome xvi., p. 17; *Pharmaceutical Journal*, vol. ix., p. 236; Liebig and Kopp's "Annual Report for 1849," English edition, p. 422.

[¶] *Pharm. Journ.*, vol. xi., p. 418.

[**] *Ann. Ch. Phys.*, 2me. sie., tome v., p. 279.

which form on cooling with ether to dissolve narcotine; that of Thiboméry,†† who precipitates hot with ammonia; that of Fordos,‡‡ who mixes an aqueous infusion, without previous evaporation, with spirit, and precipitates cold with ammonia, separating the narcotine by washing with ether and chloroform; lastly, that of Merck,§§ a most trustworthy and practical experimenter, who evaporates the watery infusion to a small bulk, adds an excess of carbonate of soda, continues the evaporation to dryness, washes with water, then with spirit, and treats the residue with very weak acetic acid as long as it is neutralised, which dissolves morphia only and not narcotine, filters through pure animal charcoal, and precipitates with ammonia.

But, whatever method is adopted, hurry is to be avoided. The thorough extraction of the opium requires time, and too much time, also, can hardly be allowed for the crystallisation of the morphia. Filtrations, too, are tedious operations if effectually performed. The use of spirit as the extracting agent is, on the small scale, much preferable to that of water for all purposes of analysis. It brings into solution all the crystalline principles, including the narcotine, and, by leaving undissolved much of the gummy matters, it renders the crystallisation and purification more easy and complete. By the process above described, I have repeatedly obtained crystals of morphia half an inch and upwards in length, even when operating upon as little as 300 grains of opium. The morphia is separated by this method with fully as much ease and accuracy as by any other, and certainly by no other means can the entire quantity of narcotine be so readily extracted—a matter of some interest now that the antiperiodic properties of this base are so fully established.

Subjoined are the results obtained by the application of this process to some little-known varieties of opium :—

No.	Name of Source.	Per Cent.				Remarks.
		Moisture, dried at 220°.	Smokable extract.	Narcotine.	Morphia.	
	Persian :—					
1	Shiraz.	(about) 5·0	62	5·74	5·69	
2	Sheereh (from Ispahan).	(do.) 6·0	80	6·00	7·67	In muller-shaped cakes.
3	Pooreh (ditto)	(do.) 7·0	84	4·56	4·46	Ditto.
4	Kerman	5·46	65	6·58	10·33	
5	Kazeroon (from Shiraz)	7·33	78	6·13	6·89	
6	Istehbanat (ditto)	8·00	73	4·58	6·58	
7	Yezd, in sticks	5·30	82	5·89	9·46	In thin sticks wrapped in coloured paper.
8	Ditto, in lump	10·81	50	5·76	5·18	Thick consistence, mouldy.
	Indian :—					
9	Benares, 1850	6·33	46 dry.	5·32	3·34	
10	Patna, 1850	3·33	53	4·90	4·53	
11	Malwa, 1850	6·00	58	4·08	5·00	
12	Punjab, No. 1	8.73		3·17	4·44	
13	Ditto, No. 2	8·67		2·73	9.26	
	Chinese :—					
14	Opium from Canton	(about) 7·0		7·15	4·63	
15	Smokable Extract, first quality, from Canton. (Qy. Indian opium?)	19·09	Nearly all soluble in water.	1.30	11·57	Consistence of treacle.

PHYSICAL SCIENCE.

On the Spectral Rays of Metallic Combinations, by M. Alexandre Mitscherlich.

M. Mitscherlich has remarked that the spectrum given by chloride of barium in presence of excess of hydrochloric acid is quite different to the spectrum of barium itself. Inclined at first to attribute this fact to the presence of another metal, he examined this point synthetically. His arrangements are very simple; he places the solution to be examined in a glass tube, closed at its upper end, and having the other drawn to a point and bent nearly to a right angle; the liquid flows slowly through a bundle of fine platinum wires lodged in the point, and allowing at the same time the re-entry of air into the tube. He usually adds a salt of ammonia to the solutions, to facilitate the volatilisation of the dissolved salt.

In studying the chloride of barium spectrum he filled one of these tubes with a solution of acetate of baryta, with the addition of acetate of ammonia, and he filled another tube with concentrated hydrochloric acid. He placed the extremity only of the first tube in a flame, and found it produced the rays characteristic of barium; concentrated hydrochloric acid alone gave no ray; but on placing the bundles of platinum wire of these two tubes in the flame together, he saw the spectrum which he had obtained directly with chloride of barium.

The author has noticed analogous facts with chlorides of calcium and strontium.

Experiments with iodides, fluorides, and sulphides of alkaline earths have not been attended with the same results; the spectra were those characteristic of the metals themselves. The explanation may no doubt be found in the reducing action of the flame.

Curious results are obtained from combinations of copper: the chloride, iodide, and metallic copper give very different results; the chlorides and iodides of copper give distinct spectra, according to the order of combination to which they belong. Sulphide of copper gives no spectrum; this fact might be attributed to the fixity of this combination; but the following experiment puts this explanation out of the question:—A solution of chloride of potassium in presence of sal-ammoniac and excess of hydrochloric acid gives no spectrum, in spite of the volatility of chloride of potassium; while this salt by itself, and much more diluted, gives the characteristic ray of potassium. This is accounted for by the reduction of the salt being in the latter instance effected by the flame, which in the first instance could not be the case.

It is important to note the fact, that certain rays in the spectrum of a metal may be effaced by the presence of a different substance in the flame; thus, the blue ray of chloride of strontium disappears in presence of the spectrum produced by chloride of copper with addition of sal-ammoniac.

Wishing to go still further, M. Mitscherlich sought to discover whether the rays are produced by the free metal itself or by its oxide; for this purpose it was requisite to avoid the reducing action of the flame. With this object, he heated the combination to redness in a porcelain tube closed at each end with glass; he commenced by receiving on the spectral apparatus the light emanating from the inside of the tube containing the combination partially reduced to vapour. This light being very feeble, he placed a bright flame at the other extremity of the heated tube, and thus the rays fur-

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†† Pereira's "Mat. Med.," *loco cit.*

‡‡ *Comptes Rendus*, vol. xxiv., June 15, 1857; *Pharm. Journ.*, vol. xvii., p. 289.

§§ Geiger und Liebig's "Handbuch der Pharmacie," band i., s. 1188.

nished by the substance were observable, only they were reversed. M. Mitscherlich's experiments were chiefly with soda, sodium, chloride of sodium, and carbonate of soda; sodium only gave a spectrum under these circumstances.

The result of these researches is, that to the free metal alone are due its characteristic rays, and that, if they are seen with its combinations, reduction takes place by the carbon and hydrogen of the flame.

It seems deducible from these experiments, that the solar atmosphere does not contain sufficient oxygen to oxidise all the sodium found in it, and that all the metals having less affinity for oxygen than sodium are free; it may, moreover, be admitted that if, in the solar atmosphere, there exist metals combined with electro-negative elements, in spite of the presence of free sodium, their affinities are inverted at the high temperature of this atmosphere. Moreover, if the rays of certain metals are not observed, it is impossible to be certain of their absence; for they may exist in a state of combination like chloride of potassium, which, in the presence of some other bodies, as above shown gives no spectrum.—*Bulletin de la Société Chimique de Paris.*

On the Stratification of the Electric Light, by M. Reitlinger.

M. Reitlinger puts forward a new explanation of the phenomenon known as the "Stratification of the Electric Light," suggested by some observations with Geissler's tubes. Many of these tubes are formed of alternately wide and narrow portions, which emit distinctive coloured lights when traversed by the electric discharge. This curious phenomenon has probably been observed by most experimentors on the electric light. M. d'Ettingshausen has shown that the spectra of diversely luminous portions are of different constitution, and he mentioned this phenomenon to M. Reitlinger as an interesting subject for study, indicating, at the same time, the following arrangements as conducive to exact experiments:—

Before a narrow slit a rectangular prism is placed so as to catch the lateral rays of light, which are wholly reflected on the face of the hypothenuse, and illuminate the upper portion of the slit, while the rays from another source fall directly on the lower portion. In this way, the spectra of the two lights may be easily compared, and their identity or their difference recognised. By this means, M. Reitlinger has shown that the spectra of narrow portions of a tube alternately widened and constricted, made and sold by M. Geissler as a hydrogen tube, showed the spectrum of pure hydrogen; while the spectrum of the wide portions showed that of oxygen. The diversity of the lights is caused, then, by the diversity of the luminous substances, and the passage of electricity seemed to separate the two mixed gases, at the same time rendering them luminous. The presence of oxygen in a hydrogen tube is probably owing to the method of preparation. M. Plücker says that aqueous vapour may be used for these tubes, and that the hydrogen is set at liberty by the first electric discharges.

Such a separation of mixed gases seemed to M. Reitlinger to explain the stratification of the electric light. He admits, for instance, that in a tube prepared with aqueous vapour, hydrogen and oxygen are disposed in alternate layers, and that the hydrogen, a much better conductor than oxygen, becomes less heated and less luminous,—that is to say, relatively obscure. Generally, stratification would result in the disposal in alternate layers of the two unequally conductive gases.

Whatever may be thought of this hypothesis, the difficulty in understanding the disposition in alternate layers of the two diverse gases,[*] the following experiments are worthy of attention:—

1. A tube full of dry air, under a pressure of 1·5 mm. gave a spectrum characteristic of nitrogen only, with no signs of stratification. Moreover, no sensible difference was observed between the two large tubes and the capillary tube of communication forming the apparatus; only the spectrum of the capillary portion showed some rays which were absent in the spectrum of the large portions. The introduction of a little pure hydrogen produced stratification in the large tubes, without perceptibly modifying the corresponding spectra. At first, no sign of stratification appeared in the narrow tube, but on the nitrogen spectrum was superposed the more vivid spectrum characterising hydrogen. The introduction of hydrogen being continued, and the pressure increased to 6 millimètres, the light of the capillary tube stratified in its turn, and the tube presented somewhat the appearance of a chaplet of brilliant beads; at the same time, the hydrogen spectrum disappeared entirely, and the nitrogen spectrum alone was visible.

2. The electric light was developed in the barometric vacuum, the mercury supplying the place of one of the electrodes. Nothing appeared but a white, homogeneous light, without stratification. The introduction of air-bubbles produced immediately a series of luminous layers, alternately more or less brilliant, but without approaching dimness. The spectrum of the light showed simultaneously the rays of both mercury and air. By properly regulating the intensity of the current and the elastic force of the air introduced, the less brilliant layers may be almost entirely obscured. The mercury rays of the spectrum at the same time lose the greater part of their brilliancy.

3. No stratification has been produced by simple gases.—*Sitzungberichte der Raiserlichen Akademie der Wissenschaften*, vol. xliii., p. 15.

PROCEEDINGS OF SOCIETIES.

PHARMACEUTICAL SOCIETY.

Wednesday, February 18.

Mr. P. Squire, *President, in the Chair.*

(Continued from page 81.)

Mr. C. H. Wood called the attention of the meeting to a matter of some interest to the Society, as it related to a discovery by one of the members, announced some years ago at one of the meetings of the Chemical Discussion Association. At one of those meetings Mr. Robbins mentioned that he had found the black oxide of iron might be converted into a peroxide, which is attracted by the magnet. The fact of its being a peroxide was doubted at the time; but analysis left no doubt of its composition.[†] Recently M. Malaguti has announced the same discovery to the

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[*] M. Reitlinger compares the arrangement of the two gases in alternate layers to the polarisation of the molecules of a compound body, preceding its electrolysis.

[†] A laboratory memorandum by Mr. Robbins, announcing the fact, was published three years ago in the first Number of the Chemical News, and was translated, we believe, into the *Repertoire de Chimie Pure et Appliquée*, and also into the *Chemisches Centralblatt*. This is another illustration of the way in which our neighbours, who ignore all sources of scientific information but the *Comptes-Rendus* and the *Annales de Physique et de Chimie*, re-discover well-known things.

Academy of Sciences of Paris as something perfectly novel and original, and the Academy has granted a commission to investigate and report on the discovery. The only difference in the method given by M. Malaguti for the preparation of the peroxide, and that given by Mr. Robbins, is that the latter ignited the protoxide with nitrate of potash, while the former used chlorate of potash. Mr. Wood said he thought it was due to the Society and to Mr. Robbins to claim priority for the discovery.

Mr. **Robbins** said that in the course of his experiments he had, of course, used chlorate of potash; but he gave nitrate of potash in his paper, because it answered equally well, and was a cheaper salt.

Mr. **Curtis** called the attention of the meeting to a bottle of aromatic confection on the table, and proceeded to make some remarks on a letter from Mr. Giles, of Clifton, in the *Pharmaceutical Journal*. The specimen of aromatic confection was of a bright yellow colour; and Mr. Curtis observed that a bright yellow colour was no proof of sophistication, as Mr. Giles seemed to allege.

Dr. **Redwood** explained that Mr. Giles had made use of the words, "unsophisticated submission to the directions of the Pharmacopœia." If the confection had been made precisely in the manner directed by the Pharmacopœia it would not possess the bright yellow colour.

Mr. **Whipple** took the opportunity of remarking, that the confection on the table had not been prepared according to the Pharmacopœia, and, therefore, it was not an article which a pharmaceutist should use.

Mr. **Deane** said that things made in large quantity sometimes differed considerably in appearance from the same article made in small quantity. It was so in the confection of senna, and might be in many others; so mere difference in appearance could not be taken as a proof of adulteration. Rhubarb varied greatly in appearance when ground, and often gave rise to suspicions of adulteration when none had been practised. The Russian Government (Mr. Deane said) had now given up the monopoly of rhubarb, and, for the future, our supply will come to us through Canton.

The conversation after this, we regret to say, became of more personal than general interest.

[For the information of those of our readers who do not know how to make aromatic confection of a bright yellow colour, we may say that it is prepared by first steeping the saffron for a short time in a small quantity of boiling water, just enough to cover the saffron, in a mortar. A portion of the chalk is then added, and triturated with the saffron, until all trace of structure in the latter has disappeared. The mixture is then carefully dried at as low a temperature as possible, after which it is re-pulverised, and mixed with the rest of the chalk and other species.—Ed. Chemical News.]

CHEMICAL SOCIETY.

Thursday, February 19.

Colonel Philip Yorke, *F.R.S., Vice-President, in the Chair.*

The minutes of the preceding meeting were read and confirmed, after which the gentlemen under-mentioned were balloted for, and duly elected Fellows of the Society:—Robert G. West, Esq.; Joseph Green, Esq.; John E. Woods, Esq., 2, Victoria Street, Westminster; C. R. Clarke Tichborne, Esq., Apothecaries' Hall; and J. A. Wanklyn, Esq., Heidelberg.

Mr. David Brown was elected an Associate.

The **Chairman** gave notice of the changes which would be proposed at the anniversary meeting, March 30, in regard to the election of members of the Council for the ensuing year.

A paper, by Dr. A. W. **Hofmann**, "*On Formiamide*," was read by the Secretary. The author adverted to the want of simple connecting links between the domains of mineral and organic chemistry, which often compelled the lecturer, for the sake of examples, to commence the history of carbon with the account of compounds higher in the series than those which formed the subject of the present communication. It was known that by extracting two equivalents of water from the formiate of ammonia, hydrocyanic acid was produced, but all attempts which had hitherto been directed to the preparation of formiamide by limiting the action to the removal of only one equivalent of water, appeared unsuccessful. The author had succeeded, by the action of dry ammonia gas upon formic ether, in producing the compound in question; for this purpose it was necessary to submit these bodies to the prolonged action of heat in sealed tubes—two days at the temperature of boiling water had converted but a small proportion of the ether into formiamide. When submitted to distillation, the excess of formic ether first passed over unchanged; but as the temperature approached 195°, a liquid was collected, which agreed both in properties and composition with formiamide. Analysis indicated the formula—$CH_3NO = CHO,NH_2$. The boiling point of this substance could not be determined with precision, on account of its tendency to decompose by heat into carbonic oxide and ammonia. By distillation at 140°, in a partial vacuum, it did not, however, undergo any change. The boiling point lies between 192° and 195°. Formiamide is soluble in water, alcohol, and ether; it remains permanently liquid when exposed to a freezing mixture, and does not exhibit any signs of crystallisation on standing at rest for a lengthened period of time. When treated with anhydrous phosphoric acid it yields hydrocyanic acid according to the following equation:—$CH_3NO = H_2O + HCN$.

Dr. **Odling** stated that, after the announcement of formiamide, he could not doubt the existence of formic aldehyde, which had never yet been isolated; also of the glyceric acid of the mono-carbon series, to which the same remark was applicable.

The **President** then adjourned the meeting until March 5, when a paper by J. B. Lawes, F.R.S., "*On the Assimilation of Nitrogen*," will be read.

Dr. **Warren De La Rue** announced that Mr. Crookes' paper "*On Thallium*" was about to be read before the Royal Society; and, inasmuch as the subject was one of great interest to chemists, he was commissioned by the President to invite the Fellows of the Chemical Society to attend the meeting of the Royal Society. This invitation was unanimously accepted and acted apon by the members assembled; an unusually large attendance was the result, and the arrangement generally appeared to give much satisfaction.

ROYAL INSTITUTION OF GREAT BRITAIN.

Friday Evening, January 23.

Sir Henry Holland, *Bart., M.D., D.C.L., F.R.S., Vice-President, in the Chair.*

A paper, by John Tyndall, Esq., F.R.S., Professor of Natural Philosophy, Royal Institution, was read "*On Radiation through the Earth's Atmosphere.*" Nobody ever obtained the idea of a line from Euclid's definition that it is length without breadth. The idea is obtained from a real physical line drawn by a pen or pencil, and, therefore, possessing width; the idea being afterwards brought, by a process of abstraction, more nearly into accordance with the conditions of the definition. So also with regard to physical phenomena; we must help ourselves to a conception of the invisible by means of proper images derived from the visible, afterwards purifying our conceptions to the needful extent. Definiteness of conceptions, even though at some expense to delicacy, is of the greatest utility in dealing with physical phenomena. Indeed, it may be questioned whether a mind trained in physical research can at all enjoy peace, without having made clear to itself some possible way of conceiving of those operations which lie beyond the boundaries of sense, and in which sensible phenomena originate.

When we speak of radiation through the atmosphere, we ought to be able to affix definite physical ideas, both to the term "atmosphere," and the term "radiation." It is well known that our atmosphere is mainly composed of the two elements, oxygen and nitrogen. These elementary atoms may be figured as small spheres scattered thickly in the space which immediately surrounds the earth. They constitute about 99½ per cent. of the atmosphere. Mixed with these atoms we have others of a totally different character; we have the molecules, or atomic groups, of carbonic acid, of ammonia, and of aqueous vapour. In these substances diverse atoms have coalesced to form little systems of atoms. The molecule of aqueous vapour, for example, consists of two atoms of hydrogen united to one of oxygen; and they mingle as little triads among the monads of oxygen and nitrogen, which constitute the great mass of the atmosphere.

These atoms and molecules are separate; but in what sense? They are separate from each other in the sense in which the individual fishes of a shoal are separate. The shoal of fish is embraced by a common medium, which connects the different members of the shoal, and renders intercommunication between them possible. A medium also embraces our atoms; within our atmosphere exists a second, and a finer atmosphere, in which the atoms of oxygen and nitrogen hang like suspended grains. This finer atmosphere unites, not only atom with atom, but star with star; and the light of all suns, and of all stars, is, in reality, a kind of music propagated through this interstellar air. This image must be clearly seized, and then we have to advance a step. We must not only figure our atoms suspended in this medium, but we must figure them vibrating in it. In this motion of the atoms consists what we call their heat. "What is heat in us," as Locke has perfectly expressed it, "is in the body heated nothing but motion." Well, we must figure this motion communicated to the medium in which the atoms swing, and sent in ripples through it with inconceivable velocity to the bounds of space. Motion in this form, unconnected with ordinary matter, but speeding through the interstellar medium, receives the name of "Radiant Heat;" and, if competent to excite the nerves of vision, we call it "Light."

Aqueous vapour was defined to be an invisible gas. Vapour was permitted to issue horizontally with considerable force from a tube connected with a small boiler. The track of the cloud of condensed steam was vividly illuminated by the electric light. What was seen, however, was not vapour, but vapour condensed to water. Beyond the visible end of the jet the cloud resolved itself into true vapour. A lamp was placed under the jet at various points; the cloud was cut sharply off at that point, and when the flame was placed near the efflux orifice the cloud entirely disappeared. The heat of the lamp completely prevented precipitation. This same vapour was condensed and congealed on the surface of a vessel containing a freezing mixture, from which it was scraped in quantities sufficient to form a small snowball. The beam of the electric lamp, moreover, was sent through a large receiver placed on an air-pump. A single stroke of the pump caused the precipitation of the aqueous vapour within, which became beautifully illuminated by the beam; while, upon a screen behind, a richly-coloured halo, due to diffraction by the little cloud within the receiver, flashed forth.

The waves of heat speed from our earth through our atmosphere towards space. These waves dash, in their passage, against the atoms of oxygen and nitrogen, and against the molecules of aqueous vapour. Thinly scattered as these latter are, we might naturally think meanly of them as barriers to the waves of heat. We might imagine that the wide spaces between the vapour molecules would be an open door for the passage of the undulations; and that, if those waves were at all intercepted, it would be by the substances which form 99½ per cent. of the whole atmosphere. Three or four years ago, however, it was found by the speaker that this small modicum of aqueous vapour intercepted fifteen times the quantity of heat stopped by the whole of the air in which it was diffused. It was afterwards found that the dry air then experimented with was not perfectly pure, and that the purer the air became, the more it approached the character of a vacuum, and the greater, by comparison, became the action of the aqueous vapour. The vapour was found to act with 30, 40, 50, 60, 70 times the energy of the air in which it was diffused; and no doubt was entertained that the aqueous vapour of the air which filled the Royal Institution theatre, during the delivery of the discourse, absorbed 90 or 100 times the quantity of radiant heat which was absorbed by the main body of the air of the room.

Looking at the single atoms, for every 200 of oxygen and nitrogen there is about 1 of aqueous vapour. This 1, then, is 80 times more powerful than the 200; and hence, comparing a single atom of oxygen or nitrogen with a single atom of aqueous vapour, we may infer that the action of the latter is 16,000 times that of the former. This was a very astonishing result, and it naturally excited opposition, based on the philosophical reluctance to accept a result so grave in consequences before testing it to the uttermost. From such opposition, a discovery, if it be worth the name, emerges with its fibre strengthened,—as the human character gathers force from the healthy antagonisms of active life. It was urged that the result was, on the face of it, improbable; that there were, moreover, many ways of accounting for it, without ascribing so enormous a comparative action to aqueous vapour. For example, the cylinder, which contained the air in which these experiments were made, was stopped at its ends by plates of rock-salt, on account of their transparency to radiant heat. Rock-salt is hygroscopic; it attracts the moisture of the atmosphere. Thus, a layer of brine readily forms on the surface of a plate of rock-salt; and it is well known that brine is very impervious to the rays of heat. Illuminating a polished plate of salt by the electric lamp, and casting, by means of a lens, a magnified image of the plate upon a screen, the speaker breathed through a tube for a moment on the salt; brilliant colours of thin plates (soap-bubble colours) flashed forth immediately upon the screen, these being caused by the film of moisture which overspread the salt. Such a film, it was contended, is formed when undried air is sent into the cylinder; it was, therefore, the absorption of a layer of brine which was measured, instead of the absorption of aqueous vapour.

This objection was met in two ways. Firstly, by showing that the plates of salt, when subjected to the strictest examination, show no trace of a film of moisture. Secondly, by abolishing the plates of salt altogether, and obtaining the same results in a cylinder open at both ends.

It was next surmised that the effect was due to the impurity of the London air, and the suspended carbon particles were pointed to as the cause of the opacity to radiant heat. This objection was met by bringing air from Hyde Park, Hampstead Heath, Primrose Hill, Epsom Downs, a field near Newport, in the Isle of Wight, St. Catharine's Down, and the sea-beach near Black Gang Chine. The aqueous vapour of the air from these localities intercepted at least seventy times the amount of radiant heat absorbed by the air in which the vapour was diffused. Experiments made with smoky air proved that the suspended smoke of the atmosphere of West London, even when an east wind pours over it the smoke of the city, exerts only a fraction of the destructive powers exercised by the transparent and impalpable aqueous vapour diffused in the air.

The cylinder which contained the air through which the calorific rays passed was polished within, and the rays which struck the interior surface were reflected from it to the thermo-electric pile which measured the radiation. The following objection was raised:—You permit moist air to enter your cylinder; a portion of this moisture is condensed as a liquid film upon the interior surface of your tube; its reflective power is thereby diminished; less heat, therefore, reaches the pile, and you incorrectly ascribe to the absorption of aqueous vapour an effect which is really due to diminished reflection of the interior surface of your cylinder.

But why should the aqueous vapour so condense? The tube within is warmer than the air without, and against its inner surface the rays of heat are impinging. There can be no tendency to condensation under such circumstances. Further, let five inches of undried air be sent into the tube—that is, one-sixth of the amount which it can contain. These five inches produce their proportionate absorption. The driest day, on the driest portion of the earth's surface, would make no approach to the dryness of our cylinder when it contains only five inches of air. Make it 10, 15, 20, 25, 30 inches; you obtain an absorption exactly proportional to the quantity of vapour present. It is next to a physical impossibility that this could be the case if the effect were due to condensation. But lest a doubt should linger in the mind, not only were the plates of rock-salt abolished, but the cylinder itself was dispensed with. Humid air was displaced by dry, and dry air by humid in the free atmosphere; the absorption of the aqueous vapour was here manifest, as in all the other cases.

No doubt, therefore, can exist of the extraordinary opacity of this substance to the rays of obscure heat; and particularly such rays as are emitted by the earth after it has been warmed by the sun. It is perfectly certain that more than ten per cent. of the terrestrial radiation from the soil of England is stopped within ten feet of the surface of the soil. This one fact is sufficient to show the immense influence which this newly-discovered property of aqueous vapours must exert on the phenomena of meteorology.

This aqueous vapour is a blanket more necessary to the vegetable life of England than clothing is to man. Remove for a single summer-night the aqueous vapour from the air which overspreads this country, and you would assuredly destroy every plant capable of being destroyed by a freezing temperature. The warmth of our fields and gardens would pour itself unrequited into space, and the sun would rise upon an island held fast in the iron grip of frost. The aqueous vapour constitutes a local dam, by which the temperature at the earth's surface is deepened: the dam, however, finally overflows, and we give to space all that we receive from the sun.

The sun raises the vapours of the equatorial ocean; they rise, but for a time a vapour screen spreads above and around them. But the higher they rise, the more they come into the presence of pure space, and when, by their levity, they have penetrated the vapour screen, which lies close to the earth's surface, what must occur?

It has been said that, compared atom for atom, the absorption of an atom of aqueous vapour is 16,000 times that of air. Now, the power to absorb and the power to radiate are perfectly reciprocal and proportional. The atom of aqueous vapour will, therefore, radiate with 16,000 times the energy of an atom of air. Imagine, then, this powerful radiant in the presence of space, and with no screen above it to check its radiation. Into space it pours its heat, chills itself, condenses, and the tropical torrents are the consequence. The expansion of the air, no doubt, also refrigerates it; but, in accounting for those deluges, the chilling of the vapour by its own radiation must play a most important part. The rain quits the ocean as vapour; it returns to it as water. How are the vast stores of heat set free by the change from the vaporous to the liquid condition disposed of? Doubtless in great part they are wasted by radiation into space. Similar remarks apply to the cumuli of our latitudes. The warmed air, charged with vapour, rises in columns, so as to penetrate the vapour screen which hugs the earth; in the presence of space, the head of each pillar wastes its heat by radiation, condenses to a cumulus, which constitutes the visible capital of an invisible column of saturated air.

Numberless other meteorological phenomena receive their solution, by reference to the radiant and absorbent properties of aqueous vapour. It is the absence of this screen, and the consequent copious waste of heat, that causes mountains to be so much chilled when the sun is withdrawn. Its absence in Central Asia renders the winter there almost unendurable; in Sahara the dryness of the air is sometimes such, that though during the day "the soil is fire and the wind is flame," the chill at night is painful to bear. In Australia, also, the thermometric range is enormous, on account of the absence of this qualifying agent. A clear day, and a dry day, moreover, are very different things. The atmosphere may possess great visual clearness, while it is charged with aqueous vapour, and on such occasions great chilling cannot occur by terrestrial radiation. Sir John Leslie and others have been perplexed by the varying indications of their instruments on days equally bright—but all these anomalies are completely accounted for by reference to this newly-discovered property of transparent aqueous vapour. Its presence would check the earth's loss; its absence, without sensibly altering the transparency of the air, would open wide a door for the escape of the earth's heat into infinitude.

MANCHESTER
LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, February 10.

E. W. Binney, *F.R.S., F.G.S., President, in the Chair.*

Mr. G. Stanley Darbishire was elected an Ordinary Member of the Society.

Among other donations which received the thanks of the Society, Mr. Parry presented a volume of MS. lectures on "Chemistry," by Mr. Thomas Henry, one of the founders of the Society, and the father of the late Dr. William Henry, the celebrated chemist.

Dr. Joule described a barometer for measuring small atmospheric disturbances. It consists of a large carboy connected by a glass tube with a miniature gasometer formed by inverting a small platinum crucible over a small vessel of water. The crucible is attached to the short end of a finely suspended lever multiplying its motion six times. When the apparatus was raised two feet the index moved through one inch; hence he was able in serene weather to observe the effect corresponding to the elevation of less than one inch. The barometer is placed in a building, the slated roof of which affords, without perceptible draught, free communication with the external atmosphere. In this situation it was found that the slightest wind caused the index to oscillate, a gale occasioning oscillations of two inches, an increase of pressure being generally observed when the gusts took place.

Dr. Clay exhibited a specimen of the snake-nut, the fruit of the *Ophiocaryon paradoxum* (*Schomb.*), nat. ord. *Sapindaceæ*, from British Guiana. For a description of the snake-nut tree, see "Ann. Nat. Hist.," vol. v., p. 202 (1840).

Mr. Joseph Sidebotham exhibited a cornelian pebble with a cavity containing a globule of water, brought from the coast of Tuscany. These pebbles are often picked up on the shore, and when broken exhibit a crystalline cavity about one-third filled with water.

Mr. Dyer read a paper entitled "*Notes on the Introduc-*

tion of Steam Navigation." Mr. Dyer stated that this subject, being of great importance, had engaged many able pens in tracing the origin of the several inventions and experiments that preceded the final triumph of steam power over that of wind for navigating ships; each writer claiming the honour of priority for his own country. It may be useful to state the order in which, and the parties by whom, the principal attempts were made to realise that object. Several letters lately appeared in the *Times,* and were thence transferred to the pages of the *Engineer,* giving a graphic account of the "first steamer in English waters," the *Margery,* built at Dumbarton, by the late William Denny, for William Anderson, of Glasgow, and passed through the canal to the Forth and thence to the Thames, where she arrived on the 23rd January, 1815." On the authority of Mr. Anderson, then, this date is fixed when the first steamboat was seen on English waters. The first steamboat, the *Claremont* was started as a regular packet on the Hudson River, in the spring of 1807; so that the first steamer seen on the American waters was fifty-five years ago, a lapse of time that should now insure a calm view of the steps that led to this first actual success in steam navigation. It will be shown that, by a long course of persevering labours, the honour of that success must be conceded to Robert Fulton, by whom it was achieved. Whilst admitting the merits of other ingenious men long engaged in the same pursuit, it is clearly proved that, either from good fortune, or by the exercise of superior judgment and skill, the race was won by eight years' priority of steam navigation, by Fulton, on the Hudson River. In 1793, Mr. Fulton sent his plan for a steamboat to Lord Stanhope, who approved of, and thanked him for the communication. Shortly after Fulton went to Paris, and made experiments, on the French waters, with the chain floats, the duck's foot paddles, the screw or smoke-jack propellers, and with the paddle wheels, to which latter he gave the preference, and constructed a boat with them in 1803, which was the model adopted in building the *Claremont* in 1806. Mr. Dyer had sailed in the *Claremont,* and remembers the sensation created by her appearance, and the high admiration bestowed on the author of so great an enterprise. That sensation in 1807 was precisely the same as the *Margery* created among the vessels on the Thames in 1815. All attempts at steam navigation were fruitless before the invention of Mr. Watt's steam-engine, his engine being the first that could be usefully applied to rotative machines on land, and, therefore, for propelling ships. The principal claims put forth by other inventors of steamboats are the following:—In France, the Marquis de Jauffroy constructed a steamboat at Lyons, in 1782, "with paddle wheels," but that this boat did not succeed is obvious, because she was not heard of until 1816, when the first Fulton boat was started to run on the Seine. In 1783, Daniel Bernoulli proposed a plan which consisted of forcing water through a tube, out at the stern of the boat. This scheme has been tried many times since, but fails on account of the defective principle of applying the force. Endless chains, with float propellers, have been many times tried, and have failed on the same ground. In 1795, Lord Stanhope made experiments with a boat on the Thames, using the reciprocating or duck's foot paddles, which also failed, from the loss of time and power by the return stroke. In 1785, James Rumsey, of Virginia, tried a boat on the Potomac, and afterwards in London, both without success; and about the same time Mr. Fitch, of Philadelphia, tried one, with paddle wheels, on the Delaware, but this boat also did not succeed, and was given up as a failure. J. C. Stevens, of New York, made experiments in 1804, with a "boat twenty-five feet long and five feet wide," which of course did no good, and was stopped as a failure, though again brought to notice as preceding Mr. Fulton's. In 1788 and 1789, William Symington, in conjunction with Patrick Millar and James Taylor, made experiments with their patents for navigating by steam, and in 1802 commenced running a boat on the canal at Glasgow, which made three miles an hour; but after many changes of her propellers and trials, the scheme was given up, and no more was heard of the steamboat of Mr. Symington until long after those of Fulton were widely spread over the American waters. In 1816, the Marquis de Jauffroy complained that the Fulton steamboat on the Seine had taken the "paddle wheels" invented by him, and used at Lyons thirty-four years before, but also abandoned by him. To this charge Mons. Royou replied in the *Journal des Debats* thus:—"It is not concerning an invention, but the means of applying a power already known. Fulton never pretended to be an inventor in regard to steamboats in any other sense. The application of steam to navigation had been thought of by all artists, but the means of applying it were wanting, and Fulton furnished them." The first ocean steamer was the *Fulton,* of 327 tons, built in 1813, and the first steamer for harbour defence was built under Fulton's direction, 2,470 tons, launched in 1814. This became the model-ship for the iron-clad batteries and rams since constructed with many changes. It will be seen by the drawings of Fulton's plans, that he had tried the several other kinds of propellers—the chain float, duck's foot, and the screw fan—before adopting the paddle wheel; for, though the screw was good in principle, it was many years before it could be constructed to act efficiently. The *James Watt* was the first boat with the screw running between London and Havre, about ten years after the advent of the *Margery.* In 1811 I endeavoured to introduce steam navigation into England, but I found a strong conviction that it would not answer in this country, our most eminent engineers saying, "We don't doubt the success of steamboats in the wide rivers and harbours of America, but in our comparatively small rivers and crowded harbours they will never answer." Even such scientific engineers as the late John Rennie, sen., and Peter Ewart, a Vice-President of this Society, both advised me to relinquish the attempt to introduce steamboats, as sure to prove a waste of time and money to no purpose. However, when conviction came over the public mind that steam navigation would answer here—but not until after more than 5,000 tons of steamboats had been launched on the Hudson in 1816, did it so come—then began the spread of steam navigation, since extended with such marvellous rapidity and perfection as to atone for the sluggish beginning. Since nations are indebted to the genius of Watt for success in using steam power, to that of Fulton for its successful application to navigation, to Stephenson for the like success on railways, the meed of praise due to each of their names should be cheerfully awarded by all who are so largely benefited by the result of their labours. In doing this we should bear in mind, that inventions do not spring into existence perfect from their birth, like Pallas from the brain of Jupiter, but they come from the prior labours of many brains, and he is the true inventor who first collects the essence of, and gives the stamp of vitality to, those labours. In this sense the invention of steam navigation will for ever illustrate the name of Robert Fulton.

MICROSCOPICAL SECTION.

January 19.

Mr. Joseph Sidebotham, *Vice-President of the Section, in the Chair.*

Captain Isaac Tessyman, of the ship *Ann Mary,* presented four soundings off the coasts of Patagonia, Singin, Malay Peninsula, and at Algoa Bay, taken during his late voyage to San Francisco, Singapore, &c.

Captain J. B. Husband, of the ship *Mutlah,* presented three soundings taken off Orissa, and the Black Pagoda, on the coast of Bengal.

Mr. Latham presented mounted slides of the skin of the *Murœna guttata*, cocoon of silk worm, and cuticle of *Gladiolus*.

Mr. John Slagg, jun., presented mounted specimens of floats and ovaries of *Ianthina;* shells of ditto; berry of *Fucus natans* covered with Membranifera*;* and *Cresia aciculata*, a Pteropod.

Mr. John Hepworth referred to the mildew mentioned in the proceedings of the previous meeting, which had been observed at Gibraltar on fustians stiffened with bone size, and stated that he had noticed a peculiar fungoid growth upon mounted sections of bone, and that it would be desirable if possible to compare them.

Mr. John Leigh, M.R.C.S., read a paper "*On the Use of Dialysis in Microscopical Investigations.*" After describing the researches of Professor Graham, the present Master of the Mint, and explaining the nature of the division into two classes of all natural bodies, namely, into crystalloids and colloids, their affiuities and means of separation, the author proceeded to describe some curious bodies found in cleaning out a steam boiler, which have almost exactly the external form and internal structure of the concretions formed by Mr. Rainey, and figured in Dr. Carpenter's work on the "Microscope," third edition, p. 769. They are composed chiefly of carbonate of lime and organic matter aggregated in the presence of the aluminous colloidal mud in the boiler, and are, on a large scale, a singular illustration of Mr. Rainey's experiments. The author then enlarged upon the advantages of this method of investigation to the microscopist and chemist, who may go hand in hand in the examination of the crystalloidal constituents of organic bodies. "The microscopist (says the author) will often be able to direct his fellow worker into new channels of research. A careful study of minute crystals, with accurate measurements of their angles and observations on the effects of polarised light, may, to speak medically, lead to an accurate diagnosis of them, as is afforded to the tests of the chemist, to whose larger operations they may be referred for further analysis. Dialysis affords us the means of separating the saline constituents of the juices of plants from the salts fixed in their tissues, and similarly in regard to animal bodies. By one or more dialytic operations on a limited scale the crystalloids of any vegetable juice may be obtained in solution of great limpidity, and, by careful evaporation over a water bath, they will crystallise out in a state fit for examination." Mr. Leigh concluded his paper by the quotation of some apposite remarks by the late Professor Johnstone, of Durham, and exhibited the small trays he uses in his experiments, consisting of a double rim of gutta percha securing a disc of parchment paper in the form of a sieve; also specimens of the mulberry-shaped nodules found in a steam boiler as before named.

The Chairman said that for minute experiment he had used the parchment paper in the form of a filter.

Mr. Dancer stated that porous earthenware could be advantageously used as a dialyser.

Professor Williamson indicated a number of subjects upon which dialysis would probably throw light, both in vegetable and animal physiology. He especially dwelt upon the phenomena of calcification and silification, illustrating his remarks by reference to what occurs in the formation of calcareous and silicious growths in the colloid sarcode of sponges and polypifera, in the development of the dental plates of the teeth of Echinus, in the calcification of the derms of the crustacea, the shells of mollusca, the scales of fishes, and in the chondriform and membraniform bones and teeth of the vertebrate animals. The Professor suggested that a natural process of dialysis probably underlay all these formations. He specially called attention to the close resemblance subsisting between the primary spherical and concentric granules seen in the derms of the crustacea, in the scales of cycloid and ctenoid fishes, in the outermost layers of many teeth, and the artificial concretions produced by Mr. Rainey, to which Mr. Leigh alluded in his paper. Professor Williamson further suggested for inquiry, how far the structureless basement membrane seen underlying the calcareous layer of many calcified structures (*e.g.*, the pulp membrane of the tooth) played some part equivalent to the parchment dialyser of the Master of the Mint.

Mr. Mosley read extracts from a Report to the Cotton Supply Association, of a microscopical examination of a sample of cotton supposed to have some peculiarities. On comparison with good American cotton it was found to contain a greater proportion of round and partially flattened filaments, all more or less twisted, but full and well developed; the polarised colours were more bright and vivid, all indicative, he considered, of strong and vigorous growth in a congenial soil, and careful gathering when the pod was at its highest stage of development. The fibres varied in size, from flattened ribbons of $\frac{1}{80}$th of an inch broad to cylindrical fibres of $\frac{1}{1500}$th of an inch in diameter; the variation being due mainly to the amount of compression of the cylinder rather than to actual difference in bulk; the staple measured from 1 inch to 1¼ inch in length. The contrast with some inferior cottons was strongly marked, as regards their twisted, flat, tape-like appearance, and faint polariscopic colouring, which he attributed either to weakly growth or to having been picked from over-ripe pods, when the fibre had become dry and sapless. Too little is, however, known to form an exact opinion; dissection of buds and pods in all stages of growth would be necessary for a full and exhaustive investigation of the subject.

In reply to a question from Dr. Roberts, Professor Williamson stated that, like all vegetable hairs, the cotton fibre in its early stage is unquestionably cylindrical.

Mr. Sidebotham exhibited a convenient and effective form of binocular microscope, by Mr. Dancer, suitable for naturalists and others.

Mr. Brothers exhibited a mounted slide of *Foraminifera* and a drawing of *Colochœta scutata*, a minute fresh-water Alga.

Mr. Whalley exhibited *Trichoda lynceus*, marine infusoria; also an objective, $\frac{1}{25}$th of an inch focus, by Messrs. Powell and Lealand.

NOTICES OF BOOKS.

Contributions towards a History of Electro-Metallurgy, Establishing the Origin of the Art. By H. Dircks, C.E. E. and F. Spon, Bucklersbury. 1863.

This *brochure* consists of a series of letters and other documents, claiming the invention of the art of electro-metallurgy for Mr. C. J. Jordan. The only result of the mass of evidence adduced seems to be the confirmation of the conclusions long since arrived at by the scientific world, that Mr. Spencer announced his discovery to the world on May 9, 1839, not, however, giving full details of his process, and that, on the 22nd of the same month, Mr. Jordan wrote to the *Mechanics' Magazine*, giving details of some experiments he had been carrying on in the same direction. The real gist of the matter seems to be that these two gentlemen (Messrs. Spencer and Jordan) had both hit upon the same great idea, but that Mr. Spencer had the advantage of Mr. Jordan in having announced his discovery sooner. It seems a pity to renew this controversy after it has been dormant for so long, more especially as all the world is wishing to give both chemists the credit due to them. We are particularly sorry to see that certain imputations of piracy are renewed against Mr. Spencer. These, although, perhaps, to a certain extent, excusable in the warmth of a first discussion, should not have been again brought forward. Mr. Spencer is too well known as a man of integrity and honour for those unworthy insinuations to do him the slightest harm. On the whole, we think it would have been much better if the author of this book had acted up to the good old rule—"*Quieta non movere.*"

NOTICES OF PATENTS.

2900. *Manufacture of Iron and Steel.* G. Parry, Ebbw Vale Iron Works, Monmouth. Dated November 18, 1861.

These improvements have for their object the production of bar or wrought iron superior to that obtained in the ordinary manufacture of iron, also the production of cast-steel in large masses, of a superior quality to that obtained by the direct decarbonisation of crude pig iron in the manner now carried out. In order to accomplish these objects, the patentee takes wrought iron which, from its having already undergone the puddling process, has been purified from sulphur and phosphorus,—or he takes wrought iron scrap, and introduces it, together with coke or other fuel and suitable fluxes, into a blast furnace, which is urged to a temperature higher than that ordinarily employed in the reduction of pig iron.

By this means, he is enabled to effect the rapid and economical carbonisation of the wrought iron under treatment. When thus carburetted, the iron is run into moulds, or direct into an adjacent puddling furnace, where a further proportion of sulphur and phosphorus is removed, and the iron is increased in strength and value. This process may be repeated if thought desirable.

In the production of cast-steel direct from pig iron by partial decarbonisation, the sulphur and phosphorus are difficult to remove, and their presence exerts a very pernicious influence upon the quality of the metal; but by submitting the crude cast-iron to an intermediate puddling operation, the larger proportion of these injurious elements is got rid of, and in this manner nearly all the qualities of pig iron in the market become available for the manufacture of cast-steel.

The removal of phosphorus appears to be more certainly accomplished by repeatedly puddling the iron, than is the case with the sulphur; and, unless a high quality of coke or anthracite be employed as fuel, there is every probability of the latter element being continually absorbed by the scrap iron during its progress through the furnaces. The mode of proceeding above described is directly applicable to the manufacture of steel upon Mr. Bessemer's principle.

2903. *Improvements in the Manufacture of Starch, and of a Vegetable Sizing Powder.* Theophilus Redwood, Montague Street, Russell Square, London. Dated November 19, 1861.

In the manufacture of starch from rice, the inventor recommends the employment of an alkaline solution composed, by preference, of caustic soda, in which is dissolved the oxide of zinc. The finely-ground rice is macerated in this liquid until all the gluten is removed, and the starch, which is collected, washed and dried, appears whiter than the ordinary product. The second part of the invention relates to the treatment of starch so as to impart properties to it which increase its value as a material to be used in sizing paper, and for similar applications. The process adopted consists in mixing the starch with an acid solution of chloride of zinc, then drying the same, and subsequently heating the mixture in an oven to the temperature of about 240° F.

The effect of this treatment will be, to a certain extent, similar to the action of sulphuric acid, which is employed in the manufacture of the "Glenfield Patent Starch;" in both cases, a small proportion of the starch is converted into soluble dextrine, which serves a useful purpose in promoting the adhesion of starch to the fabrics, and improves the lustre of the sizing when applied to surfaces of paper.

2913. *Manufacture of Coke.* E. F. Smith and T. Swinnerton, Dudley, Worcestershire. Dated November 20, 1861. (Not proceeded with.)

The patentees employ a peculiar mode of burning coke in heaps. The coals are stacked in a circular pile covered outside with coke-dust, and to the top is adapted an iron tube serving as a means of connection with any furnace flue or adjacent chimney, by the aid of which a draught of air may be created, and the combustion proceed from without inwards,—conditions under which it is said that the sulphur will be more completely expelled than when the coke is burnt in ovens.

2972. *Indelible Anti-Corrosive Ink.* C. Stevens, Charing Cross, London. A communication. Dated November 26, 1861.

This is a proposal to employ the coal-tar colours, violet, red, and blue, in the manufacture of coloured inks, the other ingredients being water, acetic acid, gluten, gelatine (animal glue), and Turkey gum. Such inks are said to be capable of resisting all ordinary chemical agents, not to corrode the metal of the pen, not liable to crystallisation or deposits in the inkstand, and the writing is permanent as regards atmospheric influences.

In contradiction of the indelible character it would not be difficult to find several means of erasing by chemical solvents the writing executed with these inks; their introduction is, however, an improvement upon the crude red and blue inks in common use.

Grants of Provisional Protection for Six Months.

91. Edward Powers, Manchester, and John Gallemore Dale, Warrington, Lancashire, "Improvements in the manufacture of caustic soda and potash, and carbonates, chromates, and stannates of the same alkalies."—Petitions recorded January 10, 1863.

97. William Clark, Chancery-lane, London, "Improvements in the preparation of green colouring matter."—A communication from Charles Jules Usèbe, Boulevart St. Martin, Paris.

105. John Thomas Stroud, Birmingham, "Certain improvements in fixed and portable lights for domestic and other uses, applicable for burning gas and the mineral oils or spirits now so commonly used."

117. Jules Albert Schlumberger, Bâle, Switzerland, "An improved process for manufacturing colours for dyeing and printing."—A communication from Jean Jaques Muller, Bâle, Switzerland.

126. William Johnson, Buchanan-street, Glasgow, Lanarkshire, N.B., "Improvements in the manufacture of chlorine and bleaching powder, carbonate of soda, and soda ash and sulphate of iron."—A communication from Thomas Macfarlane, Acton Vale, Bagot, Canada.—Petitions recorded January 14, 1863.

133. George Graham, Dalquhoun Turkey-Red Dye Works, and John McLeod, Renton, Dumbarton, N.B., "Improvements in apparatus to be used in Turkey-red dyeing."

157. Ephraim Sabel, Moorgate-street, London, "Improvements in the manufacture of artificial stone."—A communication from Joseph Sépulchre, Huy, Belgium.

132. John Harrop, Manchester, "Improvements in the treatment of organic, fecal, and urinous matters for the purpose of deodorising the same, and in the preparation of a portable manure therefrom, and in the treatment of ashes or other refuse of combustion to be combined therewith; also for improvements in machinery to be employed in the manufacture of the said manure."

162. Richard Archibald Brooman, Fleet-street, London, "Improvements in the manufacture of sulphate of soda for conversion into soda and other uses."—A communication from Germain Adolphe Thibierge, Versailles, France.

163. William Henry Harrison, Haverfordwest, "Improvements in covering wire and other iron articles for the purpose of protecting them from oxidation, and in the mode or method employed therein."—Petitions recorded January 19, 1863.

167. Joseph Mosheimer, Dolgelly, Merioneth, Wales, "Improvements in machinery for crushing and grinding quartz and other substances."

186. William Clark, Chancery Lane, London, "Improvements in desiccating and in preserving matters from decay."—A communication from Jean Baptiste Pierre Camille Bergouhnioux, Boulevart St. Martin, Paris.

2788. Richard Archibald Brooman, Fleet Street, London, "Improvements in refrigerating and freezing, and in apparatus employed therein." A communication from D. J. Kennelly, Bombay. Petition recorded October 15, 1862.

3363. Rudolph Schomburg, Onslow Terrace, Lorrimore Road, and Adolph Baldamus, Surrey Terrace, Lorrimore Road, Surrey, "Improvements applicable to all kinds of oils used for illuminating purposes, whereby combustion thereof is rendered more perfect, smoke prevented, and the purity of the light increased." Petition recorded December 16, 1862.

89. Louis Henry Emmanuel Lepreux, Bordeaux, France, "Improved plates or slabs for preserving apartments from the effect of damp, and which are also applicable in the composition of plinths and foundations for walls." A communication from François Jaffary, St. Médard de Guizières, France. Petition recorded January 10, 1863.

171. Henri Adrien Bonneville, Rue du Mont Thabor, Paris, "Improvements in colouring, bronzing, and preserving iron and steel." A communication from Claude Joseph Thirault, Rue du Mont Thabor, Paris.

Notices to Proceed.

2842. James Spencer, Portsmouth, "Improvements in non-conducting compositions for preventing the radiation or transmission of heat or cold, and in coating metallic and other surfaces therewith."—Petition recorded October 22, 1862.

2903. Edward Scripps Tudor, Upper Thames Street, London, "Improvements in the purification of lead."

2906. Thomas Sutton, St. Brelade's Bay, Jersey, "Improvements in preparing albumenised paper for photographic purposes."—Petitions recorded October 28, 1862.

3418. Matthew Clark, Glasgow, Lanarkshire, N.B., "Improvements in treating waste liquors obtained when dyeing Turkey-red colours."—Petition recorded December 22, 1862.

3443. Ebenezer Stevens, Cheapside, London, "Improvements in machinery for preparing dough and paste suitable for making bread, biscuits, pastry, cakes, and similar articles."—Petition recorded December 24, 1862.

133. George Graham, Dalquhoun, and John McLeod, Renton, Dumbartonshire, N.B., "Improvements in apparatus to be used in Turkey-red dyeing."—Petitions recorded January 15, 1863.

2698. James Newnam, Crayford, Kent, "Improvements in apparatus for crystallising and for evaporating." Petition recorded October 6, 1862.

2712. John Beale, Hope Street, Maidstone, Kent, and Mary Ann Beale, Upper Brunswick Terrace, Barnsbury, London, "Improvements in the preparation or manufacture of manure." Petitions recorded October 7, 1862.

2770. Richard Archibald Brooman, Fleet Street, London, "Improvements in apparatus for carburetting gas." A communication from Louis Bricout, Julien Bricout, and Emile Berlet, Reims, France. Petition recorded October 14, 1862.

2783. Pasquale Potenza, Naples, Italy, "The extraction, preparation, and spinning of the silky fibre contained in the bark of mulberry trees, and the manufacture of the same into textile fabrics."

CORRESPONDENCE.

Noad's Chemical Analysis.

To the Editor of the Chemical News.

Sir,—You have been kind enough to refer a correspondent to my work on "Chemical Manipulation and Analysis" for a description of the chemical balance. This book has been long out of print, but I am now engaged on a new edition, the first part of which will soon be ready.

I am, &c. Henry M. Noad.

Chemical Laboratory, Medical School of St. George's Hospital.

Valuation of Chloride of Lime.

To the Editor of the Chemical News.

Sir,—As the import duties under the French tariff have enabled the soda manufacturers of this country to introduce bleaching-powder into France as an article of commerce, it has become desirable that the manufacturers should be in possession of ready means to convert the method of estimation adopted in France, with respect to this article, to that which is ordinarily used in this country; therefore, you may, perhaps, consider the following calculations and rules to be suitable for your valuable publication.

The system of chlorimetry arranged by Gay-Lussac, and adopted by the manufacturers and purchasers of bleaching-powder in France, is based upon the number of litres of chlorine gas, at mean temperature and pressure, which are contained in one kilogramme of the chloride tested.

Thus: Bleaching-powder at 100° (Gay-Lussac) contains in 1 kilogramme, 100 litres of chlorine gas; and, as the weight of this quantity is 326 grammes, it contains 32·6 per cent. of chlorine.

Table Constructed on the Foregoing Data.

70° Gay-Lussac = 22·82 per cent. chlorine
75° " = 24·45 " "
80° " = 26·08 " "
85° " = 27·71 " "
90° " = 29·34 " "
95° " = 30·97 " "
100° " = 32·60 " "
105° " = 34·23 " "
110° " = 35·86 " "

Rule: To convert degrees (French) into per-centage of chlorine multiply by 32·6, and divide by 100.

Thus: 80° × 32·6 = 2608·0 ÷ 100 = 26·08 per cent.

To convert per-centage of chlorine into degrees (French) multiply by 306·7, and divide by 100.

Thus: 32·6 × 306·7 = 9998·42 ÷ 100 = 99·9842°,—say 100°.

I am, &c. William Gossage.

Widnes, February.

MISCELLANEOUS.

The New Metal, Thallium.—As Englishmen, we naturally feel interested in that remarkable metal, since the honour of its discovery clearly belongs to a fellow-countryman. The question of priority of discovery has, however, been raised between Mr. Crookes and M. Lamy; it is one which, by reference to certain dates, admits of being satisfactorily disposed of. In cases of disputed discovery or invention, the dictum is often appealed to that "priority of publication constitutes priority of invention." This no doubt in many cases is a safe rule, but it is, nevertheless, one under which, if invariably acted upon, much injustice might be committed. We believe that in deciding claims of priority, as other claims, *all the evidence* ought to be considered and receive its due weight. However, in the present case, according to the strict letter of the rule adverted to, the priority of Mr. Crookes' claim may be firmly established. On March 30, 1861, Mr. Crookes announced in the Chemical News, "The Existence of a New Element, probably of the Sulphur Group." On May 18 of the same year, and in the same journal, "Further Remarks on the supposed New Metalloid" appeared. In January, 1862, Mr. Williams, as he has stated publicly in the Chemical News, saw the metal in Mr. Crookes' laboratory. Early in April of the same year, Messrs. Silverlock, the well-known printers of labels, printed, as can be shown by the books of that firm, labels of "Thallium," and many of its salts. On May 1, 1862, the International Exhibition opened; and there, in a case sent in some days previously, and open, of course, to public inspection and observation, and challenging the special attention of the jurors, was a specimen of the new metal, with the following explanatory labels attached:—"Thallium, a new Metallic Element,"—"Chemical Reactions of Thallium, by which it is distinguished from every other known Element; it appears to have the Characters of a Heavy Metal," &c. Now this public exhibition of the metal, and the nature of which was so clearly expressed by the labels, cannot be regarded otherwise than as an act of publication on the broadest scale; and from it, even if no weight were allowed to the other acts and dates referred to, the discovery must be dated. Such, very briefly stated, is the evidence upon which the claim of our countryman rests. Let us now see what evidence can be advanced on behalf of the French claimant. M. Dumas, in his report on Thallium, states—"M. Lamy announced his discovery to the Société Impériale of Lille, on May 16, 1862; and on June 10 he submitted to the Jury of Chemists in London, in the presence of Mr. Crookes, a beautiful ingot of thallium." The case, then, stands thus:—On March 30, 1861, Mr. Crookes announced the existence of a new element; and on May 1, 1862, he exhibited at the International Exhibition the metal, accompanied by certain labels, showing that he was well acquainted with its metallic character and properties—facts of which he was cognisant, as proved by Mr. Williams and Messrs. Silverlock, at dates antecedent to May 1, 1862. On the other hand, the first announcement of thallium by M. Lamy did not take place until May 16, 1862, and it was not until June 10 that he exhibited the metal, and that under the name bestowed upon it by Mr. Crookes. It is true that Mr. Crookes' specimen was in the form of a black powder, as precipitated by means of zinc; while M. Lamy's sample was in that of a metallic button—a condition to which Mr. Crookes' specimen might have been brought by simple fusion under certain precautions. While desiring to do justice to Mr. Crookes in this matter, and to give honour where honour is due, we have no wish to disparage the services rendered to science by M. Lamy through his researches on thallium. We advise Mr. Crookes to submit the case to an impartial jury of *savans* for adjudication, although the result of such a proceeding does not appear to us to admit of a doubt.—*Lancet.*

Gas-Light Head-Dresses.—A new feature in the application of gas to domestic purposes is said to be on the point of introduction. Fire-flies of gold and brilliants already sparkle upon the graceful white feathers and fresh green leaves that adorn the brows of some of our youthful queens. Clusters of diminutive gas-lights are now to spring from the elaborate tresses of beautiful matrons; the jets will issue from burners measuring a twentieth of an inch per hour, within transparent shades exquisitely cut, not larger than a cherry. The tubing is to be of solid gold, connected with a reservoir of the same valuable metal, which is to lie concealed in the meshes of luxuriant hair behind the head. The pressure will be applied to the golden tank, which is supported by an elaborate back-comb, the top of which forms a row of little gas-lights. Before entering the ball-room, the husband will "turn on the gas," light up his blushing bride, and usher her into her sphere of conquest, revolving, like her prototype the moon, among the lesser lights around.

SCIENTIFIC SOCIETIES.

MEETINGS FOR THE ENSUING WEEK.

March 2. Monday.

Royal Institution—Albemarle Street. 2 p.m. *General Monthly Meeting.*

Entomological—12, Bedford Row. 7 p.m.

Medical—32a, George Street, Hanover Square. 7 p.m. *Annual Election.*

Medical and Chirurgical—53, Berners Street. 8 p.m. *Anniversary.*

Asiatic—5, New Burlington Street. 3 p.m.

3. *Tuesday.*

Royal Institution—Albemarle Street. 3 p.m. Professor Marshall, "*On Animal Mechanics.*"

Civil Engineers—25, Great George Street, Westminster. 8 p.m.

Pathological—53, Berners Street. 8 p.m.

Photographic—King's College, Strand. 8 p.m.

Ethnological—4, St. Martin's Place. 8 p.m.

4. *Wednesday.*

Society of Arts—John Street, Adelphi. 8 p.m. E. P. Alexander, "*On the Sewing Machine: its History and Progress.*"

Geological—Burlington House. 8 p.m.

Pharmaceutical—17, Bloomsbury Square. 8 p.m.

5. *Thursday.*

Royal Institution—Albemarle Street. 3 p.m. Dr. Frankland, "*On Chemical Affinity.*"

Royal—Burlington House. 8.30 p.m.

Chemical—Burlington House. 8 p.m. J. B. Lawes, F.R.S., "*On the Assimilation of Nitrogen by Plants.*"

Antiquaries—Somerset House. 8.30 p.m.

Linnæan—Burlington House. 8 p.m.

Royal Society Club—St. James's Hall. 6 p.m.

6. *Friday.*

Royal Institution—Albemarle Street. 8 p.m. Dr. W. A. Miller, "*On the most Recent Spectrum Discoveries.*"

Archæological—26, Suffolk Street, Pall Mall East. 4 p.m.

7. *Saturday.*

Royal Institution—Albemarle Street. 3 p.m. Prof. Max Müller, "*On the Science of Language.*"

ANSWERS TO CORRESPONDENTS.

⁂ All *Editorial Communications* are to be addressed to the Editor, and *Advertisements* and *Business Communications* to the Publisher, at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

R. W.—Received. Answer as requested.

J. D.—Common glue will answer. Press well together.

S. P.—Add some dilute acid, and see whether effervescence takes place; or add lime-water, and see if a cloudiness is produced.

C. W. Bingley.—Received with thanks. We intend to go further into the matter immediately,

A Subscriber.—The ignition is produced by the intense chemical action. If you use a dilute acid it will not occur.

S.—It is colourless iodide of starch. See Chemical News, vol. iii., pp. 16-48.

G. F. Cusiter.—$C_4 H_2$, or equal volumes of hydrogen and carbon. As a permanent gas. No determinations have yet been made that we are aware of, but we know of some in progress, which will be reported. The amount is, no doubt, very variable.

Errata.—Last Number, page 92, four lines from bottom of first column, *for* "thus," *read* "then;" and at the end of the sentence add, "and carbonic acid." Page 94, three lines from top of first column, *for* "28," *read* "23."

THE CHEMICAL NEWS.

Vol. VII. No. 170.—March 7, 1863.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Analytical Notes on Thallium—Separation of Thallium from Bismuth, by William Crookes.

In a note recently communicated to the *Pharmaceutical Journal*, by Dr. W. Bird Herapath,[*] he states that he has probably recognised the presence of thallium in some specimens of commercial bismuth salts. In the course of my experiments upon this metal, I have frequently had occasion to search for thallium, when combined with a very large excess of other metals, and have noticed that the new metal is a not unfrequent impurity in preparations of bismuth.

Dr. Herapath gives a method of detecting thallium, based upon the assumption that when a solution of this metal is introduced into a hydrogen apparatus, the resulting gas will contain a volatile thalliuretted hydrogen, analogous to the arsenic and antimony compounds, and states that the gas evolved burns with a green flame, and deposits a mirror upon cold porcelain. I have not been fortunate enough to obtain satisfactory evidence of the existence of a compound of thallium and hydrogen, as I have invariably found the whole of the thallium precipitated in the metallic state when metallic zinc is added to any of its solutions. Upon dissolving alloys of thallium and zinc, containing varying quantities of the two metals, in sulphuric acid, the thallium is likewise left behind as a black powder so long as any alloy remains unacted upon, but neither during the solution of the zinc, or that of the residuary thallium, have I detected any of this latter metal in the evolved hydrogen.

When hydrogen is passed over metallic thallium at a red heat, the metal is volatilised, and mechanically carried forward by the current of gas, and communicates to the flame a brilliant green colour. I am, however, inclined to believe that the thallium is not chemically united with the hydrogen, as it is gradually deposited as a brown powder upon the cold sides of the glass tube through which the gas passes. This hydrogen flame has all the properties ascribed by Dr. Herapath to the gas evolved by his process: it burns with a green flame, and deposits a reddish brown mirror with a metallic lustre upon a cold porcelain surface. This mirror has many similarities to the one given by arsenic. Upon moistening it with sulphide of ammonium it becomes rather darker in colour, but is not otherwise affected. An aqueous solution of chloride of lime dissolves the mirror readily. Upon exposing the deposit to the vapour of iodine, it turns permanently yellow, and, upon subsequent addition of sulphide of ammonium to the yellow iodide, the latter becomes of a dark reddish brown colour, but does not dissolve. Upon strongly heating the glass tube, through which the hydrogen containing thallium is passed, no metallic deposit is formed on the glass. The two former observations serve to distinguish the thallium mirror from antimony, whilst the two latter distinguish it from arsenic.

By analytical processes the detection of thallium in bismuth is very delicate. The bismuth preparation is to be first obtained in the form of a dilute solution, any convenient acid being used for this purpose. A slight excess of carbonate of soda is now added, and then a little cyanide of potassium free from sulphide. The mixture is now gently warmed, and allowed to stand for ten minutes. Filter, and add a few drops of sulphide of ammonium to the clear liquid. If the slightest trace of thallium were originally present in the bismuth compound, it will be precipitated in this place as a sulphide, which, upon gently heating the liquid (not to the boiling-point), gradually collects together in deep brown, almost black flakes, after the characteristic manner of sulphide of thallium.

This process is one of extreme delicacy. By means of standard solutions of thallium, and weighed quantities of specially purified bismuth compounds, I have found that one part of thallium can be detected by it in the presence of more than 100,000 parts of bismuth. In some cases the thallium is present in so small a quantity as to occasion only a slight darkening of the liquid when the sulphide of ammonium is added. Upon allowing this to digest at a gentle heat it will generally collect in the form of a few flakes at the bottom; these may be collected together on a small filter, washed to the apex, and tested in the spectroscope. When the precipitate is only present in sufficient quantity to produce a faint dark stain on the filter paper, the latter may be partially dried by pressure between blotting-paper, opened, and the stained surface scraped up with a knife. The dark fibres are now to be twisted up in a platinum wire loop, and held in the flame of the spectroscope, when they will give abundant indications of the presence of thallium.

Most specimens of bismuth preparations employed in pharmacy will be found by the above test to contain more or less abundant traces of thallium. They may be readily purified from this metal, by digesting the precipitated carbonate with cyanide of potassium, and washing. Boiling with carbonate of soda alone is not so satisfactory, as although carbonate of thallium is soluble, it is carried down with carbonate of bismuth, and is not readily washed out.

On Chinoline Blue, by C. Greville Williams, *F.R.S.*

Dr. Hofmann, in his elaborate examination of pelamine (known in commerce under the term cyanine), expresses his surprise at that substance having remained so long without further investigation. I may mention, that at

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[*] *Pharmaceutical Journal*, vol. iv. p. 302, and Chemical News, vol. vii. p. 77.

the time of the publication of his paper, pelamine had long been under investigation in my laboratory. The principal point having been gained by the determination of the distinct difference between the chinoline and leukaline series,* the experiments had gradually drifted into other channels.

My time for investigation being exceedingly limited, I proposed to my friend, Mr. Church, to join me in studying minutely the nature of pelamine itself. The pressure of his own researches, however, caused the matter to stand over until the appearance of Dr. Hofmann's paper rendered it essential for me to publish the results obtained up to the present time.

These results I have incorporated in a paper, hichw will shortly be laid before the Chemical Society.

On the Equivalent and Spectrum of Cæsium,† *by* S. W. Johnson *and* O. D. Allen, *Sheffield Laboratory of Yale College.*

In the last volume of this Journal a method is described of separating cæsium from rubidium by fractional crystallisation of the bitartrates of these metals.‡ The analyses of the bitartrate of cæsium there given, while perfectly according with each other as regards carbon, disagree with the numbers deduced from Bunsen's equivalent to such an extent that we have undertaken to ascertain whether the salt was impure or Bunsen's equivalent incorrect.

From the great care used in preparing the bitartrate, and especially from the fact that its spectrum remained unaltered though the salt was repeatedly re-crystallised, we were inclined to suppose that Bunsen had not operated with a pure substance.

This might easily happen, on account of the small quantity of material at his disposal, without at all detracting from the merit of this distinguished chemist.

A quantity of bitartrate of cæsium purified by concentrating its solution and re-crystallisation as described in the paper referred to, and containing no foreign matters recognisable by the spectroscope, except an inevitable trace of sodium, and (to judge from a certain red line) perhaps some lithium, was treated directly with bichloride of platinum in quantity sufficient for complete precipitation. This platinchloride of cæsium, after thorough washing, was reduced in hydrogen, the chloride of cæsium dissolved from the platinum, and evaporated to dryness with addition of a little hydrochloric acid.

We thus obtained an amorphous mass of a pure white colour, which, unlike Bunsen's chloride, was not perceptibly deliquescent even in a very moist atmosphere. The spectrum of the chloride thus prepared was identical with that of the original bitartrate. Both salts gave a red line nearly coincident with the α line of lithium. In order to determine whether this line was due to a trace of lithium, or belongs to the spectrum of cæsium, a portion of chloride was again precipitated with a relatively small quantity of bichloride of platinum, the precipitate was most thoroughly washed, and from it a new sample of chloride of cæsium was prepared. The red line was no less apparent in this than in the former preparations. The same process of partial precipitation was repeated to the third time without altering the spectrum.

Again, from a hot dilute solution of 15 grammes of chloride of cæsium, about 1 gramme of cæsium was thrown down as platinchloride; the product thus procured gave a spectrum identical with that from the original bitartrate.

We concluded from these experiments that our chloride of cæsium was free from lithium, and as pure as it is well possible to obtain any substance without the most extraordinary, and, for most purposes, unreasonable precautions.

As to the properties of the chloride of cæsium, we observed that not only is it not deliquescent, but it is hardly hygroscopic. The unfused and porous salt may be weighed in moist air with as much accuracy as chloride of sodium. After it has been fused, it does not alter in weight during twenty-four hours' exposure to the air in cold dry weather. It may be fused in a platinum capsule over the gas-flame when the air is dry, without acquiring an alkaline reaction. In a damp atmosphere it is apt to lose chlorine during fusion. The residue, after the reduction of platinchloride of cæsium by hydrogen at a gentle heat, is alkaline. It is hardly possible to fuse chloride of cæsium without loss, by volatilisation. Hence our first estimations of cæsium in this form were too low by $\frac{4}{10}$ths to $\frac{7}{10}$ths of 1 per cent.

For determining the equivalent of cæsium we have made four chlorine estimations. Two of these, I. and II., were made in the chloride obtained as already described. Their details and results are given below. The filtrates from these analyses containing nitrates of cæsium and silver, after the latter had been removed, were added to a solution of several grammes of the original chloride, and the whole was partially precipitated with bichloride of platinum, and a second portion of chloride of cæsium procured, on which determination III. was made. Finally, the nitrate of cæsium from this analysis was mingled with repeatedly purified chloride obtained in the previous study of the spectrum, about half the cæsium was again thrown down as platinchloride, and with this product another estimation of chlorine, IV., was made.

The determination of chlorine was conducted in the usual manner by precipitation with nitrate of silver and filtration. Washed Swedish filters were employed, which gave each an ash weighing $\frac{4}{10}$ths of a milligramme. The weighings were taken on a balance by Becker and Sons, of Brooklyn, N.Y., which with an ordinary load indicates $\frac{1}{20}$th of a milligramme with great decision and perfect constancy.

The data of our determinations are as follows:—

grms.
I. 1·8371 CsCl gave 1·5634 Ag. Cl = ·386598 Cl, & 1·4505 Cs
II. 2·1295 ,, ,, 1·8111 ,, = ·447848 ,, 1·68165 ,,
III. 2·7018 ,, ,, 2·2992 ,, = ·56853 ,, 2·13327 ,,
IV. 1·56165 ,, ,, 1·3302 ,, = ·32893 ,, 1·23272 ,,

The percentage composition of chloride of cæsium, and the equivalents deduced from the above figures are as follows; silver being considered = 107·94, and chlorine = 35·46, Stas:—

	Per cent of		Equiv. of Cs.	
	Cl	Cs		
1.	21·044	78·956	133·050	Allen.
2.	21·031	78·969	133·150	Johnson.
3.	21·043	78·957	133·054	Johnson.
4.	21·063	78·937	132·892	Allen.
Average,	21·045	78·955	133·036	

We may accordingly assume the round number, 133, as the equivalent of cæsium.

Calculated by this equivalent, the formula of bitartrate

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[*] Chemical News, vol. i. p. 15. "On Isomeric Alkaloids."

[†] From the *American Journal of Science and Arts*, vol. xxxv., January, 1863.

[‡] "Observations on Cæsium and Rubidium," by O. D. Allen, Chemical News, vol. vi., page 263.

of cæsium corresponds well with the results of experiment. As mentioned in the paper referred to, the analyses of this salt furnished the following data :—

I. 0·4718 grm. gave { 0·0786 grm. water, and
0·294 ,, carbonic acid.
II. 0·5966 grm. gave { 0·101 grm. water, and
0·372 ,, carbonic acid.
III. 1·3086 grm. gave 0·7708 grm. chloride of cæsium.

In two other estimations since made :—

IV. 2·0347 grm. gave 1·206 grm. chloride of cæsium.
V. 1·8271 ,, 1.0857 ,, ,,

Calculated.

	Cs = 123·35			Cs = 133·	
C_8	48·00	17·62	C_8	48·	17·02
H_5	5·00	1·83	H_5	5·	1·77
O_{11}	88·00	32·31	O_{11}	88·	31·21
CsO	131·35	48·24	CsO	141·	50·00
	272·35	100·00		282·	100·00

Found.

	I.	II.	III.	IV.	V.
C_8	16·99	17·02	...	...	...
H_5	1·85	1·88	...	...	...
O_{11}	...	...	...	...	...
CsO	...	...	49·30	49·61	49·73

The equivalent number 133, brings cæsium into a triad with rubidium and potassium. We have then two alkali triads, viz. : lithium (eq. 7), sodium (eq. 23), and potassium (eq. 39·1),

$$\frac{7+39}{2}=23$$

and potassium, rubidium (eq. 85·36) and cæsium.

$$\frac{39+133}{2}=86$$

The correction of the equivalent of cæsium implies a revision of its spectrum, since the data given by Kirchhoff and Bunsen with reference to both, were obtained from the same impure material.

The cæsium spectrum, as we have procured it, is, perhaps, from the number, colour, and definition of its lines, the most beautiful to be observed among all the alkali and earthy metals. Kirchhoff and Bunsen, in the figure given by them (*Pogg. Ann.* 1861, and *Fres. Zeitschrift für Analyt. Chemie, Heft*, 1, 1862), represent 11 lines. We find without difficulty 7 more lines, and observe further that some of those figured by K. & B. are not mapped in their correct positions. To enable other chemists to compare their cæsium preparations with ours, we will attempt to describe the cæsium spectrum as seen in our instrument, which has a single flint glass prism.

Beginning at the left or red extremity of the spectrum, we will indicate the lines in the order of their occurrence by Roman numerals ; I. is a red line of medium brightness nearly equidistant between the Fraunhofer lines *a* and B ; II. is a bright line partly coincident with, but slightly to the left of and narrower than the α line of lithium ; III. is a faint line nearly approaching Fraunhofer's line C ; IV. is the faintest of the red lines ; V. is a faint line midway between the α and β lines of lithium ; VI. is a bright red line midway between the sodium line and α lithium ; VII. is an orange-red line of medium intensity directly to the right of the α strontium line ; VIII. is a fine yellow line just to the right of and close upon the sodium line. The position of the green lines it is difficult to describe. First comes a group of three, IX. X. and XI., which are separated by very narrow spaces, and which are represented well in the spectrum plate of Kirchhoff and Bunsen, though placed a trifle too far to the right. Then, after an interval scarcely wider than the lines themselves, come XII. and XIII., which are very near each other. After another space as broad as these lines we encounter XIV. Midway between XIV. and XVI. is XV. The latter, XV., coincides with the dark line E. Finally, the two pale blue lines, XVII. and XVIII., complete the list.

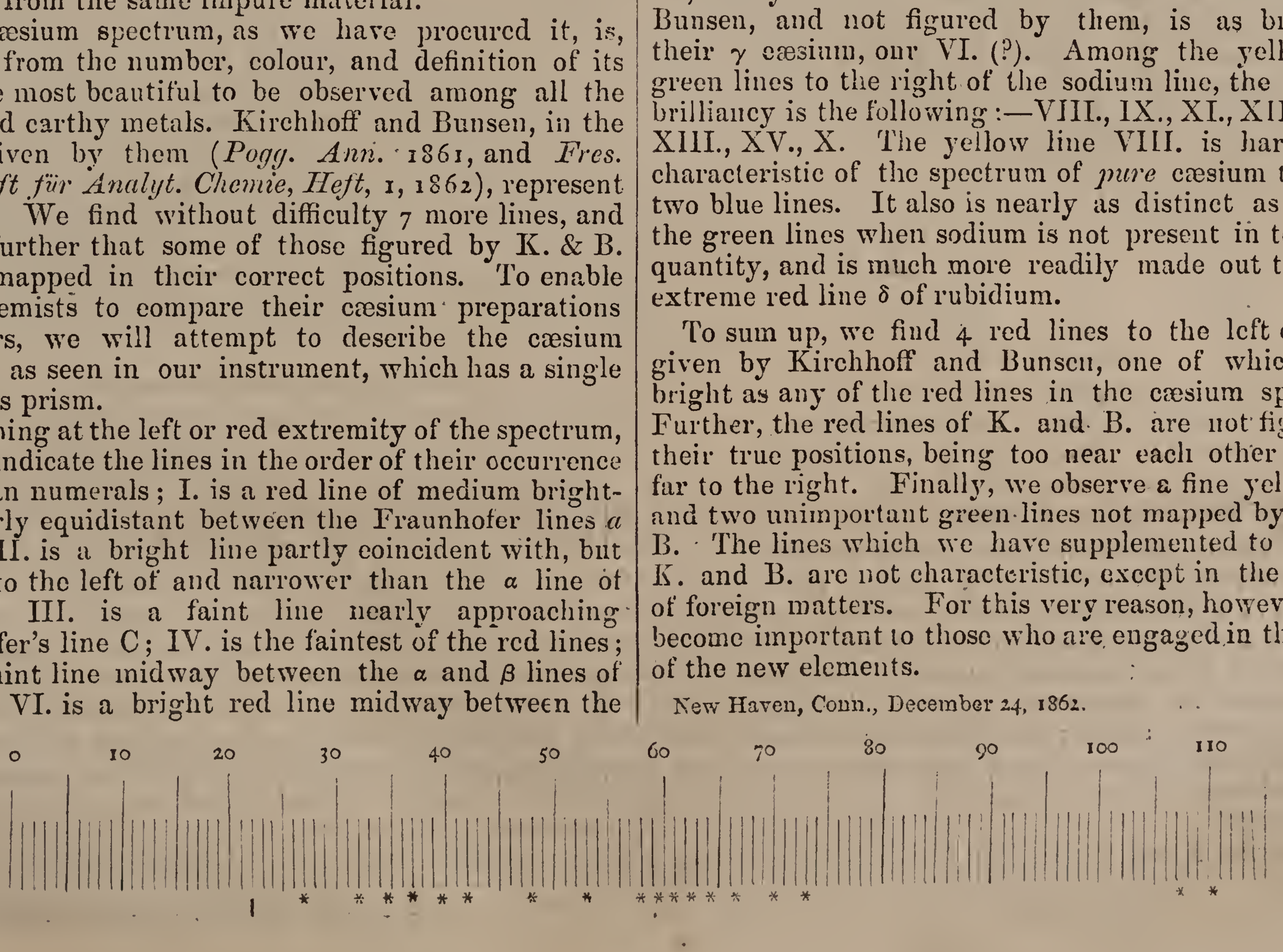
For the convenience of those who may use spectroscopes of the same construction as ours, we will mention the degrees on the scale of our instrument, which correspond to the cæsium lines. In our observations we have brought the degree 100 (10 on the scale) into the sodium line. Then the blue of strontium is at 156°, the violet of potassium 257°, the red of potassium at 65—6°; the red of lithium at 80—1°. With this adjustment the cæsium lines are as follows, beginning with the red:— I. 75°; II. 80°; III. 82—3°; IV. 85°; V. 87—8°; VI. 91°; VII. 97—8°; VIII. 101°; IX. 106°; X. 107—8°; XI. 109°; XII. 111°; XIII. 112—13°; XIV. 114—15°; XV. 118°; XVI. 121°; XVII. 157—58°; XVIII. 160°.

The position of the cæsium lines on the scale figured at the top of the spectrum plate in Fresenius' Zeitschrift, is approximatively given in the accompanying diagram, by help of which our results may be directly compared with those of Kirchhoff and Bunsen.

The order of brilliancy in the lines of what we suppose to be the spectrum of pure cæsium, with but the minutest trace of sodium, is for the red lines as follows :—VI., II., VII., I., V., III., IV. The line IV. is only made out under the most favourable conditions. II., nearly coincident with α lithium of Kirchhoff and Bunsen, and not figured by them, is as bright as their γ cæsium, our VI. (?). Among the yellow and green lines to the right of the sodium line, the order of brilliancy is the following :—VIII., IX., XI., XII., XIV., XIII., XV., X. The yellow line VIII. is hardly less characteristic of the spectrum of *pure* cæsium than the two blue lines. It also is nearly as distinct as any of the green lines when sodium is not present in too large quantity, and is much more readily made out than the extreme red line δ of rubidium.

To sum up, we find 4 red lines to the left of those given by Kirchhoff and Bunsen, one of which is as bright as any of the red lines in the cæsium spectrum. Further, the red lines of K. and B. are not figured in their true positions, being too near each other and too far to the right. Finally, we observe a fine yellow line and two unimportant green lines not mapped by K. and B. The lines which we have supplemented to those of K. and B. are not characteristic, except in the absence of foreign matters. For this very reason, however, they become important to those who are engaged in the study of the new elements.

New Haven, Conn., December 24, 1862.



On Tellurbismuth from Dahlonega, Georgia, *by* David M. Balch.

The specimen of tellurbismuth submitted to the following examination was obtained at Field's mine, near Dahlonega, Georgia, by Dr. C. T. Jackson, who presented it to me with the request that I would carefully ascertain its composition. It appeared to be part of a tabular crystal, and was easily separable into thin folia, very splendent, and quite free from impurities. Before the blowpipe a small portion entirely volatilised, and the peculiar odour of selenium was faintly recognisable. The specific gravity was found to be 7·642 at 18° C. Two portions weighing respectively ·827 and ·552 grm. were analysed as follows, the folia being first dissected to ascertain if there was any mechanical admixture of gold in thin layers, which is often the case.

The mineral was digested in hot chlorhydric acid, to which a little nitric acid had been added; it dissolved quickly, and left no residue. The solution was now evaporated till all traces of nitrous oxides were expelled, somewhat diluted, and tested for sulphuric acid by chloride of barium; the non-formation of a precipitate proved the absence of sulphur in the ore (any selenium which may have been present would of course not be thrown down, since selenite of baryta is soluble in acids). After the excess of barium had been separated, the solution was evaporated to a small bulk (a few c. c.), mixed while hot with an excess of bisulphite of ammonia, and set aside for some hours. All the tellurium and selenium present are thus thrown down as a black powder easily washed by decantation. To avoid the precipitation of basic tellurium salts and small quantities of bismuth, it is necessary that the solution should contain much free chlorhydric acid; it should also be concentrated and warm. The precipitate, after the decantation of the supernatant liquid, was washed with dilute sulphurous acid to which some chlorhydric acid had been added, then on a tared filter with water containing a little sulphurous acid, dried at 125° C. and weighed.

The filtrate and washings from the tellurium were evaporated nearly to dryness, the residue dissolved in largely diluted chlorhydric acid, and from this solution the bismuth determined as teroxide in the usual manner.

Analysis a.

·827 grm. gave,

·4256 Bi	=	51·46 per cent.
·3990 Te	=	48·26 ,,
·8246		99·72

Analysis b.

·552 grm. gave,

·2847 Bi	=	51·57 per cent.
·2690 Te	=	48·73 ,,
·5537		100·30

To ascertain whether this tellurium contained selenium in appreciable quantity, a portion (·186 grm.) was fused at a dull red heat with six times its weight of a mixture of nitre and carbonate of soda, to convert any selenium present into selenic acid. The fused cake was dissolved in water, and a little nitric acid and nitrate of baryta added: no precipitate formed, even after several days, which proves the absence of selenium, except in minute traces, as evinced by the blowpipe test before noticed.

The ore was found to be free from gold, silver, and iron.

The specimen that I have analysed is therefore a pure tellurid of bismuth, Bi Te$_3$; thus—

		Calculated.	Dahlonega. *a.*	*b.*
Bi	208	52·00	51·46	51·57
Te$_3$	192	48·00	48·26	48·73
	400	100·00	99·72	100·30

The tellurbismuth from this locality has already been analysed by Dr. Genth, with nearly the same results as above; he also finds the same formula for the Fluvanna county, Va., mineral.

Allow me to offer a few remarks on the compounds of bismuth and tellurium, suggested by an examination of the many published analyses of this ore from both American and foreign localities. It appears that selenium is present only in traces, and sulphur (where it exists at all) in quantities not exceeding 5 per cent. The mineral called tetradymite, taking Berzelius's analyses of that from Schoubkau as an example, has the following formula, $(BiTe_3)^2+BiS_3$, and other analyses agree closely with this. Examples of a compound or complex mineral formed by the union of two simple ones are common; for instance, bromyrite (AgBr) and kerargyrite (AgCl) unite to form embolite (AgCl + AgBr); and others might be cited. It seems therefore probable that when sulphur is present in a tellurbismuth, it is due to an admixture of bismuth glance (BiS$_3$), and that tetradymite, like embolite, is formed by the union of two simple minerals; in the case of tetradymite these minerals are tertellurid of bismuth, BiTe$_3$, and tersulphid of bismuth, BiS$_3$, (bismuth glance, a mineral much resembling the other in its physical properties).

The native tertellurid of bismuth in a pure state, has been observed only at Dahlonega, Ga., and the "Tellurium Mine," Fluvanna Co., Va., and is up to this time, at least, a mineral peculiar to the United States.

Taking this view of the subject, the American tertellurid of bismuth should be considered a new species, to which the term tetradymite is hardly applicable.

In conclusion, I would call attention to the fact, that, although by artificial means bismuth and tellurium can be fused together in all proportions, in their native combinations one equivalent of the former appears to be always united to three equivalents of the latter metal; the bornite of Brazil offers the only exception to this rule, and according to Damour's analysis differs entirely from the other tellurbismuths.—*American Journal of Science*, xxxv., 99.

On the Carbonates of Alumina, Glucina and the Sesquioxides of Iron, Chromium, and Uranium,[*] *by* Theodore Parkman, *Ph.D.*

All the precipitates produced by the alkaline carbonates in solutions of alumina glucina, and the sesquioxides of iron, chromium, and uranium, have been investigated by a large number of observers; but their results are so discordant, that the true composition of the substances in question is still in doubt. The following investigation was undertaken at the suggestion of Prof. Charles W. Eliot, under the idea that the contradictory results obtained by previous writers may have been owing to a loss of carbonic acid during the processes of washing and drying. This would be probable enough in itself, from the weak affinities of both

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[*] *American Journal of Science and Arts*, vol. xxxiv., page 321.

acid and base, in each case, and is, I think, fully confirmed by the analyses which follow.

1. *Carbonate of the sesquioxide of iron.*

Fe_2O_3, CO_2.

The precipitate, formed by mixing carbonate of potash with nitrate of the sesquioxide of iron, after washing with cold water, consists, according to Gmelin,[†] of hydrated sesquioxide of iron, free from carbonic acid. Berzelius[‡] considers that the compound $Fe_2O_3, 3CO_2$ is formed, but that it exists only momentarily. Langlois[§] gives an analysis of a precipitate, produced by carbonated alkali in a sesqui-salt of iron, perfectly freed from alkali by washing, and dried at 100° C. His results were 88·47 p. c. Fe_2O_3, 10·17 HO, 1·36 CO_2, from which he does not deduce any formula. Wallace[‖] finds the precipitate by carbonate of soda in sesquichloride of iron, after drying over sulphuric acid, to be $3Fe_2O_3, CO_2, 6HO$: in the nitrate, $9Fe_2O_3, CO_2, 12HO$. According to Barratt,[¶] the precipitate produced by carbonate of soda in sesquichloride of iron, when dried in the air, has the formula $3Fe_2O_3, CO_2+8HO$; when dried at 100°, $3Fe_2O_3, CO_2+4HO$.

The carbonate of iron which I have examined was precipitated, at the ordinary temperature, from pure crystallised iron-potash-alum by carbonate of soda in slight excess. The precipitate was not washed or dried, but simply pressed between folds of porous paper, under a heavy weight, for about twelve hours, and while still moist, introduced into a bulbed tube of hard glass, and the whole weighed. It was then ignited in a slow stream of dry air, and the water caught by a weighed chloride of calcium tube. The loss of weight in the bulbed tube and substance, minus the increase of weight in the chloride of calcium tube, would give the weight of carbonic acid corresponding to the carbonate of iron, while the carbonate of soda and sulphate of soda would remain unchanged. The sesquioxide of iron remaining in the bulb was washed and weighed. Of course, by this method, the water cannot be estimated, which, according to all previous observers, exists in all the carbonates in question. In my first analyses the substances, though not washed, were dried over sulphuric acid: but three analyses of the same substance, which gave successively the percentages of carbonic acid, 15·74, 14·45, and 12·95, showed conclusively that the substance lost carbonic acid by standing. All the other analyses made, both of this and of the other carbonates, were therefore made with the precipitate while still moist. The formulas obtained therefore express simply the relation between the acid and the base, and a certain amount of water must probably in every case be understood. In every case, more than one preparation was analysed, to ascertain whether the precipitate had always the same composition.

Of the carbonate of iron four analyses were made, with the following results:—

	Calculated.		Prep. I.	Prep. II.	Prep. III. 1.	Prep. III. 2.
Fe_2O_3	80	78·43	79·88	79·47	79·09	80·52
CO_2	22	21·57	20·12	20·53	20·91	19·48
	102	100·00	100·00	100·00	100·00	100·00

Considering the ease with which the substance loses carbonic acid, the above results agree with the calculated percentage probably as closely as could be expected—closely enough, at any rate, to fix the composition of the substance with sufficient certainty.

2. *Carbonates of the sesquioxide of chromium.*

$Cr_2O_3, 2CO_2$
Cr_2O_3, CO_2

The earliest analysis of carbonate of chromium is by Meissner,[**] who found it to contain 77·30 Cr_2O_3, 15·54 CO_2 and 7·16 HO. Meissner's analysis, however, is quite untrustworthy, as may be seen from his details of it. According to Lefort[††] the precipitate by carbonated alkalies in the violet salts of chromium is Cr_2O_3, CO_2+4HO; in the green modification only the hydrated oxide. According to Langlois,[‡‡] the precipitate by carbonated alkalies in the salts of chromium dried at 100°C., is $Cr_2O_3, CO_2+Cr_2O_3, 6HO$. Wallace[§§] precipitates sesquichloride of chromium by carbonate of soda, in very dilute solution, and dries the precipitate over sulphuric acid. His results give the formula $Cr_2O_3, CO_2, 4HO$. Barratt[‖‖] proceeds in the same way as Wallace, and obtains the same result, which is also that obtained by Lefort.

The substance which I analysed was precipitated by carbonate of soda, in slight excess, from crystallised chrome-potash-alum, free from iron and alumina. In the first analysis, given below, the substance was dried over sulphuric acid: in the others, not. The first analysis was made like those of carbonate of iron. The wash-water from the oxide of chromium, left in the bulbed tube after ignition, was coloured yellow from alkaline chromate. This must have arisen from oxidation of a little oxide of chromium, by ignition in the air in contact with alkaline carbonate. The solution was reduced by hydrochloric acid and alcohol, precipitated by ammonia, and the oxide of chromium thus obtained added to the rest. There still remained an error, however, from the chromic acid having driven out carbonic acid from the carbonate of soda; and to avoid this, the other determinations were made in a stream of hydrogen. In this latter method there would be a possible source of error, viz., a reduction of the alkaline sulphate to sulphide. This, however, was extremely small, as the wash-water, on heating it with hydrochloric acid, gave off in every case only a trace of sulphuretted hydrogen, and either remained clear, or became only very slightly milky, from precipitated sulphur.

The analyses of a large number of preparations gave the following results:—

	Calculated.		Prep. I.	Prep. II. 1.	Prep. II. 2.
Cr_2O_3 .	77·4	63·76	66·81	66·51	66·89
$2CO_2$.	44·	36·24	33·19	33·49	33·11
	121·4	100·00	100·00	100·00	100·00

	Prep. III. 1.	Prep. III. 2.	Prep. IV. 1.	Prep. IV. 2.	Prep. V. 1.	Prep. V. 2.
Cr_2O_3 .	67·53	66·59	73·03	73·44	72·55	73·84
$2CO_2$.	32·47	33·41	26·97	26·56	27 45	26·16
	100·00	100·00	100·00	100·00	100·00	100·00

The composition of the first three precipitations comes pretty near the formula given above, while the other two vary largely from it. These two were made in summer, while the others were made in cold weather; and it is possible that there may have been a greater loss of carbonic acid at the higher temperature. I am in-

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[†] "Handbook of Chem.", v. 222.

[‡] "Lehrb. der Chem.", iii. 626.

[§] *Ann. Chim. et. Phys.*, xlviii. 502.

[‖] *Chem. Gaz.*, 1858, 410, in Jahresb., 1858, 70.

[¶] *Chem. News*, i. 110, in J. pr. Chem., lxxxii. 61.

[**] Gilbert's "Annalen der Physik," lx. 366.

[††] "Compt. Rend.", xxvii. 269.

[‡‡] *Ann. Chim. et Phys.*, [3] xlviii. 502.

[§§] *Chem. Gaz.*, 1858, 410, in Jahresb., 1858, 70.

[‖‖] *Chem. News*, i. 110, in J. pr. Chem. 82, 61.

clined to think, however, that this is not the case, as, in the last preparation, the liquids were cooled before precipitation, and the precipitate left to press in a refrigerator containing ice. It occurred to me that one cause of the varying composition of the precipitates might be, that basic compounds might at first go down before the carbonate of soda was in excess, as is the case, for example, when ammonia, not in excess, is added to solutions of copper. To ascertain whether this was the case, an analysis was made of a precipitate, produced by pouring chrome alum slowly into carbonate of soda, with constant agitation, until the alkaline reaction became comparatively feeble. In this way there would be always an excess of carbonate of soda present. Two analyses were made of the same preparation :—

	Calculated from $Cr_2O_3,2CO_2$.	I.	II.
Cr_2O_3 . .	63·76	65·03	64·96
$2CO_2$ . .	36·24	34·97	35·04
	100 00	100·00	100·00

This seems to prove pretty conclusively that there is a carbonate of chromium possessing the above formula, and that the precipitate produced by the alkaline carbonates consists chiefly of it, generally mixed, however, with variable quantities of a more basic compound. This latter may very probably be a carbonate of chromium, which will be described further on.

As already mentioned, Lefort asserts that the precipitate produced by the alkaline carbonates, in solutions of the green modification of sesquioxide of chromium, is hydrated oxide, containing no carbonic acid. The following is the result of an analysis, made by myself, of such a precipitate. The chromium solution used was prepared by boiling the violet chrome-alum for about an hour, and cooling the solution before precipitation. The percentages obtained were 59·38 Cr_2O_3 and 40·62 CO_2. The carbonic acid is probably too high, and I regret that I did not have time to repeat the analysis. It is, however, sufficient to show that the substance contains carbonic acid, and that its composition is probably the same as that of the precipitate obtained in the violet modification.

The agreement of the results of Lefort, Wallace, and Barratt, who all make the carbonate of chromium to be $Cr_2O_3,CO_2 + 4HO$, makes it probable that the bicarbonate above described loses its second atom of carbonic acid more readily than the other, and is converted, by washing and drying, into a more stable monocarbonate. This latter is also formed, when a boiling solution of chrome alum is precipitated by carbonate of soda, as is shown by the following analysis of a precipitate thus prepared :—

	Calculated.		Found.
Cr_2O_3 . .	77·4	77·86	78·68
CO_2 . .	22·	22·14	21·32
	99·4	100·00	100·00

3. *Carbonate of Alumina.*

Saussure* considers the precipitate produced by alkaline carbonates, in the solutions of alumina, to be a compound of alumina with a little alkaline carbonate. Muspratt† finds that the precipitate by carbonate of ammonia in alum is $3Al_2O_3,2CO_2 + 16HO$. Muspratt does not mention whether he tested the substance for ammonia, which it should contain, according to H. Rose,‡ who finds that the precipitate, after being washed, first with hot and then with cold water, has the composition, $Al_2O_3,3HO + NH_4O,2CO_2 + HO$. Langlois § finds the precipitate by alkaline carbonates in the salts of alumina to be $3(Al_2O_3,CO_2) + 5(Al_2O_3,8HO)$. Wallace ‖ precipitates the chloride by carbonate of soda, in very dilute solution, and dries the precipitate over sulphuric acid. He gives the formula, $3Al_2O_3,2CO_2,9HO$. Bley ¶ obtains per-centages of carbonic acid varying from 5·27 to 11·39, in the precipitate, produced by alkaline carbonates in solutions of alum, and considers it to be a mere mixture, of variable composition. Barratt ** finds that the precipitate by carbonate of soda in chloride of aluminum, after washing, drying, rubbing with water, and again washing and drying over sulphuric acid, consists of alumina, free from carbonic acid.

The precipitates, which I have examined, were precipitated by carbonate of soda, in slight excess, from alumina-potash-alum, free from iron. The mode of analysis used in the iron and chromium compounds was not applicable in this case. The strongly alkaline wash-water, from the ignited alumina, contained alumina in large quantity, showing that aluminate of soda had been formed and carbonic acid therefore driven out of the carbonate of soda. Attempts to overcome this difficulty by taking weighed quantities of alum and carbonate of soda, in equivalent proportions, and by exactly neutralising the solution of alum by carbonate of soda, did not give good results. The method finally adopted was to wash the substance with water saturated with carbonic acid, until a portion of the filtrate, after being boiled to expel carbonic acid, gave no alkaline reaction. This would probably give correct results, since the water, being already saturated with carbonic acid, would hardly wash out any from the substance. The latter, moreover, would hardly absorb any more carbonic acid from the water, since, during the precipitation, free carbonic acid is given off in great quantity, and the alumina must have combined with as much carbonic acid as it would be possible for it to take up. In other respects, the analyses were conducted like those of the carbonate of iron. The following are the results obtained :—

	Prep. I.		Prep. II.
	1.	2.	1.
Al_2O_3 . .	79·13	79·32	75·95
CO_2 . .	20·87	20·68	24·05
	100·00	100·00	100·00

	Prep. III.		Prep. IV.	
	1.	2.	1.	2.
Al_2O_3 . .	81·37	81·91	77·51	76·72
CO_2 . .	18·63	18·09	22·49	23·28
	100·00	100·00	100·00	100·00

Preparation I. was analysed in April; Preps. II. and III. in summer; Prep. IV. also in summer; but, to avoid a possible loss of carbonic acid in hot weather, it was precipitated from cooled solutions of alum and carbonate of soda, washed with ice-water saturated with carbonic acid, and pressed in a refrigerator. The variable composition of the substance led me to suppose that it was a mixture of more than one substance, and, for the same reasons as in the case of the carbonate of chromium, led me to make the following analyses of precipitates, produced by pouring alum into carbonate of soda, until the

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[*] *Jour. Phys.*, lii., 290.

[†] *Chem. Soc.*, Qu. J., ii., 216.

[‡] *Pogg. Ann.*, xcii., 452.

[§] *Ann. Chim. et. Phys.* [3], xlviii., 502.

[‖] *Chem. Gaz.*, 1858, 410, in Jahresb., 1858, 70.

[¶] *J. pr. Chem.*, xxxix., 1.

[**] Chem. News, i., 110, in J. pr. Chem., lxxxii., 61.

alkaline reaction became weak. Both were prepared in hot weather, but with the precautions above mentioned.

	Calculated.		Prep. I.		Prep. II.	
			1.	2.	1.	2.
Al_2O_3 .	51·4	70·03	70·44	71·06	77·79	77·02
CO_2 .	22·	29·97	29·56	28·94	22·21	22·98
	73·4	100·00	100·00	100·00	100·00	100·00

The composition of the first preparation comes pretty near the formula, while that of the second is very much like that of the substances precipitated in the other way. Possibly in this last precipitate, the alum may have been poured in too rapidly, without agitation of the liquid, and thus caused, at the point where it fell, temporary excess of alum, thus affording the same probable cause for error as in the other mode of precipitation. The above results lead to this, I think, as the most probable conclusion, viz., that the normal carbonate of alumina possesses the composition Al_2O_3,CO_2, analogous to that of the carbonates of iron and glucina and one of the carbonates of chromium; and that the precipitate, produced by the alkaline carbonates in solutions of alumina, consists chiefly of this normal carbonate, generally mixed, however, with more or less of a more basic salt or of hydrate of alumina.

(To be continued.)

On some New Properties of Sulphur,
by M. DIETZENBACHER.

A SMALL quantity of iodine, bromine, or chlorine modifies in a remarkable manner the physical and chemical properties of sulphur. It becomes soft and malleable at the ordinary temperature, and maintains this form for a long time. This modification of sulphur, discovered by M. Charles Sainte-Claire Deville, and by him called *insoluble sulphur*, is almost entirely transformed by this process.

1. By heating a mixture of 400 parts of sulphur and one part of iodine, to about 180°, and then cooling it, a sulphur results remaining elastic for a considerable time. By pouring the sulphur on a glass or porcelain plate, flexible laminæ are obtained. This takes place with even a much smaller proportion of iodine.

Iodide of potassium acts in the same manner as iodine.

Treated in this way by iodine, sulphur becomes insoluble in sulphide of carbon. The liquid turns violet.

2. The action of bromine on sulphur is analogous to that of iodine; only instead of being black with a metallic lustre, the sulphur is of a wax-yellow colour, and is much softer, and remains soft. This modification is produced with 1 per cent. of bromine, and about 200° of heat.

This sulphur is composed of from 75 to 80 per cent. of sulphur insoluble in sulphide of carbon.

3. By passing a current of chlorine on sulphur heated to about 240°, a soft kind of sulphur is obtained, easily drawn out, and the fragments of which readily adhere and unite.

With sulphide of carbon it behaves in the same way as sulphur treated by bromine. When freshly prepared, however, the sulphur, modified by the chlorine, yields about 10 per cent. more matter soluble in sulphide of carbon.

After being worked up for one or more hours, sulphur hardens suddenly, and becomes completely insoluble in sulphide of carbon.

These facts may throw light on some of the details of the manufacture of India-rubber vulcanised by sulphur and chloride of sulphur. Some of them confirm the results already obtained by M. Berthelot.—*Comptes-Rendus.*

Contributions to the History of the Metals associated with Platinum, by M. C. CLAUS.

Ammoniaco-Ruthenic Bases.—According to M. Claus these bodies are not connected with the ammoniacal type; they are copulated ammoniacal combinations, allied to the water type, in which combinations the metal plays its usual part of basic radical, and determines the capacity for saturation on which ammonia has no influence. Ammonia acts only on the insoluble metallic oxide, transforming it into a soluble and energetic base.

To distinguish these new ammoniacal bases, M. Claus calls them *ammiacs;* for instance, the body NH_3Ru is ruthen-monoammiac; $2\ NH_3RuO$ is oxide of ruthen-diammiac, &c.

Thus, as has been shown in a previous memoir, ruthenium bases are prepared by means of double chloride of ammonium and ruthenium $NH_4Cl.RuCl_2$; an easy method for obtaining these bodies will be given further on.

Chloride of Ruthen-Diammiac.—$2\ NH_3RuCl + 3HO$. To 16 grammes of red ammonio-ruthenic chloride, dissolved in 250 grammes of water, is added half a litre of caustic potash, and 16 grammes of carbonate of ammonia. The mixture is kept boiling for at least an hour, until the colour changes from cherry to golden yellow, the whole is then evaporated in a water bath. The crystalline mass, reduced to fine powder, is digested with 16 grammes of water, then thrown on a filter, and washed with weak alcohol until the free ammoniacal salt is dissolved. After drying, the salt is dissolved by heat in 60 grammes of water, with the addition of a small fragment of carbonate of ammonia, and the solution is filtered while hot. On cooling it deposits the salt in beautiful oblique, rhomboidal crystals of a golden yellow colour. They are not very soluble in cold water, and quite insoluble in alcohol. The salt contains 3 molecules of water, which it does not lose even at 120°; the water is disengaged when the temperature is much raised; but at the same time the salt decomposes, leaving silvery white metallic ruthenium in a spongy state.

Its solution is very apt to form very slightly soluble combinations with the solutions of other metallic salts. Caustic potash at the ordinary temperature causes no disengagement of ammonia; on the contrary, liver of sulphur immediately sets the ammonia at liberty. Oxide of silver transforms it into a soluble base, having most of the properties of potash.

On the addition of chloride of platinum to a solution of the above salt, a precipitate is immediately obtained, containing $2NH_3RuCl+PtCl_2$, and resembling double chloride of platinum and ammonium. It is distinguished from the latter, however, by crystallising in small microscopic needles.

The oxygenated salts of the base are prepared by double decomposition with chloride of ruthen-diammiac, and the salts of silver of various acids. They are very soluble in water, and generally insoluble in alcohol.

Sulphate of Oxide of Ruthen-Diammiac, $2NH_3RuO.SO_3+4HO$ takes the shape of beautiful golden yellow rhombic tablets, perfectly transparent, but losing a portion of their water by the action of the air, and then becoming opaque and of a metallic aspect.

The *nitrate* contains $2\,HO$, and forms small rhomboidal prisms of a bright sulphur-yellow colour. When heated it melts, and then decomposes with a slight detonation. It has been analysed by decomposing it, while boiling with hydrosulphate of sulphide of barium, collecting the disengaged ammonia, and estimating the nitrate of baryta remaining in the liquid, and also the ruthenium after transforming it first into sulphate, and then into oxide.

The *carbonate* $2\,NH_3.RuO.CO_2 + 5HO$ is soluble and very alkaline. It crystallises in light yellow rhomboid prisms.

The *free base* $2\,NH_3.Ru.O$ cannot be obtained in a solid state; it decomposes, losing half its ammonia, into another equally energetic base. Oxide of ruthen-diammiac behaves when in solution like potash; it attracts atmospheric carbonic acid, expels ammonia from its combinations, precipitates metallic oxides, dissolves alumina, but not oxides of copper and silver.

Oxide of Ruthen-Monoammiac, $NH_3.RuO + 5HO$ obtained by evaporation in a vacuum from the solution of the foregoing base. It has then the appearance of a brownish-yellow spongy mass, composed of small crystalline deliquescent flakes, and, when in solution, possesses the properties of oxide of ruthen-diammiac. On the tongue it has a more caustic action than caustic potash. Its salts resemble those of the diammiac, but their colour is darker.

The two series of salts of the above bases correspond to those of the bases of Reiset.

Preparation of Red Ammonio-Ruthenic Chloride, $NH_4Cl.RuCl_2$. and different methods proposed for attacking Osmide of Iridium. Among all the methods proposed for attacking osmide of iridium, M. Claus gives the preference to Vauquelin's ancient method of attacking by fusion with saltpetre; he modifies it, however, by the addition of caustic potash to the saltpetre. He objects to the use of peroxide of barium, recommended by MM. H. Sainte-Claire Deville and Debray, because, he says, when the baryta is precipitated by sulphuric acid, a notable quantity of the platinum metals is carried away by the sulphate of baryta.

The following is the *modus operandi:* osmide of iridium is kept at a red heat for about an hour and a-half, in portions of 90 grammes, with 180 grammes of nitrate of potash and 90 grammes of caustic potash, in a silver crucible of 1 litre, which is protected by a Hessian crucible, the bottom being covered with magnesia. The melted mass is carefully decanted into an iron capsule, and the fusion repeated with a fresh portion of the ore. The unattacked portion, remaining at the bottom of the crucible, is again treated two or three times by nitre and potash, and is then reduced to 30 grammes which are put aside. The melted mases resulting from the several operations are pounded, put into a stoppered bottle, with 14 litres of water, and shaken until dissolved. After remaining four hours in the dark, the clear liquid is decanted by means of a syphon, and the black precipitate remaining at the bottom of the bottle, is thrown on a funnel, stopped with asbestos, and carefully washed.

The liquid contains free potash, nitrite, osmite' rutheniate, and nitrate of potash, a little free osmic acid, but no trace of the other platinum metals.

(To be continued.)

TECHNICAL CHEMISTRY.

On the Action of Chloride of Zinc on Silk,

by M. J. Persoy, *Jun.*

Silk rapidly dissolves in a hot concentrated solution of chloride of zinc, and more slowly in a weak and cold solution.

But though chloride of zinc readily dissolves silk, it does not destroy the texture of wool nor vegetable fibres, so that, by means of this re-agent, the complex nature of certain tissues can now be more easily distinguished. Thus, the silk may be dissolved by chloride of zinc, and the wool destroyed by soda, so as to leave only the vegetable fibres. The sample of wool and silk tissue which I have presented to the Academy, a portion of which has been immersed in chloride of zinc, will give an exact idea of this reaction.

The solvent I employed was chloride of zinc, concentrated to about 60° by the areometer. This had been kept boiling with excess of oxide of zinc till it became sensibly neutral to litmus paper. It is then a basic chloride, which, indeed, is slightly troubled on the addition of distilled water, but has the advantage of causing no alteration in vegetable tissues, which may have to be isolated in the course of the experiment.

On contact with chloride of zinc, the silk is converted into a gummy mass, preserving at first the form of the threads of the tissue, but changing gradually to transparent clots, and finally becoming completely dissolved.

Chloride of zinc at 60° gradually dissolves considerable quantities of silk, but, under the influence of heat, the solution will be effected in a few instants, becoming viscous and threading like a thick syrup. It then resembles a concentrated solution of gum arabic. Ammonia produces in the weak solution, diluted with water, a white precipitate, which dissolves completely in an excess of re-agent.

Being in solution in chloride of zinc, I tried in vain to separate the silk from its solvent by ordinary chemical agents, till, at last, M. Graham's dialyser occurred to me.

I first diluted the silk solution by pouring it into water acidulated with hydrochloric acid. This acid prevents the formation of the precipitate of basic chloride of zinc referred to previously, which water simply would induce.

In a former experiment I twice filtered the solution without getting rid of its slightly opalescent appearance, and I placed it in this state on the dialyser. A larger quantity of chloride of zinc passed directly, and after a few hours the liquid had become much more viscid; then it increased in volume and formed into an opaline jelly, resembling starch. This jelly contained yet a little chloride of zinc, which could not be separated on account of the new physical condition of the matter. It had the insipid taste and smell of starch, while its chemical characteristics were dissimilar. In fact, it did not puff up with caustic potash, nor liquefy with sulphuric acid. Soluble in acetic acid as steeped starch, yet, if dried, it no longer dissolves in this re-agent, being transformed into vitreous and brittle fragments. But I have found it to possess one singular property: evaporated to dryness in a thin layer in a platinum capsule, then carefully and gradually heated, it yields a vivid gooseberry-red matter, similar in colour to murexide. The colour thus formed has, however, no stability. As the reaction is produced at a very high temperature, nothing up to this point indicates the animal nature of the product.

Not until the temperature reaches dull red does the matter completely decompose and disengage the disagreeable odour of torrefied silk.

In the following experiments I prevented the formation of this kind of fibroine starch by diluting the solution with more water before submitting it to the action of the dialyser, and especially by heating it for a few instants, which effects the removal, by filtration, of the matters in suspension, to which is due the opaline appearance of the liquid.

With these precautions, and by means of the dialyser, all the chloride of zinc can be separated, and a limpid, colourless, and insipid liquid obtained, which, by evaporation, gives a gold-coloured friable varnish.—*Comptes-Rendus.*

PHYSICAL SCIENCE.

On a new Form of Spectroscope. (*From a letter of Dr.* WOLCOTT GIBBS *to* B. SILLIMAN, *Jun.*)

MESSRS. J. and W. GRUNOW, the well known opticians, have just completed, at my suggestion, a spectroscope involving a new principle, or, rather, one for the first time applied to instruments of this kind. In this instrument, the prism of flint glass has a refracting angle of only 37°: the rays which diverge from the slit are rendered parallel in the usual manner by an acromatic lens having the slit in its principal focus. The bundle of rays then falls upon the first surface of the prism at a perpendicular incidence, and of course makes an angle of 37° with the second surface. Under these circumstances, the refraction takes place at an angle so near the limiting angle that the refracted rays emerge nearly parallel to the second surface of the prism. The amount of dispersion produced in this manner is very great, while the loss of light, occasioned by reflection at the first surface in prisms of 60° placed in the position of least deviation is avoided. The spectrum thus produced possesses remarkable intensity, and the dark lines are seen in countless numbers and with great distinctness. The instrument in this form is sufficient for all chemical purposes, but it is so constructed as to permit the use of a second prism by which the length of the spectrum is of course greatly increased. Though the telescopes are only 6 inches in length, with a magnifying power of about 6, the spectrum compares very advantageously with that of a large apparatus with telescopes of 18 inches focal length, and 1½ inch aperture, and a prism of 60°. I may mention that the centre of the second surface of the prism lies in the vertical axis of the instrument, and also that in a prism of this kind the refracted rays diverge as if from a single radiant point, which is not the case with prisms of the ordinary construction, the angular dispersion being at the same time much greater. So far as I have been able to find, this form of prism was first employed by Matthiessen. In a lithographed copy of Regnault's lecture on "Optics" at the College de France, in 1848, prisms on this principle of various forms are figured and described, together with the spectra produced. These last exhibit an extraordinary extension of the violet end of the spectrum. A Matthiessen prism of flint glass in which the first surface is concave, so as to admit the addition of a double convex lens of crown glass, appears to be preferable for the spectroscope, in consequence of the saving of light.—*American Journal of Science*, XXXV., 110.

NOTICES OF PATENTS.

2986. *Manufacturing Gas for Illuminating Purposes.* H. BAMBACH, Cologne. Dated November 27, 1861. (Not proceeded with.)

THIS proposal consists in preparing hydrogen gas in the usual manner from dilute sulphuric acid and iron or zinc, and in passing this gas through some readily volatile hydrocarbon—such as benzol or turpentine—whereby it becomes sufficiently charged with the vapour of these substances to permit of its employment for illuminating purposes.

This suggestion has been already so many times patented in this country, that it will only be necessary to refer to page 94 of the present volume to find evidence bearing upon the subject.

Grants of Provisional Protection for Six Months.

199. Robert Penney, Carbrook, Cheshire, "An improved solution or mixture for 'fixing' certain colours employed in printing calico and other fabrics."

219. Edward Booth and George Booth, Gorton, near Manchester, and Andrew Swallow, Stalybridge, Lancashire, "Certain improvements in the mode of fixing colouring matter on cotton, silk, wool, and other fibres and materials, and certain improvements in finishing such like and other textile fabrics and yarns."

247. Elijah Freeman Prentiss, and John Carrington Sellars, Birkenhead, Cheshire, "Improvements in treating rock oil, petroleum, paraffin oil, coal oil, and paraffin and other like mineral oils and products therefrom."

326. Henry Dircks, Blackheath, Kent, and John Henry Pepper, Regent Street, London, "Improvements in apparatus to be used in the exhibition of dramatic and other like performances." Petitions recorded February 5, 1863.

267. John Pouncy, Dorchester, Dorsetshire, "Improvements in obtaining, transferring, and printing from photographic pictures or images, also in preparing materials for the same."

271. Charles Hanson Greville Williams, Rumford Street, Glasgow, Lanarkshire, N.B., "Improvements in the manufacture of red colouring matters."

287. John Grossmith, Newgate Street, London, "An improved mode of producing the aura-electric gas."

289. William Drummond, Bank of England, London, "Improvements in apparatus for stopping the supply of gas to burners when the light is put out."

307. William George Valentin, Royal College of Chemistry, Oxford Street, London, and Frederick Levick, Blaina, Monmouthshire, "Improvements in generating combustible gases, and in the apparatus employed therefor."—Petitions recorded February 3, 1863.

343. Jean Sirou, Castel Sarrazin, Tarn et Garrone, France, "A new medicinal preparation for internal and external application."

Notices to Proceed.

2909. George Darlington, Minera, Denbighshire, "Improvements in the manufacture of zinc oxide."

2913. William Clark, Chancery Lane, London, "Improvements in the treatment of copper ores, and in apparatus for the same." A communication from Frederic Le Clerc, Boulevart St. Martin, Paris. Petitions recorded October 29, 1862.

199. Robert Penney, Carbrook, Cheshire, "An improved solution or mixture for 'fixing' certain colours employed in printing calico and other fabrics." Petitions recorded January 22, 1863.

3010. Charles Otto Heyl, Berlin, Prussia, "Improved means and machinery to be used for the purpose of extracting fatty oils from oleaginous seeds, and for purifying the said oils, and for extracting the agents employed from the exhausted residue."—Petition recorded November 7, 1862.

3175. Alfred Vincent Newton, Chancery Lane, London, "An improved mode of preparing oxide of zinc as a pigment."—A communication from George Lewis, Philadelphia, U.S.—Petition recorded November 26, 1862.

157. Ephraim Sabel, Moorgate Street, London, "Improvements in the manufacture of artificial stone."—A communication from Joseph Sepulchre, Huy, Belgium.—Petition recorded January 19, 1863.

CORRESPONDENCE.

Preservation of Stone.

To the Editor of the Chemical News.

Sir,—A Mr. Je Veux de Bonne Guerre has taken upon himself the responsibility of declaring that he has fully examined specimens of the dried residue of the stone-preserving solution prepared by us.

What he means by examined he will better explain perhaps when we tell you that not one atom of the solidified compound has ever left the factory, nor one drop of the prepared solution ever been parted with, but that applied to specimens of stone sent us by various persons to operate upon, and from which it would be impossible to obtain the residue Mr. Guerre speaks of.

We are, &c. Jesse Rust & Co.

Lambeth Glass Works, Carlisle Street, S.

Foreign Aconitine.

To the Editor of the Chemical News.

Sir,—Can you or any of your readers inform me what it is that is sold as foreign aconitine? The alkaloid is generally regarded as a most powerful poison, and I believe that which is made by English manufacturers is a poison. The foreign article, however, I fancy is a comparatively harmless one. I gave a small dog three grains of a substance, labelled "Pure Aconitina Exot," and obtained from a very respectable source, but it had not the slightest effect on the animal. I don't think it occasioned a moment's discomfort. A few days afterwards I gave three grains of another sample, not said to be "exot," at least that was not on the label, and it was a very different looking article. This made the dog dreadfully sick; he vomited and retched almost incessantly for more than an hour, and then he got better, and the next day was quite well. I think these facts should be known, in order that medical men may know what is the value of the foreign article, and I shall be glad, as I said before, if you or any of your readers can tell me what it really is. I am, &c.

Enquirer.

Manufacture of Iron and Steel.

To the Editor of the Chemical News.

Sir,—You appear to have misconstrued my specification in your last Number, or you could not have remarked,—"The mode of proceeding above described is directly applicable to the manufacture of steel upon Mr. Bessemer's principle." The fact is, the whole of my principle is as diametrically opposite to Mr. Bessemer's as it is possible for any two to be. In as few words as possible I will explain.

A distinguished metallurgist, writing to the *Times* in August last, referring to the manufacture of steel, observes:—"The processes now in operation are founded on two opposite principles, namely, putting carbon into wrought iron and taking carbon out of pig iron." The latter has been adopted by Mr. Bessemer, the former by me; but both principles are as old as the hills, the improvements being in the mode of manufacture. On the one principle, cast steel, in large masses, from pig iron direct, are produced; on the other, the same from wrought iron,—and from both without the use of crucibles. The one requires the careful selection of pig iron from selected ores; the other embraces all kinds of wrought iron, made from any known iron ores, with but rare exceptions.

On Mr. Bessemer's principle, the puddling furnace is entirely dispensed with,—in mine, its use forms the whole foundation of success. He makes his steel rails direct from pig iron,—I manufacture mine from old, worn-out wrought iron, rails, &c. The first cast steel rail (a bridge rail) rolled by my process, was made from an old, worn-out wire rope. How could this be done "upon Mr. Bessemer's principle?" Again, upon his principle, pig iron is converted into cast steel without the use of fuel,—in my conversion of wrought iron, fuel is used, both in the solid and the gaseous state. I am &c.

George Parry.

Ghbw Vale, Monmouthshire, March 2.

MISCELLANEOUS.

Extension of the Act for Preventing the Adulteration of Food.—Viscount Raynham, we learn, is preparing a bill for the above object. Many of our readers probably could give facts and suggestions which would be of value to his lordship.

Influence of Ozonised Air upon Animals.—Dr. Ireland says: "These experiments were most carefully performed, and all sources of complication avoided as carefully as possible; and, as I felt satisfied of their correctness, I saw no reason to sacrifice the lives of more animals in repeating them. I submit to the reader the following conclusions:—1. Ozonised air accelerates the respiration, and, we may infer, the circulation. 2. Ozonised air excites the nervous system. 3. Ozonised air promotes the coagulability of the blood, probably by increasing its fibrine. In the blood, however, ozone loses its peculiar properties, probably entering into combination with some of the constituents of the circulating fluid. 4. Animals can be subjected to the influence of a considerable proportion of ozone in the air for hours without permanent injury; but in the end ozone produces effects which may continue after its withdrawal, and destroy life."—*Edinburgh Medical Journal.*

Royal Institution of Great Britain.—General Monthly Meeting, Monday, March 2, 1863.—William Pole, Esq., M.A., F.R.S., Treasurer and Vice-President, in the Chair.—Edward Atkinson, Esq., the Rev. Henry Blunt, Col. Craven Hildesley Dickens, Frederick William Gingell, Esq., Ernest Hart, Esq., William Hartree, Esq., John Hogg, Esq, M.A., F.R.S., William Wood Humphry, Esq., William Edward Kilburn, Esq., Henry Lainson, Esq., Thomas Leckie, M.D., James Lees, Esq., William McKeand, Esq., Abraham Pope, Esq., John Rivington, Esq, John Rutherford Russell, M.D., John Benjamin Smith, Esq., M.P., and George S. Trower, Esq., were elected members of the Royal Institution. The thanks of the members were returned to Mr. James Glaisher, Professor Frankland, the Rev. George Williams, and to Mr. John Lubbock, for their discourses on the evening meetings on Fridays, February 6, 13, 20, and 27. The presents received since the last meeting were laid on the table, and the thanks of the members returned for the same.

The Midland Society of Chemists and Druggists.—The chemists and druggists of Nottingham some time ago took the praiseworthy step of instituting "a society for interchange of sentiment on the trade, and the protection of interests;" and they now propose to go a step further, and give a scientific and educational interest to the proceedings. The rules of the new society we subjoin, and we may add that we heartily wish the promoters success. We should be glad to hear of a similar society in every large town.

"1st.—That a society shall be formed in Nottingham, consisting of the chemists of the town, as members, and the assistants and apprentices as associates, and that the society shall be called the 'Nottingham Chemists' Association.'

"2nd.—That the meetings shall be held in one of the public rooms of the town fortnightly or monthly, as shall be determined, for the reading of papers relative to pharmacy, chemistry, or any subject likely to interest the society.

"3rd.—That a library be formed of books of reference and standard elementary works on chemistry, pharmacy, botany, and materia medica, &c., for circulation amongst the members and associates of the society; and that subscriptions and contributions be solicited on its behalf.

"4th.—That those physicians, surgeons, and others of standing and scientific attainments in the town, taking an interest in the society, be elected honorary members.

"5th.—That the society shall consist of president, vice-president, secretary, treasurer, and council of eight members, to be elected annually.

"6th.—That the subscriptions be limited to 10s. 6d. per annum for members, and 5s. for associates, payable in advance.

"7th.—That short courses of lectures, at intervals, shall be instituted in the elementary branches of science, for the benefit and instruction of the associates.

"8th.—That, if found practicable, after a year or two, a museum shall be formed, either in connection with the museum at the Mechanics' Institute, or separate, as shall be determined.

"9th.—That all topics relating to the private arrangement and management of business be studiously avoided.

"10th.—That a *conversazione* shall be held annually in the society's room or rooms, and that objects of interest be solicited, and invitations addressed to the different scientific and literary societies in the town, and to those persons taking an interest in the society."

The Coming Illuminations.—Among the many splendours to be seen in the metropolis on the evening of the 10th inst., there will be one which promises to eclipse every other light in its neghbourhood, and is of such a character as to deserve some notice in a scientific journal. Most of our readers are, no doubt, aware that it is intended to place an electric light on the top of the monument, but few, perhaps, have any idea of the intensity of the light which it is proposed to exhibit. By the kindness of Mr. Ladd, who has undertaken the superintendence of the arrangements, we are enabled to give the following particulars:—In the first place, it is intended to place a lantern at each angle of the gallery, the light from which will be reflected upon the gilt ball, meant to resemble a mass of flame, at the top. Each of these lanterns will receive the electricity from eighty cells, having platinums of five and a-half inches by two and a-quarter inches. The effect of this, it is supposed, will be to give the appearance of strong sunlight. On the summit of the ball will be placed another light, of a power which, we believe, has never before been seen. To produce this a hundred cells will be employed, the platinums having the dimensions of six inches by seven and three-quarter inches. Those of our readers who are acquainted with the ordinary means used for obtaining the electric light will be able to form some idea of what (should all the arrangements succeed, of which, however, we have no doubt) the splendour of this illumination will be. The light, it is calculated, will be plainly visible at Windsor, and at all distances within twenty-five miles, and, probably, far beyond this range. In the aggregate, more than 16,000 square inches of platinum will be used on the occasion. We could almost wish that the proposal to put another light on St. Paul's will be abandoned, as the intensity is not likely to approach that of the display of the monument, and it will have the effect of spoiling the distant view of the latter. We shall be glad to learn from our country friends, who may have the opportunity of making the observation, how far this light is seen, and what may be the effects produced.

Mummy Wheat.—The *Presse Scientifique des Deux Mondes* contains a description of a series of experiments made in Egypt by Figari-Bey on the wheat found in the ancient sepulchres of that country. A long dispute occurred a few years ago, as to what truth there might be in the popular belief, according to which this ancient wheat will not only germinate after the lapse of three thousand years, but produce ears of extraordinary size and beauty. The question was left undecided; but Figari-Bey's paper, addressed to the Egyptian Institute at Alexandria, contains some facts which appear much in favour of a negative solution. One kind of wheat which Figari-Bey employed for his experiments had been found in Upper Egypt, at the bottom of a tomb at Medinet-Aboo, by M. Schnepp, secretary to the Egyptian Institute. There were two varieties of it, both pertaining to those still cultivated in Egypt. The form of the grains had not changed; but their colour, both within and without, had become reddish, as if they had been exposed to smoke. The specific weight was also the same—viz., twenty-five grains to a gramme. On being ground they yield a good deal of flour, but are harder than common wheat, and not very friable; the colour of the flour is somewhat lighter than that of the outer envelope. Its taste is bitter and bituminous; and when thrown into the fire, it emits a slight but pungent smell. On being sown in moist ground, under the usual pressure of the atmosphere, and at a temperature of 25 degrees (Reaumur), the grains became soft, and swelled a little during the first four days; on the seventh day their tumefaction became more apparent, with an appearance of maceration and decomposition; and on the ninth day this decomposition was complete. No trace of germination could be discovered during all this time. Figari-Bey obtained similar negative results from grains of wheat found in other sepulchres, and also on barley proceeding from the same source; so that there is every reason to believe that the ears hitherto ostensibly obtained from mummy wheat proceed from grain accidentally contained in the mould into which the former was sown.

Solders.—Soldering is the art of uniting the surfaces of metals together by partial fusion, and the insertion of an alloy between the edges, which is called solder, it being more fusible than the metals which it unites. Solders are distinguished as hard and soft, according to their difficulty of fusion. Hard solders usually melt only at a red heat, but soft solders fuse at lower temperatures. In applying solder it is of the utmost importance that the edges to be united should be chemically clean—free from oxide—and they should be protected from the air by some flux. The common fluxes used in soldering are borax, sal ammoniac, and rosin. Hard silver solder is composed of four parts of fine silver and one of copper, made into an alloy and rolled into sheets. It is quite difficult of fusion. Soft silver solder is composed of two parts of silver, one part of brass, and a little arsenic, which is added at the last moment in melting them. It will be understood that these alloys are commonly run into convenient bars or strips for use. Silver solders are used for soldering silver work, gold, steel, and gun-metal. A neater seam is produced with it than with soft solder. It is commonly fused with the blow-pipe. A strip of thin silver solder is laid on the joint to be closed, the blow-pipe is brought to bear upon it, when it melts and runs into the joint, filling it up completely. Button solder is employed to solder white metals, such as mixtures of copper and tin. It is composed of tin ten parts, copper six, brass four. The copper and brass are first melted, then the tin is added. When the whole is melted the mixture is stirred, then poured into cold water and granulated, then dried and pulverised in a mortar for use. This is called granulated solder. If two parts of zinc are added to this alloy, it makes a more fusible solder. Fine gold cut into shreds is employed as a solder for joining the parts of chemical apparatus made of platinum. Copper cut into shreds is used as a solder for iron. Hard silver solders are frequently reduced to powder, and used in that condition. Soft solder consists of two parts of tin and one of lead. An excellent solder is made of equal parts of Banca tin and pure lead. It is used for soldering tin plate, and, if well made, it never fails. The following is a useful table of solders with their fusing points:—

No.	Parts of Tin.	Lead.		Melting deg. F.
1	1	25		558
2	1	10		541
3	1	5		511
4	1	3		482
5	1	2		441
6	1	1		370
7	1½	1		334
8	2	1		340
9	3	1		356
10	4	1		365
11	5	1		378
12	6	1		381
13	4	4	1 Bismuth	320
14	3	3	1 ,,	310
15	2	2	1 ,,	292
16	1	1	1 ,,	254
17	1	2	2 ,,	236
18	5	3	3 ,,	202

The alloy No. 8 is used sometimes for soldering cast-iron and steel; the flux used for this purpose is sal-ammoniac, but common resin may be employed. Gold and silver are sometimes soldered with pure tin and a flux of resin. Copper, brass, and gun-metal are soldered with No. 8 and a flux of resin or sal-ammoniac. The chloride of zinc is used for soldering sheet and plate iron as a flux with the same solder. Lead and tin pipes are soldered by plumbers with Nos. 6, 7, and 8 and a flux of resin and sweet oil. In soldering with soft brass, the ends of the article to be soldered are secured together by a wire, and granulated solder and powdered borax are mixed in a cup with a small quantity of water, and spread along the joint with a spoon. The article is then placed in a clear fire, and the solder melts at a bright red heat, when the article is then removed from the fire. In soldering small articles with the blow-pipe, they are supported on a piece of charcoal, or, what is better, pumice-stone, and the flame is ejected upon the solder. In soldering lead pipes, the parts to which the solder is not to be attached are usually covered with a mixture of lamp-black and size. In soldering any articles care must be exercised to have the edges of the plates or articles perfectly clean, or the solder will not adhere. A flux is employed for the purpose of preventing oxidation. Resin and sal-ammoniac, powdered and mixed together, make a good flux for copper and sheet iron soldering. In other cases, a strong solution of sal-ammoniac is used to moisten the edges of the joint, then the resin is sprinkled upon it, and the solder applied. The chloride of zinc is made by dissolving pieces of zinc in muriatic acid. It is well adapted for soldering zinc plates and pipes, and is applied with a brush to moisten the edge of the article to be soldered. The solder is then applied in the usual way with a tool. Zinc is a very difficult metal to solder, because it is so easily coated with oxide, and it also volatilises with heat.

SCIENTIFIC SOCIETIES.

MEETINGS FOR THE ENSUING WEEK.

March 9. Monday.

Geographical—15, Whitehall Place. 8.30 p.m.
British Architects—9, Conduit Street. 8 p.m.
Medical—32a, George Street, Hanover Square. 8.30 p.m.

10. *Tuesday.*

Royal Institution—Albemarle Street. 3 p.m. Professor Marshall, "*On Animal Mechanics.*"
Medical and Chirurgical—53, Berners Street. 8.30 p.m.
Civil Engineers—25, Great George Street, Westminster. 8 p.m.
Zoological—11, Hanover Square. 9 p.m.
Tyro-Egyptian—22, Hart Street, Bloomsbury. 7.30 p.m.

11. *Wednesday.*

Society of Arts—John Street, Adelphi. 8 p.m. G. R. Burnell, "*On the Influence of Social Institutions upon the Progress of Art in this and Foreign Countries.*"
Graphic—Flaxman Hall, University College. 8 p.m.
Microscopical—King's College. 8 p.m.
Literary Fund—4, Adelphi Terrace. 2 p.m. *Anniversary.*
Royal Society of Literature—4, St. Martin's Place. 8.30 p.m.
Archæological Association—32, Sackville Street. 8.30 p.m.

12. *Thursday.*

Royal Institution—Albemarle Street. 3 p.m. Dr. Frankland, "*On Chemical Affinity.*"
Royal—Burlington House. 8.30 p.m.
Antiquaries—Somerset House. 8.30 p.m.
Royal Society Club—St. James's Hall. 6 p.m.

13. *Friday.*

Royal Institution—Albemarle Street. 8 p.m. Dr. J. H. Gladstone, "*On Fog Signals.*"
Astronomical—Somerset House. 8 p.m.

14. *Saturday.*

Royal Institution—Albemarle Street. 3 p.m. Prof. Max Müller, "*On Language.*"
Royal Botanic—Inner Circle, Regent's Park. 3.45 p.m.

ANSWERS TO CORRESPONDENTS.

⁂ All *Editorial Communications* are to be addressed to the Editor, and *Advertisements* and *Business Communications* to the Publisher, at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

⁂ In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

S. P.—Answered last week.

W. D. S.—We know of nothing which will effect the object.

W. B.—There is no book that we know of at present which will give the information required by our correspondent.

C. R. asks information which we make a rule not to supply if we possess it. Our correspondent will easily see the reason.

Y. V.—Make the frame-work of the basket of iron wire, and set it in a strong solution of alum.

W. L. B.—There is no book specially on the subjects. The last edition of Fownes', just issued, gives as much information as any.

Received.—Rules of the Leeds Chemists' Association; also an Inaugural Address, by T. Harvey, Esq., President.

Mr. McB.—You can see them all at the Patent Office in Chancery Lane. There are one or two monthly journals which give particulars of some, but not all, patents taken out. A Patent agent would probably give you all the information you require.

F.C.S.—The time required for the degree of Dr. of Science at the London University is four years, dating from the matriculation examination. No particular curriculum is prescribed, but the candidate must pass two examinations in mathematics, including arithmetic and algebra, geometry, plain trigonometry, mechanical and natural philosophy, chemistry, botany, and zoology, before he can obtain the degree of Bachelor. After that, he must wait two years before he can pass for Doctor of Science. For the last examination, the candidate may choose his own subject from the sciences. See the London University Calendar. For the German Ph.D. degree, a residence at the University of a fortnight to six weeks is required; and, beyond this, the candidate must be prepared with some good testimonials, a thesis of some sort, and about £50 in hard cash.

THE CHEMICAL NEWS.

Vol. VII. No. 171.—March 14, 1863.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Contributions to the History of the Metals associated with Platinum, by M. C. Claus.

(*Continued from page* 116.)

The black powder is formed principally of oxide of iridium and acid iridate of potash, sesquioxide of ruthenium, oxides of osmium (OsO_2), of iron, and of traces of the oxides of copper and palladium, all soluble in acids; of oxides of iridium, platinum, and rhodium, insoluble in acids; and finally of a small quantity of unattacked ore. This mixture is fused with potash and nitre, and treated in the same way as the original ore. By this means a liquid is obtained which is added to the first, and a residuum still containing about one-fourth of ruthenium retained in the ore.

The solution, carefully neutralised by weak nitric acid, deposits an abundant black precipitate, and when its deposition is completed, the liquid is decanted, and the precipitate collected on a filter, and washed. This precipitate is composed chiefly of hydrated oxide of osmium, $OsO_2 + 2HO$, containing about 15 to 20 per cent. of sesquioxide of ruthenium. The liquid contains osmic acid (OsO_4), perruthenic acid, and always binoxide of ruthenium.

The black precipitate readily furnishes the red salt of ruthenium. The total amount of this precipitate is 240 grammes, containing rather over 30 grammes of hydrate of sesquioxide of ruthenium. Treated by 1 kilogramme of hydrochloric acid, and 1·5 of nitric acid in a retort furnished with a large receiver, and thoroughly cooled where the osmic acid condensed, it is to be carefully distilled until the contents of the retort become viscous; the residuum consists of bichloride of ruthenium, $RuCl_2$, with a little sesquichloride, Ru_2Cl_3, and still contains traces of osmic chloride. The presence of osmium is ascertained by calcining a small portion of the substance in a platinum spoon, by introducing it alternately into the oxidising and reducing portions of the flame of a spirit-lamp. If the matter contains osmium, a bright light is immediately produced in the oxidising flame, in which case the distillation with the mixture of hydrochloric and nitric acids is repeated until no osmium remains. There is no danger of losing the ruthenium as perruthenic acid, as this acid never forms under these circumstances.

The osmium being thus expelled, the residuum is dissolved in the smallest possible quantity of boiling water; a salt of ammonia added, and the whole left to cool. After some time, all the sesquichloride of ruthenium in the mixture is deposited in the form of a brown salt, containing $2NH_4Cl + Ru_2Cl_3$. This precipitate is separated, and a considerable quantity of sal ammoniac added to the hot solution of chloride, and then left to cool. The double chloride $NH_4Cl.RuCl_2$ is deposited as a dark red powder. The mother waters furnish a further portion when evaporated quickly. The whole is washed on a filter with weak alcohol, to remove the hydrochloride of ammonium. The double salt is then sufficiently pure to serve in the preparation of bases; it can contain only traces of brown salt, which are of no disadvantage in this preparation. To obtain it perfectly pure, it must be treated by a quantity of water, insufficient to dissolve it completely, and the solution must be crystallised by evaporation. Beautiful octohedral crystals are then deposited, which, when calcined, yield perfectly pure ruthenium. About thirty grammes of this double chloride are obtainable.

The slightly-coloured liquid, wherein the black precipitate is deposited (a mixture of hydrate of oxide of osmium and sesquioxide of ruthenium), still contains a considerable quantity of ruthenium, as perruthenic acid, together with much osmic acid.

It is distilled with hydrochloric acid until all the osmic acid is expelled. The liquid remaining in the retort contains much nitrate of potash, which is partially separated by crystallisation, and then evaporated to dryness. It is redissolved in water, and the ruthenium precipitated by hydrosulphate of ammonia, in presence of a little acid. The sulphide of ruthenium is transformed into oxide by simple calcination, or into sulphate by the action of nitric acid.

The best means to obtain the osmium is to distil the liquids containing osmic acid; the latter passes before the other acids, and may be precipitated by sulphuretted hydrogen in the state of oxisulphide. Or better still, to saturate the liquid with caustic potash, to add alcohol, to transform the osmiate of potash into osmite; and then to precipitate by powdered sal ammoniac all the osmium as chloride of the osmic base of a yellowish dove colour. The latter process has been described by M. Fremy.

It should be observed that, in operating with pure ruthenium, it is very difficult to transform sesquichloride of ruthenium into bichloride by nitric acid; the presence of oxide of osmium favours the change.

Another important fact is, that the fusing process described above succeeds ill with ores poor in osmium and ruthenium, in which case a solution of iridium in the state of basic iridate of potash may enter into the combination. For this reason the black powder must not be treated a third time by nitre and potash, as it is already so poor in ruthenium and osmium that there would be risk of dissolving part of the iridium.

The following process may be used for extracting from it the osmium and ruthenium:—Distil with aqua regia; the osmium distils in the state of osmic acid, and the whole dissolves, except the insoluble combination of oxides of rhodium and platinum, with a little oxide of iridium. Concentrate the remaining liquid in a capsule, and leave it to crystallise. Most of the iridium separates in the form of a black crystalline powder, containing

$KCl + IrCl_2$. With the addition of a little sal ammoniac the mother waters of these crystals deposit the remainder of the iridium as iridio ammonic chloride, the salts of ruthenium remaining in solution. A fresh quantity of hydrochlorate of ammonia precipitates the ruthenium in the state of nearly pure brown chloride ($2NH_4Cl.Ru_2Cl_3$). About 30 grammes of this compound are thus obtained, which, with the red salt previously collected, give about 6·5 per cent of metallic ruthenium, extracted from the amount of ore employed. At the same time 450 grammes of salt of iridium are obtained, containing yet a little ruthenium and platinum.

To attack residuums rich in rhodium, M. Claus recommends the process of MM. H. Deville and Debray, which consists in fusing them with lead and litharge, and dissolving out the lead of the metallic mass by nitric acid. But, instead of melting together the powder, insoluble in nitric acid, with peroxide of barium and nitrate of baryta, he prefers treating this powder by moist chlorine and sea salt, at dull red heat, as does M. Wöhler. This process is especially suited to rhodium, which, under these conditions, chlorine easily attacks. Add a little nitric acid to the solutions, and heat them, to transform the sesquichloride of iridium into bichloride; then separate the salt of iridium by several precipitations by means of hydrochlorate of ammonia.

The author further discusses some secondary points on which he differs from the French chemists; among others, the relations between the platinum and other metals.

Finally, he observes, on the subject of a recent work by M. Martius, that potassic osmiocyanide, obtained by this chemist in presence of ferrocyanide, ought to contain a portion of the latter salt with which it is isomorphous. Osmiocyanide, prepared with cyanide of potassium and ammonio-osmic chloride, is perfectly colourless, and crystallises in beautiful tablets of the type of square-based straight prisms. Potassic rutheniocyanide behaves in the same way; prepared with ferrocyanide it is yellow, and contains iron, while when pure it is quite colourless.—*Répertoire de Chimie pure et Appliquée.*

On the Carbonates of Alumina, Glucina, and the Sesquioxides of Iron, Chromium, and Uranium,* *by* THEODORE PARKMAN, *Ph.D.*

(*Continued from page* 115.)

4. *Carbonate of Glucina.*

G_2O_3, CO_2.

Schaffgotsch† gives an analysis of a carbonate of glucina precipitated by boiling a solution of glucina in carbonate of ammonia. He finds the percentages, 47·53 G_2O_3, 17·57 CO_2 and 34·90 HO, corresponding most nearly to the formula, $3G_2O_3$, $2CO_29 + HO$. Wecren‡ gives analyses of precipitates obtained by boiling solutions of glucina in carbonate of ammonia, under somewhat different circumstances, and obtains the formulas, $4G_2O_3 + 4CO_2 + 11HO$: $3G_2O_3 + 2CO_2 + 10HO$: $7G_2O_3 + 3CO_2 + 14HO$. For a precipitate by carbonate of ammonia, not in excess, from a neutral solution of glucina in hydrochloric acid, he obtains the formula, $11G_2O_3 + 6CO_2 + 26HO$.

The salt of glucina which I used was prepared by dissolving glucina in a slight excess of sulphuric acid and washing out the excess of acid with alcohol. The glucina used had been freed from alumina and iron, by dissolving it in sesquicarbonate of ammonia and precipitating it by boiling, and by again dissolving it in caustic soda and precipitating it by boiling. From the sulphate thus obtained the carbonate was precipitated by carbonate of soda in slight excess, and analysed in the same way as the carbonate of iron. The wash-water from the ignited glucina contained no glucina, no glucinate of soda having been formed. Analyses of two preparations gave the following results:—

	Calculated.		Prep. I.		Prep. II.
			1.	2	
G_2O_3 .	38·	63·34	63·31	63·57	63·52
CO_2 .	22·	36·66	36·69	36·43	36·48
	60	100·C0	100·00	100·00	100·00

If glucina be regarded as a protoxide, as it has been by some chemists, the above results would correspond to the formula, $3GO, CO_2$ (calculated, G.=4·7, 63·40 GO and 36·60 CO_2).

5. *Carbonate of the sesquioxide of Uranium.*

The precipitate produced by carbonate of potash in nitrate of uranium, after being washed with cold water and dried in the air, has, according to Ebelmen,§ the following composition: 3·66 KO, 3·87 CO_2, 81·98 U_2O_3 and 10·49 HO; and is probably a mere mixture.

The precipitates which I have examined were precipitated, some from the sulphate, others from the nitrate of uranium, by carbonate of soda, in as small excess as possible. The general mode of analysis, described under carbonate of iron, could not be employed here. There would be a variable mixture left in the bulbed tube, after ignition, consisting partly of uranate of soda (formed when sesquioxide of uranium is ignited with carbonate of soda) and partly of proto-sesquioxide of uranium. The substance was therefore ignited in a stream of dry hydrogen, free from carbonic acid, and the carbonic acid absorbed by a weighed soda-lime tube.‖ In this way the sesquioxide is reduced to protoxide, which does not drive out carbonic acid from carbonate of soda. The residue in the bulb was washed somewhat, ignited, evaporated to dryness with hydrochloric acid and chloride of ammonium, and ignited in a stream of hydrogen. After thorough washing, it was again ignited in hydrogen and weighed as UO. This process is, according to Rose,¶ the best method for separating uranium from the alkalies.

The three following analyses were of precipitates from the nitrate. The first two were dried: the last was not.

	Calculated.		Prep. I.		Prep. II.
			1.	2.	
$3U_2O_3$.	432	90·76	91·60	90·29	90·55
$2CO_2$.	44	9·24	8·40	9·71	9·45
	476	100·00	100·00	100·00	100·00

The five next analyses were of precipitates from the

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* *American Journal of Science and Arts*, vol. xxxiv., page 321.

† *Pogg. Ann.*, 1, 83.

‡ *Pogg. Ann.*, xc. 91.

§ *N. Ann. Chim. Phys.*, v., 189.

‖ This method of absorbing carbonic acid by soda-lime seems to work very well. In one case a tube, used for the fourth time, still absorbed the carbonic acid perfectly, as was shown by a tube, containing pieces of solid caustic potash, attached to it, not increasing in weight. The originator of the method uses, instead of a chloride of calcium tube, a U tube, containing pumice soaked in sulphuric acid, and having a little sulphuric acid at the bottom, to show the rate at which the gas is passing through. In my analyses I used a chloride of calcium tube, as usual, and to show the rapidity of the stream of gas I employed a little bulb-apparatus, passing through the cork at one end of the soda-lime tube, and filled with caustic potash. This has the advantage that it absorbs a large part of the carbonic acid, and thus makes the soda lime last longer.

¶ "Analytical Chemistry," French ed., ii., 252.

sulphate. The last was precipitated by pouring the sulphate of uranium into the carbonate of soda; the others, as usual, by pouring the carbonate of soda into the uranium solution.

	Calculated.		Prep. I.		Prep. II.		Prep. III.
			1.	2.	1.	2.	
$3U_2O_3$.	432	95·16	95·52	94·60	94·13	94·68	94·54
CO_2 .	22	4·84	4.48	5·40	5·87	5·32	5·46
	454	100·00	100·00	100·00	100·00	100·00	100·00

Notwithstanding the tolerably close agreement of the above results with the formulas just given, I am strongly inclined to think that these formulas do not express the true composition of the precipitates. A portion of one of the precipitates, after very protracted washing with cold water, still contained alkali, showing the presence of uranate of soda.** This result might indeed be inferred, almost with certainty, from the very strong affinity of sesquioxide of uranium for the alkalies.†† On account of this impurity, as well as the ease with which all these carbonates appear to lose carbonic acid, the observed percentage of carbonic acid should be less than the calculated. Instead of this, it is in nearly every case larger. I suspect therefore that the precipitates in question are mixtures of a less basic carbonate with uranate of soda. More of the latter appears to be formed in the precipitate from the sulphate of uranium than in that from the nitrate. Why this should be I cannot explain. What the true composition of the carbonate of uranium would be, if it could be obtained pure, may be with probability inferred from the composition of its double salts with carbonate of potash and with carbonate of ammonia, in which it exists, according to Ebelmen,‡‡ as U_2O_3, CO_2. On the other hand, as the salts of the sesquioxide of uranium, with the stronger acids, contain only one equivalent of acid, instead of three like those of the other sesquioxides, analogy with the carbonates just described should make the carbonate of uranium contain less than one equivalent of acid to one of base. The first view is, however, I think, the more probable one.

Summary of the above results.

The following is a brief summary of the principal results arrived at in the foregoing paper:—

1. All the carbonates in question, except, perhaps, the carbonate of uranium, lose carbonic acid readily during the processes of washing and drying. To this may, in a great measure, be attributed the discordant results of previous observers.

2. The precipitate produced by the alkaline carbonates in solutions of the persalts of iron has the composition Fe_2O_3,CO_2.

3. The precipitate by the alkaline carbonates in the violet salts of the sesquioxide of chromium varies somewhat in composition, but approaches the formula $Cr_2O_3,2CO_2$, and is probably a mixture of this with a little of the more basic salt next mentioned. The results of previous observers render it nearly certain that the above precipitate, by washing and drying, is converted into a more stable salt of the composition Cr_2O_3,CO_2. This latter is formed, when the precipitation takes place at the boiling point. The precipitate in the green solutions of chromium consists of carbonate, not of oxide, as stated by Lefort, and has probably the same composition as in the violet salts.

4. The precipitate by the fixed alkaline carbonates, in the salts of alumina, varies in composition, but approaches the formula Al_2O_3,CO_2, and is probably a mixture of this with a more basic salt. Both in this and in the case of the carbonate of chromium, the normal salt may be obtained nearly free from basic compounds by pouring the alumina or chromium solution into that of the alkaline carbonate, instead of the reverse, so as to have always an excess of the alkali.

5. The precipitate by the alkaline carbonates in the salts of glucina, has the composition G_2O_3,CO_2.

6. The precipitate by the alkaline carbonates, in solutions of the sesquioxide of uranium, is undoubtedly a mixture of uranate of the alkali and carbonate of uranium. The latter, if it could be obtained pure, would very probably have the composition U_2O_3,CO_2, in which state it exists, according to Ebelmen, in its double salts with the carbonates of potash and ammonia.

In conclusion, it will be observed that none of these carbonates conform to the theory of Berzelius, that the number of atoms of acid in a neutral salt corresponds to the number of atoms of oxygen in the base. On the contrary, their composition is generally the same as that of the salts of the protoxides—one atom of acid to one of base.

Laboratory of the Lawrence Scientific School, Cambridge, July, 1862.

TECHNICAL CHEMISTRY.

Process for Obtaining Finely-powdered Metallic Copper, by M. HUGO SCHIFF.

TO obtain finely-powdered metallic copper, mix a certain quantity of a saturated solution of sulphate of copper with some of the same salt in coarse powder, and some granular metallic zinc, and shake the mixture continuously and briskly; the cupric solution is decomposed by the zinc, which is changed into sulphate, and leaves the copper only in the form of a finely-divided powder.

As a certain quantity of the excess of sulphate of copper is constantly dissolving, the decomposing action of the zinc continues while cupric salt is in solution.

By this means large quantities of powdered metallic copper can be prepared in a very short time. When a certain quantity is formed, it is thrown on a filter and washed several times with distilled water freed from air, and then with alcohol; it is afterwards dried by pressure, guarded from air and heat, as the metal, when so finely divided, is very apt to oxidise.

During the reaction heat is developed, and singularly accelerates the decomposition of the cupric salt, and after a few minutes the temperature becomes so intense as to render it impossible to hold the flask in the hand. —*Journal d'Anvers.*

PHARMACY, TOXICOLOGY, &c.

On Pure Pepsine, by M. BRUCKE.

PEPSINE may be precipitated from its solutions by being shaken with cholesterine, phosphate of lime, or even animal charcoal dissolved in water containing phosphoric acid; it may be precipitated in the same way when neutralised by lime water, and is then contained

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** The carbonates of iron, chromium, and alumina, after washing with cold water, contained not a trace of soda. The carbonate of glucina contained a mere trace, which could probably have been removed by further washing.

†† For example: the sesquioxide of uranium cannot be separated from the alkalies by ammonia. Only the excess of alkali is separated. The rest is precipitated with the uranium, as uranate of the alkali. See Rose, "Analyt. Chem.", Fr. ed., ii., 252.

‡‡ *Ann. Chim. et Phys.* [3], v., 206—208.

in the phosphate of lime precipitate, though not always in combination.

On these facts M. Brucke has founded a new process for the preparation of this important substance; he obtains it in a state of purity hitherto unknown, which enables him to disprove many assertions in respect to it made in treatises on the subject.

Two mucous membranes from the stomach of a pig after digestion at 38° in diluted phosphoric acid, soon detach fragments; then filter and treat the residuum with a fresh quantity of phosphoric acid, and continue the maceration at 38°, until the membranes are completely disintegrated. The filtered liquid should be limpid, and yellow prussiate ought not to separate the albumen from it. After adding lime water until quasi-complete neutralisation is produced, which is indicated when it turns litmus paper violet, collect the precipitated phosphate, express and dissolve it in water containing hydrochloric acid. Reprecipitate by lime water, recover by hydrochloric acid, and filter.

Into a sufficiently large flask containing this liquid introduce a funnel with a long stem, through which pour slowly and by small portions a solution of cholesterine prepared cold, with a mixture of 4 parts of alcohol at 94 per cent., and 1 part of ether. On contact with the liquid acid the cholesterine separates into particles of great tenuity, which rise to the surface of the liquid; when this coagulum becomes about 2 centimetres thick, remove the funnel and shake the liquid briskly and frequently, in order to attach all the pepsine possible to the cholesterine; then filter, and wash first with water containing hydrochloric acid, and then with pure water, until the washings become free from acid, and have no reaction on nitrate of silver. Then treat with ether; the cholesterine dissolves, while the adherent water remains in the form of a turbid layer, which must be shaken with ether, and so on again, decanting as they form the ethereal layers; then evaporate the small remaining quantity of ether adhering, and if necessary filter, and a limpid liquid results containing pure pepsine.

Acidulated, this liquid possesses energetic digestive properties; thus it dissolves instantaneously a filament of fibrine, and one drop of the liquid, added to 5 c.c. of hydrochloric acid, containing 1 gramme of HCl to the litre of water will dissolve a filament of fibrine in the space of an hour.

However, as we observed before, this pepsic liquid is far from sharing the reactions assigned to pepsine. Thus, it is affected by none of the reagents which denote the presence of albumen, for instance, concentrated nitric acid, tincture of iodine, tannin, or bichloride of mercury. This is a point of great importance, as the pepsine analysed by M. Schmidt was prepared by precipitation with this bichloride.

Nitrate of silver renders the liquid slightly opaline. Bichloride of platinum produces a sensible opaqueness; it is abundantly precipitated by acetates of lead, which, even in presence of free acetate of lead, also renders it considerably turbid.

These facts ought to modify our notions concerning pepsine. Its analysis has yet to be made.—*Journal de Pharmacie et de Chemie.*

PHYSICAL SCIENCE.

Astronomical Observations with the Spectroscope,

by Lewis M. Rutherfurd.

In the course of a conversation, last December, with Dr. Gibbs, upon the remarkable revelations of the spectroscope, he suggested the continuation of Fraunhoffer's observations upon the spectra of the heavenly bodies. At that time I had not seen Fraunhoffer's paper on this subject. I immediately began a series of experiments with the view of determining the best form of instrument for the purpose; they resulted in adopting Bunsen and Kirchhoff's simple form of spectroscope, consisting of a condensing telescope with adjustable slit, a scale telescope with photographed scale of equal parts showing bright lines upon a dark ground, a flint-glass prism of 60°, and an observing telescope with Hygenian eyepiece, magnifying about five times, the whole firmly but lightly mounted on seasoned wood, and provided with an adapting tube in front of the slit, by means of which the spectroscope is attached to the eye-tube of the equatorial by Fitz, 11¼ aperture, and 14 feet focal distance. In order to obtain a sensible breadth of spectrum, it was necessary to throw the star out of focus, and in this manner a large portion of the light was lost upon the jaws of the slit. Most of my observations, however, have been made under this disadvantage. I subsequently found Fraunhoffer's paper in the *Transactions of the Bavarian Academy*, and there saw that he used no slit, but upon the hypothesis that the image of a star is a point, he elongated this point to a line by means of a cylindrical lens whose action is null in the direction of the length of the spectrum, but serves to give it the desired breadth. I at once adopted this idea, and my instrument is now provided with such a lens, made by Mr. Fitz, placed between the objective of the condensing telescope and the prism; the result is a very satisfactory increase of light, besides freeing the spectrum from the longitudinal lines of diffraction caused by the edges of the slit and the other annoyances consequent upon imperfections and dust upon the jaws. It is still, however, necessary to make use of the slit to confine the image to a given point in the field of view; but inasmuch as it is placed at the focus of the telescope, where the image has no appreciable dimensions, no light is lost, and the spectrum remains uniform and pure.*

It will be easily seen that the lens spoken of is serviceable only in observations upon stars, and is of no use in the spectral investigation of planets or other sources of light having appreciable dimensions. I would here incidentally remark, that the spectroscope so mounted furnishes the best means of investigating the achromatic condition of the telescope; for it is evident that if the different coloured rays have foci at different distances from the objective, it will require a change of focus for each in order that it may comply with the condition of passing the slit (in case of a star) as a point. In my telescope, I find that the luminous rays from near the outer margin of the red to the indigo are brought to a focus at one point; the ultra red require a small but measurable adjustment, and the violet and indigo quite a large change of focus. I intend to make use of this method to find more accurately the photographic focus of the instrument, which at present is determined only by experiment.

Before and after observation the spectroscope has always been examined as to the zero point, the standard being that the soda line D should coincide with division 30 on the scale, and the necessary corrections have been applied to all the observations.

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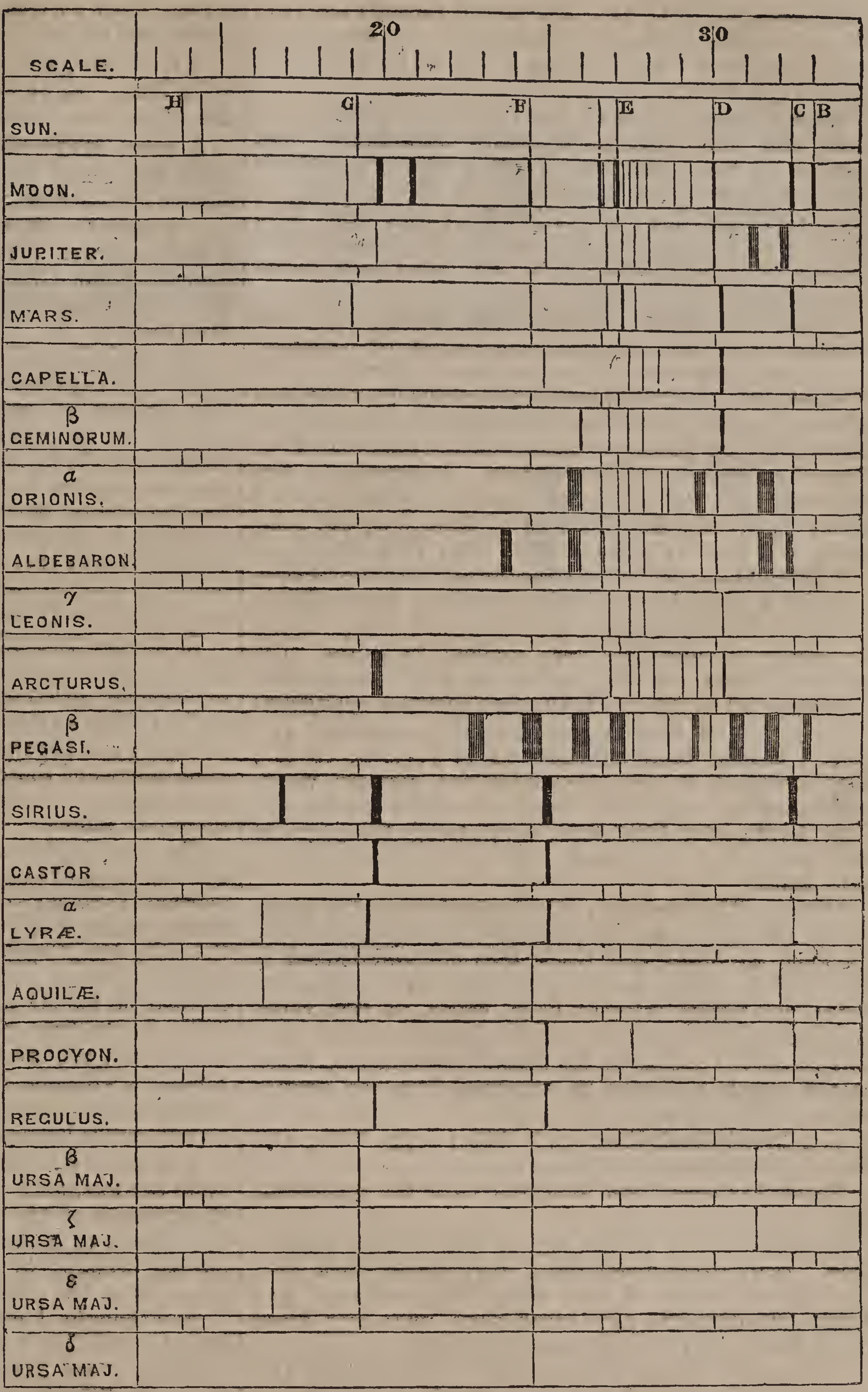
* Fraunhoffer used no condensing telescope; he simply placed a prism and cylindrical lens before the objective of a small telescope, and received the star's light directly upon the prism; being thus confined to the dimensions of the prism as the measure of the volume of light examined, he discontinued his observations, proposing to resume the subject at another time.

The observation of star spectra is of the most difficult and delicate description, requiring perfect action in the equatorial clock, great patience in the observer, and skilful management of the scale illumination. Most of the lines and bands, particularly in the ends of the spectra, are faint, and can only be seen in a good atmosphere.

The difficulty of the observations, the imperfection of the spectroscope, and the want of a sufficient accumulation of observations, render it necessary that the places assigned to the fainter lines should be received with caution, but I believe that no line is represented on the accompanying sketch which does not exist. The smallness of the scale, and the imperfection of the drawing, render it necessary that I should increase the length of this communication by giving a short note upon each object.



Sun.—I have inserted the seven principal lettered lines in the solar spectrum at the points seen on the scale, and carried them through the page as points of comparison. These places are the results of several observations, all of which agree absolutely, except as to the place of the lines H, for which I have taken the mean. At a future time, and on a larger scale, I propose to locate all the solar lines which may be visible with my instrument, and thus have further points of comparison with the stellar spectra. The reading for the sun's lines is as follows:—

B 33·1 C 32·3 D 30· E 27· 26·5
F 24·4 G 19·3 H 14·5 13·9

Moon.—These readings are the means of two observations agreeing very closely in most particulars, and coincident in all the stronger features. 33·05 broad line generally limiting the spectrum; 32·35 sharp dark line; 30·05 well defined; 29·3 faint line; 28·7 faint line; 27·8 faint line, but stronger than the last; 27·65 very faint line; 27·4 darker line; 27·25 faint line; 27·05 strong line; 26·85 faint line; 26·65 line; 25·55 strong line; 24·75 line; 24·35 strong line; 21·05 faint band; 19·9 broad line; 18·09 broad dark line.

Jupiter.—Mean of three observations. 32·1 band; 31·12 band; 30·06 line; 28 faint line; 27·5 faint line; 27·26 line; 24·7 line; 19·9 line.

Mars.—Mean of three observations. 32·4 line; 30·25 well defined line; 27·5 well defined line, but faint; 27·1 strongest line in the spectrum; 26·55 quite strong line; 24·4 band; 19·1 line. I would here remark that the line D is not present, as the observations made in different nights, one by myself, and two by Mr. Wakeley, my assistant, agree in placing a line at 30·2, but none at 30.

Capella.—Mean of observations on five nights in which the greatest discrepancy is but one-tenth of a division of the scale. 30·22 line; 27·73 line; 27·38 line; 26·75 line; 24·78 line.

β Geminorum.—Mean of four observations agreeing remarkably well, greatest discrepancy one-tenth of a division.

30·23 line; 27·7 faint line, seen but on one evening; 27·35 line; 26·8 line; 25·8 fine line, seen but on one evening.

α Orionis.—Mean of six observations agreeing well in most of the strong features, but containing some discrepancies in the faintest lines and in the limits of the bands. This star has not yet been observed with cylindrical lens. 32·4 broad line; 31·6 to 31·2 shaded band best defined at 31·2; 30·1 line shaded towards the red; 29·5 faint band; 28·4 faint line; 28·3 fine line; 27·75 faint line; 27·3 faint line; 27 line; 26·4 strong line; 25·7 to 25·4 band.

Aldebaran.—Mean of four observations, three with lens, and one without, agreeing remarkably well. 32·2 to 32 band generally limiting spectrum, still in places the red is seen beyond it; 31·6 to 31·3 band; 30 line; 29·6 faint line; 27·7 faint line; 27·4 faint line; 27 line; 26·6 rather strong line; 26·5 line not so strong as last; 25·6 faint band; 23·6 faint band seen but on one occasion.

γ **Leonis.**—Mean of two observations very concordant. 30·2 line; 27·7 line seen but on one occasion; 27·35 line; 26·8 line.

Arcturus.—This star has been observed but twice, and without lens. I have affixed a mark of interrogation to those lines, the places of which depend upon single or somewhat discordant observations. It promises a fine spectrum with the cylindrical lens. 30·2 line; 29·85? faint; 29·5? very faint line; 28·9? faint line; 28? faint line; 27·6 line; 27·32 line; 26·82 line; 19·9 band.

β **Pegasi.**—This star, considering its faintness,—scarcely a second magnitude,—presents a remarkable spectrum, which contains few lines and many bands, the limits of which are very difficult to locate. The adopted places are the means of four observations agreeing well in the mean, all made with the cylindrical lens—32·8 band limiting generally the spectrum; 31·7 to 31·3 shaded band strongest at 31·3; 30·65 to 30·3 band strongest at 30·3; 29·8 faint line between which and preceding is included a yellow band or space; 29·3 faint band; 28·5 faint line; 27·3 strong line; 27·1 to 26·7 band; 26 to 25·6 band; 24·6 to 24·1 band; 22·9 to 22·5 band.

Sirius.—The spectrum of this star is one of a group which has little resemblance to those already mentioned; its lines are broad and black, they are well defined in margin, but, unlike the band recorded in the foregoing notes, are totally without light, being, in fact, interruptions of the spectrum; no fine lines have been seen. The places are means of six nights' observations, which accord closely as might be expected from the decided nature of the lines.

32·4 broad black line; 24·8 ditto; 19·9 ditto; 16·8 ditto. The spectrum extends to 14·5.

Sirius has never been observed with the cylindrical lens.

Castor.—Mean of five nights' observations without lens. 24·78 strong black line; 19·87 strong black line.

α **Lyræ.**—Mean of four nights' observations. 32·2 broad but difficult; 24·7 broad dark line; 19·5 broad dark line; 16·3 seen only on one evening.

α **Aquilæ.**—Mean of three nights' observations. 31·8 line very faint, seen but on one evening; 24·4 strong line; 19·33 strong line; 16·4 faint line.

Procyon.—Mean of six nights' observations without lens. 32·3 faint line seen but once; 27·3 faint line seen but once; 24·75 strong dark line. Spectrum extends from 17·8 to 33·8.

Regulus.—Mean of five nights' observations, on one of which the lens was used, but without bringing out any more lines. 24·78 strong dark line; 19·9 strong dark line.

β **Ursa Majoris.**—Mean of two nights' observations with lens. 31·2 very faint line, seen only once; 24·35 strong line; 19·45 well defined line.

ζ **Ursa Majoris.**—Mean of three nights' observations with lens. 3·12 very faint line, seen but once; 24·53 strong line; 19·5 faint line. A line was seen in the violet, but too faint to bear the least illumination.

ε **Ursa Majoris.**—Mean of three nights' observations with lens. 24·53 strong line; 19·63 faint line; 16·5 faint line, seen but once.

δ **Ursa Majoris.**—Mean of two nights' observations with lens. 24·7 strong line. Two lines lower down on the scale were seen, but would bear no illumination.

α **Virginis**, β **Orionis**, ε **Orionis**, δ **Orionis**, ζ **Orionis**, and α **Ursa Majoris** have been repeatedly examined, but although many of them, particularly the first two, present bright spectra, no lines or bands have been seen.

The sun's lines, B, C, D, E, F, find their counterparts in the lunar spectrum. G does not appear; but whether this absence is real or due to errors of observation, remains to be proven. The moon was observed only twice—once by me and once by Mr. Wakeley; he placed the line nearest G at 19·85, and I at 19·95. When I re-examine the lunar spectrum I intend introducing a condensing achromatic between the great objective and the slit, and in this manner increase the intensity of the light. No doubt many more lines will be brought out, and those already observed more accurately placed.

In the spectrum of Jupiter are found two bands in the red and orange, between C and D, which are not found in the solar spectrum. It may be that these bands, as well as those so remarkable in α Orionis, Aldebaran, and β Pegasi, are absorption bands due to the action of the atmospheres of those bodies; still it is possible that the application of sufficient optical power would resolve them into lines.†

The star spectra present such varieties that it is difficult to point out any mode of classification. For the present, I divide them into three groups: first, those having many lines and bands and most nearly resembling the sun, viz., Capella, β Geminorum, α Orionis, Aldebaran, γ Leonis, Arcturus, and β Pegasi. These are all reddish or golden stars. The second group, of which Sirius is the type, presents spectra wholly unlike that of the sun, and are white stars. The third group, comprising α Virginis, Rigel, &c., are also white stars, but show no lines; perhaps they contain no mineral substance, or are incandescent without flame.

It is not my intention to hazard any conjectures based upon the foregoing observations; this is more properly the province of the chemist; and a great accumulation of accurate data should be obtained before making the daring attempt to proclaim any of the constituent elements of the stars.

One thought I cannot forbear suggesting: we have long known that "one star differeth from another star in glory;" we have now the strongest evidence that they also differ in constituent materials,—some of them, perhaps, having no elements to be found in some other. What, then, becomes of that homogeneity of original diffuse matter which is almost a logical necessity of the nebular hypothesis?

Taking advantage of past experience, I propose to re-model and improve my spectroscope, and continue to observe the stars, noting particularly the relations which may exist between the spectral revelations and the colour, magnitude, variability, and duplicity of the objects.—*American Journal of Science*, xxxv., 71.

New York, December 4, 1862.

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† Since writing the above, I have seen with Dr. Gibbs the absorption-bands produced by the vapour of iodine, bromine, and other kindred substances entirely composed of fine lines.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, March 5.

Dr. A. W. Williamson, *F.R.S., in the Chair.*

Mr. J. S. Atherton was elected a fellow of the Society.

A paper, by Mr. J. B. Lawes, F.R.S., and Dr. Gilbert, F.R.S., "*On the Assimilation of Nitrogen,*" was read by Dr. Gilbert. He commenced by stating that the main object of their experiments, which had been carried on for a period of twenty years, was to discover the sources of the nitrogen of vegetation, and especially whether free nitrogen could be assimilated by plants. On this point it was evident that little information could be gained until a thorough knowledge of the composition of the air and of water had been gained. Of the earlier experimenters, the most successful was perhaps Hale; he had not, however, sufficient data before him from which to draw decisive conclusions, and, in fact, the investigators who determined the composition of air and water were those who furnished the knowledge requisite as a foundation for the inquiry; their experiments on plants, however, were made chiefly with a view of determining the changes produced in the atmosphere with which the plant was surrounded. Among these Priestley and others considered that free nitrogen was assimilated; but Saussure thought, on the contrary, that nitrogen was given off by plants. Saussure made many important investigations, from which he was led to the conclusion that plants derive their food from air and water, the nitrogen being furnished by ammonia and from nitrogenous matter in the soil, from which the mineral constituents were also obtained; and he considered that the mineral constituents in animals were derived from the plants they consumed. Before his time it was thought that plants manufactured their constituents in some manner out of the soil in which they grew. Boussingault was another investigator who was led to turn his attention to the chemisty of farming. The authors of the present paper had determined the amounts of nitrogen harvested per acre with different crops, both with and without manure, and had found that, if no nitrogenous manure was supplied, the amount of nitrogen harvested with leguminous plants was double that obtained with graminaceous plants. It appeared that, after clover had been grown for a succession of years on the same soil, no combination of manuring would restore the clover-producing power of the soil. In their experiments on wheat they had found that the same yield of nitrogen per acre was obtained, whether the crop was raised continuously or in alternation with periods during which the ground was allowed to lie fallow — the amount harvested during the year of growth making up for the want of produce during the fallow year. A similar result was obtained when the wheat crop was alternated with a crop of beans; the wheat giving the same amount of nitrogen as before, and the bean crop furnishing an additional quantity. Leguminous crops appeared to be but little benefited by nitrogenous manures; but with graminaceous crops, on the contrary, a considerable increase in the yield of nitrogen was obtained, and nearly half of the nitrogen supplied in the manure was obtained in the crop above that yielded when no manure was employed. The loss of the remainder was probably to be accounted for by draining away, or loss by evaporation, or by the circumstance that some of the nitrogen might remain in a combined form in the soil and furnish nutriment to future crops. Their inquiries had next been directed to the sources of the combined nitrogen found in plants, and to the question whether plants could assimilate free nitrogen. One source of the nitrogen was to be found in minerals and in the soil; minerals when dug up from a depth always containing ammonia, certain nitrides contained in the earth might also furnish a small supply of combined nitrogen. A considerable quantity would be supplied from the atmosphere by aqueous deposits, the ammonia in the air being thus carried down into the soil. It appeared to them, however, to be doubtful whether this would be sufficient to produce a yield of more than eight or ten pounds of nitrogen per acre per annum. The absorptive power of the soil furnished an additional supply of nitrogenous compounds, and from a series of experiments on the absorptive powers of burnt soil, alumina, and other porous bodies, they had come to the conclusion that this was a sufficient source of the nitrogen of plants. It had been suggested that ammonia might be formed by the combination of nascent hydrogen with the nitrogen of the atmosphere, and, in fact, this combination does take place in some cases, but they considered it doubtful whether, during the evolution of hydrogen by the decay of organic matter, a formation of ammonia by its combination with free nitrogen could take place. Many experiments had been made to show that common air passed over alkaline and porous bodies furnished nitric acid; and it was well known that small quantities of this substance were formed during thunderstorms, but the amount of nitrogenous compounds formed in this manner was doubtful. It was a question whether plants were capable of taking up ammonia or nitric acid by their leaves; the very small proportion of ammonia found in the atmosphere made it doubtful that any considerable amount of nitrogen should be supplied in this manner to plants. With respect to this point, it was to be observed that leguminous plants, which assimilated more nitrogen than graminaceous plants, had, in reality, less leaf surface than these last, although they apparently had more leaves. The question as to the power of plants to assimilate free nitrogen had been inquired into by many investigators. Boussingault's experiments on this point had been made under various conditions; and eventually he excluded plants altogether from the external air, all the air supplied to the plants being previously washed with sulphuric acid, carbonic acid being occasionally supplied in addition. He came to the conclusion that plants did not assimilate free nitrogen. The experiments of M. Ville, who employed an iron case with glass windows to contain his plants, led him to an exactly opposite conclusion; there was little doubt, however, that in these experiments the increase of combined nitrogen was due to the impurity of the distilled water supplied to the plants. De Lucca considered that ozonised air might effect the oxidation of free nitrogen, and give rise to the formation of nitric acid. The question having been left in this conflicting state, the authors, with the assistance of Dr. Pugh, had taken it up. With respect to the action of ozonised air contained in the cells of leaves on nitrogen, it was not probable that ozone would be formed during the evolution of oxygen from a plant placed in the sun's rays, since these rays possessed a great reducing power, still less could ozone be formed when the plant was in the shade, for the oxygen would then be consumed in the oxidation of carbon to form carbonic acid; ozonised air, might, however, act on the nitrogen in the air, and give rise to the formation of nitric acid. In order to determine whether plants could assimilate free nitrogen, they employed an apparatus composed of a large glass shade resting upon a pan formed of a plate with two concentric cylinders rising from it; the edge of the glass shade passed between the two cylinders, and the intervening space was filled up with mercury. The soil employed was burnt, exhausted of soluble matter, ignited, and allowed to cool over sulphuric acid: it was then placed in porous pots. The air supplied to the plants was washed with sulphuric acid and carbonate of soda before it was allowed to enter the glass shade; it was forced through the apparatus instead of being drawn through by an aspirator, which was the plan adopted by former experimenters — this method of procedure preventing any errors being produced by leaks. The seeds which were allowed to germinate in this apparatus grew up to a certain extent, and in fact showed a curious power of maintaining their existence—new parts as they grew absorbing nitrogenous matter from old decaying parts. Leguminous plants, however, died almost as soon as they had germinated, not being able to sustain their existence without an additional supply of combined nitrogen; in some of the experiments nitrogenous matter was supplied to the plant until it had attained some growth but in no case did any increase in the amount of the combined nitrogen, beyond that supplied, and that originally existing in the seed take place; in fact, when additional combined nitrogen had been supplied there was a loss of nitrogen, a portion being carried off as ammonia by the current of air passing through the apparatus. With respect to a loss of nitrogen taking place from the production of free nitrogen by the decay of those portions of the plants that were dead, by which an error would be introduced, they considered that, although under certain circumstances such might be the case, as, for instance, by the decay of dead leaves, &c., still under the circumstances of the expertments mentioned no loss of this kind took place.

The CHAIRMAN remarked that great thanks were due to the authors for the care and attention they had bestowed upon the subject.

Dr. FRANKLAND considered that with respect to the opposite result arrived at by M. Ville, it was partly to be attributed to his apparatus, which was probably not air-tight, while the authors of the present paper had taken care to guard against a similar inaccuracy.

PHARMACEUTICAL SOCIETY.

Wednesday, March 4.

Mr. P. SQUIRE, President, in the Chair.

THE first paper was some "*Additional Observations on Storax,*" by Mr. D. Hanbury, jun In an ingenious and able paper communicated to the Society six years ago, the author thought to establish the following conclusions:—1. That the original and classical storax was produced by the *Styrax Officinale L.* 2. That this substance has wholly disappeared from the commerce of modern times. 3. That the resin called *Liquid Storax* is produced by *Liquidambar Orientale*, a tree indigenous to the south-west of Asia Minor, where the drug is collected. These conclusions were generally accepted, and were never disputed until a short time ago, when Professor Krinos, a teacher of Pharmacology at Athens, published in modern Greek a paper entitled "*A Pharmacographical Essay on Storax.*" In this paper the learned Professor endeavours to show 1. That *Liquid Storax* was known to the ancient Greek Physicians. 2. That the text of Dioscorides requires a slight change. 3. That the *Solid Storax* of ancient authors was not the produce of *Styrax Officinale, Lin.*, but of the *Liquidambar Orientale*, the same tree as that from the bark of which liquid storax is obtained by decoction and expression. To support these propositions, Professor Krinos reviews at length the ancient accounts of storax, quoting Paulus Ægineta who mentions a solid storax, liquid storax, and also the resin of a tree called *ζυγία*. Ætius and other authors are then cited, and the author points out that the word *ζυγία*, signifying maple, is now applied in the south west of Asia Minor to a tree which resembles a maple, and which is the *Liquidambar Orientale* of botanists. To prove that the text of Dioscorides requires an alteration, Professor Krinos contends that storax was not the produce of the *Styrax Off.*, but of a tree which resembles the maple, Dioscorides expressly stating that the tree resembled a quince. Mr. Hanbury remarks on this that if this were admitted it would be needful to alter Pliny as well. Mr. Hanbury here referred to some dried specimens of the plants, and showed that the storax exhibited very much resembled the quince as stated by Dioscorides and Pliny. This plant also had a very extensive range; it was found in the south of Europe as well as in Asia Minor and Syria. The liquidambar, on the contrary, had a very restricted range; it was only found in an extreme corner of Asia Minor, near Alexandretta, and on the Orontes. From the third proposition of Professor Krinos, Mr. Hanbury entirely dissents, while admitting that an odorous resin from liquidambar may have passed as storax in ancient times. Assuming that no solid resin from *Styrax Off.* is now found in commerce, Professor Krinos believes that the tree never yielded any, although Mr. Hanbury has shown that two respectable authors of the last century actually collected some, and has recently received information that the trees in the neighbourhood of Alexandretta still yield an odorous resin. Mr. Hanbury, however, has not been able to obtain the resin from the south of France, Asia Minor, or from Syria, in which latter country he has personally examined many bushes. In Syria the *Styrax* never attains more than the size of a bush, owing to its being periodically cut for fuel, but Mr. Hanbury believes that when it reaches the size of a tree it still yields the resin. Professor Krinos further asserts that the *Liquidambar Orientale* is common in Syria, a statement which is not borne out by the researches of botanists. The botany of Syria has been thoroughly studied; and if common there, the tree could not possibly have been overlooked by the numerous botanists who have visited that country. For the above reasons Mr. Hanbury is not inclined to abandon the opinion that the storax tree of Dioscorides, Pliny, and others, was the *Styrax Off.* of modern botanists, nor the idea that like *Sty. Benzoin*, the tree is capable of yielding an aromatic resin, which was once found in sufficient abundance to form an article of trade. Mr. Hanbury then referred to the substance known in pharmacy as *Styrax Calamita.* This drug, he had ascertained, was manufactured at Trieste, and probably other places, by mixing the residual liquidambar bark in coarse powder with liquid storax, in about the proportions of three to two. So made it constitutes a moist, clammy paste, which in a few weeks developes silky crystals which give it a mouldy appearance. When liquidambar bark is scarce, sawdust is said to be substituted for it; and olibanum, red earth, and honey enter into the composition of inferior qualities. The Greek monks of Syria prepare a cake used for incense by mixing olibanum with liquid storax, and it is this, in Mr. Hanbury's opinion, which constitutes the black storax of Pereira, Guibourt, and others, and which is used in the Church of the Holy Sepulchre at Jerusalem at Easter, and sold to pilgrims at an extravagant price. In conclusion, Mr. Hanbury expressed an earnest desire that all travellers visiting Asia Minor and Northern Syria, and residents in those countries, would examine the stems of *Styrax Officinale* with a view to discover adherent resin; and when possible perforate the trunk, and see if the operation is followed by an exudation.

The PRESIDENT, after expressing the thanks of the meeting, inquired what size the styrax-tree usually attained, and remarked that no doubt the production of the resin depended on the age and size of the trees. It was well known in this country that ivy gum was only found on old trees which had arrived at a certain size. From some very fine and old trees at Conway Castle he he had once picked about half an ounce of the gum, which had a very aromatic odour, but the gum was rarely seen.

Mr. HANBURY said that when growing wild in Syria, the trunk of the tree was seldom seen larger than four inches, but at Montpellier he had seen it as large as an apple-tree, which was probably the natural size. The chairman's explanation was no doubt correct, and the absence of solid storax was owing to the want of large trees. He had never found ivy gum anywhere in Syria, where the ivy plant is small. The ivy reaches a larger size in England than in any other country, and is here the admiration of German botanists.

Professor Bentley inquired what reasons Professor Krinos had given for the proposed alteration in the text of Dioscorides?

Mr. Hanbury said no reasons had been adduced which would induce a classical scholar to assent to the alteration. The Professor had not examined any manuscripts, but to get out of a difficulty had made Dioscorides say what he did not say.

The President asked whether the styrax was common, and why it was not allowed to come to perfection.

Mr. Hanbury replied that the tree was widely distributed. It was found in Asia Minor, the Levant, Southern France, and through the warmer regions of Europe, but became rarer towards the west. The reason it was not allowed to come to perfection was that wood was scarce in Syria, and all trees which were easily cut were taken for fuel. The styrax wood was soft, and a full-sized tree was a rarity in consequence.

Professor Bentley said that of a tree so largely distributed he should have thought some grown specimens might have been met with. It was a consolation to hear Mr. Hanbury's account of *Styrax Calamita*, and know that it really contained storax. It had been commonly supposed that it was compounded of saw-dust and something else unmentionable.

Mr. Hanbury said he was not in the secrets of the manufacture of *Styrax Calamita* further than was related in the paper. It was a variable substance, and was probably made in many ways. With regard to the age and size of trees, as they affected the production of resins, he might instance the mastic tree which was found near Kiaffir about the size of a furze bush, but did not there produce any gum. In Scio, from which island we obtain our mastic, the tree was protected and grew to a large size. It was only under favourable circumstances that trees yielded exudations.

In reply to a member who asked whether Dioscorides and Pliny gave any description of the storax tree, or merely the name, Mr. Hanbury said that Disocorides lived in the first century, and was a native of Asia Minor. He had been followed by a succession of writers on the same subject, and there could be no doubt as to the meaning of his words. The quince was perfectly well known to both Greeks and Romans, who called it by the same name. Dioscorides gave no botanical description of the storax tree, but said it resembled a quince-tree, and did not say it resembled the maple. As Asia Minor, of which country Dioscorides was a native, was covered with styrax, he must have been familiar with the tree, and as the maple grew there also, he was no doubt acquainted with it; but he did not compare the styrax to the maple, but to the quince, for the very good reason that it was like it.

Mr. Hanbury then read a communication "*On Cinchona*," from Dr. De Vrij, the Superintendent of the Dutch Cinchona Plantations in Java. The letter complained of some apparent mis-statements in Mr. Markham's "Travels in Peru and India," where it would seem to be stated that the cinchonas in the Dutch plantations are not the best for producing quinine. Dr. De Vrij thinks that Mr. Markham may have been deceived by appearances, as quinine may be easily overlooked. He states also that if the culture of cinchona succeeds in British India, the Dutch will have had a large share in promoting the success, inasmuch as when nearly all the Calisaya plants obtained by Mr. Markham from Peru had died, he was supplied with a large number of strong plants raised from seed in Java, only one of which died on the journey to the British plantations.

An interesting conversation followed on the prospects of the cinchona culture in British India. Mr. Hanbury said there were now 72,568 cinchona plants on the Neilgherry Hills, 30,000 of which were red cinchonas, 1000 *Cin. Calisaya*, 20,000 *C. Condaminea*, and a smaller number of other kinds.

Professor Bentley said that Mr. McIvor had returned the number of plants at 120,000, mostly of the species yielding quinine. There were 1600 *Calisaya* plants, the largest about seven feet high. The plants yielded alkaloids when about fifteen or twenty years old. He considered the trees now well established in India.

Mr. Hanbury said that the old flat bark was generally richer in quinine than a quill bark, and in Peru a cinchona plantation yielded no produce for forty years. He considered the proposal of Mr. Markham, to cut the trees so as to leave them shrubs, objectionable, as he did not think the barks would be so rich in alkaloids.

Professor Bentley then exhibited some ergots of wheat and rye. He stated that a thesis had recently been published in which it is asserted that ergot of wheat has all the virtues, and none of the poisonous properties, of ergot of rye, and, moreover, that it keeps very much better. The former statement, the Professor said, might be taken for what it was worth; the latter he could confirm by specimens he had found in the Society's Museum. They had come from the collection of the late Dr. Pereira, and had been in the Society's possession over ten years. The specimen of ergot of rye was injured to some extent, but the ergot of wheat was untouched by acari. Professor Bentley hoped the Medical Profession in England would make a trial of the ergot of wheat.

A discussion on the best way of keeping ergot of rye followed, from which we gathered that when well dried the ergot could be kept good in any way, with free access of air, or with air completely excluded, but that perhaps the most convenient way for a dispensing chemist to preserve it was in a dry place in a stoppered bottle, and a lump of camphor with it.

At the close of the meeting, Mr. Long proposed a motion to the effect that no subject should be brought forward for discussion which had not been announced in the previous journal. As no seconder was found, the motion was not put, and the meeting adjourned.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, February 24, 1863.

E. W. Binney, *F.R.S., F.G.S., President, in the Chair.*

Among other donations for which the thanks of the Society were voted were an extensive collection of Dr. Dalton's manuscript correspondence, presented by Dr. W. C. Henry; and a framed photograph of Professors Bunsen and Kirchhoff, presented by Mr. H. Petschler.

Professor Roscoe stated that he had been for some little time, and is still, engaged in an interesting examination of the spectrum produced by the flame evolved in the manufacture of cast steel by the Bessemer process, on the works of Messrs. John Brown and Co., of Sheffield.

The spectrum of this highly luminous and peculiar flame exhibits during a certain phase of its existence a complicated but most characteristic series of bright lines and dark absorption bands. Amongst the former the sodium, lithium, and potassium lines are most conspicuous; but these are accompanied by a number of other, and as yet undetermined bright lines; whilst among the absorption bands those formed by sodium vapour and carbonic oxide can be readily distinguished. Professor Roscoe expressed his belief that this first practical application of the spectrum analysis will prove of the highest importance in the manufacture of cast steel by the Bessemer process, and he hoped on a future occasion to be in a position to bring the subject before the Society in a more extended form than he was at present able to do.

A communication was likewise made by Professor

Roscoe concerning "*The Existence of a Crystallisable Carbon Compound and Free Sulphur in the Alais Meteorite.*" Through the kindness of R. P. Greg, Esq., of Manchester, I was placed in possession of about a gramme and a-half of this peculiar meteorite, which fell near Alais, in France, on the 15th March, 1806, and was examined by Berzelius in 1834. This distinguished chemist states* that the Alais meteorite is remarkable as containing an organic carbon compound, soluble in water, which turns brown on heating, deposits a black carbonaceous mass, and burns without residue. In the year 1860, Wöhler† discovered the presence of small traces of a crystallisable hydrocarbon, soluble in alcohol and ether, in two meteorites, one of which fell at Kaba, in Hungary, on the 15th April, 1857, and the other at Bokkevelde, in South Africa, on October 13, 1838. The fact thus undoubtedly proved of the existence in these two meteorites of crystallisable carbon compounds, which in terrestrial matter are solely the results of vital action, rendered a further confirmation of the existence of organic matter in the Alais meteorite of special interest. In general appearance the small fragments of the meteorite experimented upon coincided exactly with the minute description of the substance given by Berzelius; the white efflorescence which covers the surface of the mineral was found to consist mainly of small crystals of sulphate of magnesium; the only other bodies which could be detected by spectrum analysis were soda and lime. Iron was not contained in the soluble salts. On extracting 1·0583 gramme of the meteorite with water, 0·1155 gramme of soluble salts was dissolved, corresponding to 10·91 per cent., and thus closely agreeing with Berzelius's estimation of 10·3 per cent. Ether was found to dissolve from the residue 1·94 per cent. on the original meteorite, a substance which on evaporation was deposited in distinct crystals. The crystals possessed a peculiar aromatic odour, and melted at 114° C., subliming on heating, and leaving a slight carbonaceous residue. Under the microscope the crystals were seen to be of two forms, one acicular, the other rhombic. The acicular crystals were difficultly soluble in absolute alcohol, but easily soluble in ether, bisulphide of carbon, turpentine, and cold nitric acid, and dissolved in sulphuric acid with formation of a brown colouring matter; the rhombic crystals likewise dissolved in ether and bisulphide of carbon, but were unaltered by cold nitric and sulphuric acids, or turpentine. The ethereal extract gave no reaction for sulphuric acid, but after boiling with nitric acid, a copious precipitate of sulphate of barium was deposited. When burnt in a stream of dry oxygen gas, 0·0078 gramme of the extract, dried at 100° C., yielded 0·010 gramme of sulphurous acid, 0·008 gramme of carbonic acid, and 0·003 gramme of water. Hence the meteorite contained 1·24 per cent. of free sulphur, 0·54 per cent. of carbon, and 0·1 per cent. of hydrogen, in a form soluble in ether. The meteorite contains a considerable quantity of carbon (probably as graphite) which is insoluble in ether. The total percentage of carbon found on igniting the meteorite in oxygen amounted to 3·36 per cent.; this closely corresponds with the amount found by Berzelius, viz., 3·05 per cent. From the above it is evident that the Alais meteorite contains at least a-half per cent. of a hydrocarbon, which is deposited in acicular crystals when the mass is treated with ether, together with considerable quantities (more than one per cent.) of free sulphur, crystallising from the ethereal solution in rhombic octohedra. To judge by the melting point, the hydrocarbon may be analogous to a mineral wax called Könlite, discovered by Kraus in the lignite of Uznach, which contains an equal number of atoms of carbon and hydrogen, and melts at 114° C.

The Rev. T. P. Kirkman, M.A., F.R.S., read a Paper "*On Maximum Groups.*"

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* *Pogg. Ann.* xxxiii. p. 113.

† Wien. Acad. Ber. xxxv. 5, ditto xli. 565.

MATHEMATICAL AND PHYSICAL SECTION.

February 12, 1863.

Robert Worthington, *F.R.A.S., President of the Section, in the Chair.*

Mr. W. L. Dickinson read a Paper "*On the Eclipse of the Sun, May 17, 1863.*" This eclipse, which is a partial one, will not be of great magnitude to any part of the United Kingdom. In this neighbourhood about three-tenths of the sun's diameter will be obscured by the moon. The following particulars, which have been computed from the numerical equations in the *Nautical Almanack*, are offered to the members, to enable them to compare the calculated results with their own observations.

Eclipse of the Sun, on Sunday Afternoon, May 17, 1863.

Computation for Manchester (Royal Infirmary), lat. N. 53° 29′, long. W. 2° 14′:—

	h.	m.	s.
Begins	5	37	12
Greatest Phase	6	25	32
Ends	7	11	4

Greenwich mean time.

Magnitude of the eclipse (sun's diameter = 1) 0·307.

Angle, from North Pole, of
first contact, 49° towards the West,
last contact, 45° towards the East,

Angle, from vertex, of
first contact, 88° towards the West,
last contact, 9° towards the East,
for *direct* image.

The position of the points of contact may be familiarly illustrated in the following manner. If we suppose a Victoria shilling to represent the sun, the moon will appear first to touch it on the right side, at the last *r* in the word *Britanniar*, and to leave it on the left side near the vertex, at the letter *i* in the word *Gratia*. A line drawn between these letters will indicate the direction of the moon's path.

There will not be another eclipse of the sun visible in England until October 19, 1865.

NOTICES OF PATENTS.

Grants of Provisional Protection for Six Months.

346. William Tyndon Cooper, Tooley Street, London, "Improvements in distilling apparatus."

348. William Clark, Chancery Lane, London, "Improvements in the application of gas for the preparation of woodwork generally and iron ships for their better preservation and reception of paint or other protecting coating, and for disinfecting ships, hospitals, and other places."—A communication from Henri de Lapparent, Boulevart St. Martin, Paris.

350. James Miller, Glasgow, and Wilson Struthers, Hamilton, Lanarkshire, N.B., "Improvements in securing the corks, stoppers, or lids of bottles, jars, and other similar vessels."

385. George Henry Birkbeck, Southampton Buildings, Chancery Lane, London, "Improvements in processes or means employed for separating or extracting silver or other metals from lead."—A communication from M. Clement Roswag, Paris.

395. Jules Albert Schlumberger, Golden Square, London, "Improvements in treating coal-tar dead oils, and for producing phenic or carbolic acid."—A communication from Jean Jacques Müller, Bâle, Switzerland.—Petitions recorded February 13, 1863.

Notices to Proceed.

2865. Louis Groux, Victoria Soap Works, Woodhouse Junction, near Sheffield, Yorkshire, "Improvements in the manufacture of soap, and in machinery for that purpose." Petition recorded October 24, 1862.

2894. Alfred Peek, Manchester, "Improvements in apparatus for evaporating saccharine and saline solutions."—Petitions recorded October 27, 1862.

2937. William Renwick Bowditch, Wakefield, Yorkshire, "Improvements in carburetting or naphthalising gas, and in the apparatus employed therein."

3011. William Clark, Chancery Lane, London, "Improvements in the means of utilising refuse and azoted matters of commerce."—A communication from MM. Amédee Gélis and Lucien Dusart, Boulevart St. Martin, Paris.—Petition recorded November 7, 1862.

CORRESPONDENCE.

Volumetric Analysis of Water.

To the Editor of the Chemical News.

Sir,—I did not till to-day see your Number of February 14, in which occurs a notice of a debate at the Chemical Society on the plan devised by Assistant-Surgeon Nicholson for analysing water. Mr. Nicholson is now on his way to India, and cannot, therefore, write in his own defence; and, without constituting myself his advocate, I do not think it right to allow the expressions of Dr. Paul to pass unnoticed.

The difficulty which Dr. Paul has found in determining the amount of sulphuric acid from the loss of barytic hardness (a process devised by MM. Boutron and Boudet, and not by Mr. Nicholson), must be owing to some imperfection in operating. It has succeeded well in my hands, and Mr. Sutton, in his late work on "Volumetric Analysis," gives two instances in which the results corresponded most closely with those given by the balance.

With regard to the statement that Mr. Nicholson's plan of determining the free carbonic acid must be fallacious, "as the calcium hardness is invariable, whether it be present in the form of sulphate or bicarbonate of lime," I am at a loss to understand the force of the criticism, for Mr. Nicholson's process is based on this very fact, viz., that the calcium hardness is invariable.

The amount of free carbonic acid necessary to dissolve a certain amount of carbonate of lime is so imperfectly known that any criticism founded on the small amount of free carbonic acid in Mr. Nicholson's analyses must be unsatisfactory. The only mode of testing his accuracy must be by comparing the soap test and ordinary methods. But I may observe that the amount of free carbonic acid in the water supplied by the Chatham Water Company (as determined by the method of Boutron and Boudet) varies a good deal, no doubt from different periods of retention of the water in the reservoirs and pipes; it may be as low as three or four cubic inches per gallon, and as high as eight or ten, or perhaps more. When the water is allowed to stand, it often happens that a great number of bubbles of carbonic acid are given off before there is the least trace of a precipitate of carbonate of lime. It is therefore possible that when carbonate of lime has once been brought into solution, a smaller amount of free carbonic acid may suffice to keep it dissolved than was necessary to dissolve it in the first instance.

I am, &c.

E. A. Parkes, M.D.

Fort Pitt, Chatham.

Discovery of Coal in Holland.

To the Editor of the Chemical News.

Sir,—While on a visit to my native country, I had an opportunity of obtaining reliable information concerning a discovery of coal on the easternmost frontier of that portion of the kingdom of the Netherlands, or, as it is called in Great Britain, Holland, termed the province of Overyssel, now some six years ago. I have been aware of this discovery for a good number of years past, and was also acquainted with the fact that samples of this coal had been analysed as well at Utrecht by Professor Dr. G. J. Mulder as elsewhere. I have often tried to obtain a sample of this mineral, but, until a few weeks ago, failed in this purpose. Most of your readers will be aware of the fact that the whole kingdom of the Netherlands is, almost without exception, a flat, low-lying country, hardly above sea level, that as regards its geological formation it is alluvial and diluvial, and that, except where it touches Prussia or Hanover, no older formation appears to occur. The country is for all practical purposes void of building-stone, and of mineral fuel, except peat. It is true, in the dukedom of Limburg, about Maestricht, a solitary coal deposit is found, and a pit worked near Kerkrade; but, as far as is known, no other similar deposit exists within the limits of this country. The discovery of the coal above referred to took place near the township of Gildenhuis, situated across the Dutch border, on Hanoverian territory, in that portion thereof which up to the year 1805 was the sovereign county of Bentheim. The nearest Dutch place to Gildenhuis is the small frontier town of Oldenhaal, distant about 4¼ English miles. The county of Bentheim contains a number of localities where sandstone is quarried, which stone is in good condition for building purposes. As a rule, this stone does never contain any fossil remains. It appears that while boring for sandstone was going on, now some six years ago, near Gildenhuis, a deposit of coal was found, which attracted much attention in a scientific, as well as in an industrial point of view, the more so, as the locality is not far distant from a district on Dutch territory where coal as fuel for steam-boilers is in great request. The coal—a genuine cannel coal—is found about forty feet below the surface, in what, according to the best information I could obtain, is the *terrain néocomien*. Near the spot and in the environs fossil remains have been dug up, and a sandstone containing such, whereas otherwise, as already remarked, the Bentheim sandstone is quite void thereof; my informant could not precisely define what kind of fossil remains have been met with. This coal, beside having been used as fuel, was applied to gas-making, for which it seems eminently suited, as well as for obtaining oils—the so-called paraffin oils. The following parties have analysed the coal, and some of them have also tested it for its gas-making and for its oil-yielding qualities:—Professor Mulder, at Utrecht; the late Dr. Bleckrode, at Delft; Dr. Vohl, at Bonn; Dr. Werther, at Konigsberg; Dr. Noellner, at Haarburg; Dr. Dumas, at Paris; and Dr. Hofmann (P.W.), at Lille. I quote here Mulder's analysis, which, as far as I have been enabled to see the results of the analysis of others, agree together. Specific gravity of the coal at 15° C., 1·11; water lost by careful treating, 1·30 per cent.; volatile at bright red heat, 55 per cent.; coke left, 45 per cent. Elementary composition, in 100 parts dry sample: carbon, 84·10; hydrogen, 8·61; nitrogen, 1·08; oxygen, 5·46; ash, 0·75. On being treated with ether, this menstruum exhausted from the coal 1·08 per cent. of a resinous matter. From the analysis made by the other parties above mentioned, it appears that this Gildenhuis coal far surpasses both for gas-making and for yielding oils any of the coals yet found, not even excepting the celebrated Boghead or Torbane Hill. Now, as regards the supply of this mineral, I have the following information in writing from a landed proprietor, at the same time chief magistrate, at Oldenhast—C. W. Eekhout, Esq. The seam (der flötz) originally discovered contains, as far as ascertained, which is only partly done as yet, 80,000 Berlin scheffel, which measure is equal to 50 kilogrammes each, or about 1 cwt. each, so that as far as the workings have been opened this seam yet contains 4000 tons. Six other seams (flötze) have been found, but at present nothing is known as to their richness. The whole tract of country has been granted for working for coal by the Hanoverian Government to three proprietors, I believe all Dutchmen. One of these is H. P. Gelderman, Esq., at Oldenhaal, Holland, to whom parties who might feel industriously interested may apply. From what I learned from Mr. Eekhout it would appear as if this coal deposit,—far distant from any other known in that region, the nearest being about forty miles south-south-eastward off, near Rheina,—even dips towards the Dutch territory, as he told me that, while some of his workmen were digging for marl, traces of coal were found not far from Oldenhaal. I obtained a small sample of this coal, and forwarded specimens to the geological museum of the Government School of Mines, Jermyn Street, London; to the Industrial Museum of Scotland, Edinburgh; to the Natural History Museum, Newcastle-on-Tyne, to the Museum of the Universities of Oxford, Durham, Glasgow, and the Andersonian Institute there, further to some private parties, chiefly scientific men, but also men engaged in industrial pursuits. I am sorry the samples I had to dispose of were not such as to admit of me sending large lumps, although the coal is found and wrought out in large pieces, and though not so difficult to break as some of the Scotch cannels, is pretty hard to break after all. The pit at Gildenhuis is not being worked just now, owing to having got filled with water; it is intended to establish the necessary machinery to withdraw this, and resume working, chiefly it would appear to apply the coal to the making of oily produce.

There is in construction a line of railroad from Almelo to Salzbergen, which will almost touch at Gildenhuis. From Almelo easy transport by water begins, leading to the ports of Amsterdam and Rotterdam. As will be seen from the experiments quoted above, made by Professor Mulder, this coal yields a pretty large percentage of a resinous matter, soluble in ether. I have been busy testing British coals in that aspect,—trying upon them the solving power of ether, chloroform, and sulphide of carbon. I found that as coals stand higher in value for gas-making purposes, as ascertained in daily practice by practical men, so much the larger also is the proportion of parts in these coals soluble in ether, &c.

In requesting you to insert in your valuable paper this notice, I wish it understood that I do not write with the view of puffing up a concern, although, as regards industrial parties who might, by reading this, take an interest in the matter, I may state that, on applying to Mr. H. P. Gelderman, they may learn on what terms the coal is to be had, or the concession granted both to work the seams and erect works for the manufacture of oily products from coals. I have rectified in this paper a mistake made by me while writing to some parties when forwarding the samples of coal, of which I also offer you a small lump as specimen. I am, &c.

Dr. A. Adriani.

Rotterdam.

MISCELLANEOUS.

Deaths from Nitric Acid.—A very painful sensation has been caused in Edinburgh by the death of Mr. Stewart, one of the masters, and also of the janitor of the Edinburgh Institution, a large educational establishment in Queen Street. Mr. Stewart was in the laboratory of the school preparing for some chemical experiments, and while carrying a jar of nitric acid across the room, it fell on the floor and was broken. He called the janitor to his assistance to wipe the floor and to endeavour to save a portion of the fluid. In this effort both unwittingly inhaled its deadly fumes. Mr. Stewart went home to dinner unconscious of the injury he had received. After an hour or two he began to experience difficulty of breathing, and sent for medical advice, but he very rapidly became worse, and expired at two o'clock on Wednesday morning, about ten hours after the accident. The janitor was also taken ill, and, though he rallied for a time on Wednesday, he afterwards sank, and expired at five o'clock on Thursday morning. Several of the most eminent medical men in the city were called in to the unfortunate patients, but their skill proved unavailing. The case, it appears, is almost without a parallel in medical experience.

SCIENTIFIC SOCIETIES.

MEETINGS FOR THE ENSUING WEEK.

March 16. Monday.

Medical—32A, George Street, Hanover Square. 8.30 p.m.

Asiatic—5, New Burlington Street. 3 p.m.

17. *Tuesday.*

Royal Institution—Albemarle Street. 3 p.m. Professor Marshall, "*On Animal Mechanics.*"

Civil Engineers—25, Great George Street, Westminster. 8 p.m.

Statistical—12, St. James's Square. 8 p.m.

Pathological—53, Berners Street. 8 p.m.

Ethnological—4, St. Martin's Place. 8 p.m.

18. *Wednesday.*

Meteorological—25, Great George Street. 7 p.m.

Society of Arts—John Street, Adelphi. 8 p.m. C. B. King, "*On the Suppression and Extinction of Fires.*"

Geological—Burlington House. 8 p.m.

London Institution—Finsbury Circus. 7 p.m.

19. *Thursday.*

Royal Institution—Albemarle Street. 3 p.m. Dr. Frankland, "*On Chemical Affinity.*"

Royal—Burlington House. 8.30 p.m.

Chemical—Burlington House. 8 p.m. Professor Abel, F.R.S., "*On the Composition of some Varieties of Native Copper.*" Dr. Divers, "*On the Spontaneous Conversion of Gun-cotton into Pectic and Parapectic Acids.*" C. Greville Williams, F.R.S., "*On the Chinoline and Leucoline Series.*" Dr. Attfield, "*Note on Oxamide.*"

Antiquaries—Somerset House. 8.30 p.m.

Linnæan—Burlington House. 8 p.m.

Numismatic—13, Gate Street, Lincoln's Inn Fields. 7 p.m.

Royal Society Club—St. James's Hall. 6 p.m.

20. *Friday.*

Royal Institution—Albemarle Street. 8 p.m. Mr. Balfour Stewart, "*On Magnetic Forces.*"

Philological—Somerset House. 8 p.m.

21. *Saturday.*

Royal Institution—Albemarle Street. 3 p.m. Prof. Max Müller, "*On Language.*"

ANSWERS TO CORRESPONDENTS.

⁂ All *Editorial Communications* are to be addressed to the Editor, and *Advertisements* and *Business Communications* to the Publisher, at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

⁂ In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

Vol. VI. of the Chemical News, containing a copious Index, is now ready, price 10s. 8d., by post, 11s. 2d., handsomely bound in cloth, gold-lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. if sent to our Office, or, if accompanied by a cloth case, for 6d. Vols. I. and II. are out of print. All the others are kept in stock. Vol. VII. commenced on January 3, 1863, and will be complete in 26 numbers.

C. V. B.—The suspension is only temporary. They will be reported whenever arrangements can be made.

S.—We have not received the report of the meeting yet.

B. P.—In vol. iv. of the Chemical News.

THE CHEMICAL NEWS.

Vol. VII. No. 172.—March 21, 1863.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Analytical Notes on Thallium—Separation of Thallium from Copper, by William Crookes.

These two metals are frequently associated in nature, and, when that is the case, the thallium generally accompanies the copper in the various metallurgical and manufacturing processes through which it passes, and may be detected in the commercial products. The analytical separation of thallium from copper is remarkably easy. The metals must in the first place be obtained in acid solution; any mineral acid will answer the purpose, unless, indeed, a considerable quantity of thallium is present, when hydrochloric acid should be avoided, as it might determine the formation of the slightly soluble protochloride of thallium. With this proviso any acid may be used. and an excess does no harm. To the acid solution add ammonia in excess; if the thallium is present in the state of a protosalt it will dissolve along with the copper, but if it be in a higher state of oxidation it will come down in the form of a flocculent brown precipitate, scarcely distinguishable from sesquioxide of iron: its colour is, however, rather darker. Now add to the blue ammoniacal solution without filtering a solution of cyanide of potassium, free from sulphide, until it is decolourised. A little more than is sufficient for this purpose is to be added. Let the liquid digest at a gentle heat for half an hour, and then filter; to the filtrate add a drop or two of sulphide of ammonium, mix well, and allow it to stand in a warm place for half an hour: according to the quantity of thallium present a greater or less amount of sulphide will be precipitated. If only a very minute trace be present, the precipitate will not be very apparent until it has settled. It is a very characteristic property of sulphide of thallium, that all the minute particles suspended in a liquid gradually collect together into a few large clots at the bottom of the vessel.

This test is exceedingly delicate; many specimens of copper wire and sheet contain more or less abundant traces of thallium. It has a tendency to make the copper very brittle; the 200th part greatly injuring its ductility, malleability, and conducting power for electricity. Dr. Matthiessen has kindly placed at my disposal some metallic copper prepared from Spanish copper pyrites. The metal is remarkable for its very low electrical conducting power; its value in this respect being only 15, pure copper being 100. It is called "cement copper," from the way it is prepared. Copper pyrites are allowed to oxidise in the air, and the resulting sulphate of copper is washed out; scrap iron is now placed in the liquid, which precipitates the copper in the powdery state. The metal is then collected together, dried, strongly compressed, and heated to the melting point. It is brought over to this country in the form of rectangular cakes, weighing about 20 lbs. each.

Upon examination, thallium was found to be present in this copper in considerable quantities. I have not completed the quantitative determination, but it is so rich that, had I discovered this six months ago, I should have welcomed it as an available source of the new metal.

It is obvious how the thallium occurs in such quantities in this copper; the pyrites is evidently thalliferous. I have not had an opportunity of examining some of the identical mineral used for the extraction of this copper, but I have found so many different samples of Spanish pyrites which contain thallium, especially some from the south of Spain, for which I am indebted to the kindness of Dr. Thornthwaite, that there is no doubt the pyrites in this case also contained thallium. I have found out, experimentally, that upon allowing thalliferous pyrites to oxidise by exposure to the atmosphere, sulphate of thallium is formed as well as sulphate of iron and copper.[*] From the solution of these, metallic iron precipitates thallium along with the copper, and upon subsequent fusion a considerable portion of the thallium would alloy with the copper and escape volatilisation.

I have endeavoured to form an artificial alloy of thallium and copper; the two metals melt together without much difficulty, but a homogeneous mixture cannot be obtained unless they are kept melted for some time together, the thallium volatilising all the time. Ten per cent. of thallium forms a yellowish alloy very hard and brittle, and tarnishing readily in the air. A larger quantity of thallium whitens the copper.

On Bathvillite, a new Inflammable Mineral, by C. Greville Williams, *F.R.S.*

Adjoining the lands of Torbanehill, well known for containing the valuable seam of inflammable schist bearing its name, lie the grounds of Bathville Large quantities of mineral of almost absolutely similar composition to torbanite are extracted there, and employed chiefly as a source of photogen. The Bathville mineral, at least all that I have seen of it, only differs from torbanite in being rather paler in tint. It often contains hollows, generally filled with carbonate of lime, pyrites, or other foreign matters; but sometimes, although very rarely, the hollows are filled with lumps of a brown and exceedingly friable substance, in appearance re-

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[*] I may here draw attention to the rapid oxidation which precipitated sulphide of thallium undergoes on the filter. If the water with which it is washed is not charged with sulphide of ammonium, as soon as the washing commences sulphide of thallium is formed, which passes through, and is re-precipitated as sulphide upon meeting with the excesss of sulphide of ammonium in the first portions of the filtrate.

sembling tripoli. It is this substance which forms the subject of the present communication. I am informed by Mr. George Miller that the new mineral has not yet been found in the shale from the adjoining estate of Torbanehill.

It was about two years ago that I first happened, on breaking a lump of Bathville torbanite, to observe this substance, and it was so totally unlike anything I had seen accompanying coals or shales before, that I was induced to examine it carefully. It was in one fragment weighing about five grammes. Its fawn-brown colour, and friability between the fingers, recalled the idea of wood in the last stage of decay. These properties rendered a determination of the specific gravity by ordinary methods extremely difficult, and I was disinclined to subject any of it to treatment which would have prevented its further examination, as I wished to retain a portion for future comparison with other minerals. The density of one small fragment was about 1·010, after the air in the pores had been as far as possible removed by the air-pump.

It does not melt when heated, and it yields nothing to benzole. Moderately dilute nitric acid is without action; but concentrated sulphuric acid, assisted by heat, effects complete carbonization. Heated in a platinum crucible it evolves a fatty odour like that emitted under similar circumstances by torbanite. The evolved gases burn with a dense smoky flame, the final result of the ignition being a brilliantly white ash.

The following results were obtained on analysis:—

I. 0·2876 grm. gave 0·6210 carbonic acid and 0·2216 water.
II. 0·3080 ,, ,, 0·0780 ash.

The above numbers reduced to percentages give the annexed values:—

	Experiment.	Torbanite (Miller).
Carbon	58·89	63·10
Hydrogen	8·56	8·91
Nitrogen, oxygen, sulphur, &c.	7·23	8·21
Ash	25·32	19·78
	100·00	100·00

This analysis differs considerably from Dr. Miller's analysis of torbanite, but the difference is owing to the ash; for if we deduct it, and then calculate the carbon and hydrogen in the organic matter only, we find the composition to be almost identical.

	Experiment calculated free from ash.	Torbanite ash free. (Miller.)
Carbon	78·86	78·67
Hydrogen	11·46	11·11

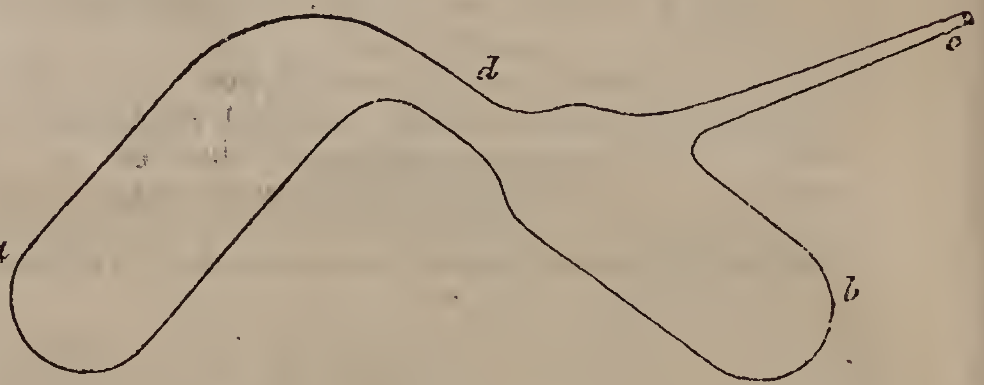
If we assume the difference between these latter numbers and the hundred parts to be oxygen, it will be found that the organic portion of the mineral corresponds closely in composition to the percentages required by the formula $\text{Ȼ}_{30}\text{H}_{50}\text{Ɵ}_{3}$, as may be seen by the following numbers:—

	Experiment.		Calculation.	
Carbon	78·86	Ȼ_{30}	360	78·60
Hydrogen	11·46	H_{50}	50	10·92
Oxygen	9·68	Ɵ_{3}	48	10·48
	100·00		458	100·00

I do not, however, place much reliance upon the formula of a substance of the purity of which there is no guarantee.

It is remarkable that the mineral, as removed from the cavity in the torbanite, was almost absolutely dry, for it did not lose a tenth of a per cent. of moisture on being heated to 300° F.

I was desirous of ascertaining whether the quantities of tar, coke, ammoniacal liquor, &c., yielded by the substance under examination, were similar in amount to those afforded by torbanite; but the quantity in my possession being so small, I was obliged to employ a special arrangement. A retort and a receiver were therefore blown in one piece, the end *a* of the retort being left open. The apparatus being inverted, a weighed fragment of the mineral was inserted, and the end closed with the blowpipe. A small tube, *c*, was then drawn out from the upper part of *b*. The apparatus having been weighed, heat was cautiously applied to *a*, *b* being kept cold by a stream of cold water; the tar and ammoniacal liquor distilling over condensed in *b*, while the moist gases escaped by the tube *c*. The loss of weight of the entire apparatus consequently gave the amount of volatile matter. The retort was then cut from the receiver at *d*, and the latter was weighed. The tar having been dissolved out with benzole, and the receiver rendered perfectly clean and dry, it was weighed again, the difference between the two weighings giving the amount of tar and ammoniacal liquor. The coke was then removed carefully from *a*, and reheated in a closely covered platinum crucible for a few minutes. The loss of weight was added to the volatile matters. The lid was then removed, and the coke burnt until a brilliantly white ash was obtained. By proceeding in this manner the tar, volatile matters, coke, and ash can all be determined upon three decigrammes of substance. Where great accuracy is desired, it is necessary to make two or three experiments at varying temperatures, and obtain a mean result.



The following table contains the results of a comparative examination of the mineral with the torbanite with which it was associated:—

	Experiment.				Dr. Miller.	
	Tar, &c.	Gases.	Coke.	Ash.	Coke.	Ash.
Torbanite	54·3	14·8	31·8	20·1	30·2	19·8
Bathvillite	43·0	19·8	36·1	25·3	—	—

I can say but little as to the origin of the mineral. Its composition, and the manner in which it was protected by its envelope of torbanite, are evidence against its being a product of the disintegration of that substance. It is more probable that it consists of the material from which torbanite would have resulted had it undergone the pressure of the overlying strata. That the excess of ash is accidental would appear to be proved by the almost identity of composition of the organic portion of the two minerals.

I am far from supposing that all specimens of bathvillite will yield exactly the numbers given above; the organic part of the mineral will probably always correspond with that of the mineral with which it may be associated.

As I have been unable to identify the substance with any of the inflammable minerals yet known, I have given it a distinctive appellation derived from the locality where it was first found.

On the Preparation of Bismuthic Acid, by MM. Boedeker *and* Deichmann.

A solution of nitrate of bismuth when treated with excess of concentrated solution of cyanide of potassium, produces, it is well known, a dark brown powder, and the water employed becomes red.

This brown precipitate, hitherto supposed to be BiO_4, is in fact hydrated bismuthic acid BiO_5+2HO. It is purified by boiling and washing in distilled water; without this precaution the product will retain a little cyanide of potassium.

The hydrating water only begins to be disengaged towards 150° C.

This reaction, during which bismuthic acid forms, is not produced by the cyanate of potash which the cyanide may contain; the authors have ascertained by direct analysis that even when pure cyanate gives no analogous reaction.—*Chem. Centralblatt.*

TECHNICAL CHEMISTRY.

On the Presence of Sub-Oxide of Silver in Silver Scorias, by M. Leussen.

After treating residuums of silver with excess of sulphide of ammonium, and melting with nitre and carbonate of soda, the precipitate, containing besides sulphide of silver sulphides of iron and zinc, M. Leussen found above the button of silver a compact olive-green crust, covered with a fine powder of a beautiful colour.

The compact scoria contained:—

Oxide of zinc	37·87
Peroxide of iron	8·39
Soda	6·02
Oxide of silver.	39·20
Insoluble portion	8·76
	100·24

The powder yielded by analysis the following numbers:—

Insoluble portion	5·62
Peroxide of iron	12·13
Oxide of zinc	46·30
Protoxide of silver	35·73
	99·78

Analogies which the author explains by the formulas:—

$$Fe_2O_3+Ag_2O+7ZnO \text{ or } 2FeO+2AgO+7ZnO.$$

He is disposed to admit that the silver contained in this compound exists in the state of sub-oxide.—*Journal für Praktische Chemie*, vol. lxxxv., p. 97.

PHARMACY, TOXICOLOGY, &c.

On Phosphomolybdic Acid as a Test for Alkaloids in Medicinal Preparations, by Fred. F. Mayer, *of New York.*

At the time when I undertook this investigation I was not aware that another chemist had been engaged on the same subject, and in the same direction, nor what results he might have arrived at. But as I was informed last year by Professor Maisch, a pupil of his in Philadelphia, Mr. Hayes, of Georgia, had undertaken to examine the value of phosphomolybdic acid for the same purpose. Professor Maisch could not, however, give me any data as to the conclusions Mr. Hayes had arrived at, and being unable to communicate with the gentleman, as well as unwilling to deprive him of the merit belonging to him, I concluded upon entering only into a general examination, the more so as I have never been very favourably impressed with any belief in the practicability of the test for volumetrical analysis.

For the relative value of a method of wet assay depends not so much on the extreme nicety of the reaction which is required in chemical analysis by weighing, but in the readiness of the means with which the reaction is brought about. Another requisite of a volumetrical method is that the assay be not interfered with by other ingredients of the liquids acted on, which might produce similar results or render the reaction indistinct.

It always appeared to me that phosphomolybdic acid was inapplicable for every one of the reasons just given.

This compound was introduced by Struve and Svanberg, as a test for the presence of ammonia in acid liquids, and to this day is used for this purpose, as is the molybdate of ammonia in testing for phosphoric acid. Its preparation, quoting Sonnenschein, is as follows:—

"Molybdate of ammonia is precipitated by common phosphate of soda; the yellow precipitate, after being well washed, is suspended in water and heated with carbonate of soda until dissolved. This solution is then evaporated to dryness, and calcined to drive out every trace of ammonia. In case the molybdic acid should in part have been reduced during this process, the whole must be moistened with nitric acid, and again calcined. The resulting salt, when cool, is then warmed with water, nitric acid is added in excess, and the strongly added solution diluted with water, in such a proportion that the resulting liquor should contain 10 per cent. of the dry salt. The golden coloured liquid must be kept from the influence of ammoniacal vapours."

After this reagent had been established by Sonnenschein as a test for ammonia or any of its salts, De Vrij, in 1853, first noticed that it produced a similar reaction, not only with ammonia, but also with alkaloids, and in 1857, Sonnenschein, without crediting De Vrij, published the same as an extension of the original test, for the determination of the amid-bases derived from ammonia, as well as all the alkaloids, which had come under his observation, (*Ueber ein neues Reagens auf Stickstoffbasen, Berlin, E. Kuhn*, 1857.) and an abridged account of his paper was republished in the *Pharmaceutical Journal* and the *American Journal of Pharmacy* for 1858.

As regards delicacy of reaction this test leaves nothing to be desired, and is in this regard exceeded only by Scheibler's metatungstic acid.

Not to mention, however, the costliness of the reagent in either case, which would in itself be sufficient to preclude its adoption for general analysis, there is one great objection to its use, and this objection in all probability has prevented Sonnenschein from giving the quantitative application of the test which he promised when publishing the qualitative—it is that a precipitate with phosphomolybdic acid always leaves us in doubt as to whether it be caused by an alkaloid or by ammonia, or one of its compounds; appearance, texture, and colour in nearly all cases differing very little, which difference in coloured solutions amounts to nothing.

This I consider the fatal objection to the reagent in its application for the testing of narcotic extracts. An extract containing as much of a salt of ammonia as it should of its proper alkaloid, if phosphomolybdic acid were used for quantitative precipitation, would show the same strength as a good extract. There would therefore be no safeguard against error or fraud. But it is a question as yet whether of all narcotic extracts, and extracts generally containing nitrogenous compounds, ammonia, or some of its salts does not form a constituent.

We know how liable the alkaloids of Belladonna, Stramonium, Conium, and Hyoscyamus, are to decomposition during their preparation, and that ammonia is always a product of such decomposition; and it may be safely assumed that it is a constituent of all narcotic extracts—preparations which have to undergo so much manipulation and exposure to extraneous influences.

An experiment made in this direction gave me a doubtful result; but as the method employed is capable of improvement, and the point well worth examining, I can only promise to continue the experiments.

A few grammes of Extr. Belladonnæ (English) diffused in water, were mixed with dilute caustic soda lye, and placed in a flask, through the cork of which passed first a tube conveying a current of washed hydrogen gas; and, secondly, a tube provided with a large bulb, the upper end of which was connected with a bent tube passing into a flask with dilute nitric acid.

The purpose was to obtain, if possible, the most volatile product of the action of the caustic alkali on the extract in an atmosphere of less density than that of the atmospheric air, free from the products of that action at a higher temperature; the bulb tube to retain anything condensible, other than gas.

Belladonna and its extract, when mixed with a fixed alkali, evolves an odour closely resembling that of conia. The current of gas after passing through the bulb tube, but before reaching the acid, possessed the same odour and gave white fumes with acetic acid, and the nitric acid into which the gas was afterwards conducted for a considerable length of time, gave a faint reaction with phosphomolybdic acid. The question is therefore unsettled, whether these fumes with acetic acid arise from ammonia, or from the substance which causes the narcotic odour of the extract and of impure atropia; and my experiments in that direction are not yet concluded.

The third objection, and which relates especially to the application for which the test is wanted, is that phosphomolybdic acid, when brought into a solution of an extract, however the same may be acidulated with nitric acid, is decomposed, the green modification of molybdic oxide being one of the products, which produces not only a green precipitate, but also a deep green, cloudy liquid in which no test-reaction can be observed with any degree of safety. The action of the saccharine or mucilaginous constituents of the extracts is in this case similar to that of most other deoxidising agents, but appears not to be overcome even by the presence of nitric acid.

For one purpose, however, and this is one of the applications I believe Mr. Hayes has made of the test, it may be valuable to the pharmaceutist; this is in determining, during displacement or extraction of a vegetable drug, the point of its exhaustion. In liquids containing but little extraneous organic matter, the reaction, after acidulating with nitric acid, shows itself distinctly enough; but in such where the organic non-nitrogenous matter preponderates, phosphomolybdic acid cannot be used, at least as a quantitative test. These we may presume are some, if not the principal reasons, why the test has not been further extended in its application.—*Proc. Am. Pharm. Association.*

On Liquid Rennet, or Rennet Wine,
by William Procter, *Junr.*

It has long been known that the mucous coat of the stomach of the calf, pig, and other young mammals possesses the power of coagulating the casein of milk, as in the making of cheese; but it is only of later years that attempts have been made to introduce a solution of *rennet*, as this substance is called, into Pharmacy with a view to the *cuisine*, as well as for use in medicine. For several years *pepsin*, the proximate principle to which this coagulating power is attributed in an impure state, has been an article of commerce with a view to medicinal use in diabetes and other diseases; that prepared by M. Boudalt, of Paris, having acquired some celebrity.

In several pharmaceutical works, recipes for making "Liquid Rennet" may be found, but these vary much, both as regards the strength of the solution and the preservative ingredients. The dried stomach of the calf prepared by salting it, and stretching it over a bent hoop to facilitate its desiccation, is familiarly known in the markets under the name of rennet, and most housekeepers prefer to keep and use it in this state, or by cutting this up and macerating it in wine, but for those who aim at manufacturing the solution for commerce, it is preferable to employ the recent membranes both on account of economy and efficiency.

Pepsin has never been isolated in a state of purity; it seems to be secreted by glands located in the mucous coat of the stomach, and in connection with hydrochloric, and perhaps lactic acid, constitutes the active portion of what is called the *gastric juice*, concerned in the process of digestion. It is to this mucous coat of the organ, therefore, that attention must be given; and inasmuch as the rennet tends to exercise its digestive power on the membrane itself, there is a propriety in using mechanical means to effect the rupture of the mucous coating to facilitate the extraction of the active principle without cutting it up, by malaxating them in water with salt, to which weak alcohol or wine is subsequently added. Some prefer at once to separate by the knife the mucous coat with the glands, but generally the whole organ is cut up, mixed with salt and water, and well malaxated at intervals, for a time varying from twenty-four hours to two months. Soubeiran gives the following recipe as that of Wislin:—He takes of the stomachs of young calves *ten* parts; chloride of sodium *three* parts; alcohol, of 80 per cent., *one* part; and water *sixteen* parts. The stomachs are slit with a scissors so as to expose the interior, they having been previously gently rinsed to remove foreign particles adherent; they are then malaxated with the dry salt thoroughly, and left in a cool place until the cheesy odour at first apparent becomes replaced by that of rennet, which requires one or two months. At this period it is macerated in the water for a short time, the alcohol is added, and the liquid portion strained off and filtered for use.

Dorvault, in his "L'Officine," gives the following recipe:—Take of recent rennet 375 parts; common salt 60 parts; alcohol of 75 per cent. 60 parts; white wine 1000 parts. Digest the whole for a month and filter. A teaspoonful is sufficient for a quart of milk."

Some believe that the rennet during the protracted process of curing by salt is rendered more active, and that it is necessary to delay the final process of solution until after that operation; but this is, doubtless, incorrect, in fact, though it may seem to be true, as the truth lies more in the mechanical effect of salting in breaking or causing the shrinking of the tissue, and in altering the resistant or pulpy condition of the mucus, than in any development of increased power; and hence, there appears to be no good reason why it should be protracted beyond a few days, unless with the intention of curing the rennets for future use. After several trials the following is offered as a practicable formula for making

Liquid Rennet, or Rennet Wine.

Take of fresh rennets (about three),	twenty-four troy ounces,
Chloride of sodium,	three troy ounces,
Alcohol,	six fluid ounces,
White wine,	sixteen fluid ounces,
Water,	a sufficient quantity.

Having turned the rennets inside out, and washed them by a gentle stream of water for a few moments without any friction, and having placed them in a shallow dish, sprinkle half of the salt over them, and with the hands malaxate them vigorously for fifteen minutes, aiming to disengage the mucous secretion, then add a pint of water, again malaxate, and after standing several hours strain off the thick mucilaginous liquid by pressure in a loose-textured cotton cloth. Return the rennets to the dish, sprinkle on the remainder of the salt, again repeat the process of malaxation, maceration, and expression. Let these operations be repeated a third time, or until the fluid obtained measures forty-two fluid ounces; then, having mixed the alcohol with the wine, add them to the expressed liquid, and agitate the mixture several times at intervals of an hour or two; after which allow it to stand until the mucus which is precipitated by the alcoholic addition subsides, when the nearly clear solution of rennet may be drawn off with a syphon, and filtered for use. The wine may be substituted by a mixture of one part of alcohol and three parts of water. A tablespoonful of this solution readily coagulates a quart of milk to a firm curd, if added after gently warming the milk (to about 100° F.), stirring it well for a moment, and allowing it to stand undisturbed for half an hour or an hour. *Cold custard* is made in the same manner by previously adding a tablespoonful or more of sugar, and some vanilla or other flavouring before adding the rennet. When liquid rennet is employed for medicinal purposes it is preferable to make it with wine, as being more acceptable to the stomach.—*Am. Journ. Pharmacy.*

PHYSICAL SCIENCE.

*On the Fixed Lines of Stellar Spectra,** *by Professor* Donati. (*Abstract by* S. M. Drach, *Esq.*)

The first memoir, dated August, 1860, is on the "Striæ of Stellar Spectra." After quoting Fraunhofer, in Schumacher's *Astron. Abhand.*, Part ii., 1823, and in Gilbert's *Annalen der Physik* (vol. lxxiv., 1823), on the "Spectra of Stellar and Planetary Light Rays," the author states that he used a large burning lens kept in the Museum since 1690, and whereon Targioni, Averani, and Sir H. Davy had experimented. This lens has a diameter of 0·41 metres, and a focal distance of 1·58 metres (15 and 62½ inches). The lens was mounted parallactically on a movable stand: a piece with a fine slit was placed a short distance within the focus of the lens, through which the stellar rays passed. Within this lens-tube is a cylindrical lens with its very short focus, coinciding with that of the great lens. The stellar ray then falls on a prism fixed on a graduated circle, to which is attached a small achromatic telescope (object-glass, aperture 0·024; focal distance, 0·17 metre; eye-piece magnifies twelve times). There is also at the end of the great lens-tube an achromatic lens equal to the object-glass of the telescope. Thus the stellar rays are passed through the cylinder-lens, the tube-achromatic, the prism, and the telescope-lenses, before reaching the observer's eye. In the focus of the achromatic telescope are two metallic bars, one fixed, the other movable by a micrometer-screw. A cylinder-lens was preferred to a simple slit, for the purpose of getting the two lateral segments of the fasciculus of rays, which are lost in the ordinary diaphragm. But as by changing the position of the star observed, as respects the direction of the optical axis of the collecting lens, we change the position of the focal straight line produced by the cylindrical lens, making it impossible to keep the said straight line always in the same position as regards the prism, the author placed a metallic lamina at a distance from the cylinder-lens, equal to the focal distance of the latter. In the said lamina is a thin longitudinal slit, through which one sees the light-line formed in the focus of the cylinder-lens; thus certifying, when that line is visible, that it is always in the same position relative to the prism.

The author then proceeds as follows:—

I observed by day the solar spectrum, placing over the great lens a cloth, so fixed that it transmitted only the requisite quantity of light, and fixed the prism p to correspond to one or the other of the solar striæ; then, fixing the telescope, I turned the movable bar, till one of its limbs touched the said stria at its minimum, and I read the corresponding division of the micrometer-screw. Leaving everything in this state, I turned the great lens in the evening to the star I wished to observe, and by the micrometer-screw immediately obtained the position of the stellar striæ, referred to the solar one observed in the morning. Besides the position of the stellar striæ, I have tried to determine the extremes of the spectra themselves, for which see table.† I denote the striæ of the stellar spectra by the Greek letters, α, β, γ, proceeding from the least refracted end of the spectrum. The unlettered striæ have not been measured, but simply estimated, as being very difficult. I have grouped separately the separate-coloured stars, giving in each group the first place to the most conspicuous striæ, and so in order; classifying them from Humboldt's *Cosmos*, and Schmidt, in *Astr. Nachr.* The table indicates that the stellar striæ have a certain relation to the star's colour. Thus the white stars seem to have a family likeness; and so the yellow, orange, and red ones. There is likewise a great difference in the lengths. The three yellow stars have nearly the same maximum refraction end, but different minimum refraction ends; the reverse with the red stars. True, the measures of these ends (and indeed of the maximum refraction) are very uncertain, depending on the glass of the lenses and prism, and on the observer's eye. (Professor Amici has constructed a prism of very great dispersive power, without altering the axis of vision. It is composed of three prisms; the two outer ones of crown-glass, the inner one of boro-silicate of lead. Looking through this prism at a slit or luminous line, light is decomposed, with the same striæ

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* *Monthly Notices of the Astronomical Society.*

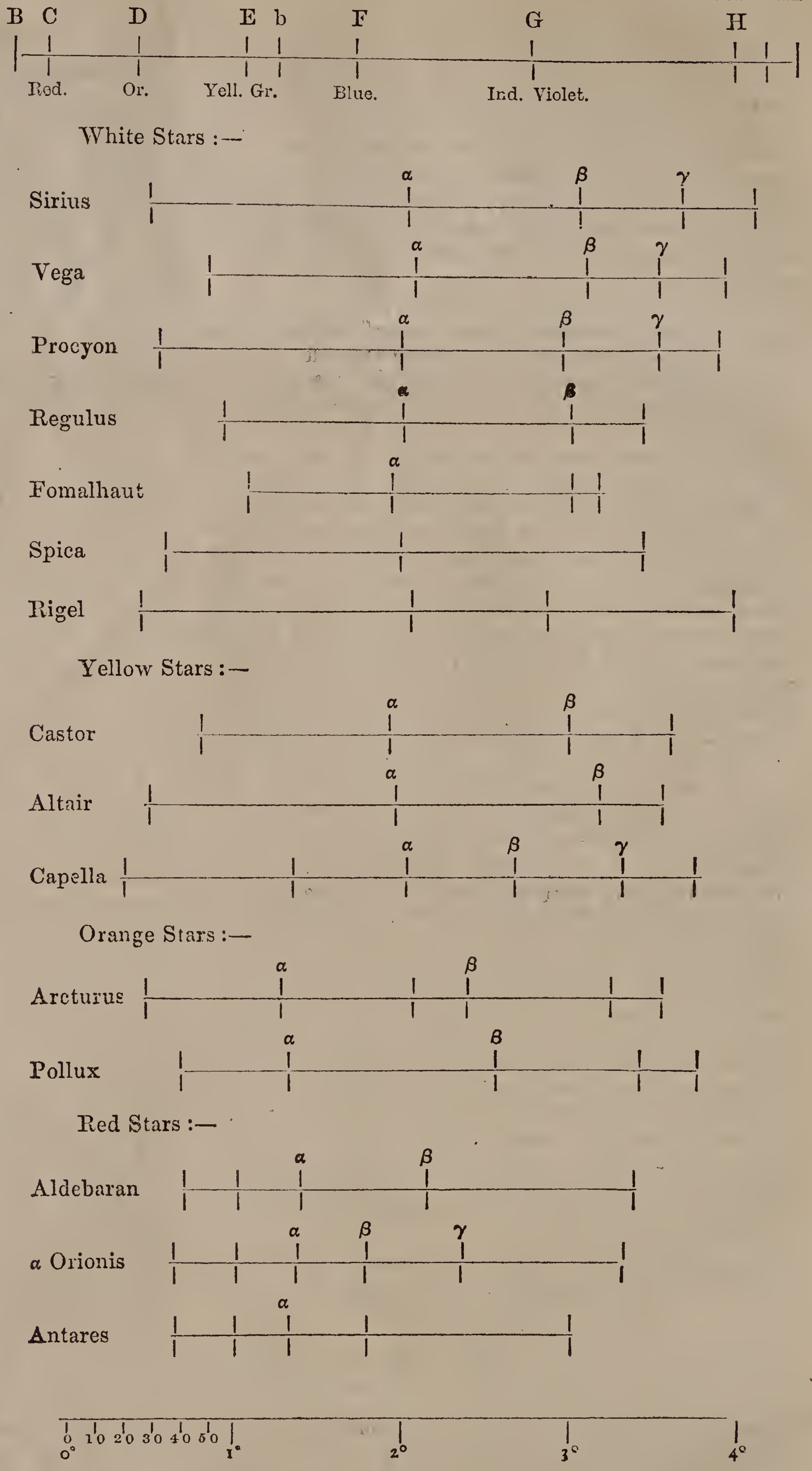
† See the figure roughly reduced from Professor Donati's Memoir, and the numerical values.

as through a simple prism of flint glass.) My prism was of flint glass; refraction angle, 60° 52′ 39″; angle of refraction of stria D of the solar spectrum (*i.e.*, the angle which the refracted ray corresponding to D makes with the incident ray), is 49° 55′ 05″. The intermediate angles of the solar spectrum were found—

	′	″			″
B C =	10	57	b F =	29	16
C D =	30	35	F G =	72	9
D E =	40	41	G H =	65	56
E b =	7	45			

All these angles I have measured by the theodolite as accurately as possible. The rim of the micrometer-screw = 11′ 5″; each of the 100 divisions of the head = 6·65″. Its movable bar was broad enough to be seen without illuminating the telescope; and I always placed the same limb in contact with the measured striæ. I observed when the star was close to the meridian, giving me more definite striæ; but we must not conclude that the striæ change with the altitude of the star; the horizontal scintillations naturally disturb the precision of the spectrum. Whilst observing, I tried to be in the dark as much as possible; ordering another to read the micrometers; for when the eye is fatigued, even by the feeble light of a lantern, it could not perceive the feeblest striæ of the stars. In cloudy or moonlight nights, the observations were uncertain and sometimes absolutely impossible. Although the great lens was not achromatic, yet, looking through the telescope at the slit illumined by the rays collected by the cylinder-lens, it appeared perfectly white and without perceptible coloured edges.

The following measures are the means of many observations thus obtained. Each star has its stria α referred to the solar F. Thus, for *Sirius*, $\alpha = \mathrm{F}\odot - 15''$ means that *Sirius* α is refracted 15″ less than Sun F; and for *Vega*, $\alpha = \mathrm{F}\odot + 40''$, that *Vega* α is refracted 40″ more than Sun F. $(r\,\alpha)$ means the angle between the minimum refracted end of the stellar spectrum with its stria α; $(\alpha\,\beta)$, $(\beta\,\gamma)$, the angles between α and β, β and γ. $(\alpha\,v)$, $(\beta\,v)$, $(\gamma\,v)$, are the angles made by α, β, and γ, with the maximum refracted end v of its spectrum.



A short description of each stria is appended, and its eye-estimated breadth referred to the micrometric bars. These said angles do not correspond to the central part of the striæ, but to their least refracted limb, which limb I always collimated with the movable bar of the micrometer. Thus *Sirius* α is refracted 15″ less than F Sun; but, since breadth of this $\alpha = 50''$, the central part of the said α is refracted 10″ more than F Sun. Similarly, *Vega* $\alpha = \mathrm{F}\odot + 40''$, breadth 40″, has its centre refracted 60″ more than F Sun, &c.

		$(r\,\alpha)$	$(\alpha\,\beta)$	$(\beta\,\gamma)$	$(\gamma\,v)$
Sirius	$\alpha = \mathrm{F}\odot - 15''$	85′40″	66′37″	42′27″	20′17″

α very fine, breadth 50″; β very fine, double, breadth 80″; γ as broad as α, faint.

Vega	$\alpha = \mathrm{F}\odot + 40''$	77′15″	67′56″	34′55″	10′18″

α, β, very fine; α 40″, β 60″ broad; γ very broad, faint.

Procyon	$\alpha = \mathrm{F}\odot - 33''$	78′55″	65′30″	37′ 1″	9′32″

α well seen, 15″ broad; β scarcely seen; γ ditto, rather broader than α.

		(r α)	(α β)	(β γ)	(γ v)
Regulus	α=F⊙—60″	68′30″	66′17″	30′55″	—
α very clear, 20″ broad; β seen at intervals.					
Fomalhaut	α=F⊙—55″	61′44″	70′36″	—	—
α fine, 35″ broad.					
Castor	α=F⊙—30″	69′43″	62′ 4″	30′ 9″	—
α very fine, 40″ broad; β ditto, 60″ broad.					
Altair	α=F⊙+5″	73′42″	66′ 3″	24′23″	—
α fine, 20″ broad; β indistinct, faint, 40″ broad.					
Capella	α=F⊙—2′40″	81′ 1″	31′55″	34′35″	23′36″
α very thin, well observed; β, γ, scarcely visible, uncertain observation.					
Arcturus	α=F⊙—29′22″	51′26″	44′27″	66′23″	—
α very thin, seen with trouble; β very difficult to be seen.					
Pollux	α=F⊙—28′42″	47′33″	45′46″	68′56″	—
α very thin, seen with trouble; β very difficult to be seen.					
Aldebaran	α=F⊙—22′29″	50′39″	29′40″	64′17″	—
α very fine, 30″ broad; β somewhat thinner than α, very fine.					
α Orionis	α=F⊙—30′35″	50′19″	17′24″	16′51″	57′18″
α very fine, 40″ broad; β very fine, 50″ broad; γ frothy, well seen.					
Antares	α=F⊙—33′28″	47′20″	18′17″	65′45″	—
α fine, 30″ broad; β rather fine, 20″ broad.					

Now, comparing my results with Fraunhofer, there are very great differences. Thus, he says, *Sirius* has a very fine stria in the green, whilst I find this stria in the blue of the spectrum. He finds in *Pollux*, *Capella*, α *Orionis*, *Procyon*, a stria with the same position as D Sun, which line I have never seen. Perhaps attributable to the reason that the stellar light is never strong enough to distinguish the colours; saving a reddish tinge at the minimum-refraction end, they appear of a lavender tint in their whole length, which is always smaller than the solar one under the same conditions, from the great difference of light between the Sun and stars. If the stellar stria has not been measured, but merely eye-estimated (probably Fraunhofer's process), one may very easily get deceived, and mistake one stria for another. This occurred even to me at the beginning. Thus, comparing the distance between the extreme red of *Sirius* and its α with that between the extreme red of the Sun, and E ⊙, I judged that α *Sirius* = E ⊙; but, passing to exact measures, I directly found that α *Sirius* = F ⊙.

Perhaps, as the stars change in colour, their stria-positions may change, or my more brilliant striæ may not have been the more brilliant ones to Fraunhofer's eye. (Thus Fraunhofer is silent on the difficulties of *Capella*, and speaks of the difficulties of *Procyon*. I found *Capella* very troublesome, and the stria of *Procyon* much clearer and sharper than any in *Capella*.) I merely point out the utility of making these observations at various times with the extremest accuracy, without hazarding any hypothesis.

However, I must remark that almost all my fifteen stars have a stria, very little differing from F ⊙. One may therefore presume that all these F-like striæ are constant, but with a greater or less refraction, from a difference in the refractive quality of the light of the particular star. On this hypothesis (partly agreeing with Fraunhofer's) it follows that the light of *Capella*, with a stria refracted 2′ 40″ less than F ⊙, must suffer a less refraction than the light of *Vega*, which is refracted 60″ more than F ⊙, whence the declinations of those two stars, observed from two places, with very different (meridian) altitudes for each, must result differently. (Since 2′ 40″ + 60″ = 220″, and the refraction of F ⊙ = 51° 13′ = 3073′, with an horizontal refraction of 33′. Hence the different refractions of these two striæ transferred to horizontal refraction $= \frac{33}{3073} + 220'' = 2{\cdot}36''$.)

I therefore compared Mr. Airy's "Cambridge Observations," 1828—1835, with Mr. Henderson's at the Cape of Good Hope, 1832—3 (*Mem. R. Ast. Soc.* vols. x. and xi.), which give these results:—

Camb.—C.G.H. 1830.		Mean Declination.					A.P.M. in Deci.
		Camb. 1830·0.			C.G.H. 1833·0.		
″		°	′	″	′	″	″
+1·94	Fomalhaut	—30	31	13·92	30	19·1	—0·166
+1·71	Antares	—26	0	46·81	3	14·3	—0·034
+0·81	Sirius	—16	29	22·22	29	36·2	—1·198
+1·12	Spica	—10	16	15·60	17	13·7	—0·040
+0·57	Rigel	— 8	24	16·22	24	2·9	—0·012
—0·06	Procyon	+ 5	39	14·65	38	48·5	—1·023
+0·28	α Orionis	+ 7	22	3·72	22	7·1	+0·002
+1·32	Altair	+ 8	25	33·75	25	59·6	+0·380
+0·07	Regulus	+12	47	44·17	46	49·1	+0·006
—0·51	Aldebaran	+16	9	35·68	9	59·6	—0·174
+1·43	Arcturus	+20	4	16·41	3	18·0	—1·983
+0·72	Pollux	+28	25	45·50	25	20·4	—0·057
+0·49	Castor	+32	15	9·44	14	47·2	—0·078
+1·88	Vega	+38	37	50·56	37	57·7	+0·282
—2·00	Capella	+45	48	52·88	49	8·4	—0·425

I have extracted the Annual Proper Motion from vol. ii., p. 199, of "Annals of the Paris Observatory." With them, and the values in p. 181, to calculate the Precession, I have reduced the Cape observations to 1830·0, whence I find as above. Whence *Vega* has at Cambridge a greater declination than at the Cape of Good Hope, and *Capella* a less declination at Cambridge. This seems to agree with my supposition, since *Vega* is low at the Cape with great horizontal refraction, and at Cambridge it is very high, with scarcely any refraction; and since the light of *Vega* seems from the above to be refracted more than the light of any other star, the refraction correction used at the Cape (which would increase the declination) is too small, making the star's declination less than that observed at Cambridge. The same horizontalities being verified for *Capella*, but, as contrary to *Vega*, the light of *Capella* is less refracted than that of *Vega*, the refraction correction for the Cape of Good Hope must be too great for this star, causing a greater Cape of Good Hope declination than at Cambridge, as is the case. I chose the above two catalogues, as being much esteemed and in approximate years: thus eliminating the uncertain influences of stellar proper motion. Also the stria *Vega* α is very fine, and *Capella* α, though thin and difficult, has been often observed by me with sufficient precision. This does not occur with Fomalhaut, which, rising to a very small altitude above Florence, and only seen for a short time, has been seldom observed by me, causing with its horizontal scintillations unreliable results. Indeed, my observations would lead to a contrary sign of the difference in declination between Cambridge and the Cape of Good Hope than to that which is actually obtained.

The sign for *Sirius* should be +, since the centre of its α is refracted 10″ more than F ⊙; the refraction correction applied to this star's observed place may be smaller than the true one, when the observed declination at Cambridge has not been increased enough, giving a smaller result at the Cape of Good Hope. The difference, *Altair* + 1·32″, also confirms my hypothesis; for, since the centre of its α stria is refracted 20″ more than F ⊙, one may suppose the horizontal refraction to be less than the true one, causing the Cambridge declination to be increased, and the Cape of Good Hope declination diminished; and therefore the former greater than the latter.

My measures are uncertain, more from the difficulty of hitting the exact stria-point than from the micrometer graduation stopping at 6·6″. The quickest way of reconciling the two catalogues would be to make simultaneous observations at both places.

I would further recommend for observations of the stellar striæ a very large lens on Fresnel's principles for lighthouse lenses, as we do not so much require an image of achromatic sharpness as the concentrating of a great quantity of light, and such a lens would, perhaps, enable us to see as many stellar striæ as we find in the solar spectrum.

We further require to know the very small dispersive power in our atmosphere of the stellar light; also the stellar spectra breadth seen when near the horizon by a good telescope; also the maximum brightness point of various spectra. Such a series would be of first-rate value to modern astronomy.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

Friday Evening, March 6.

The lecture was by Professor Wm. Allen Miller, F.R.S., on "*The Most Recent Spectrum Discoveries.*"

The subject of the lecture, the learned Professor said, was perhaps the most extensive and fascinating which presented itself to scientific men. On the present occasion he intended to limit himself to but a few of the interesting discoveries made in this great field of research, and hoped he should not be considered egotistical if he referred to his own experiments. Among the rays emitted by the sun there were three kinds, interesting as endowed with special action—those which conveyed heat, light, and chemical action. With heat he should have but little to do on this occasion; about light he had something to say; but he was now principally concerned with the rays which manifested themselves by producing chemical action. It was well known that transparent substances did not transmit all these rays with equal facility. Glass was only imperfectly transparent to the chemically active rays, which were found in the most refrangible rays of the spectrum, heat rays being in the least refrangible portion, and light occupying the middle place. It had been found that rock crystal was one of the few substances which perfectly transmitted rays, those highly refrangible, which glass absorbed.

The Professor then showed that some kinds of light were without chemical action, the light from a mixed air-gas flame possessing scarcely any, while that from an ordinary gas flame did possess a little. The oxyhydrogen flame, while attended with intense heat, was endowed with very little chemical action. A prepared collodion plate exposed to this light for twenty seconds gave a very faint picture. But when the flame was thrown on lime, although the temperature was lower, the light had sufficient chemical activity to produce a strong picture on a similarly prepared plate, exposed for the same time. In the case of the chemically acting rays the intensity, number, and position of the lines on the spectrum had been found to vary with the source of light. The most remarkable illustration of this was the different spectra produced by the electric spark of an induction coil between poles of different metals and projected upon a photographic plate.

The spectrum produced by the spark from silver poles, for example, was found to be three times the length of the whole of the solar spectrum transmitted by quartz. In order to obtain views of this invisible spectrum it was necessary to transmit the rays through a medium more transparent to chemical rays than glass, which, it had been said, was opaque to the higher rays of this kind, and various experiments had been made to ascertain what substance allowed them to pass most freely. The results were shown in the following table of the—

Photographic Transparency of Solids.

Rock crystal .	74	Sulphate of magnesia	62
Ice . .	74	Borax . . .	62
Fluor spar .	74	Bromide of potassium	48
Topaz . .	65	Thin glass . .	20
Rock salt .	63	Mica . . .	18
Iceland spar .	63	Iodide of potassium .	18
Diamond .	62	Nitrate of potash .	16

The above numbers being founded upon an arbitrary division of the spectrum.

The photographic transparency of liquids differed still more, as would be seen by the following diagram:—

Photographic Transparency of Liquids.

Water . .	74	Wood spirit . .	20
Alcohol . .	63	Acetic acid . .	16
Chloroform .	26	Oil of turpentine .	8
Benzole . .	21	Bisulphide of carbon	6

Various gases were also found to interfere with the transmissibility of these rays, as exhibited in the table of the

Photographic Transparency of Gases.

Hydrogen .	74	Benzole vapour . .	35
Nitrogen .	74	Hydrochloric acid . .	55
Oxygen .	74	Hydrobromic acid .	23
Carbonic acid	74	Hydriodic acid . .	15
Olefiant gas .	66	Sulphurous acid .	14
Marsh gas .	63	Sulphuretted hydrogen	14
Coal gas .	37		

The diamond and rock crystal allow the chemical rays to pass freely, but other substances, in which no difference of transparency can be discovered by the eye, considerably affect the transmission of these rays. Chloride of potassium allowed them to pass less freely; and nitrate of potash, and the nitrates generally, offered still more obstruction. It was the same with fluids, and also with gases, as would be seen by a reference to the diagrams. It was remarkable, too, that solid bodies when dissolved or melted maintained exactly the same power as when in the solid state. The same was the case when they were converted into vapour, which showed that this power was part of the nature of the substance.

The lecturer then described the phenomenon of fluorescence, and showed that the chemical rays of the spectrum corresponded with the rays of fluorescence by taking a photograph in that part of the spectrum which, though otherwise invisible on the screen, lighted up a solution of æsculine. He then showed that all metals give characteristic photographic spectra, some of them bearing a strong family resemblance to each other, as in the cases of iron, cobalt, and nickel, the last metal giving one of the longest spectra observed, and which extended to 190° of the scale. Arsenic, antimony, and tin showed as great differences in the invisible as visible part of the spectrum. The most interesting of the metals to study in this respect was magnesium, which opened a wide field for investigation. There were certain points of resemblance between the spectrum of magnesium and that of the sun, which led to the supposition that this metal existed in the solar atmospheres. The comparison of the spectrum of magnesium with that of the sun led also to some important considerations as to the temperature of the sun. It was known that the higher the temperature the more refrangible were the rays of light emitted by a body. We have no conception of the temperature of the electric spark. The heat of the strongest wind furnace was estimated at 4500° F., and that of the oxyhydrogen jet was supposed not to exceed 15,000° F., yet with neither of these could the same effects be produced as with the electric spark. The lines of the photographic spectrum of magnesium were not seen in photographs of the solar spectrum, and yet there was no doubt that this metal was present in the solar atmosphere. Kirchoff had discovered that solids when heated give a continuous spectrum, but that bodies in the form of gas give rays of definite and limited refrangibility, each substance emitting light of definite property. He had also noticed that light from a luminous mass, by passing through ignited vapour which, *per se*, would give bright lines in the spectrum, became furrowed out in dark bands occupying exactly the same position in the spectrum as the bright lines. Now, ignited magnesium vapour emitted green rays which were absolutely identical with the group of fixed lines *b* in the solar spectrum, and it was therefore certain that magnesium was a constituent of the sun. It was, moreover, probable that the heat of the sun was inferior to that of the electric spark, inasmuch as it was insufficient to bring out the highly refrangible lines observed in photographs of the magnesium spectrum

There were thirteen bodies known on earth which these researches lead us to suppose existed in the solar atmosphere. Nor are they limited merely to the sun. Fraunhöfer had examined the spectra of several stars, and found that although they presented no similarity to that of the sun, nor to each other, yet that some general relationship between them was observable.

Mr. Huggins and the lecturer had recently been investigating this subject, and had obtained very perfect maps of the visible spectra of several stars. They had also obtained a photograph of the spectrum (which was exhibited) of Sirius. This star is 130,000,000,000 of miles distant, and the light which produced the photograph must have left it twenty-one years ago.

A photograph of the spectrum of Capella, which is three times further distant than Sirius, had also been obtained, the light to produce which, the lecturer said, must have left that star when the oldest in the room was a little boy. Professor Miller concluded an eloquent address, of which the above is a mere outline, by remarking how much these wonderful facts enlarged our ideas of the power of the great Author of the universe, whose will "creates, sustains, and animates the whole."

NOTICES OF BOOKS.

Inaugural Address Delivered before the Leeds Chemists' Association. By T. Harvey, Esq., President.

We have read this Address with pleasure, and, for the sake of our many pharmaceutical readers, wish that space permitted us to print it at length. It is a thoughtful, well-reasoned paper, in which the author shows that he is fully aware of the somewhat anomalous social and political or legal position a pharmaceutical chemist now holds, and points out clearly the only way in which, in course of time, he may extricate himself, and assume a proper *status*. This, it is clear, cannot be accomplished at once by Act of Parliament. The chemist must elevate himself, and, where numbers are concerned, this can only be done by association. Noticing a previous meeting at Leeds, the author remarks :—

"Reference was made on that occasion to the combinations that have sprung up among the members of our calling within the last twenty years;—the Pharmaceutical Society, the United Society, and the various local associations; we shall probably all admit that the formation of these societies is a sign of the times—a token that a want has been felt, which individual unassisted effort cannot supply. This need of association arises from the fact that ours is not a mere trade. It might be presumptuous to call it a profession, yet it evidently joins on to a profession. It demands the habits of business of the merchant and tradesman; but it demands also some knowledge of the learned and other languages, and no small amount of scientific training, the more exact and thorough the better. Now, in all professions, *association*, in some shape or other, for the purposes of mutual improvement, is found to be a necessity. The advance of science, especially in its practical applications, is so extraordinarily rapid, that we need all possible aid to enable us to keep pace with it; and it has been found by experience that as solitary study is the most laborious, so associated investigation and mutual instruction are the readiest and most agreeable means of adding to our stores of knowledge."

We quote the next paragraph mainly for the sake of the last sentence. Years ago it was fashionable to say that science was declining in England, but the balance has long since been restored, and, with regard to our own profession, we might easily point out why English Chemists (not pharmaceutical) have not a higher reputation than they have. In practical scientific pharmacy, however, we are quite certain that our country is not surpassed. It is quite true that the Continental pharmaceutist is required to possess himself of a smattering of many sciences, which lead innocent people to suppose him a profoundly educated man; but a slight acquaintance with him in his own country soon convinces the studious observer that for the practical part of his business he is not one whit superior to the better class of English pharmaceutists. Our readers will see that we candidly admit there is a better class of pharmaceutists in England, which, of course, implies another less educated. To the notice of the latter we commend the last sentence of the following paragraph, assuring them that very little effort is necessary to place them quite on a par with their Continental *confrères* :—

"In most countries the professional education required to make a competent pharmaceutist is *compulsory*; the State demands it, and makes more or less provision for its supply. There is this grand difference running between British and continental arrangements in matters educational. The latter are compulsory, while with us all is left to voluntary zeal. Now, while we may believe that a higher style of national and individual character results from our greater independence of action, yet it must be admitted that whatever we gain is purchased at a considerable price. Some of the greatest authorities among our scientific men believe that we are falling behind several of the continental nations in science; and what is of far more consequence to us, that we are losing our grand pre-eminence in manufactures; that, in short, while we are making progress, our continental competitors are improving at a faster rate, and bringing more science and more taste, the result of better educational training, to bear on their several branches of manufacturing industry. However this may be, with reference to our particular calling, we are bound in all propriety to do our best to maintain as high a standard of knowledge by voluntary effort as our French and German brethren are compelled to reach by less spontaneous means."

To local and spontaneous efforts at improvement we recommend our pharmaceutical friends to confine themselves. The time is not yet ripe for any serious change in the circumstances and relations of the trade, as we must call it. Before many years, possibly before long, it is likely that considerable changes may take place in the medical profession, and no thorough medical reform can take place which does not affect the condition of dealers in medicines. In the meantime, we are pleased to see springing up societies like the Nottingham and the Leeds Chemists' Association, and hope the latter will persevere in the course suggested in their President's address.

Les Mondes. Revue Hebdomadaire des Sciences. Paris : E. Giraud.

We have received several copies of a new French contemporary, lately issued under the above title, edited by the Abbé Moigue, the late well-known editor of *Cosmos.* It contains a number of well selected and condensed articles on both pure and applied science, from home and foreign sources, besides several papers from the pen of the learned editor. The proceedings of the French Academy are reported at considerable length, we believe by the Abbé himself. We sincerely wish our new brother all the success the literary and scientific reputation of its accomplished editor will no doubt ensure.

A Manual of Metallurgy. By G. H. Makins, M.R.C.S., F.C.S., &c. F. S. Ellis, Convent Garden.

This is a very valuable work by Mr. Makins, who is particularly well fitted for the task, being one of the principal assayers at the Bank of England, and Lecturer on Metallurgy at the Dental Hospital. As might be readily expected, Mr. Makins gives particular prominence to the metallurgy of the nobler metals; the commoner metallic bodies are, however, not neglected, copious descriptions being given of their chemical and mechanical properties, and the method of extracting them from their ores. Having devoted four chapters to describing the physical and chemical properties of the metals generally, their salts and alloys, the author gives valuable instructions for the construction and use of furnaces and heating apparatus, supplementing them by one of the best treatises on fuel that we have read for some time. The practical information relative to the management of gas, ordinary coal, anthracite, coke, and charcoal, may be read by the most expert chemical manipulator with very great profit. The author then goes on to describe most fully the noble metals and the ordinary economic metals, finishing with details respecting the manufacture of sodium and aluminium. The book ends with a description of the principles and practice of electro-metallurgy, giving the latest possible information on the subject. As might be expected, the chapters on the extraction and assaying of gold and silver are exhaustive, and stamp the work with a standard character. To the gold and silver worker, Mr. Makins' book is a desideratum, the only works available being rendered long since obsolete by the introduction of new processes. Mr. Makins' long practice in the art of lecturing has had a very perceptible influence on his style, which has resulted in his presenting the public, in a small volume of 450 pages, with an amount of information which a mere bookmaker would spread out into several volumes. Mr. Makins' descriptions of processes and apparatus are exceedingly clear and concise, and the engravings distributed through the book, to the number of fifty, are very beautifully executed. We most cordially recommend this work, not only to the attentive study of those amongst our readers whose business or pleasure connects them with metallurgical subjects, but to our younger friends, as a model of clear, concise, and unpedantic composition.

NOTICES OF PATENTS.

2920. *Treatment of Zinc Ores.* J. H. Johnson, Lincoln's Inn Fields, London. A communication. Dated November 20, 1861.

As a preliminary step in the reduction of zinc ores, the inventor subjects the mineral to a roasting operation in a peculiarly constructed reverberatory furnace, and whilst still hot transfers the metallic oxide thus formed to another (blast) furnace, wherein the metal is reduced according to the usual processes. The reverberatory furnace is provided with beds or floors at different elevations, and the crude material being introduced at the top is subjected at first to a moderate degree of heat, and after a sufficient interval is removed by raking to a position at a lower level, where it becomes exposed to a more intense heat, and where consequently the roasting operation is readily completed.

3002. *Treatment of Ores for the Manufacture of Sulphuric Acid.* P. Spence, Newton Heath, near Manchester. Dated November 28, 1861.

This invention relates to the extended application of a method of treating sulphurous ores, for which a patent (No. 1695, July 3, 1861,*) has already been granted to the present patentee. In the specification referred to the sulphuretted ores of copper were alone proposed to be treated; the inventor now finds, however, that the same principle may be applied successfully in the case of iron pyrites, galena, and blende, and to other metallic ores containing sulphur, but which do not contain sufficient copper to allow of their being classed with copper ores. The present invention consists in submitting such minerals to the action of heat in separate charges, which are successively moved forward from one end of the furnace to the other, whilst a current of air is passing over them. With regard to a condition formerly specified, the inventor prefers to allow the current of air to flow in the same direction as the course of the material under treatment in the furnace, but this arrangement may be varied according to the nature of the ore, or the special requirements of the case. Another part of the invention relates to the employment of the furnace above described for smelting or calcining ores, with or without fluxes, and from which it is desired to extract the metals; the power of conducting the roasting process very gradually and at a moderate degree of heat constitutes the great advantage of this invention.

3076 *Manufacture of Sulphate of Copper and other Salts of the Same Metal.* B. W. Gerland, Newton-le-Willows, Lancashire. Dated December 7, 1861.

For the production of the sulphate of copper, the patentee dissolves malachite directly in dilute sulphuric acid and crystallises the solution. A second claim refers to the use of powdered malachite or other form of carbonate or oxide of copper, for the purpose of precipitating iron and other metallic impurities from cupreous solutions.

There are occasionally large supplies of red oxide and blue and green carbonate of copper in the market; whenever they can be procured, it appears far more satisfactory to prepare the soluble salts of copper from these ores rather than reduce them at once to metal in the smelting-furnace.

2994. *Soap.* M. Henry, Fleet Street, London. A communication. Dated November 27, 1861.

In the manufacture of soap according to the usual process certain constituents of the oil or fat, particularly the glycerin, will not combine with the alkali, cannot be saponified, and therefore cause considerable loss of material. To avoid this waste the patentee treats the fat with sulphuric acid, whereby the glycerin becomes converted into sulpho-glyceric acid, and is then capable of uniting with alkalies, whilst the fatty acids suffer no change, and are equally available for the manufacture of soap.

3073. *Candles.* H. W. Bristow, Jermyn Street, London. A communication. Dated December 7, 1861. (Not proceeded with.)

This proposal relates to the use of Japan wax in combination with the fatty acids obtained from tallow as a material suitable for the manufacture of candles.

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* *Vide* Chemical News, vol. vi., p. 68.

3043. *Manufacture of Potash and Salts of Potash.* W. H. Balmain, St. Helens, Lancashire. Dated December 4, 1861.

This invention consists in the economisation of the spent bark, or other woody fibre from which tannin or colouring matter has been extracted, as a source of potash and its salts. The mode of carrying out this object consists in incinerating the spent bark or wood, or the charcoal prepared therefrom, and then lixiviating the ashes so as to obtain a solution of potash, from which either the pure alkali or any of its salts may be produced by known processes. The residuum containing the impure potash may also be applied to the uses for which salts of potash are at the present time employed, or may serve for the direct production of potash salts by treatment with suitable acids.

The recommendation which forms the subject of this patent might be advantageously carried out in connection with the process of Mr. W. Larberg,† who claims the manufacture of chemical products, especially charcoal, tar and other oily matters, pyroligneous acid, and gas, by submitting to destructive distillation the spent bark of the tan-yards. By operating upon the residual charcoal so obtained the series of products is complete, and at no time could this scheme be more successful commercially than at the present hour, when potash commands an unusually high price in the market.

CORRESPONDENCE.

Nottingham Chemists' Association.

To the Editor of the Chemical News.

Sir,—I observed in your impression of March 7, a paragraph headed "Midland Society of Chemists and Druggists." As that is a mistake, and one likely to mislead, I shall be glad if you will insert this note in your next.

The chemists of this town have instituted a society upon the basis correctly given in your last, to be called the Nottingham Chemists' Association, having for its object the advancement of chemical and pharmaceutical science, the better education of the assistants and apprentices, and the formation of a library of scientific works.

This movement is purely of a scientific and educational character, carefully avoiding that rock upon which most societies of this nature get broken, namely, the introduction of trade questions and regulations.—I am, &c.

J. H. Atherton, Hon. Sec.

Long Row, Nottingham.

Sulphide of Zinc.

To the Editor of the Chemical News.

Sir,—Can any of your readers inform me by what process Black Jack (sulphide of zinc) may be smelted beneficially?

I am, &c. J. C.

Swaledale.

Deaths from Nitric Acid.

To the Editor of the Chemical News.

Sir,—I read with great regret the account in your Journal of the death of Mr. Stewart, one of the Masters, and also of the janitor, of the Edinburgh Institution, from the effects of inhaling the fumes of nitric acid.

It certainly is a very singular occurrence, and I cannot altogether believe that it was from the fumes of the nitric acid alone that the unfortunate parties met their deaths. There must have been some poisonous article on the floor of the laboratory, or rather mixed with the dirt upon the floor.

I have myself inhaled times without number the fumes of the same acid without feeling the slightest unpleasant sensation, beyond at times a slight stuffing at the chest, but this has entirely disappeared on my taking a draught of cold water. I doubt very much if the fumes which Mr. Stewart and the janitor would inhale were sufficient of themselves to cause death. Yours is the only report I have been able to find of the unfortunate case, and it would no doubt be interesting to most of your readers to have further particulars, especially those who pay any attention to toxicology. I think any one having had much experience in the manufacture of various acids will agree with me as regards the case in question.

I am, &c. J. H. Swindells, Jun.

Meadows Bridge, Wigan.

Volumetric Analysis of Water.

To the Editor of the Chemical News.

Sir,—The very decisive manner in which Dr. Parkes disposes of the remarks I made, at a recent meeting of the Chemical Society, on the method of analysing water proposed by Mr. Nicholson, strikes me as remarkable, especially as it is unaccompanied by any evidence in opposition to the opinion I expressed as the result of experiment.

"Imperfection in operating" is a misfortune to which most chemists are more or less liable, and I do not pretend to claim immunity from it; but I may at least express my surprise that it should be regarded as the necessary explanation of the results I have obtained in testing Mr. Nicholson's method of estimating sulphates in water.

I have precisely stated the case, in which I found that it was not merely difficult to estimate sulphates by Mr. Nicholson's method, but that it is impossible to do so. It rests with the supporters of that method to show that the results stated are incorrect.

Upon those results I base my opinion that the method in question is fallacious. The circumstance that Dr. Parkes believes it to have succeeded well in his hands, I can only accept as further evidence that the method is also delusive.

Since, in reference to the estimation of the carbonic acid present in water as the solvent of earthy carbonates, Dr. Parkes states that he does not understand the force of my criticism, I may explain that I believe the hardness communicated to water by a given quantity of earthy base to be the same whether that base exists in the state of carbonate, bicarbonate, or sulphate. I consider this to be the necessary consequence of the data on which Dr. Clark based the application of the soap test; and I may add, that it was the remark of one of our leading chemists, with whom I conversed on this subject some time since, "that if Mr. Nicholson's method were correct, all the results which have been obtained by means of the soap test must be wrong."

It is unnecessary to enter at length into the hypothetical possibility which Dr. Parkes puts forward as to the quantity of carbonic acid requisite for the solution of carbonate of lime, since that assumed possibility is inconsistent with probability and with experience. All the analyses of water in which this carbonic acid has been estimated indicate that it is present at least in such proportion as to form bicarbonates of the earthy bases, allowance being made for the solubility of the carbonates in water. In illustration of this fact, I may refer to the report by Messrs. Graham, Miller, and Hofmann on the water supplied to London.—I am, &c. B. W. Paul.

8, Gray's Inn Square.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Tungstic Acid.—M. Debray announces (*Comptes-Rendus*, t. lxiv., p. 278) that tungstic acid is easily obtained by passing a current of hydrochloric acid gas over a mixture of tungstate of soda and carbonate of soda placed in platinum boat, and heated to redness in a porcelain tube.

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† Chemical News, vol. iv., p. 288.

The tungstic acid crystallizes in the chloride of sodium formed. By using a white heat, and passing a rapid current of the gas, the tungstic acid is carried out and deposited on the sides of the tube, but the crystals are not then well defined. The crystals obtained in the former way are rectangular prisms of a deep olive green colour.

II. ORGANIC CHEMISTRY.

Transformation of Cinchonine into a Base Isomeric with Quinine.—Strecker has accomplished this transformation (*Annalen der Chem. und Pharm.*, bd. cxxiii., s. 379) by first preparing bromated cinchonine by acting moist on hydrochlorate of the alkaloid with bromine, and removing the excess of the last by a small quantity of alcohol. He then dissolved the residue in boiling alcohol, added ammonia, and concentrated the solution. In this way he obtained crystals of dibromated cinchonine, $Ȼ_{20}H_{22}Br_2N_2Ɵ$. By boiling the alcoholic solution of this body with oxide of silver, bromide of silver was formed. On boiling a similar solution with an alcoholic solution of potash, there was a separation of bromide of potassium. Carbonic acid was then passed through the alkaline liquor, which was then evaporated to dryness, and the soluble salts removed by cold water. The residue was very slightly soluble in ether, but dissolved in alcohol, and crystallized out in the form of colourless plates, which had the composition of quinine—$Ȼ_{20}H_{24}N_2Ɵ_2$. It is not, however, identical, though isomeric, with quinine. It is soluble in acids, but the solutions are not coloured given with chlorine and ammonia, and are not fluorescent.

Composition of Manna.—M. Berthelot (*Annales de Chemie et de Physique*, t. lxvii., p. 82) has analysed a sample of the manna from Sinai. It was the produce of the *Tamarix mannifera*, and was composed, when dried and after the separation of vegetable *débris*, of the following constituents:—

Cane sugar	55
Levulose and glucose	25
Dextrine and analogous products . .	20
	100

The author also analysed a specimen of Kurdistan manna, which, it seems, falls from all sorts of trees. It has the following composition:—

Cane sugar	61
Levulose and glucose . . .	16.5
Dextrine and analogous products .	22.5
	100·0

M. Berthelot says that the composition of the above bodies is almost identical with that of honey, and points out that the Sinai manna alone would not suffice for food, in consequence of the absence of nitrogenous matter. The Kurds at the present day eat this manna with meat. The Israelites of old, too, used animal food with manna.

MISCELLANEOUS.

ROYAL POLYTECHNIC INSTITUTION.

Mr. Pepper, the enterprising manager of the Royal Polytechnic Institution, always anxious for the scientific instruction of the public on the questions of the day, gave them a short time ago a specimen of his pleasant method of treating such subjects in the form of a lecture on "Burning to Death, and Saving from Burning." Mr. Pepper illustrated his lecture by a series of brilliant experiments, showing the cause and effect of fire, and the means used for its prevention and extinction. He demonstrated most conclusively that the most inflammable tissues that were ever worn in a ball-room may be easily rendered uninflammable by enduing them with starch prepared with tungstate of soda and other chemical compounds. The lecture was concluded by two very startling experiments. A large figure dressed in combustible material was ignited at the bottom, the flames spreading with fearful rapidity, but Mr. Pepper showed that by the prompt application of wet blankets the blazing effigy might be extinguished. Having thus given his hearers a very useful practical lesson in the art of rescuing one's fellow-creatures from a horrible death by burning, he introduced a young lady attired in a voluminous muslin dress distended by a crinoline of the most fashionable dimensions. In front of the lecture table was an iron platform, along which ran two gutters about three feet apart containing naphtha or some other inflammable liquid. The naphtha being ignited the young lady, the whole of whose garments were prepared with the "Patent Incombustible Starch" passed backwards and forwards between the two rows of blazing liquid, sweeping her skirts through the flames with no other effect than the blackening of those portions of her habiliments which came in contact with the flames, and, we presume, a slight amount of roasting of the ankles.

Mr. Lionel Brough has succeeded Mr. Buckland as the expositor of the fairy extravaganza of Cinderella. The reception he met with from a crowded audience indicated that he will before long become a standing favourite with the frequenters of the Polytechnic Institution.

SCIENTIFIC SOCIETIES.

MEETINGS FOR THE ENSUING WEEK.

March 23. *Monday.*

Geographical—15, Whitehall Place. 8.30 p.m.
British Architects—9, Conduit Street. 8 p.m.
Medical—32a, George Street, Hanover Square. 8.30 p.m.

24. *Tuesday.*

Royal Institution—Albemarle Street. 3 p.m. Professor Marshall, "*On Animal Mechanics.*"
Medical and Chirurgical—53, Berners Street. 8.30 p.m.
Civil Engineers—25, Great George Street, Westminster. 8 p.m.
Zoological—11, Hanover Square. 9 p.m.

25. *Wednesday.*

Society of Arts—John Street, Adelphi. 8 p.m. C. R. Markham, "*On the Present Sources of the Supply of Quinine, with special reference to the Introduction of the Chinchona Plant into India and other Countries.*"
Royal Society of Literature—4, St. Martin's Place. 4.30 p.m.
Archæological Association—32, Sackville Street. 8.30 p.m.

26. *Thursday.*

Royal Institution—Albemarle Street. 3 p.m. Dr. Frankland, "*On Chemical Affinity.*"
Royal—Burlington House. 8.30 p.m.
Antiquaries—Somerset House. 8.30 p.m.
Philosophical Club—6 p.m.
Artists and Amateurs—Willis's Rooms, St. James's. 8 p.m.

27. *Friday.*

Royal Institution—Albemarle Street. 8 p.m. Mr. Crookes, "*On Thallium.*"

28. *Saturday.*

Royal Institution—Albemarle Street. 3 p.m. Prof. Max Müller, "*On Language.*"
Royal Botanic—Inner Circle, Regent's Park. 3.45 p.m.

ANSWERS TO CORRESPONDENTS.

⁂ All *Editorial Communications* are to be addressed to the Editor, and *Advertisements* and *Business Communications* to the Publisher, at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

J. B.—Received too late.

THE CHEMICAL NEWS.

Vol. VII. No. 173.—March 28, 1863.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Analytical Notes on Thallium.—Separation of Thallium from Cadmium, by William Crookes.

These two metals are frequently associated together in nature. Different specimens of metallic cadmium which I have examined contain it, and, as might have been expected, thallium is also frequently met with in commercial salts of cadmium, these being generally prepared from the metal. Sulphide of cadmium, owing to its brilliant yellow colour and permanence, is largely used by artists as a pigment. As met with in commerce, it varies greatly in colour, some specimens being of a much deeper orange than others. I have found by spectrum analysis, as well as by the wet way, that thallium is generally present in the dark-coloured varieties; and it is therefore most probable that the variations in colour of sulphide of cadmium are due to traces of thallium.

To detect thallium analytically, when associated with cadmium, I proceed as follows:—To the acid solution add a few drops of chromate or bi-chromate of potash, and then supersaturate with ammonia. Heat to the boiling point, when the insoluble yellow chromate of thallium will be precipitated. This may be collected on a filter, and the presence of thallium verified by spectrum analysis.

This test is tolerably delicate; by its means I have detected one part of thallium in the presence of 800 parts of cadmium. It is not, however, equal in delicacy to some of the analytical processes I have already described.

On the Properties and the Preparation of Rubidium, by M. Bunsen.

The first material used in these researches was extracted from lepidolithe residuums from Dr. Struve's lithia manufactory, Leipzig. In separating carbonate of cæsium from the corresponding salt of rubidium, advantage has been taken of the great solubility of neutral deliquescent tartrate of cæsium, compared with slightly soluble bitartrate of rubidium.

The reduction of carbonate of rubidium by carbon is more difficult than the reduction of sodium, and easier than that of potassium.

The mixture, treated by heat in a potassium furnace, was composed of

Bitartrate of rubidium	89·55
Neutral tartrate of lime	8·46
Turpentine soot	1·99
	100·00

The metal was collected in a receiver containing naphtha: 75 grammes of bitartrate gave 5 grammes of metal.

The metal melts at 38°·5 C., its density being equal to 1·516.

Sodium melts at 95°·6, potassium at 62°·5, and lithium at 180°, according to the new determinations made in the Heidelberg laboratory.

Rubidium burns on water, revolving about like potassium.

The reduction of cæsium has not yet been attempted for want of material to operate upon, M. Bunsen having been able to extract but a few grammes of the salts of this metal from 15,000 litres of the water of the Murquelle at Baden.

In its other properties rubidium resembles potassium. —*Comptes-Rendus.*

Soda in Coal, by E. S. Wayne.

I have not seen the presence of soda in coal mentioned in any analysis of it that has come under my notice, and presume the cause of its having been overlooked, is the minute quantity in it, only to be detected by operating upon much larger quantities than are generally used in analysis.

My detection of it in coal was unlooked for and accidental. Having frequently observed a pale ochreous deposit upon the casting stills I was using for the distillation of coal oil, curiosity at last induced me to make an analysis of it; and I found it to be composed of alumina, silica, oxide of iron, and sulphate of soda. Now, under this still had been burnt thousands of baskets of coke from cannel coal, from which the crude coal oil had been extracted, and which was unquestionably the source of both the soda and sulphuric acid in the sulphate of soda found in the deposit. In what state the soda existed in the original coal, or in the coke, I cannot say. Whatever it was, it must have been reduced by the carbon of the coke during its combustion to sodium, and its vapour deposited upon the bottom of the still, and being exposed to the sulphurous products of combustion, combining with them, and finally, through the agency of heat and air, was oxidised to a sulphate as found.

The presence of soda in coal is an interesting fact, in several respects, particularly so to the palæontologist and geologist, as it tends to point out the habitat of the vegetation, whose remains it consists of.

PHARMACY, TOXICOLOGY, &c.

On Albuminate of Iron and Soda as a Therapeutic Agent, by Angellico Fabbri.

Simple contact, at the ordinary temperature of the atmosphere, of white of egg with a salt of iron and soda, is capable of instantly producing a soluble albuminate of iron and soda, or an albuminferrate of the alkaline base. The chemical combination of this compound is such that it is not altered by the yellow ferrocyanide of potassium, the most delicate test of the salts of iron, unless a few drops of acid—as, for example, the hydrochloric—be previously added to the soluble albuminate; thus proving that this decomposition cannot be effected by the agency of the alkalies, but only by some acids, since the potassium of the cyanide is not able to displace the iron, as occurs with the other ferruginous preparations. Considering that we find in the blood albumen, soda in excess, and iron, and having shown how these three bodies, by simple direct contact, form a soluble salt, the chemical combination of which is so powerful that it is not destroyed by the most delicate reagent, may we not fairly infer that the iron exists in the blood as an albuminate of iron and soda; and would it not therefore be reasonable to administer iron in the various diseases in which it is prescribed, principally in reference to the state of the sanguineous system, in the form of albuminate, as that in which Nature itself has placed it within our organism—one of the products, so to speak, on which our life depends? When I read in works of chemistry that the yellow ferrocyanide of potassium is not capable of demonstrating the presence of iron in the blood until a stream of chlorine has first been passed through the latter to destroy its colouring matter, I am confirmed in the opinion that the iron exists in that fluid as an albuminate of iron and soda, because this salt, requiring the addition of an acid to render it capable of detection by the cyanide, is supplied with it by the chlorine, which, in destroying the organic colouring matter, becomes converted into hydrochloric acid by uniting with their hydrogen. Physicians have been long puzzled, and are still at a loss, how to administer iron, a most valuable remedy, in the manner most suitable to the internal organism; hence the great number of preparations of this metal. Some object to its saline combination with mineral acid, on the ground that these are inorganic, and they prefer giving it in the metallic or oxidised state, leaving the acids of the stomach to form with it compounds which may be carried into the circulation. Others, unwilling to run the risk of having the greater part of the iron—little or not at all acted upon—expelled with the fæces, prescribe it in the saline state, but combined with organic vegetable acids, hence we have the malate, tannate, citrate, &c., of iron. Others, still more scrupulous, wish to have it united to acids of an animal nature, and prefer the lactate, &c.; and I, going still farther, would recommend its employment in the state of albuminate of iron and soda, requesting physicians to take into consideration what I have advanced, and to ascertain if practice will in this instance corroborate theory.

In preparing the albuminate of iron and soda, I employed the following process:—Take 112 grs. of caustic soda, and 104 of sulphate of iron. Having dissolved both in a sufficient quantity of distilled water, let the solutions be poured on the whites of four eggs previously beaten up; let all now be shaken together and poured upon a filter to separate the hydrated oxide of iron which has precipitated, since all the iron is not in this case converted into albuminate. To the filtered liquid, which now contains, in addition to the albuminate, sulphate of soda, formed by the decomposition of sulphate of iron by the soda present in excess, lime-water is to be added, to decompose the sulphate of soda, by which an insoluble sulphate of lime is precipitated. To separate the latter, the mixture is to be again filtered; and as the filtered fluid will contain an excess of lime, it is to be subjected to the action of a stream of carbonic acid, care being taken to avoid using an excess of the latter, and again filter to get rid of the insoluble carbonate of lime thus formed. The filtered fluid is now to be allowed to evaporate in a wide, shallow vessel, and with the aid of the heat of a stove, until it is reduced to a pint. A clear orange yellow, slightly saltish, chalybeate solution is thus obtained, which, as already mentioned, does not give a precipitate with ferrocyanide of potassium without the addition of an acid. Each ounce of this liquid contains, approximately, four grains of the albuminate, *plus* an excess of albumen and soda, as may be seen by referring to the process employed; the solution consequently has a slightly alkaline reaction. It is desirable that the soda should thus be present in excess, in order that the compound shall be conformable to the state in which it exists in the blood, where we find the albumen rendered alkaline by an excess of soda. I have neglected the excess of the albumen, which, though united to the liquid, perhaps with some other soluble salts of the albumen of the egg (chlorides), I do not consider to form a part of the saline compound, which may be obtained in radiated crystals by evaporating the solution to dryness. —*Journal of Rational Medicine.*

PHYSICAL SCIENCE.

Note on the Spectrum of Thallium, by Professor William Allen Miller, *M.D., LL.D., Treasurer and V.P.R.S.*

My friend Mr. Crookes, the discoverer of the new metal thallium,* having kindly put into my hands a small quantity of the metal, which he believes to be chemically pure, I have been enabled to make some experiments upon its spectrum, the results of which may not be without interest to the members of the Royal Society.

Thallium, as is well known, when examined in the usual way by the spectroscope, yields a spectrum of remarkable simplicity, furnishing a single intense green line, the occurrence of which, as is familiar to chemists, led Mr. Crookes to the discovery of the metal, and suggested to him the name by which it is known. In order to try the effect of a progressively increasing temperature upon the spectrum furnished by the metal and its compounds, the following experiments were made:—

Portions of metallic thallium, as well as of an alloy formed by fusing a bead of thallium upon the end of a platinum wire, and portions of the sulphate of the metal were introduced successively, first, into the flame of burning hydrogen, and then into the oxyhydrogen jet, and were in each case viewed by the spectroscope. As the temperature increased in intensity, the brilliancy of the thallium green line increased also, but no new lines made their appearance.

Two pieces of stout thallium wire were then arranged as electrodes to the secondary wire of an induction coil. A continuous torrent of sparks was maintained without melting the wires, or producing very rapid oxidation, or volatilisation of the metal; the light, however, was

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* Note by Dr. Miller.—It has been made the subject of question abroad, whether Mr. Crookes or M. Lamy was the first to recognise the metallic nature of thallium, and thus to dispute the claim of Mr. Crookes to the full credit due to him for his investigation (with only about twenty grains of the element) of its leading characters where no previous clue existed to guide him. It may be sufficient to state in answer to this suggestion, that Mr. Crookes had exhibited it at the International Exhibition, and marked as *metallic* his scanty store, though in the form of a precipitate, in the beginning of May, unquestionably before M. Lamy had published anything relating to thallium.

much whiter than its ordinary monochromatic character would have led us to expect. Mr. Crookes, who was with me during the experiments, projected the image of the points by means of a lens upon a distant white screen, when it was at once obvious that the extremities of the spark were of a fine green colour, whilst the flickering luminous arc, which filled up the interval, due chiefly to ignited air, was much whiter.

On viewing the sparks from the induction coil by the spectroscope, several new lines, independently of well-marked air-lines, made their appearance. These lines were distinguished from air-lines by the peculiar character which distinguishes most metallic lines, viz., the much greater intensity of their extremities than of their central portions. Besides the usual intense line in the green, five others were particularly observable; first, a very faint one in the orange; next, two of nearly equal intensity in the green, more refrangible than Tlα, with a third much fainter, these three lines in the green being nearly equidistant; whilst fifth in the blue was a bright well-defined line: all these were strong at each extremity, and evanescent in the central portions.

The induction spark of thallium was then observed when produced in a current of hydrogen gas. The air-lines disappeared, the peculiar lines of hydrogen were very manifest, particularly the line in the red and one of the lines in the blue; whilst the new thallium lines were preserved, with the exception of the feeblest, though all were reduced in intensity.

Finally, a photographic impression of the thallium spectrum upon collodion was obtained by the method which I have described in a paper communicated to the Royal Society in June last. An impression extending to about division 154 of the scale then adopted was obtained. This spectrum contains several very characteristic groups of lines; it recalls the features of the spectra of cadmium and zinc, and less strongly that of lead.

Measuring by the scale already adopted in my former paper, it is found that there are two strong groups of lines at about 103 and 106. At 116, 121, and 126 are three groups—the first two less intense than the third, which is of about the same strength as the earliest two. Several feebler pairs of dots follow, and the spectrum terminates rather abruptly with four nearly equidistant groups, commencing respectively at 136, 141, 145, and 151. The first of these groups is very strongly marked, the others are fainter, but of nearly equal intensity.

The remarkable way in which a spectrum at low temperatures so simple becomes increased in complexity, both in the visible and in the extra-visible portions, is of high interest considered in relation to the physical cause of these phenomena; and it is not without interest in a chemical sense, from its bearing upon the view supported by Dumas, that thallium belongs to the alkaline group. Potassium and sodium exhibit no new lines in the induction spark, merely a diffuse light filling up the air-lines, and lithium but a single strong group at about 124. This physical character, added to the more purely chemical ones of the insolubility of the sulphide, the chromate, the iodide, the sparing solubility of the chloride, the phosphate, the oxalate, the ferrocyanide, the occurrence of a powerfully basic oxide, and of a higher feebly acid oxide, may therefore assist in showing the resemblance of thallium to silver or to lead, which latter metal in density, colour, softness, and external appearance it so closely simulates.

It would be easy to point out other particulars in which the properties of thallium are in strong contrast with those of the alkali metals. The chemical energy of these metals, lithium, sodium, potassium, rubidium, and cæsium, increases in the order mentioned, which is that of their equivalents. Thallium, with a higher equivalent than any of these, shows a greatly diminished chemical activity. The metal is readily reduced by zinc from its solutions. Its oxide, instead of being like that of all the alkalies, excessively deliquescent, is permanent in air, and forms a closely adhering coat like that which is produced upon the surface of zinc or lead, protecting the metal beneath from further change.

In many points the chemical reactions of thallium resemble those of silver, to which metal it is also further approximated by the circumstance that the atomic heat of the metal, like that of silver, is double that of the series to which lead belongs. Although therefore in other physical properties thallium differs greatly from silver, it seems to be more closely allied to that metal than to any other.—*Proceedings of the Royal Society, January 15, 1863.*

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*On the Long Spectrum of Electric Light,** *by* G. G. Stokes, *M.A., D.C.L., Sec. R.S., Lucasian Professor of Mathematics in the University of Cambridge.*

Introduction.

The experimental researches described in a former paper† led me indirectly to the conclusion that the electric spark, whether obtained directly from the prime conductor of an ordinary electrifying machine, or from the discharge of a Leyden jar, emits rays of very high refrangibility, surpassing in this respect any that reach us from the sun—and that these rays pass freely through quartz, while glass absorbs them, as it does also the most refrangible of the solar rays. I was induced in consequence to procure prisms and a lens of quartz, which were applied in the first instance to the examination of the solar spectrum, and which immediately revealed the existence of an invisible region extending as far beyond that previously known as the latter extends beyond the visible spectrum, and exhibiting a continuation of Fraunhofer's lines.‡ A map of the new lines was exhibited at an evening lecture delivered before the British Association at their meeting in Belfast in the autumn of the same year; and I then stated that I conceived we had obtained evidence that the limit of the solar spectrum in the more refrangible direction had been reached. In fact, the very same arrangement which revealed, by means of fluorescence, the existence of what were evidently rays of higher refrangibility coming from the electric spark failed to show anything of the kind when applied to the solar spectrum. At least, the only link in the chain of evidence which remained to be supplied by direct experiment related to the reflecting power, for rays of high refrangibility, of the metallic speculum of the heliostat which was employed to reflect the sun's rays into a convenient direction; and this was shortly afterwards tested by direct experiment on rays from an electric discharge separated by prismatic refraction.

In making preparations for a lecture on the subject delivered at the Royal Institution in February, 1853, in which I had the benefit of the kind assistance of Mr. Faraday, recourse was naturally had to electric light, on account of the extraordinary richness which it had been

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* Abstract from the *Proceedings of the Royal Society.*

† "On the Change of Refrangibility of Light," *Phil. Trans.* fo 1852, p. 463.

‡ Ibid., p. 559.

found to possess in rays of high refrangibility. Although fully prepared to expect rays of much higher refrangibility than were found in the solar spectrum, I was perfectly astonished, on subjecting a powerful discharge from a Leyden jar to prismatic analysis with quartz apparatus, to find a spectrum extending no less than six or eight times the length of the visible spectrum, and could not help at first suspecting that it was a mistake arising from the reflexion of stray light. A similarly extensive spectrum was obtained from the voltaic arc, and this was sufficiently bright to be exhibited to the audience, the arc passing between copper electrodes, and the pure spectrum formed by quartz apparatus being received on a piece of uranium glass cut for the purpose. The spectrum thus formed was found to consist entirely of bright lines,§ whereas the spectrum of the discharge of a Leyden jar had appeared (perhaps from not having been truly in focus) to be continuous, or at least not wholly discontinuous.

The mode of absorption of light by coloured solutions, as observed by the prism, affords in many cases most valuable characters of particular substances, which, strange to say, though so easily observed, have till very lately been almost wholly neglected by chemists. Having obtained the long spectrum above mentioned, I could not fail to be interested with the manner in which substances, especially pure but otherwise imperfectly known organic substances, might behave as to their absorption of the rays of high refrangibility. But the difficulties attending the habitual use of a nitric acid battery of thirty or forty cells deterred me from entering on this investigation, and I determined to confine myself to the solar spectrum.

On account of some inconvenience attending the tarnishing of the speculum of my heliostat, I was induced to order a quartz plate, intended to be either silvered or coated with the usual amalgam of tin. On trying on a small scale the reflecting power of such plates with respect to the invisible rays, which may be done by means of fluorescence almost as easily as if those rays were visible,‖ I noticed a remarkable falling off in the reflecting power of the silvered plate for the most refrangible of the solar rays, which I readily found was due to a peculiarity of the metal silver. This metal is highly reflective for the invisible as it is for the visible rays up to about the fixed line S,¶ when its reflecting power falls off with remarkable rapidity, and for the more refrangible rays of the solar spectrum is comparable with that of a vitreous substance rather than with that of a metal. Steel, gold, tin, &c., showed nothing of the kind, but copiously reflected the invisible rays.

A few years ago, as Dr. Robinson was showing me some experiments with the induction coil, it seemed worth while to try whether the spark obtained when a Leyden jar has its coatings connected with the secondary terminals might not be sufficiently strong to exhibit by projection the long spectrum shown by electric light. On projecting a spectrum formed by a prism and lens of quartz on a piece of uranium glass, the long spectrum was in fact exhibited. It was not, indeed, so bright as when formed by means of a powerful voltaic battery, but, nevertheless, was quite bright enough to work by. It was discontinuous, consisting of bright lines. On changing the metals between which the spark passed, we found that the lines were changed, which showed clearly that they were due to the particular metals.

A wide field of research was thus thrown open to any one taking the very moderate trouble attending the use of an induction coil. It remained to study the lines given by different metals and gases, and the absorbing action of various substances with respect to the invisible rays of different refrangibilities.

Various observations were made from time to time in this subject. As regards the metallic lines, it is perfectly easy to view them at pleasure; but to obtain faithful delineations of them is another matter. Even an accomplished artist would find difficulty in obtaining by mere eye-sketching a faithful representation of an object which requires to be seen in the dark. I tried different methods without being able to satisfy myself as to the accuracy of the drawings which could be thus obtained, and frequently thought of resorting to photography.

Meanwhile the mode of absorption of the rays of high refrangibility by a good number of substances was observed. Nothing is easier, to a person provided with a cell with parallel faces of quartz, than to observe by means of fluorescence the mode of absorption of these rays by a given solution; but to draw safe conclusions as to the optical character in this respect of the substance deemed to be in solution is not so easy as it might appear; for the rays of high refrangibility are liable to be absorbed by an exceedingly small amount of an impurity which may chance to be present without the observer's knowledge. Thus I found that about a quarter of a square inch of clean filtering-paper sufficiently contaminated the water contained in a small cell to interfere sensibly with its transparency. Should the solution be transparent there would be no difficulty, for the effect of an impurity would not be to render transparent a solution which otherwise would be opaque. Should it, on the other hand, absorb the invisible rays, or some of them, with great energy, or in a peculiar manner, we might again conclude that we had obtained the true character of the substance deemed to be observed. The most remarkable example of this kind which I met with among inorganic colourless solutions was in the case of nitric acid and its salts, such as nitrate of potash, soda, ammonia, baryta, which absorb the rays of high refrangibility with great energy and in a peculiar manner, exhibiting a maximum of opacity followed by a maximum of transparency, beyond which the absorption becomes still more energetic than before. But if the solution should be found to absorb the rays of high refrangibility with only moderate energy, it would be left doubtful whether the observed absorption might not be due to some impurity; and I did not see how this doubt could be solved otherwise than by a laborious system of re-crystallisations.

After having obtained these results, I found by conversation with my friend Dr. Miller that he also had been engaged at the same subject, working by photography, and had prepared a number of photographs of metallic spectra, and studied by the same means the absorption of the rays of high refrangibility by a great variety of substances, chiefly inorganic acids, bases, and salts, and the commoner organic bodies. Although a large part of the task which I had proposed to myself has thus been accomplished in another way, there are many results which I have met with which are not

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§ *Proceedings of the Royal Institution*, vol. i., p. 264.

‖ *Philosophical Transactions* for 1852, p. 537.

¶ According to the notation employed in the map published in the *Philosophical Transactions* for 1859, plate xlvii. In this plate the group S should have been represented as three lines, of which the middle (specially named S) divides the interval between the first and third in the proportion of three to two nearly, the spaces between the lines being a little darkened by shading.

likely to have been obtained by one working by photography, and I have therefore thought it well to draw up a paper embodying these results, and thus forming, as it were, a supplement to the paper by Dr. Miller.

Preparation of a Screen by Means of a Salt of Uranium.—Few substances are more powerfully fluorescent than several of the salts of sesquioxide of uranium; and a piece of glass coloured by uranium and polished along at least two planes at right angles to each other is exceedingly convenient, from its powerful fluorescence and its permanence, for a screen on which to receive a spectrum. Nevertheless, such a screen, which must be viewed in particular directions in order to get the strongest effect, is in many cases less convenient than a screen would be which was prepared by means of a highly fluorescent powder treated like a water colour, which could be viewed in all directions indifferently. This is especially the case in taking measures by a method which will be mentioned presently. Besides, I find an excellent piece of such glass defective in fluorescent power as regards the extreme lines shown by aluminium; and some specimens are defective to a much greater extent, which is doubtless due to impurities. Accordingly, I have long regarded it as a desideratum to obtain by precipitation an insoluble or very sparingly soluble salt of sesquioxide of uranium, which should be as fluorescent as the best salts of that base, and which might be treated like a water colour. I have now succeeded in preparing such a salt, though not by direct precipitation.

The ordinary phosphate obtained by precipitation, the composition of which, independently of water of hydration, is $PO_5(U_2O_3)_2HO$, is only slightly fluorescent. If, however, this salt, with as much water as remains when it is washed by decantation, be put into a saucer, a little free phosphoric or sulphuric acid added, and then crystals of phosphate of soda, phosphate of ammonia, microcosmic salt, or borax be added in excess, the original salt is gradually changed into one which is powerfully fluorescent. The change seems to take place most rapidly with borax; but as an excess of this salt is liable slowly to decompose the fluorescent salt first formed, it is better to employ a phosphate. The quantity of acid should be sufficient to leave a decided acid reaction when the liquid is fully saturated by the alkaline phosphate employed. The change may be watched by observing from time to time the fluorescence of the salt by daylight, with the aid of absorbing media. It is complete in a few days at furthest, when the salt is ready to be collected.

This requires precaution, as the salt is quickly decomposed by dilute acids (and accordingly by its own mother-liquor if diluted), and even, though more slowly, by pure water, with the formation apparently of the original phosphate. It is also decomposed, at least in time, by alkaline carbonates, with the formation of a beautiful yellow non-fluorescent salt resembling the precipitate given by alkaline carbonates in salts of sesquioxide of uranium. The salt may be collected by adding at once, instead of water, a saturated solution of borax, in quantity at least sufficient to destroy the acid reaction. The salt is then poured off in suspension from any undissolved crystals of the alkaline phosphate employed, and collected on a filter. A pressed cake of this salt, or a porous tile on which the salt is spread, having been moistened with a solution of borax, forms an admirable screen, and is what I have chiefly employed of late. It shows, of course, the visible as well as the invisible rays—the former by ordinary scattering, the latter by fluorescence.

From the circumstances of its formation, the salt is probably (abstraction being made of the water of hydration) the original phosphate with the equivalent of constitutional water replaced by an equivalent of an alkali, which would make it analogous to the highly fluorescent natural yellow urinate. At any rate, this hypothesis guides us to its successful preparation, the conditions of which it would not have been easy to make out by observation alone. Without the use of free acid the fluorescence is not fully developed, which is accounted for by the insolubility of the original phosphate and the fluorescent salt, which presents an obstacle to the complete conversion of the one into the other.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

Friday Evening, March 13.

The lecture was by Dr. Gladstone, on "*Fogs and Fog Signals.*"

A fog, the lecturer said, was a cloud on the surface of the earth, and quoting Dr. Tyndall's definition of a cloud as the visible capital of a pillar of invisible vapour, added that a fog was the capital without the pillar. The constitution of a fog was the same as that of an ordinary cloud: it was composed of water in the form of minute globules; there was no ground for believing that the water existed in the form of vesicles. A simple fog had the same appearance as a cloud, and the peculiar colour of a London fog was due to smoke. The lecturer then dwelt on the importance of fog signals to mariners, and showed the obstruction fogs offered to the passage of light, explaining that a simple mist offered great obstruction. He then remarked that the character of the light exhibited on lighthouses was a matter of great importance, and showed that a fog entirely cut off the blue end of the spectrum, relating that the electric light displayed on the Monument on the evening of Tuesday, the 10th, was seen through the fog at Hampstead as a red light. Red light had been found to penetrate farthest, and Dr. Gladstone showed the effects of the coloured glasses used in the French and English lighthouses, the French being composed of red and orange, and the English of red only, the latter being visible at the greatest distance. The lecturer then said that for practical purposes it was necessary to distinguish between a mist and a fog, but this was not an easy matter. The same thing had a very different appearance to different observers. Landsmen in general had very loose notions about a fog, but a sailor's ideas on the subject were more definite. The lecturer showed by a reference to the reports of several lighthouse keepers on one corner of Scotland that what was evidently the same fog was variously reported as fog or as simple mist. In order to get uniformity in these reports, Mr. Cunningham had proposed to erect a pole, painted vermilion, as a test object, and suggested that everything which rendered this invisible at a hundred yards should be considered a fog. This test had been accepted by Mr. Glaisher, and may be generally received. The returns from various lighthouses round our coasts showed that fogs were sometimes of very small, and at others of very large extent. In London they were generally local; but a comparison of the returns of lighthouse keepers had shown the lecturer that some extended over a large area. There was one on April 14, 1861, which enveloped nearly the whole coast of Ireland, and was also seen in Wales, Scotland, and on one point of Cornwall. On April 1 of the same year it was foggy over nearly all Europe. Fogs had been found to be more common at one time of the year than another. November was considered the foggiest month in London; but in the South they prevailed most in January and June. The east of Scotland was in general free from fog in January, but had many in September, May, and June. In Ireland there was seldom any fog in February. Some years, too, were remarkable for fogs. England, the lecturer said, did not deserve its reputation for fogginess, statistics proving that fogs were not more frequent here than in other countries. The sea was much more uniformly covered with fog than the land; and it was found on our coasts that high headlands with a south-west aspect were peculiarly liable to fog, which was explained by the fact of a south-west wind coming loaded with moisture from the Atlantic.

The lecturer then entered upon the subject of fog signals, and showed that, light failing, it was necessary to have recourse to sound. Many means of producing sound had been used—bells alone, and bells with mirrors behind them (as at Boulogne) to collect and send forward the sound. Gongs had been used on lightships, guns were also employed, and steam-whistles, which last were highly spoken of. Sea birds had been encouraged on the Skerries, and were believed to have been of some use, until some rats escaped from a shipwrecked vessel and began to devour them. A cat was introduced to remedy this, but it was found, unfortunately, that she preferred birds to rats. The objection to the use of sound was that it did not indicate the direction of the danger, but still it had at times been useful to steerers, and Dr. Gladstone related one or two instances in which vessels had been safely steered, the steerer being guided by sound and the constant use of the lead. Another objection was that one sound was easily confounded with another sound. Bells were rung for many purposes, guns were used as signals of distress, steam-whistles were always going; but still it might be found possible to agree on some plan to regulate a system of sounds. One more objection was that a breeze interfered with the passage of sound. It was true that in a dense fog there was generally a calm; but then came the most serious objection of all—that fogs affected the transmission of sound, not so much as of light, but still to an appreciable extent. The composition of a fog, too, had a great influence on sound. Perfectly damp air was a much better conductor than a dry atmosphere mixed with some fog. As the use of sound, however, could not be discarded, it was necessary to find out the quality of the sound which was able to travel farthest, and fall back on experiments to ascertain this. Many had already been made, and more were in progress. Horns, trumpets, whistles, and whistles combined with a rattle, guns on a level and guns on a height, had all been experimented on, as well as the influence of pitch, and the repetition of discordant rather than concordant sounds. The influence of the background had also been examined. When on this part of the subject, the lecturer exhibited a small model of Professor Holmes' trumpet, which was sounded, and which gave the audience a satisfactory idea of the power of the instrument. The transmission of sound through water was then considered, and, as this would be totally unaffected by waves or fog, Dr. Gladstone deemed this the most fruitful line of investigation.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, March 11, 1863.

E. W. Binney, *F.R.S., F.G.S., President, in the Chair.*

The Rev. Robert Harley, F.R.A.S., read the following communication "*On Bring's Reduction of the Equation of the Fifth Degree to a Trinomial Form.*"

A paper was read, "*On the Solution of the Differential Resolvent,*" by W. H. L. Russell, A.B., communicated by the Rev. Robert Harley, F.R.A.S.

Mr. Crookes, F.C.S. exhibited a specimen weighing 450 grains of the new metal thallium, which he discovered by spectrum analysis. He stated that he had been fortunate enough to find this element in comparatively large quantities in the deposit from the flues of Mr. Spence's pyrites burners.

Dr. Joule made the following communication respecting a new and extremely sensitive thermometer:—"Some years ago I remarked the disturbing influence of currents of air on finely suspended magnetic needles, and suggested that it might be made use of as a delicate test of temperature. I have lately carried out the idea into practice, and have obtained results beyond my expectation. A glass vessel in the shape of a tube, two feet long and four inches in diameter, was divided longitudinally by a blackened pasteboard diaphragm, leaving spaces at the top and bottom, each a little over one inch. In the top space a bit of magnetised sewing needle, furnished with a glass index, is suspended by a single filament of silk. It is evident that the arrangement is similar to that of a 'bratticed' coal pit shaft, and that the slightest excess of temperature on one side over that on the other must occasion a circulation of air, which will ascend on the heated side, and, after passing across the fine glass index, descend on the other side. It is also evident that the sensibility of the instrument may be increased to any required extent, by diminishing the directive force of the magnetic needle. I purpose to make several improvements in my present instrument, but in its present condition the heat radiated by a small pan, containing a pint of water heated 30°, is quite perceptible at a distance of three yards. A further proof of the extreme sensibility of the instrument is obtained from the fact that it is able to detect the heat radiated by the moon. A beam of moonlight was admitted through a slit in a shutter. As the moon (nearly full) travelled from left to right the beam passed gradually across the instrument, causing the index to be deflected several degrees, first to the left and then to the right. The effect showed, according to a very rough estimate, that the air in the instrument must have been heated by the moon's rays a few ten-thousandths of a degree, or by a quantity no doubt the equivalent of the light absorbed by the blackened surface on which the rays fell."

E. W. Binney, F.R.S., the President, said that of late years considerable attention had been devoted to the examination of the beds of sand and gravel found in the valleys formed since the deposition of the till or boulder clay. Sir Charles Lyell, in his valuable work on the "Geological Evidences of the Antiquity of Man," has given us many facts connected with these valley gravels, especially relating to the terraces of the higher and lower level gravels found in the valley of the Somme, in which the flint instruments have been met with. As these two deposits are seen in the neighbourhood of Manchester, he wished to direct attention to all excavations that were being made in them, in order that any remains or implements which might be met with should be preserved. Doubtless many interesting specimens have perished, owing to the parties finding them being ignorant of their value. Many years since a former member of the Society, the late Mr. F. Looney, F.G.S., in speaking of the superficial gravel found in this neighbourhood, at page 23,* says:—"Imbedded in the gravel near the river courses are occasionally found the stone celts of the ancients, from which it is presumed that the rivers, since the country was inhabited, have either sawn their beds deeper, or much exceeded their present volume of water; several large trees have been dug up from the sand and gravel; part of one is now lying near the residence of the Rev. J. Clowes, at Kersal Moor, which was dug from the Show Field, on his estate,

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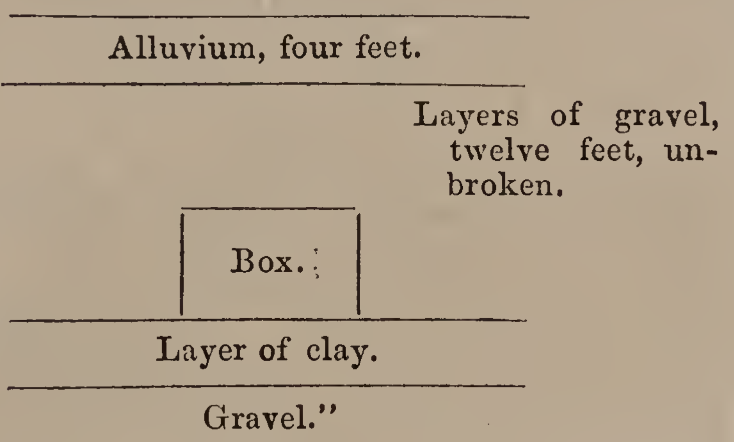
* "List of Organic Remains, &c., and Where Found, to accompany Mr. Elias Hall's Introduction and Map," by Mr. Francis Looney, member of the Literary and Philosophical Society of Manchester, published in 1836.

at upwards of twenty feet elevation above the present level of the river. A case more illustrative of this was beautifully shown in the winter of 1820, during the cutting away part of the high ground at Castlefield, near the tunnel mouth; for sixteen feet below the level of the grass a wooden box was found. It was square, and formed of four upright posts, driven into a bed of clay; the sides and bottom were closed in with logs of wood; the logs were rudely hewn, had been riven, not sawn, from five to six inches square; some greenstone boulders lay at the bottom, and the whole was covered with sixteen feet of sand and gravel; twelve feet of the lower part had never been disturbed, the continuity of the layers being unbroken.

SECTION.



Mr. Looney was well known to be a most accurate and intelligent observer, and his mention of stone celts having been found at levels above the present river courses appears to afford us fair hopes of their being found in the lower level gravel if carefully looked for. But the most interesting fact is the finding of the wooden fabric, by the author termed a box, which, although smaller in size, bears some resemblance to the *crannoges* found sometimes in Ireland under the peat bogs. He (the President) had lately been to examine the place where it was found, which is to the south of and just outside of the old Roman Station of Mancunium, in company with a party who saw the fabric when it was first exposed, and stood in it afterwards. The locality of course is now much changed, but the person was able to point out to him the position where it was found, which is now covered by No. 6 arch of the Altrincham Railway, and said that it was about six feet square and four feet high. The four upright posts driven into the clay were about five inches thick, and roughly riven, as were also the slabs forming the sides and bottom. His informant did not notice any top to the fabric, or how the slabs had been fastened to the posts, but he remembers a layer of cobble stones, each about six inches in diameter, forming a kind of pavement, being in the bottom of it. Upon these stones were some bones, which he examined, but did not recognise any of them as human. All the wood appeared to be oak, but it was very rotten, and fell to pieces soon after it was exposed to the air. None of it was preserved. Under the clay the red rock (Trias) was found at a short distance. The geological position where the fabric was found is in the lower level gravel, about 29 feet above the water of the present river Irwell. For whatever purpose it was made, there can be little doubt as to its having been the work of man, and as we are assured that the overlying sand and gravel was quite undisturbed, there can be as little question of its great antiquity, reaching as far back as the age of the lower level gravel of the valley of the Somme in France, where the flint implements have been found. Doubts have been raised as to these flints having been fashioned by human hands, but as to the origin of the wooden fabric, if, as stated, it was covered by 12 feet of undisturbed sand and gravel, no such doubts can rationally be raised.

A paper was read, entitled, “*Notes on the Action of Heat and Force upon Matter*,” by J. C. Dyer, V.P. Confining his observations to the “action of mechanical forces,”—apart from those depending on the electrical, magnetical, and vital forces—he held the former to depend on the inherent properties of matter, as gravity, inertia, and elasticity, each co-extensive with the material universe. That force cannot in any case be treated as an abstraction or as apart from the matter in which it resides, though several authors have had recourse to such an abstraction to account for phenomena attending the operation of force. Mechanical action, depending on the elastic force generated by the agency of carbon and water, being of so great importance, it is desirable to obtain some settled knowledge as to how this arises. We all know that by combustion of the former the latter is converted into steam. That which passes from the burning coals through the boiler plates and into the water to form the elastic vapour, is called heat; but whether it be a union of heat with the water, or only a transmitted mechanical action on the water, causing its atoms to separate and assume the elastic state, remains a question for solution; and of late several able writers have offered explanations of this phenomenon founded on the supposed nonentity of heat and the agency of mechanical force to form steam, and thus rejecting the latent heat theory, which before seemed to account for the heat absorbed to form steam. So far as the latent heat theory serves to explain phenomena, the mechanical creation of heat is uncalled for, since it is unphilosophical to assign new causes when those before accepted clearly meet the case. Yet we have seen two elaborate essays, by Dr. W. J. M. Rankine and Professor Tyndall, each based on the force heat assumption, but without affording any solid proofs of its application to the phenomena adduced by them.

Mr. Rankine states that “Heat produces an increase of temperature and of expansive force in bodies; that the changes from the solid and liquid states to the gaseous state are accompanied by an increase in volume; that heat which produces those effects is known by the name of sensible heat, as attaining the form of heat, and in short making the body hotter.” Again, that “changes of volume are attended with changes of molecular arrangement, perceptible or imperceptible, and that the latent heat of expansion or evaporation consists of heat that disappears in overcoming pressure and the attraction of the particles of the body.” In reference to the non-production of heat by the moving force exerted in waterfalls, the following solution is offered, viz., “It is natural to suppose that the motion during this phenomenon (the falling of a mass of water) has not been destroyed, but has been converted into revolutions of the particles in vortices or eddies too small to be perceptible by any of our means of observation, and that the centrifugal force of such eddies is the cause of the tendency of hot bodies to expand, melt, and evaporate.”

The first class of phenomena above cited are plainly accounted for by the doctrine of latent heat, but the latter explanation about waterfalls passes Mr. Dyer's comprehension. He submits that what is said to be natural to suppose concerning the revolutions of particles in vortices and eddies too small to be perceived is too obscure and questionable to be accepted in explanation of anything. If Mr. Rankine has no better mode of accounting for the non-appearance of heat from the force expended in waterfalls than the above, his defence of the generation of heat by force fails, and the balancing the force by the resistance at the foot of the falls is just as open to view as the falling force itself; so that the motions, too small to be perceived, are supererogatory inventions. The dynamical origin of heat is also set forth by Professor Tyndall, in a paper “On the Forms and Action of Water.” Examples are therein adduced of vast mechanical forces exerted by the formation of water, and by its changes from the states of a solid, liquid, and vapour. He says that “on the combination of oxygen and hydrogen to form water weighing 10 lbs. an energy was expended, the atoms clashing together with a force equal to that of a ton weight let fall from a height of 23,757 feet.

In falling from the state of vapour to that of water an energy was expended equal to that of a ton falling from a height of 3700 feet; the moving force of the stone avalanches of the Alps was but as that of snow-flakes compared with the energy involved in the formation of a cloud. In passing finally from the liquid to the solid state, the atoms of 10 lbs. of water exercised an energy equal to that of a ton weight falling down a precipice 550 feet high."

Now, the union of the gases to form water, the evaporation of it to form steam, and freezing it into ice, are chemical changes, and the forces called into action by them are chemical forces, which cannot be measured by that of gravity in falling tons or even ounces; but the mutations of heat attending those changes are measured by degrees, when passing from the latent to the sensible state, and *vice versâ*. Of vapour in the air, the Professor says, that "though forming only about 0·5 per cent. of the atmosphere, for every ray of terrestrial heat struck down by the air, fifty, sixty, or seventy rays were destroyed by the aqueous vapour. This vapour permitted the solar rays to reach the earth, but intercepted the terrestrial rays in their escape towards space," and he adds that "in the presence of such experiments, it was easy to see how the snow of the Alpine mountains, and how the ice should be squeezed through the moulds formed by the valleys, and then that these mountain glaciers were as much a proof of heat as of cold, and to produce the vapour to condense and form glaciers required as much heat as would raise five times the weight of the glaciers of cast-iron to its melting point." The heat required to convert water into steam as it passes from the sensible into the latent state is a known quantity, so also is the amount required to convert ice into water; but that required to melt a pound of ice and evaporate a pound of water would fall far short of melting 5 lbs. of cast iron.

That no mechanical energy is exerted in the conversion of water into steam is obvious to our senses, since evaporation is the most gentle, imperceptible, as well as the most universal action in Nature's laboratory; and although the elasticity of the steam itself exerts mechanical force equal to the atmospheric pressure, chemical force alone is concerned in evaporation, and therefore cannot be compared with falling bodies, so that the alleged energy and clashing force of atoms, in the actions of water cited, are wholly imaginary; nor can philosophy assert any fact relating to mechanical action which cannot be proved by experiment or established by plain induction; and in the cases cited, no one can know anything about the occult forces and energies of the motions of atoms attending the mutations of water. Great respect is due to both of the gentlemen whose essays have invited the above strictures; but the search after the truths of science "should not halt before high names."

Mr. Rankine, in offering demonstrations of the dynamical origin of heat, allows that heat disappears and becomes latent in water, in passing from the solid to the liquid state, and from this into vapour, as also that the same amount of heat is given out and reappears as sensible heat, upon the reversal of those changes; thus in substance admitting the latent heat doctrine, whilst denying the materiality of that which does so enter into a latent and reappear in a sensible state, and calling this heat the production of force, when no mechanical force whatever is called into action by those mutations of water. They are, however, plainly explained by the agency of heat, in its chemical union with water and its evolution from water. Although the evolution and absorption of heat from the compression and expansion of elastic fluids bears a constant relation to the amount of compression and to the compressing force, yet this is not the case, as Mr. Rankine supposes, in the resistance to compressing forces by liquids and solids; the amount of compression in these latter cases depends upon the relative mobility of the particles of the bodies, and we find forces of great energy, exerted against yielding solids, as in carriage and other springs and elastic bodies, are in continuous action without producing any sensible heat when no compression takes place. If it were true that acting forces always produced their equivalent in heat, then the powerful movements of railway springs, hydraulic presses, and the like, should raise intense heat in the steel springs and water exerting such force, and the absence of any heat in such cases must be fatal to the dynamic creation of heat.†

A Paper, "*On the Chemical Constitution of American Rock Oil*," by Mr. Schorlemmer, Assistant in the Laboratory of Owens College, was communicated by Professor Roscoe.

MICROSCOPICAL SECTION.

February 16, 1863.

Mr. Joseph Sidebotham, *Vice-President of the Section, in the Chair.*

Captain Fletcher, of the ship *Tigris*, presented a portion of harbour mud from Singapore, and five soundings from the coasts of Java and Sumatra.

Mr. R. D. Darbishire presented specimens of mud and fossil shells (received through Dr. P. P. Carpenter) from the post-pliocene or latest tertiary deposits at Logan's Farm, Mile-end Quarries, near Montreal, Canada, described by Sir C. Lyell ("First Travels in North America," vol. ii., p. 135), and in Papers by Dr. J. W. Dawson in the *Canadian Naturalist*, 1858 and in 1859. Mr. Darbishire, in a note to the Secretary, stated that one of the peculiarities of the deposit is that it seems to have been formed in a quiet hollow. Spiculæ of sponges are found in position, as if the sponge had grown and been quietly buried on the spot. Amongst other characteristic fossils are numerous Foraminiferæ, and a siliceous and close-textured sponge referred to Tethea, of the species Logani, which is now found in water from the tide line to 200 fathoms deep. Mr. Nevill undertook to examine and report upon the specimens.

Mr. H. A. Hurst presented a copy of Part IV., Vol XII., of the "Journal of the Agricultural and Horticultural Society of India," published at Calcutta, containing the Prize Essay on Cotton Cultivation in India from Foreign Seed, by Dr. J. Shortt, F.L.S., Zillah Surgeon, Chingleputt, for which the prize of 1000 rupees and the gold medal of the Manchester Cotton Supply Association were awarded. Mr. Hurst read a paragraph from page 499 relating to the early stage of the cotton pod, which, bearing upon points lately in dispute, and not before published in this country, is given entire:—"On examining a cotton pod soon after the ovary has been impregnated (which is known by the change in colour and the fading of the petals, or flower leaves, or corolla), it is found to contain a number of seeds according to its particular variety. If a single seed be separated and examined by the naked eye, nothing is visible; but, when seen through the microscope, it is found covered with a villous coat, formed apparently of elongated cells joined end to end. These are filled with sap. The young seed itself is somewhat pear-shaped, and resembles, in miniature, some of the China candied fruits with the frosted crystals of sugar covering it. On letting out the contents of a single cell, it is found to consist of granular cells containing a centro-lateral nucleus. On examining a pod between three and four weeks old, the seed still retains somewhat of its pyriform shape, and appears quite shaggy. The fibres, tapering to a point at their free end, resemble hollow cylindrical tubes filled with fluid, and vary in length; and on submitting a single fibre, com-

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† I have experimentally determined the thermal effects of compressing or dilating metals and fluids, and have found these effects to be strictly in accordance with the dynamical theory of heat. See *Phil. Trans.*, 1859.—Ed.

pressed between pieces of glass, to the microscope, the flattened surfaces become distinctly visible. Again, on substituting a mature fibre before it gets dried, the filament is found to consist of tubular hairs, which are now quite cylindrical. After the dehiscence of the mature capsule by the contraction and separation of its valves, the wool becomes dry from exposure. A filament now placed under the microscope is found to resemble a flattened piece of tape, twisted upon itself, and apparently formed of an extremely thin and transparent membrane, interspersed with dark granular matter, which, after a certain time, disappears in some of the varieties."

Mr. J. G. Lynde, F.G.S., M. Inst. C.E., read a paper "*On the Action of Magenta upon Vegetable Tissue*," in which he described a series of experiments upon cuttings of Vallisneria immersed in a solution of that dye in cells under the microscope, and its effect upon the circulation in that plant. He found that so long as the vital action continued, the cell walls and the moving chlorophyll retained their green colour, but the injured cells were immediately deeply reddened, and their contents gradually acquired the same colour, the intensity of which was in proportion to the thickness or density of the tissue. Between the cell walls it would appear that there exists an intercellular membrane, devoid of vital action, which becomes rapidly coloured whilst the circulation continues active. On the inner surface of the cell wall, whilst rotation is going on, the author observed a luminous stratum suggesting the action of ciliæ, but in every observation as the dye permeated the tissue and the circulation ceased, the true cell wall became covered with irregular markings, either corrugated or having raised excrescences, scarcely alike in any two cells; in no case were the markings visible until the rotation had ceased, and they had the appearance which would be produced by ciliæ falling against the cell wall in all positions upon the suspension of vital action.‡

The chlorophyll vesicles appear in three forms: in a gelatinous sac or mass rotating altogether in the cells; as independent vesicles apparently homogeneous in their structure, rendered opaque by colouring matter; and lastly, as independent vesicles somewhat increased in size, of a pale green colour, almost transparent, containing nuclei, one, two, or three in number, which in reality appear to be immature vesicles within the parent, similar to Volvox globator, without rotatory motion. The chlorophyll vesicles appear to resist the action of the magenta for some time after their rotation has ceased, indicating a vitality, at least to a certain extent, independent of that of the cell. In some of the experiments a few of the cells assumed a purplish colour, whilst in the adjoining cells the circulation was active and the chlorophyll green; in those the chlorophyll appeared to be decomposed, and the cell to be nearly full of very minute dots, swarming like the granules in Closterium lunula. Upon this subject the author offered no opinion. The observations were made with $\frac{1}{5}$th and $\frac{1}{8}$th objectives, and the paper contained minutia of several experiments, with the hours of observation, temperature of the room, and other particulars.

Dr. Roberts observed that Mr. Lynde's remarks upon the separate vitality of the cell and cell contents were very suggestive; he had noticed that fresh blood discs (for instance from a pricked finger) were not immediately affected by magenta, but that some little time was required for the dye to permeate the envelopes.

Mr. Nevill exhibited a new form of cell cut out in cardboard, containing seven divisions for seven different objects; it is very suitable for Foraminferæ, Diatomaceæ, &c., and may contain seven species, which will economise space in cabinets and facilitate exhibition. The perforations are about a quarter of an inch in diameter, and are made with a saddler's hand-punch; the disc is then covered with black varnish and secured to the glass slide.

Mr. J. B. Dancer exhibited new cells for opaque objects of various sizes; they are made of composition, and are cast in a mould.

Mr. J. G. Dale exhibited with the polariscope crystallised films of santonine and of picrate of aniline; they are very rich in colour, and some of the forms are believed to be new.

MATHEMATICAL AND PHYSICAL SECTION.

Annual Meeting, March 5, 1863.

Joseph Baxendell, *F.R.A.S., Vice-President of the Section, in the Chair.*

On the motion of Mr. Atkinson, seconded by Mr. Dickinson, it was resolved that Rule 4 be altered by adding the following words:—"No person shall hold the office of President for more than two years in succession."

The following gentlemen were elected officers of the Section for the ensuing year:—*President:* Mr. Joseph Baxendell, F.R.A.S.; *Vice-Presidents:* Mr. Robert Worthington, F.R.A.S., Mr. E. W. Binney, F.R.S., F.G.S.; *Treasurer:* Mr. George Mosley; *Secretary:* Professor R. B. Clifton, M.A., F.R.A.S.

In illustration of the abnormal character of the season, two barometric curves, neatly laid down on engraved forms by Mr. Sidebotham, from observations made by him at Ashton-upon-Mersey during the months of January and February last, and a summary of meteorological observations taken at Thelwall by Mr. Atkinson, F.G.S., during the month of February, were communicated to the Section. The mean height of the barometer at Thelwall for February, reduced to 32° F., and to mean sea level, had the unusually high value of 30·218 inches; the mean temperature was 42·5°, and the total fall of rain was only 0·811 inches.

Mr. Baxendell communicated a table which he had calculated, showing the monthly sums of the oscillations of mean daily temperature at Greenwich during the thirteen years 1848—60; and also the mean daily values for the different months. The mean monthly results were as follows:—

	°		°
January	102·8	July	82·8
February	84·9	August	78·4
March	83·4	September	72·3
April	90·6	October	84·4
May	83·9	November	94·5
June	87·3	December	107·1

The oscillations of mean daily temperature have hitherto been strangely neglected by meteorologists, although they undoubtedly form an important element in the question of climate. The curve laid down from the above numbers differs considerably from that of any other element of temperature, the principal maximum of disturbance occurring in December, and the principal minimum in September; and it is perhaps worthy of remark that the course of this curve appears to bear a well-marked relation to the annual progress of the growth and decay of plants, the period of maximum disturbance of mean daily temperature corresponding with that when the vital principle in plants is least active; and the time of minimum with that when the ripening of the more important cereals and fruits takes place. Mr. Baxendell also drew attention to the attempt which is now being made to organise an association for the systematic observation of variable stars, and presented to the Section a chart of the vicinity of the

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‡ Eminent microscopists do not entertain the idea of the circulation in Vallisneria being due to ciliary action: "this appearance is decidedly affirmed by Mr. Wenham to be an optical illusion." (See Dr. Branson, in "Quarterly Journal of Microscopical Science" for 1855, Vol. III., p. 274, and Mr. Wenham, p. 277, quoted by Dr. Carpenter, "Microscope and its Revelations," 3rd ed., p. 408.) This opinion was no doubt formed solely upon observations made during vital action, and may be modified upon examination of the supposed dead and dying ciliæ rendered visible by the action of the magenta dye.—(Sec. Micro. Section.)

variable star R *Canis minoris*, neatly executed by Mr. George Knott, F.R.A.S., of Woodcroft Observatory, Cuckfield, Sussex. This chart extends over one square degree: the central portion of half a degree square includes all the stars which are visible with a seven inch object glass, and the outer portion all Argelander's stars to the 9·5 magnitude. A list of twelve comparison stars is given, the magnitudes of which have been photometrically determined by the method of limiting apertures. In reference to the objects of the Association, Mr. Baxendell remarked that the importance of a careful study of the phenomena of variable stars will be apparent when it is considered that all the so-called fixed stars—our own sun included—are supposed to have a general similarity of constitution; and as several eminent astronomers have doubted whether the emission of light from the sun is absolutely constant, it is not improbable that an extended knowledge of the phenomena of variable stars may ultimately assist us in obtaining a more intimate acquaintance with the constitution and phenomena of the great luminary of our own system, and lead to the detection of changes at present not recognised, but which in their influence upon the earth may have an important bearing upon many questions of considerable interest in magnetical and meteorological science, and also upon the economy of the various forms of vegetable and animal life. The recent results of spectral analysis tend to show that the light of the sun has a general resemblance to that of the class of stars to which nearly all the known variables belong, and thus give additional weight to the probability that it may also be slightly variable.

Professor CLIFTON made a communication, in which he endeavoured to show that the principles of the mechanical theory of heat afford an explanation of phenomena attending the production of light by bodies, either when this production of light is due to incandescence or to fluorescence. His remarks and explanations were illustrated by a series of highly interesting experiments.

CHEMICAL SOCIETY.

March 19, 1863.

Professor A. W. HOFMANN, *Ph.D., F.R.S., President, in the Chair.*

THE minutes of the previous meeting were read and confirmed, after which the following gentlemen were balloted for and duly elected Fellows of the Society:—Robert Warington, jun., George Warington, William Crossley, J. Robert Colman, and W. B. Kemshead. T. A. Malone was elected Associate.

A paper, by C. GREVILLE WILLIAMS, Esq., "*On the Chinoline and Leucoline Series,*" was read by the SECRETARY. The author commenced by referring to the recent publication of researches in this direction by Dr. A. W. Hofmann,[*] and to the particular description therein given of the chinoline blue, which was fully in accordance with the author's views upon the subject. The bases higher in the series had also been investigated by Mr. Williams; they gave rise to the production of compounds precisely similar, and were both in chemical and physical characters, excepting colour, almost identical with the substances already described. Thus, by acting with iodide of amyl upon the heavy coal-oils, or those separated from Boghead naphtha, mixed products were obtained which could be purified by fractional distillation; the unchanged iodide of amyl distilled over, and the remaining product yielded with potassa a dark coloured oil which possessed the odour of triamylamine; a small quantity of this body was actually formed and could be identified in the crude product. The oil itself was decidedly bibasic; with hydrochloric acid it dissolved, forming two layers, the upper one, like melted paraffin, solidified on cooling into a white crystalline mass, whilst the lower portion consisted of a saturated aqueous solution of the same hydrochlorate; its formula was,—

$$C_{20}H_{32}N_2,2HCl.$$

This compound might be viewed as the hydrochlorate of diamyl-lepidyl-diamine, or as containing—

$$C_{10}H_{23}N,C_{10}H_9N.$$

The platinum salt had been prepared and analysed, and the determination of the vapour-density gave the expected result. The base itself had a boiling point of 175° C., and for a provisional name the author proposed to call it "lepoline."

The PRESIDENT remarked that the number of isomeric bodies was multiplying fast; in these two series, the one obtained from coal, the other from cinchonine, it was extraordinary to find that the latter alone afforded the coloured ammonias, but that in all the remaining characters, such as the boiling point, density, and even the odour, they were very similar. The circumstance that the first was scarcely affected by potash, whilst the chinoline series yielded coloured bodies, seemed indeed the only difference. There were, on the other hand, many cases of isomerism in which all the physical characters appeared completely changed—he might refer, for example, to the diatomic base prepared from dinitro-benzol by a process of semi-reduction, or from aniline by oxidising with nitric acid; the composition of these products was identical, but their fusing points were 100 degrees apart; the one furnished kinone in large, or rather theoretical, quantity, whilst that made from dinitro-benzol did not give any under the same treatment.

Dr. J. H. GLADSTONE pointed out the advantage of appealing to optical tests as a means of discriminating between isomeric bodies which had many characters in common. The base lepidine exhibited a very high degree of refraction, and it was probable that bodies containing a larger number of carbon atoms would prove even more highly refracting.

The PRESIDENT stated that we were not more anxious to find methods of distinguishing between such bodies than to know *why* we were enabled to distinguish them.

The next paper read was by Dr. EDWARD DIVERS, of Queen's College, Galway, and entitled "*On the Spontaneous Conversion of Gun-Cotton into Pectic and Parapectic Acids.*" The author having prepared some pyroxyline from Swedish filtering-paper by the action of mixed nitric and sulphuric acids, a portion remained after the object of the experiment had been satisfied; this was enclosed in a stoppered bottle, and put away in a position exposed to diffused daylight, but never to the direct rays of the sun. After two years the stopper was lifted, and a violent escape of gas ensued; this appeared to be chiefly binoxide of nitrogen, for it produced red fumes on mixing with the air; the stopper was not replaced, but left resting loosely upon the neck of the bottle, and another year and a-half elapsed. At the end of that time the gun-paper had become converted into a kind of gelatinous pulp, which was strongly acid to test-paper, and partially soluble in water. The aqueous solution furnished precipitates with a variety of metallic salts, especially with basic acetate of lead, the nitrates of mercury and silver, sulphate of copper, and perchloride of iron, but none either with salts of manganese or zinc. The acid solution immediately reduced the tartrate of copper, and gave a yellow colouration to potassa. These reactions appeared to justify the conclusion that pectic acid was the chief product of the decomposition, and that parapectic acid was left insoluble on treatment with water. This latter substance could be dissolved in alkali, and reprecipitated by an acid; the soda salt was thrown down from its aqueous solution by alcohol in the form of a gummy mass, and, in consequence of the impossibility of preparing a crystalline product, no quantitative analysis was made. There was no evidence either of oxalic or saccharic acid

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[*] *Vide* CHEMICAL NEWS, vol. vii., p. 7.

in this specimen of decomposed pyroxyline; the first was carefully sought for, inasmuch as Dr. Hofmann and others had already detected this substance among the products of the spontaneous decomposition of gun-cotton.

The President explained, with reference to the sample examined by him, that it was put into his hands by Dr. Percy. The gun-cotton had been kept in a close stoppered bottle exposed to light for several years, and the residual product was visibly crystalline.

The next communication was by Dr. Attfield, in the form of a "*Note on Oxamide.*" A very interesting reaction had been observed by his friend, Mr. John Robbins, to the effect that when hydrocyanic acid was mixed with the peroxide of hydrogen and set aside for an interval of ten days or more, a white crystalline product was deposited, which had been put into the author's hands for chemical examination. The reactions were such as to point to the formation of oxamide, and this supposition was confirmed both by analysis and by comparison with a sample of this substance prepared as usual by the action of ammonia upon oxalic ether. The change was correctly expressed by the following equation :—

$$HO_2 + HC_2N = N \left\{ \begin{matrix} H \\ H \\ C_2O_2 \end{matrix} \right.$$

Or by doubling the formula—

$$2\,HO_2 + 2\,HC_2N = N_2 \left\{ \begin{matrix} H_2 \\ H_2 \\ C_4O_4'' \end{matrix} \right.$$

By the action of hydrochloric acid, the substance in question was resolved into ammonia and oxalic acid; and the effect of concentrated sulphuric acid was similar, excepting that the oxalic acid split up immediately into equal equivalents of carbonic acid and carbonic oxide. A somewhat parallel instance of formation had been described by Dr. Playfair, who obtained oxamide as one of the products of the action of nitric acid upon ferrocyanide of potassium. The author had succeeded likewise in producing small quantities of this substance by treating a mixture of binoxide of manganese and cyanide of potassium with a minimum of sulphuric acid, but had failed in the attempt to produce ethyl-oxamide by the action of peroxide of hydrogen upon the cyanide of ethyl. The mode of production above described was interesting from the circumstance that oxamide was, in this instance, formed without the intervention of ammonia or a derivative of that base.

Dr. Odling, as well as the President, objected to the word "derivative" being understood in the sense just now expressed by Dr. Attfield. According to Gerhardt's definition, it would be better to confine its meaning to a substitution product or body formed on the original type; between ammonia and hydrocyanic acid there was, however, an intimate relation, the formiate of ammonia being, in fact, equivalent to hydrocyanic acid and water.

Mr. Church referred to a process, published by Liebig, as forming oxamide without the direct intervention of ammonia; it consisted in passing cyanogen gas through a mixture of aldehyde and water. The action of what might be termed nascent oxygen could be explained in the same manner as that of the peroxide of hydrogen.

Dr. Attfield replied to the objection, on the part of the President, by citing the decomposition of the nitrate and nitrite of ammonia by heat; by the same rule that hydrocyanic acid was to be considered an ammonia-derivative, we should be compelled to enumerate nitrogen and protoxide of nitrogen among the derivatives of ammonia.

The President would not hesitate to adopt such an expression, since protoxide of nitrogen gas gave rise to the production of ammonia on passing over hydrate of potash in the state of fusion.

The fourth and last contribution, by F. A. Abel, F.R.S., had reference to the composition of certain samples of native copper, the report of which is unavoidably postponed until the next Number.

The meeting stood adjourned until the 30th instant, the twenty-second anniversary of the foundation of the Society.

NOTICES OF PATENTS.

3021. *Manufacture of Certain Colours for Printing and Dyeing Fabrics.* A. Schultz, Paris. Dated December 2, 1861. (Not proceeded with.)

For the production of various shades of black, olive, chocolate, and other colours upon cotton, wool, silk, or mixed fabrics, the inventor employs a decoction of logwood mixed with caustic soda.

3024. *Preparing and Applying a Certain Material on the Hulls of Iron or Wooden Ships, &c.* G. Ralston, Tokenhouse-yard, London. A communication. Dated December 2, 1861.

The inventor employs a composition prepared from the following materials :—

Dry and finely-powdered graphite .	10 parts
Raw linseed oil	3 „
Yellow beeswax	1 „

The last ingredient is liquefied by heat, and the whole triturated together in a "mixer," or ground on a paint mill, in either arrangement a suitable degree of heat being maintained by a coil of steam-pipes or otherwise. It may be applied to the iron surface in this condition, or thinned with a larger proportion of oil.

3163. *Manufacture of Glue or Size.* J. Dale, Manchester. Dated December 17, 1861.

This invention refers to a mode of treating the cuttings, parings, and scrapings of hides, and is especially applicable to that kind of tanner's refuse ordinarily called "flushings." For the purpose of speedily reducing these matters to a proper condition for the separation of the gelatine and the manufacture of glue and size, the patentee mixes with them an acid (by preference muriatic acid), and then washes them to remove the excess of this latter, employing if necessary a small proportion of lime or alkali, and boils to obtain the fatty matters and the glue or size separately, as in the ordinary manufacture.

The addition of a small proportion of any mineral acid has a very marked effect in coagulating and promoting the separation of animal matters from dilute solutions of the character referred to in the specification.

Grants of Provisional Protection for Six Months.

528. Thomas Vincent Lee, Bank Chambers, Lothbury, London, "Improvements in machinery for digging, compressing, and moulding peat or turf, and for retorts and kilns for drying peat or turf, and making peat or turf charcoal through the agency of hydro-caloric or superheated steam, and for collecting the products of distillation while charring the peat or turf."

3469. William Billinghurst and Josephus Regua, Rochester, New York, U.S., "An improved portable battery."—Petition recorded December 29, 1862.

285. John Lightfoot, Accrington, Lancashire, and Frederick Trachsel, Manchester, "Improvements in machinery or apparatus used in the process of distillation."—Petition recorded January 31, 1863.

379. Frederick Oppenheim, Strand, London, "An improved plastic compound for dental purposes, to be used instead of wax, gutta-percha, or resinous gums, in taking the impression of the mouth."—Partly a communication from Edouard Billard, Paris.—Petition recorded February 12, 1863.

388. Jabez Jones, Liverpool, "Improvements in the manufacture of lead, tin, and other metals, or amalgamation of metals of a like fusible nature into sheets of any required thickness and length, and also coating one or both sides of pipes and sheets of lead and other metal or amalgamation of metals with tin or other substances, and in the apparatus connected therewith."

421. John Morgan, Stephen's Green North, Dublin, "Improvements in embalming and preserving from decay human bodies and bodies of other animals, also pickling, curing, and flavouring animal bodies."—Petitions recorded February 14, 1863.

458. Nathan Thompson, Abbey Gardens, St. John's Wood, London, "Improvements in apparatus for stopping bottles, jars, and other vessels, and in tools for producing parts of such apparatus."—Petitions recorded February 19, 1863.

522. Edward Brown Wilson, Parliament Street, Westminster, "An improvement or improvements in the manufacture of an alloy or alloys of titanium and iron."

546. James Humby, Old Broad Street, London, "Improvements in furnaces and apparatus for manufacturing oxide of zinc or zinc white."—A communication from Heinrich Felsch, Witten, Westphalia, Prussia.

Notices to Proceed.

3017. George Henry Ogston, Mincing Lane, London, "Improvements in treating nitrous acid and nitric oxide in order to convert them into nitric acid."

3296. Victor Mirland, Frameries, Belgium, "Improvements in manufacturing paste with the dried pulp of rhubarb, to be used as preserve."—Petition recorded December 9, 1862.

132. John Harrop, Manchester, "Improvements in the treatment of organic fecal and urinous matters for the purpose of deodorising the same, and in the preparation of a portable manure therefrom, and in the treatment of ashes or other refuse of combustion to be combined therewith, also for improvements in machinery to be employed in the manufacture of the said manure."—Petition recorded January 13, 1863.

458. Nathan Thompson, Abbey Gardens, St. John's Wood, London, "Improvements in apparatus for stopping bottles, jars, and other vessels, and in tools for producing parts of such apparatus."—Petitions recorded February 19, 1863.

2988. Arthur Wall, Canton-street, East India Road, Poplar, Middlesex, "Improved processes for purifying lead, and extracting and separating silver therefrom, and in machinery for those purposes."—Petitions recorded November 4, 1862.

CORRESPONDENCE.

Volumetric Analysis of Water.

To the Editor of the Chemical News.

Sir,—With reference to my letter on Mr. Nicholson's process of analysing water, I need hardly say that I had no intention of discourtesy to Dr. Paul.

On the scientific part of the subject I shall only say one word more. The proposed plans of Boutron and Boudet, and of Nicholson, are, as far as I understand them, based entirely on the same data as the plan of Dr. Clark, of which they are modifications and amplifications, but which they cannot, as far as I can see, subvert.

The same quantity of earthy base will always continue with the same quantity of soap; herein Mr. Nicholson would entirely agree with Dr. Paul. The points which may be urged against Mr. Nicholson's plan refer to its details and not to its principle. These details must be judged of by chemists. I can merely say, in justice to Mr. Nicholson, who cannot reply for himself at present, that he spent a great deal of time in perfecting and testing his method, and, to say the least of it, he is entitled to a considerate hearing. I am, &c.

E. A. Parkes.

Deaths from Nitric Acid.

To the Editor of the Chemical News.

Sir,—A correspondent in your last issue questions the statement that the cause of death in the recent accident at the Edinburgh Institution was the fumes of nitric acid.

In the *Medical Times and Gazette* for March 14, 1863, is a note to the effect that "in the year 1854 a similar occurrence took place at Sheffield, and it is worth remarking that the victim in that case, Mr. Haywood, a chemist, experienced no great uneasiness until three hours after the accident, when difficulty of breathing came on." I also can contribute one or two facts to the history of the action of such fumes on the air passages and lungs. In the latter part of the year 1861 I had occasion to frequently work with a Bunsen's electric battery of forty large cells. Its charge of nitric acid was about two gallons. The man who usually dismounted it had often been cautioned against inhaling the nitrous fumes. Both he and I, moreover, had already suffered from them to a slight extent. Our symptoms were shortness of breath for two days; deep inspirations required much effort, and were accompanied by some pain and a short cough. But one night the man delegated his duty to a brother porter, neglecting to warn the latter of incident dangers. The result was that the substitute, an active, careless fellow, was shortly after seized with difficulty of breathing and general pain in the chest region, was soon attacked by inflammation of the lungs and inner surface of the trachea, only partially recovered after a month's serious illness, and has had a cough ever since.

These two cases tend, I think, to corroborate the statement concerning the cause of the deaths in Edinburgh.

I am, &c. J. Attfield, Ph. D.

Pharmaceutical Society, 17, Bloomsbury-square.

SCIENTIFIC SOCIETIES.

MEETINGS FOR THE ENSUING WEEK.

March 30. Monday.

Chemical — Burlington House. 8 p.m. *Anniversary Meeting.*

Institute of Actuaries—12, St. James's Square. 7 p.m.

Medical—32a, George Street, Hanover Square. 8.30 p.m.

31. *Tuesday.*

Civil Engineers—25, Great George Street, Westminster. 8 p.m.

April 1. Wednesday.

Geological—Burlington House. 8 p.m.

Pharmaceutical—17, Bloomsbury Square. 8 p.m.

2. *Thursday.*

Chemical—Burlington House. 8 p.m.

Linnæan—Burlington House. 8 p.m.

3. *Friday.*

Philological—Somerset House. 8 p.m.

ANSWERS TO CORRESPONDENTS.

⁂ All *Editorial Communications* are to be addressed to the Editor, and *Advertisements* and *Business Communications* to the Publisher, at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

A.R.—Received. We are sorry to say the day escaped our reporter's memory, but we will endeavour to obtain the information.

R. H.—Lardner's Handbook (the volume is sold separately) is perhaps the "most comprehensive and moderate priced." Precise information on the particular point mentioned will not be found, we believe, in any book.

THE CHEMICAL NEWS.

Vol. VII. No. 174.—April 4, 1863.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Decomposition of Chlorate of Potash at a Low Temperature, in Presence of Peroxide of Manganese, by M. E. Wiederhold.

The author set himself to study the catalytic decomposition of chlorate of potash in presence of oxides of manganese and copper. Contrary to M. Schönbein's hypothesis, he found that oxygenated bodies are not the only ones capable of determining this decomposition; for spongy platinum he found induced the same effect.

An intimate mixture of two parts of chlorate of potash and one part of artificial peroxide of manganese begins to disengage oxygen between 200 and 205°; and this phenomenon takes place with the same proportions of chlorate mixed with oxide of copper at 230°; with platinum black, or natural peroxide of manganese at 260 to 270, and with peroxide of lead at 280—285°.

The author believes that the action of these bodies is most likely due to their great power of absorbing heat, and in part also to the relative volume of the catalysing substances.

An interesting fact observed by him is the rising of the temperature of the mixture when the metallic bath wherein the retort is plunged reaches 250° for the mixture of chlorate and peroxide of manganese, and 290 for that of chlorate and oxide of copper.

In none of these decompositions is perchlorate of potash produced.—*Poggendorff's Annalen der Physik und Chemie*, vol. cxvi., p. 171.

On the Chemical Constitution of American Rock Oil, by Mr. Schorlemmer, Assistant in the Laboratory of Owen's College.[*]

In a paper published in the October number of the *Chemical Society's Journal*, I showed that the products of the distillation of cannel coal at a low temperature contain a series of homologous hydrocarbons of the formula C_nH_{n+2}. I further showed that these are the hydrides of the alcohol radicles, as upon treatment with chlorine they yielded, by substitution of one atom of hydrogen by one of chlorine, the corresponding chlorides, from which other derivatives may be obtained.

In the portion of the oil boiling below 120° C., I found the following four hydrides:—

$C_{10}H_{12}$	hydride of amyl,	boiling point	39° C.
$C_{12}H_{14}$	hydride of hexyl	,, ,,	68° C.
$C_{14}H_{16}$	hydride of heptyl	,, ,,	98° C.
$C_{16}H_{18}$	hydride of octyl	,, ,,	119° C.

Of these the hydride of heptyl or oenanthyl is the most interesting, as it was previously unknown, and I therefore undertook the investigation of its derivatives, concerning which likewise our knowledge is very limited and contradictory. Thus, for instance, many chemists state that the alcohol obtained by the distillation of castor oil with potash is heptylic alcohol, whilst others regard it as octylic alcohol; and it is only by the most recent experiments of Bouis[†] that we learn with certainty that this substance is octylic alcohol, inasmuch as he obtained the true heptylic alcohol by the action of nascent hydrogen on oenanthol. For the purpose of this investigation, I endeavoured to obtain the hydride of heptyl from the American petroleum, as the yield of this substance from the cannel oils is but small, and the labour of purification tedious and disagreeable.

The existence of this hydride in the petroleum was rendered probable by the fact of the discovery of hydride of hexyl by Pelouze and Cahours.[‡]

The oils which I examined are those known by the name of turpentine substitute, and obtained as the first products in the rectification of the crude oil. Different samples of the commercial articles possess very different properties; the specific gravity lies between 0·70—0·75. One sample began to boil at 30° C., and the greatest portion distilled over below 100° C., whilst others between 80°—150° C., and others between 100°—200 C.

When these oils are subjected to fractional distillation, no product of constant boiling point is obtained, the oils requiring for this purpose a preliminary purification with concentrated nitric acid.

The greatest portion of the oils remains unattacked, and the acid solution contains nitrobenzol (from which aniline was prepared), nitrotoluol, and binitrotoluol, and small quantities of fatty acids produced from traces of olefines which are probably contained in the crude oils.

I tried to separate these olefines by adding bromine to the crude oil until the colour of the latter no longer disappeared; a few drops, however, are sufficient for a large quantity of the oil, and when the whole is subjected to distillation, a very few drops of bromine compounds of a high boiling point remained behind, the quantity of which was too small for further examination.

The oil after this treatment was well washed, dried over potash, and rectified repeatedly over sodium. By fractional distillation, the following four hydrides were obtained, and found to be identical with the hydrides from the cannel tar:—

$C_{10}H_{12}$	hydride of amyl,	boiling point	34° C.
$C_{12}H_{14}$	hydride of hexyl	,, ,,	68° C.
$C_{14}H_{16}$	hydride of heptyl	,, ,,	98° C.
$C_{16}H_{18}$	hydride of octyl	,, ,,	119° C.

In addition to these, I obtained a small quantity (about one gramme) of a liquid boiling between 20°—30° C., and hence we may infer that also hydride of butyl is present in small quantities.

I stated, in the paper above alluded to, that hydride

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[*] Communicated by Professor Roscoe to the Manchester Literary and Philosophical Society.

[†] *Comptes-Rendus*, lv. 140.

[‡] *Comptes-Rendus*, liv. 1241.

of amyl boils at 39° C. The same compound from petroleum boils at 34° C. I find, however, that the presence of traces of foreign substances alter the boiling point of this body very considerably. Thus, for instance, I obtained from the crude oil about one ounce of a liquid which boiled from 15°—20° C.; after treatment with nitric acid, the volume of the liquid remained almost unchanged, showing that only a very small quantity of substance had been removed; but, after drying with potash and rectifying over sodium, it was found that a mere trace of the liquid boiled below 30° C.; nearly the whole distilled at 34° C., and consisted of hydride of amyl. Of the four hydrides which I isolated, I have only prepared the hydride of heptyl in quantity; four gallons of turpentine substitute boiling between 80°—150° C., yielded three pounds of the pure compound.

In order to obtain from this other heptyl compounds, it was transformed into the chloride according to the excellent method described by Hugo Müller,§ which consists in the addition of a small quantity of iodine to the substance which is to be treated with chlorine. The substitution occurs much more rapidly in this case than when chlorine alone is employed, and goes on in absence of the daylight, so that a rapid current of chlorine gas can be led into the liquid without any chlorine escaping with the hydrochloric acid vapours.

Hence it is seen that the constitution of American petroleum, at least that portion boiling below 120° C., is quite analogous to that of the oil from cannel tar.

Petroleum consists mainly of the hydrides of the alcohol radicles; it contains very small quantities of benzol and toluol, and probably traces of olefines; whilst in the cannel coal oil the hydrides are found in smaller quantities, and benzol and toluol in proportionally larger amounts. In the oil obtained by distillation of boghead coal, Greville Williams has discovered a series of hydrocarbons possessing the composition and physical characters of the hydrides C_nH_{n+2}, also benzol and its homologues and olefines.‖

The rock oils obtained in other countries appear to possess a somewhat similar constitution.

Thus, for instance, Warren de la Rue and Hugo Müller¶ found in the Rangoon tar, benzol, toluol, xylol, and cumol and hydrocarbons of the formula C_nH_{n+2}. They were, however, unable to isolate from these a compound of definite composition and boiling point. The rock oil from Sehnde, in Hanover, consists, according to the investigation of Busenius, Eisenstuck,** and Uellsmann,†† of hydrocarbons of the same general formula, but they likewise failed to obtain definite products.

Pebal and Freund‡‡ found in the rock oil from Gallicia benzol and homologues, carbolic acid and homologues, and hydrocarbons which are not attacked by the strongest acids, and probably identical with those previously mentioned.

On the Estimation of Nitric Acid, by M. H. ROSE.

M. F. REICH has shown that pulverised quartz expels nitric acid from nitrates, especially from alkaline nitrates, at a barely visible red heat. This reaction may be utilised in estimating nitric acid by difference; for which purpose the nitrate is heated with four or six times its weight of powdered quartz.

It is better to use quartz than bichromate of potash, because the mass calcines without melting, and there is no fear of its projecting pieces of the mass. The presence of chlorides and sulphates is no inconvenience; nitrates only are decomposed. Amorphous and crystallised silica behave in almost exactly the same way.

Silica may be replaced by bichromate of potash in the estimation of carbonic acid; to expel all the carbonic acid a higher temperature is necessary than that required for the decomposition of nitrates; however, the red heat produced by Bunsen's simple gas jet is sufficient.—*Poggendorff's Annalen der Physik und Chemie*, vol. cxvi., p. 635.

On Some Reactions of Subformiate of Ethyl, by H. BASSETT, *Esq.*

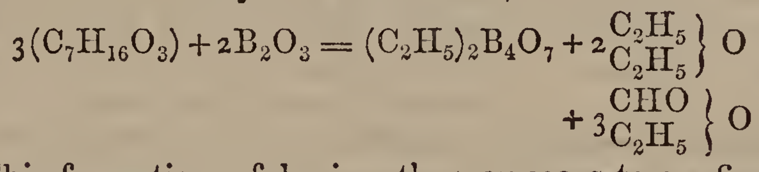
THE formation of this substance, described by Kay and Williamson,* from ethylate of soda and chloroform, the chloride of a triatomic radical, suggested the possibility of regarding it as a derivative of the glycerine of the mono-carbon series, the three equivalents of hydrogen being replaced by three of ethyl, and the following experiments were made in pursuance of this idea:—

In the first place, it was advisable to find a process for its easy preparation, as those recommended by Kay are troublesome, and, moreover, produce only a small quantity. The following method gave a product nearly equal in bulk to the chloroform used:—

Eighteen ozs. absolute alcohol, and 3 ozs. chloroform introduced into a flask furnished with an upright condenser—1½ oz. sodium then added by small portions, the action being assisted by a gentle heat in the water-bath. The alcohol then distilled off in the water-bath, the residue dissolved in water, and the supernatant oil dried with chloride of calcium and rectified.

Three equivalents of the substance thus obtained were heated with two equivalents of boric anhydride in a sealed tube for six hours at 100°. The resulting solution heated in a retort up to 200°. The distillate agitated with a small quantity of water, when an ethereal layer floats on the top, in which can be distinctly recognised the odours of ordinary ether and formic ether; the boiling point (35° to 55°) also indicates such a mixture. In a solution of this in a larger quantity of water formic acid is easily detected by chloride of mercury and sesquichloride of iron. The residue in the retort presents all the characters of biborate of ethyl; is decomposed violently by hot water with evolution of vapours of alcohol.

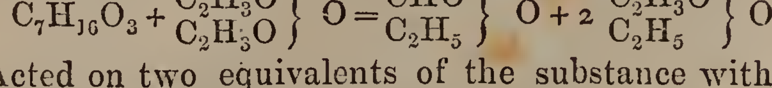
The reaction may be written:—†



$$3(C_7H_{16}O_3)+2B_2O_3=(C_2H_5)_2B_4O_7+2\left.\begin{matrix}C_2H_5\\C_2H_5\end{matrix}\right\}O+3\left.\begin{matrix}CHO\\C_2H_5\end{matrix}\right\}O$$

This formation of boric ether appears to confirm the formula given by Ebelman similar to that of anhydrous borax.

The action of acetic anhydride in excess, at 150°, gave similar results, only one equivalent of the anhydride entering into the reaction.



$$C_7H_{16}O_3+\left.\begin{matrix}C_2H_3O\\C_2H_3O\end{matrix}\right\}O=\left.\begin{matrix}CHO\\C_2H_5\end{matrix}\right\}O+2\left.\begin{matrix}C_2H_3O\\C_2H_5\end{matrix}\right\}O$$

Acted on two equivalents of the substance with one equivalent ter-iodide of phosphorus, and the result distilled, a thick, very acid liquid remained in the retort,

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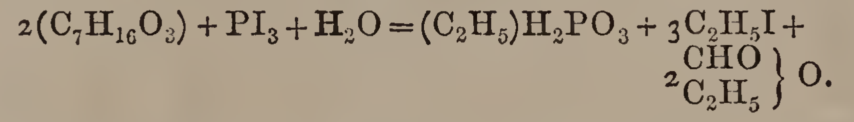
§ *Journal of Chemical Society*, xv. 41.
‖ *Philosophical Transactions*, 1857.
¶ *Jahresbericht*, ix. 606.
** *Liebig's Annalen*, 113—115.
†† *Ibid.*, 114—279.
‡‡ *Ibid.*, 115, 19.

* *Proceedings Royal Society*, June, 1854.
† C = 12 O = 16.

presenting the characters of ethyl-phosphorous acid. The distillate was digested with an excess of dry phosphate of silver to separate the iodide of ethyl. On distillation in the water bath a volatile inflammable liquid came over, soluble in a large quantity of water, having the peculiar smell of formic ether—boiling-point 52° to 56°. In its watery solution formic acid could be readily detected. The reaction may be represented as follows, the water arising probably from moisture in the iodine:—



$$2(C_7H_{16}O_3) + PI_3 + H_2O = (C_2H_5)H_2PO_3 + 3C_2H_5I + 2\left.\begin{matrix}CHO\\C_2H_5\end{matrix}\right\}O.$$

These experiments tend to show that the compound in question functions as a true tribasic formic ether, similar in composition to the best defined metallic subsalts of the fatty acids.

Analytical Notices on Uranium, by M. H. Rose.

Oxide of uranium may be completely precipitated from its acid solutions, having previously saturated them with ammonia, by addition of hydrosulphate of ammonia. No inconvenience results from the solution containing many ammoniacal salts, excepting, of course, carbonate of ammonia and all alkaline carbonates. The precipitate is black, or reddish brown if the hydrosulphate be greatly in excess. It is washed in water, to which is added a small quantity of hydrosulphate of ammonia.

The precipitate is formed essentially of protoxide of uraniam, and contains no sulphide of uranium.

After being dried, it is ignited, to expel what little sulphur may be retained; then it is calcined in a hydrogen current, at a high temperature, and left to cool in the hydrogen. Pure protoxide is thus obtained. Should the solution contain much salts of potash, or of other strong non-volatile bases, the precipitate will retain a small quantity of these bases.

Oxide of uranium is separable in the following manner from most metals, especially from those which are completely precipitated from their solutions by hydrosulphate of ammonia:—Add to the solution excess of carbonate of ammonia mixed with hydrosulphate. All the oxides which hydrosulphate transforms into sulphides are precipitated, while the protoxide of uranium is dissolved in the carbonate of ammonia. Leave the mixture to deposit in a closed vessel, wash the precipitate by decantation, with water containing carbonate and hydrosulphate of ammonia, and then filter. Gently heat the filtered liquid, to expel most of the carbonate; decompose the hydrosulphate with hydrochloric acid; oxidize the protoxide of uranium by nitric acid, and precipitate the oxide by ammonia, and, before weighing, calcine it in a hydrogen current.—*Poggendorff's Annalen der Physik und Chemie*, vol. cxvi., p. 352.

PHARMACY, TOXICOLOGY, &c.

On the Assay of Alkaloids—Pure and in Preparations,

by Ferdinand F. Mayer.

In a paper laid before the last meeting of the American Pharmaceutical Association (August, 1862), I pointed out the facility with which the iodohydrargyrate of potassium could be used for the quantitative determination of all vegetable alkaloids, either pure or contained in pharmaceutical preparations.

This test, which was first described by F. L. Winckler, in 1830,[*] as a qualitative reagent, and was introduced in 1846 by A. von Planta-Reichenau,[†] is simply a solution of corrosive sublimate in an excess of iodide of potassium, the strength of which for volumetric analysis is 13·546 grammes of corrosive sublimate, and 49·8 grammes of iodide of potassium per litre, constituting a *tenth normal solution.*

Of this solution it requires
1 cubic centimetre for the precipitation of

				In grammes.
1-10·000	of an equivalent of	Aconitia	=	0·0267
1-20·000	,, ,,	Atropia	=	0·0145
1-20·000	,, ,,	Narcotina	=	0·0213
1-20·000	,, ,,	Strychnia	=	0·0167
1-20·000	,, ,,	Brucia	=	0·0233
1-20·000	,, ,,	Veratria	=	0·0269
1-30·000	of a double equiv. of	Morphia	=	0·020
1-20·000	,, ,,	Conia	=	0·00416
1-40·000	,, ,,	Nicotia	=	0·00405
1-60·000	of a double equiv. of	Quinia	=	0·0108
		Cinchonia	=	0·0102
		Quinidia	=	0·0120

The compounds formed are the hydriodates of the base with iodide of mercury; in consequence of which a part of the mercury used for precipitation remains in solution. For this reason a solution of chloride, and not of iodide of mercury must be used, inasmuch as with a solution of the latter the results very much differ; nor must the solution of alkaloid be added to the mercuric solution, but the latter to the former. These precipitates form in acid, neutral, and slightly alkaline solutions, and, as Nessler noticed,[‡] permit the separation of the bases from ammonia.

They are further not interfered with by the usual constituents of pharmaceutical preparations, except alcohol and acetic acid, in both of which the precipitates are soluble. In this the iodohydrargyrates and Groves'[§] bromohydrargyrates differ from all other precipitants of the alkaloids, which as a rule do not allow of the presence of starch, gum, albumen, or tannic acids.

As to the intensity of the reaction, it falls in some cases short of that produced by phosphomolybdic acid, while in others it far exceeds it.

Distinct reactions are obtained with iodohydrargyrate of potassium in solutions, containing—

1-2500 of Morphia,	1-150000 of Strychnia,
1-7000 of Atropia,	1-50·000 of Brucia,
1-60·000 of Narcotina,	1-125000 of Quinia,
1-8000 of Conia,	1-75·000 of Cinchonia,
1-25000 of Nicotia,	1-50·000 of Quinidia.

In mixtures with inert, colouring or other ingredients, the end of the precipitation is determined by filtration on a watch-glass, and testing the small quantity so filtered. The reaction is, however, rendered incomparably more distinct when a certain measure or bulk of solution has been prepared from the extract or mixture by means of dialysis through parchment-paper.

But where no colouring matter, or substances affecting nitrate of silver are present, the excess of iodohydrargyrate and of iodine and chlorine may at once be

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[*] "Buchner's Repertorium," vol. xxxv. p. 57. On some Precipitates caused by solutions of Hydrochlorate of Quinia and by Iodide of Mercury.

[†] "Das Verhalten der Alkaloide gegen Reagentien." Heidelberg, bei J. C. Mohr., 1846.

[‡] "Verhalten des Jodquecksilbers zu Ammoniak und eine neue Reaction auf Ammoniak. Inaugural Dissertation," Freibug, 1856. Also, *Chemisches Centralblatt*, 1856. No. 34.—*Jahresbericht der Chemie* for 1856.

[§] *Quarterly Journal of the Chemical Society*, vol. xi. p. 97. *Pharm. Journal and Trans.*, vol. xviii. p. 131. *Jahresbericht der Chemie* for 1858.

determined without filtration by the tenth normal solution of nitrate of silver, using Mohr's indicator, the neutral chromate of potassa; and where great exactness is required, and but small quantities of the substances are operated upon, the hundredth normal solution of silver should be used.

Each cubic centimetre of the tenth normal solution iodohydrargyrate requires for the separation of its iodine and chlorine 4 cubic centimetres of tenth normal, or 40 c. c. of hundredth normal solution of silver. Each cubic centimetre of the $\frac{1}{10}$th normal, and every 10 c. c. of the $\frac{1}{100}$th normal solution of silver correspond to 0·25 c.c. of $\frac{1}{10}$th normal solution of iodohydrargyrate.

The precipitate of iodide of mercury and of alkaloid, at the concentration of the test-solutions, being in no way affected by them in testing a clear solution of an alkaloid, it is therefore only necessary to add enough iodohydrargyrate to be in excess, and then the $\frac{1}{10}$th or $\frac{1}{100}$th normal solution of silver with the precautions given by Mohr, until the red colour of chromate of silver remains stationary.

For the use of pharmaceutists not in possession of metrical weights or graduates, a solution prepared and to be used with Troy weights must be of the following strength:—

16¼ grains of corrosive sublimate, and 100 grains of iodide of potassium, are dissolved in enough pure water to make up 12½ ounces Troy = 6000 grains.

Of this solution every

10 grains will precipitate 0·0534 of a grain of Aconitia.

,,	0·0289	,,	Atropia.
,,	0·0389	,,	Atropiæ Sulphas.
,,	0·0334	,,	Strychnia.
,,	0·0466	,,	Brucia.
,,	0·0538	,,	Veratria.
,,	0·040	,,	Morphia.
,,	0·050	,,	Morphiæ Sulphas
,,	0·0083	,,	Conia.
,,	0·0081	,,	Nicotia.
,,	0·0216	,,	Quinia.
,,	0·0296	,,	Quinæ Sulphas.
,,	0·0204	,,	Cinchonia.
,,	0·025	,,	Cinchoniæ Sulphas
,,	0·024	,,	Quinidia.
,,	0·0284	,,	Quinidiæ Sulphas

From a flask with this solution balanced on the scales, the test-liquor is added as long as it produces a precipitate, which is to be found by repeatedly filtering; the balance is then restored by additional weights, which express the quantity of test-liquor consumed. The results obtained are sufficiently correct for pharmaceutical purposes. When the operator has sufficient faith in his ability to carry out a more lengthy operation, he may finish the assay with a solution of silver, containing, in 6000 grains, 81½ grains of pure nitrate of silver, every 40 grains of which solution correspond to 10 grains of the Troy solution of iodohydrargyrate.

This method, like all those of volumetric analysis, precludes the presence of other alkaloids besides the one immediately concerned. It is directly applicable in solutions of a single alkaloid, or when only one alkaloid is assumed to be present. Mixtures of alkaloids will have to be parted by preparatory manipulations, unless, as in the case of the sulphates of the cinchona alkaloids, their different solubility in water or other solvents permits the direct application of the test in a certain degree.

An application of particular value to the pharmaceutist will be the testing of the strength of such preparations, the value or action of which depends on the presence of one alkaloid, or two very closely related (conia, conhydria), and about the qualitative nature of which he is not in doubt.

If such doubts exist, the alkaloid can be separated from the precipitate in the following manner:—

A small quantity of the solution of alkaloid is precipitated by the iodohydrargyrate; the precipitate is collected on a small filter, washed thoroughly with cold water, and, after draining, dissolved in the smallest quantity of boiling dilute alcohol. To this solution a drop, or more, according to the quantity of precipitate, of freshly-prepared hydrosulphate of ammonia is added, and after this a drop or two of tincture of iron, taking care to have an excess of ammonia present. The whole is then thrown on a filter, washed with hot alcohol, and the filtrate, after being acidulated with sulphuric acid, passed through animal charcoal, if necessary. It is then treated by Stas' and Otto's method for the elimination of alkaloids.

The subjoined assays of atropia and preparations of belladonna and stramonium will in part exemplify the details of the process, and also show some of the variations and changes of these preparations.

A number of the extracts of these narcotics, included among those mentioned below, have been tried in an informal manner for the dilatation of the pupil by one of the surgeons of the New York Eye Infirmary; and whenever the effect had been closely watched, the strength of the extract was found to correspond with the quantitative test or percentage of alkaloid. The dilatation of the pupil is, however, a much more delicate re-agent for very minute quantities of the mydriatics than either the iodohydrargyrate of potassium or phosphomolybdic acid.

Assay of Atropia, and Preparations of Stramonium and Belladonna.

Equiv. of atropia (Daturia) $= 289 = C_{34}H_{23}NO_6$.

,, Sulphate of atropia $= 338 = C_{34}H_{23}NO_6 + SO_3HO$.

1 c.c. $\frac{1}{10}$th solution of iodohydrargyrate = 0·0145 of a gramme of atropia.

1 c.c. $\frac{1}{10}$th solution of iodohydrargyrate = 0·0169 of a gramme of atropiæ sulphas.

1 c.c. $\frac{1}{10}$th solution of iodohydrargyrate = 4 c.c. $\frac{1}{10}$th solution of silver.

1 c.c. $\frac{1}{10}$th solution of silver = 0·25 c.c. $\frac{1}{10}$th solution iodohydrargyrate.

10 grains of Troy solution iodohydrarg. = 0·0289 of a grain of atropia.

10 grains of Troy solution iodohydrarg. = 0·0338 of a grain of atropiæ sulphas.

In order to test a solution of atropia, or a salt of it, it must not exceed 1 per cent. in strength, must be free from alcohol, acetic acid, or ammonia, in the presence of which precipitation does not take place. Nor must any soluble sulphuret or cyanuret be present, because they precipitate or decompose the re-agent before its combination with the alkaloid.

A.—The Solution of 0·5 Gramme of pure Sulphate of Atropia Dissolved to 100 c.c. in Water.

10 c.c. with 4 c.c. $\frac{1}{10}$th solution iodohydrarg. showed 12·2 c.c. $\frac{1}{10}$th solution silver, equal to 3·05 c.c. $\frac{1}{10}$th solution iodohydrarg., instead of 2·95 c.c., or 0·0517 gramme of sulphate of atropia, instead of 0·0500 gramme.

10 c.c. with 3·5 c.c. $\frac{1}{10}$th sol. iodohydrarg. showed 11·1 c.c. $\frac{1}{10}$th sol. silver, equal to 2·9 c.c. $\frac{1}{10}$th sol. iodohydrarg., instead of 2·95 c.c., or 0·0491 gramme of sulphate of atropia instead of 0·0500 gramme.

10 c.c. with 3·5 c.c. $\frac{1}{10}$th sol. iodohydrarg. showed 11 c.c. $\frac{1}{10}$th sol. silver = 0·0501 gramme instead of 0·0500.

The average of these three assays, which were the first made by this method, is 100·6 per cent., instead of 100 per cent.

The precipitation is performed in a small beaker, placed first on a sheet of dark glazed paper, while the iodohydrargyrate is being added. An excess of the latter is readily perceived from the non-increase of the precipitate, which is at first curdy, then falls together into a canary-coloured, resinous mass, which strongly adheres to the glass, and shrinks considerably before it hardens; exposed to the air it forms pearls of the appearance of pale resin. It is a hydriodate of atropia with one equivalent of iodide of mercury, $C_{34}H_{23}NO_6HI + HgI$, one-half the mercury of the test-liquor remaining in solution.

After the precipitation, the breaker is placed on white paper, some carbonate of soda, and a few drops of a solution of chromate of potassa added, and the $\frac{1}{10}$th sol. silver dropped in until the red colour of chromate of silver remains permanent after stirring.

The number of cubic centimetres of $\frac{1}{10}$th sol. silver divided by 4, and subtracted from the number of cubic centimetres of $\frac{1}{10}$th sol. iodohydrargyrate, leaves the quantity of the latter that has been consumed for precipitation. The last $\frac{1}{20}$th c.c. of $\frac{1}{10}$th sol. silver is not counted.

The solutions to be tested should be divided into at least four equal parts, in order to enable the operator to repeat the test, and thus to control it.

B.—Preparations of Datura Stramonium.

I. *Juice from the Fresh Leaves.*

36 grammes of juice from 100 grammes of fresh leaves gathered in September, during the last period of flowering.

They were diluted to 100 c.c. with the water with which the residual cake had been again pressed; then filtered. A little of the solution evaporated and calcined, and the ashes exhausted with water gave a slight precipitate, with acid nitrate of silver. It was therefore tested with $\frac{1}{10}$th sol. iodohydrarg. alone, as was done in the case of all the other preparations.

25 c.c. of the filtered diluted juice required for precipitation 1·3 c.c. $\frac{1}{10}$th sol. iodohydrarg.

25 c.c. of the same required 1·4 c.c. $\frac{1}{10}$th sol. iodohydrarg.
,, ,, 1·3 c.c. ,, ,,

For 100 c.c. therefore 5·33 c.c. = 0·0772 gramme, or per cent. of atropia in the fresh leaves.

II. *Juice from 30 Grammes of Dried leaves, expressed after Maceration with Water, Acidulated with Oxalic Acid, and Diluted to 200 c.c.; then Filtered.*

50 c.c. precipitated required 2·9 c.c. $\frac{1}{10}$th sol. iodohydrarg.
,, ,, 3·15 ,, ,,

200 c.c. therefore = 12·1 c.c. = 0·17545 gramme of atropia = 0·527 per cent. of atropia in the dried leaves.

The precipitation from the last assay, which was of a pale brownish colour, was decomposed in the manner delineated above, using oxalic acid to acidulate, evaporated very slowly nearly to dryness, redissolved in water, filtered, and again tested with $\frac{1}{10}$th sol. iodohydrarg., required 3 c.c. of the latter. This may be assumed as proof that the resinous colouring matter (pseudo toxin?)[||] which is carried down with the precipitate does not carry with it any of the iodide of mercury.

III. 50 c.c. of the above solution from dried leaves were evaporated in the water bath, to the consistence of extract, then redissolved in water to 100 c.c. and filtered.

70 c.c. of this solution required for precipitation 0·5 c.c. $\frac{1}{10}$th sol. iodohyd.

Before evaporation, 2·1 c.c.

Hence it would appear that about three-fourths of the atropia present in the original solution was driven off during the evaporation.

From an aqueous infusion of the dry leaves acidulated with oxalic acid, which, after the precipitation of the atropia still retained the narcotic odour, an acid distillate was obtained, possessing an odour of tobacco, but containing neither ammonia nor alkaloid.

IV. *Tinctura Stramonii Fol.* (By displacement.)

100 c.c. were acidulated with oxalic acid and left to spontaneous evaporation. The residue was diluted to 100 c.c. and filtered.

25 c.c. required 1·1 c.c. $\frac{1}{10}$th sol. iodohydrarg.

100 c.c. of the tincture therefore = 0·0638 gramme or weight per cent. of atropia.

V. *Tinctura Stramonii Sem.* U. S. P.

50 c.c. acidulated with oxalic acid were left to spontaneous evaporation, then diluted to 100 c.c. and filtered.

50 c.c. required 1 c.c. $\frac{1}{10}$th sol. iodohydrarg.
= 0·029 per cent. of atropia.

VI. *Extractum Stramonii Fol. Fluidum.* (By Professor Procter's formula.)

50 c.c. acidulated with oxalic acid, spontaneously evaporated, then diluted to 100 c c. and filtered.

25 c.c. required 5 c.c. $\frac{1}{10}$th sol. iodohydrarg.
= 0·59 per cent. of atropia.

(The same leaves extracted by water as under II. had yielded 0·527 per cent.)

VII. *Extractum Stramonii Fol. Alcoholicum.* (U. S. P.)

5 grammes dissolved in water acidulated with hydrochloric acid to 100 c.c.

40 c.c. required 2·6 c.c. $\frac{1}{10}$th sol. iodohydrarg. = 1·886 per cent. of atropia.

VIII. 20 grammes of the same extract rubbed up with part of 400 c.c. of water acidulated with hydrochloric acid and dialysed through parchment paper into the remainder of the 400 c.c. required fifty-four hours to give a solution of the same strength, at a medium temperature of 50° F.

IX. *Extractum Stramonii Fol. Siccum.* (Ph. Boruss. half strength.)

10 grammes diffused in dilute oxalic acid to 100 c.c. then filtered.

25 c.c. required 4·5 c.c. $\frac{1}{10}$th sol. iodohydrarg.
= 2·61 per cent. of atropia.

C.—Preparations of Belladonna.

I. *Tinctura Belladonnæ*, U. S.

30 c.c. with oxalic acid left to spontaneous evaporation, then diluted to 75 c.c. and filtered.

25 c.c. required 1·5 c.c. $\frac{1}{10}$th sol. iodohydrarg. = 0·2175 per cent. of atropia.

II. *Tinctura Belladonnæ ex herb. rec.* (Hahnemann's.)

100 cubic centimetres with oxalic acid, spontaneously evaporated, then again diluted to 100 c.c. and filtered.

25 c.c. required 3·75 c.c. $\frac{1}{10}$th sol. iodohydrarg. = 0·2175 per cent. atropia.

III. *Extractum Belladonnæ Fol. Fluidum.* (By Prof. Procter's formula.)

a. 30 c.c. of the fluid extract acidulated and left to spontaneous evaporation, then diluted to 100 c.c. and filtered.

[||] From alcoholic preparations this colouring matter is mostly chloryphyll.

20 c.c. required 4·5 c.c. $\frac{1}{10}$th sol. iodohydrarg.
= 1·087 per cent. atropia.

b. 60 c.c. of the same fluid extract, acidulated with oxalic acid, and evaporated on the water-bath, then diluted to 100 c.c., and filtered.

30 c.c. required 11 c.c. $\frac{1}{10}$th sol. iodohydrarg.
= 0·88 per cent. of atropia.

This extract, according to this assay, had lost one-fifth of the alkaloid by the evaporation.

IV. *Extractum Belladonnæ Alcoholicum* (U. S. P.), from selected leaves, showed 4·03 per cent. of atropia.

V. *Extractum Belladonnæ Alcohol* (U. S. P.), from another source, showed 3·56 per cent. of atropia.

VI. *Extractum Belladonnæ Siccum* (Pharm. Boruss. half strength), gave 0·906 per cent. of atropia.

VII. *Extractum Belladonnæ Aquosum*, from selected leaves, gave 3·26 per cent. of atropia.—*American Journal of Pharmacy.*

PHYSICAL SCIENCE.

On the Long Spectrum of Electric Light,[*] *by* G. G. Stokes, *M.A., D.C.L., Sec. R.S., Lucasian Professor of Mathematics in the University of Cambridge.*

(*Continued from page* 149.)

Metallic Lines.—These may be viewed, as already mentioned, by passing the spark of an induction coil between two electrodes formed of the metal to be examined (the secondary terminals being respectively in connection with the coatings of a jar of suitable size), forming a pure spectrum by a prism and lens of quartz, the faces of the prism being equally inclined to the axis of the crystal, and the lens being cut perpendicular to the axis, and receiving the spectrum on a suitable screen, for which, if a fluorescent liquid be employed, it is to be placed in a quartz-faced vessel, in default of which a piece of filtering-paper may be saturated with the liquid.

If the visible spectrum and the very beginning of the invisible be excepted, the lines thus seen vary from metal to metal, and therefore are to be referred to the metal and not to the air. They are further distinguished from air lines by being formed only at an almost insensible distance from the tips of the electrodes, whereas air lines would extend right across. The spectrum is far too extended to allow us to regard the whole at once as in the position of minimum deviation; and if the prism be placed at all near the electrodes, without which we should have comparatively little light to work with, the effect of the different divergency, converted by the lens into convergency, of the rays in the primary and secondary planes is very great. In order to obtain a pure spectrum, the screen must be in focus as regards the primary plane; and if a particular point P of the spectrum be at a minimum deviation, the lines immediately about P are reduced almost to points, which are the images, for light of that refrangibility, of the tips of the electrodes, or, to speak more exactly, of the part of the spark just outside the tips. But in the secondary plane the rays on one side of P have not yet reached their focus, and on the other side have passed it; so that the image of a point is a line, the primary focal line, of a length increasing on receding from P in either direction, and accordingly the spectral image of either tip, assumed to be a mere point, would be a pair of slender triangles vertically opposite, and having their common vertex at P, their lengths lying in the plane of refraction. The invisible spectrum is in fact made up of two such pairs of triangles corresponding to the two tips respectively, as may be readily seen when the electrodes are not too close. At a distance from P, at which the length of the primary focal line becomes equal to that of the image of the spark the two lines which are the images, for rays of the refrangibility answering to that distance, of the tips of the electrodes meet in the middle of the spectrum, and beyond that distance they overlap, so that a line appears to run across the spectrum, though it relates to rays which emanated only from the immediate neighbourhood of the tips of the electrodes, as may be seen by turning the prism till that part of the spectrum is at a minimum deviation, and focusing afresh.

Besides the bright lines, evidently due to metals, which have been mentioned, other weaker light is perceptible, too faint for precise observation. A portion of this is probably due to the air.

The chief part of the visible spectrum, as seen by projection, appears plainly to belong to the air; for the lines stretch across the interval separating the electrodes, while the lines belonging to the metals extend but a little way, even in the visible spectrum, and the former reappear when the electrodes are changed. With some metals, however, lines belonging to the metal appear in the visible spectrum which are comparable in strength with the invisible lines of high refrangibility; but in general it is rather remarkable how poor is the visible spectrum, and even the invisible region for a good distance beyond, compared with the part of the spectrum of still higher refrangibility, with respect to strong lines characteristic of the metal.

I have lately adopted a mode of laying down positions in the invisible spectrum, which is extremely simple and convenient, and yields results agreeing well with one another. It might be applied to the formation of maps of the metallic lines; but this is unnecessary, as the subject has been worked out by Dr. Miller. It is still useful, however, for laying down the positions of bands of absorption, being more convenient and exact than estimating their place with reference to the known metallic lines.

The method is as follows:—The quartz prism is placed on a block, raising it to a convenient height above a long drawing-board, to which the block is screwed, and is fixed at pleasure by a screw pressing upon it from above. The lens is fixed in a blackened board screwed edgeways to the drawing-board near the prism, so as to be ready to receive the rays of all refrangibilities after refraction through the prism. The focal length of the lens actually used was about 12 inches, and its diameter 1¼ inch. A convenient distance of the spark from the prism having been selected (I chose 30 inches), the drawing-board was turned round till it attained such a position that, on placing the prism in the position of minimum deviation for the middle of the long spectrum, the rays belonging to that part fell perpendicularly, or nearly so, on the lens, which had previously been placed so that this should be a convenient position relatively to the drawing-board. The prism was then fixed by its screw, and to mark the angle of incidence a pin was placed at the edge of the shadow of one of the blocks. On account of the increasing refraction by the lens of rays of increasing refrangibility, the locus of the foci of the different rays formed an arc of a curve, or nearly a straight line, lying very obliquely to the axes of the

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[*] Abstract from the *Proceedings of the Royal Society.*

pencils coming through the lens. The projection of this line on the board having been marked, a line was drawn bisecting this at right angles, and at a point in the latter line situated $11\frac{2}{3}$ inches from the former,† the board was pierced for the insertion of a pivot, which carried two wooden rulers, which could be clamped together at any convenient angle. The shorter of these carried a vertical needle, which as the ruler was turned moved in front of the focus of the different rays at the distance of about a quarter of an inch. The longer ruler carried a pricker, destined to mark on a sheet of paper, temporarily fastened to the drawing-board, the position of any object observed. Thus the prism, the lens, the axis of motion of the needle and pricker, and the pin for fixing the angle of incidence retained an invariable relative position when the drawing-board was moved. In observing, the electrodes were placed at the proper distance, and the board turned till the edge of the shadow fell on the pin. The rulers were then turned together till any bright line or other object was eclipsed by the needle, and its place was then pricked down. To obtain a fixed point of reference, I generally pricked down the position of the extreme red visible on a screen, such as a piece of paper; but if great accuracy were required, it might be better to employ a well-marked green air-line.

The metals the spectra of which I have observed are platinum, palladium, gold, silver, mercury, antimony, bismuth, copper, lead, tin, nickel, cobalt, iron, cadmium, zinc, aluminium, magnesium. Several of these show invisible lines of extraordinary strength, which is especially the case with zinc, cadmium, magnesium, aluminium, and lead, which last, in a spectrum not generally remarkable, contains one line surpassing perhaps all the other metals. Other metals exhibit lines which in certain parts of the spectrum are both bright and numerous; so that, in taking a rough view of the whole, certain parts of the spectrum are bright and tolerably continuous, while other parts are comparatively weak. This grouping of the lines is especially remarkable in copper, nickel, cobalt, iron, and tin. Of the metals mentioned, magnesium gives by far the shortest spectrum, ending in a very bright line, beyond which, however, excessively faint light may be perceived to a distance about as great as the extent of the longer spectra. Aluminium, on the other hand, stands at the head of the above metals for richness in rays of the very highest refrangibility; and it is to this part of the spectrum that the strong lines above mentioned belong. In calling these lines strong, it must be understood that some allowance is made for their very high refrangibility; for when observed as above described they do not appear *absolutely* quite so strong as the bold lines of zinc or cadmium. This is partly due to the defective transparency of quartz, which for this part of the spectrum shows itself by no means perfect; and indeed the highest aluminium line, which is a double line, can only be seen by rays which pass through the prism near its edge.

Besides the lens above mentioned, I sometimes employ in a different manner another of $\frac{1}{2}$ inch diameter and $2\frac{1}{2}$ inches focal length, and accordingly large for its focal length. This is used for forming an image of the spark, which is received on the substance that is to be examined, or that is used for examining the spark. The difference of focal length for the different rays is so enormous that, while one part of the spectrum is in focus, other parts are utterly out of focus, and thus we may judge in a general way of the refrangibility of the rays by which any particular effect is produced. In this way such concentration of the rays is obtained, that effects may be studied which would not bear examination by prismatic analysis. In speaking of this lens I shall call it the 2·5-inch lens, from its focal length.

Absorption of the Invisible Rays by Alkaloids, Glucosides, &c.—Before examining these substances, it is requisite to dissolve them, and we must first inquire into the transparency of the solvent. Fortunately, the most useful of all solvents, water, is transparent when pure; and as to reagents, we may employ sulphuric or hydrochloric acid for an acid, these acids being transparent, and ammonia, suppose, for an alkali. In speaking of a substance as transparent, I wish it only to be understood that it is of a transparency comparable with quartz. As to ammonia, although it absorbs the more refrangible rays when in quantity (unless the observed absorption were due to some impurity), it may be deemed transparent in the small quantity which alone it is requisite to employ. Even alcohol, which in the state in which it is to be had is defective in transparency, is sufficiently transparent to be employed as a solvent for such substances as those under consideration, provided it be used in small thickness only.

The alkaloids and glucosides which I have examined are almost without exception intensely opaque for a portion at least of the invisible rays, absorbing them with an energy comparable for the most part to that with which colouring matters (such as alizarine, &c.) absorb the visible rays. The mode of absorption also is frequently, I might almost say generally, highly characteristic; so that by this single property they might be distinguished one from another. It frequently happens, too, that the mode of absorption decidedly changes according as the solution is acid or alkaline, which assists still further in the discrimination.

In the examination I sometimes employ a small cell with parallel faces of quartz, sometimes a wedge shaped vessel, having its inclined faces also of quartz, but more commonly the former. The cell being filled with the solvent, a minute quantity of the substance is introduced, and the progress of the absorption is watched as the substance gradually dissolves, the fluid meantime being of course stirred up. In this way it is easy to seize the most characteristic phase of the absorption, which may be then registered by the pricking instrument. When minima of opacity occur, it is best to seize that stage of the absorption at which they are well developed. When no minima occur, a greater or less part of the more refrangible region is quickly absorbed, after which the absorption creeps on towards the less refrangible side. When once it has become tolerably stationary, the limit of the rays transmitted may be marked. It seems desirable not to go beyond this point in the absorption, lest some possible impurity in the substance examined, which if it had formed the whole of the specimen would have absorbed rays of lower refrangibility, should begin to make itself perceived, and its mode of absorption should be mistaken for that of the substance professed to be examined.

All the metallic spectra are discontinuous, which prevents the mode of absorption of even a solid or liquid from being observed quite so well as in the solar spectrum, even independently of the greater intensity of the latter, and would greatly interfere with the observation of narrow bands like those shown by the absorption of certain gases in the visible spectrum, and of which

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† A longer distance would have been better.

chlorous acid gas (ClO_4) shows a splendid system in the invisible part of the solar spectrum. Should a general absorption take place in a part of the spectrum where previously a bright group of lines was seen, with weaker light for some distance on both sides, it is evident that at a certain stage of the absorption the bright group would be left isolated, and the effect might be mistaken for a maximum of transparency. In doubtful cases of this kind it is requisite to change the electrodes, so as to use the spectrum of some other metal; but practically the difficulty is not so great as might be supposed.

It is desirable to choose a metal which gives a spectrum that is bright and tolerably continuous in the region in which the distinctive features of the absorption are most likely to occur. For general use in the examination of substances such as here considered, I prefer tin—the electrodes (or one of them at least) being broad, for a reason which will be mentioned presently. Tin, indeed, is weak in the most refrangible region, though after a long interval of weakness it shows one pretty strong line between the second and third of the strong aluminium lines; but with these substances the distinctive features of the absorption hardly ever occurs so late. For combined strength and continuity, copper answers well for the highly refrangible region in which tin is weak; while mercury, which may be employed in the form of amalgamated zinc, is the richest metal for the invisible region just beyond the visible spectrum; but I have employed tin almost exclusively.

A glance will show how distinctive is the mode of absorption of the rays of high refrangibility by these different substances. Indeed, this one character would serve to distinguish all these substances one from another, unless it be morphine from codeine, and caffeine from salicine. A solution of brucine cuts off the invisible end of the solar spectrum about midway between the lines S and T, and accordingly not far from the end of the region which it requires a quartz prism and lens to see. Accordingly, when these substances are examined by solar light their distinctive characters are almost wholly unperceived, the solutions of some appearing quite transparent, and those of others merely cutting off the extreme rays to a greater or less distance. With æsculine alone the maximum of opacity lies within the solar spectrum; but even in this case we should have little idea of the great increase of transparency about to take place.

The effect of acids and alkalies on all the glucosides referred to presents one uniform feature. When a previously neutral solution is rendered alkaline, the absorption begins somewhat earlier, when rendered acid somewhat later. With salicine there is merely an indication of this change, falling within the limits of errors of observation; but in the other cases it is quite perceptible, and with phlorizine the shifting of the band of absorption produced by an acid is very large. Fraxine (or paviine) agrees remarkably with æsculine in all its optical characters; the maximum of absorption is merely situated a little nearer to the red, and the tint of the fluorescent light corresponds to a slightly lower mean refrangibility.

Quinine presents no decided maximum of transparency. With this and the other bases observed, with one exception, the absorption, if changed at all, is changed in an opposite manner to the glucosides when the base is set free by ammonia.

Bands of absorption occur also with neutral substances, for example, coumarine and paranaphthaline, which last exhibits a system of such bands in the invisible part of the solar spectrum.

Aconitine, atropine, and solanine exhibit no bands of absorption, but merely a general opacity for the more refrangible rays. The last, indeed, when dissolved in dilute sulphuric acid, is, for this class of bodies, remarkably transparent; while when the base is set free, the solution, contrary to what takes place with the other bases, becomes much more opaque, but the absorption is vague. I am not sure, however, how far the purity of the specimen examined may be trusted, though it was white, and regularly crystallised. It would be easy to examine more such substances; but what precedes is sufficient to show the value of the study of the absorption of the rays of high refrangibility, as affording distinctive characters of substances little known.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

March 19, 1863.

Professor A. W. Hofmann, *Ph.D., F.R.S., President, in the Chair.*

In continuation of our report of this meeting, given at page 155, the fourth and last paper, "*On the Composition of some Varieties of Native Copper*," by F. A. Abel, Esq., F.R.S., Chemist of the War Department, remains to be described. The author refers to a previous communication to the Society conjointly by himself and Mr. Field,* wherein were stated the results of a series of analyses of manufactured coppers; to compare with these he had lately analysed samples of the native metal from Russia, Lake Superior, and Chili. The examination included a search for bismuth, lead, silver, tin, antimony, arsenic, and also certain metalloids. Bismuth and lead were tested for by the process formerly described, and which is based upon the precipitation of the mixed phosphates of these metals by adding a small quantity of phosphate of soda and ammonia in excess, by converting the phosphates into sulphides, and ultimately into nitrates, when they could readily be separated by digesting with hydrated oxide of copper, the bismuth alone being precipitated, and the lead in solution easily recovered in the form of chromate. The amount of arsenic in one of the specimens was considerable. For the detection and estimation of this metal recourse was had to the mode of precipitation with oxide of lead, afterwards separating the same by digestion with oxalic acid, and passing sulphuretted hydrogen through the ammoniacal solution; on adding hydrochloric acid to the filtrate the pure yellow sulphide of arsenic was precipitated. The specimen of native copper from Chili was of the description known as "Charqui copper." It was in the form of a flat slab coated on both sides with steatite, and a small quantity of chlorite, the latter being in some places peroxidised, and exhibiting variations in colour from green to red. The purity of this sample of copper was remarkable; it contained only traces of bismuth and silver. Two samples of Minnesota copper from Lake Superior were examined. The first of these was massive, and contained ·56 per cent. of silver, no other foreign metal being detected. The other sample was in the form of water-worn nodules, some of the largest of which weighed nearly an ounce, and contained as much as one-third of their bulk of pure silver occurring in thick veins. Besides this metal, a trace of lead was found in the specimen. The remaining specimen examined was a portion of the great mass, from a Siberian locality, exhibited last year in the International Exhibition. The gangue con-

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* *Vide* Chemical News, vol. iv., p. 264.

sisted of carbonate of lime in rhombic crystals, which appeared externally to have been modified by heat; they were friable, and exhibited a green colouration due to copper. The metallic surfaces in contact with the calc-spar were partially converted into suboxide of a ruby colour and crystalline. The copper itself was very impure, containing not less than 1·28 per cent. of arsenic, besides small quantities of silver, bismuth, and lead. Mr. Abel's communication concluded with an explanatory statement referring to the use of the term "cake copper" in his former paper; he wished it to be understood as meaning not "tough cake," from which the ordinary sheet and bolt copper are manufactured, and which always contains lead, but as referring to blister copper, and to "best selected," and foreign refined coppers, which commercial qualities seldom contain more than traces of this metallic impurity.

Dr. Odling announced that Mr. Crookes had detected thallium in several samples of commercial copper; in consequence of this statement, it would in future be necessary to include this element in the general scheme of examination.

NOTICES OF BOOKS.

A Dictionary of Chemistry and the Allied Branches of other Sciences. Founded on that of the late Dr. Ure. By Henry Watts, B.A., F.C.S. Longmans.

It is with much pleasure that we are enabled to announce the publication of the first part of this work, which is to be continued in monthly parts until completed. The dictionaries published by Dr. Ure have always been favourite works with those engaged in pursuits connected with chemical science, and deservedly so, for, in addition to the accuracy and extent of the information they contained, they were eminently characterised by an elegance of style and a clearness of exposition which are somewhat rare in the scientific literature of this country.

The present part of the new issue of this dictionary gives evidence of the maintenance of these important features, and it is a matter for congratulation, both to the publishers and to the public, that the task of reproducing the "Dictionary of Chemistry" should have been placed in the hands of a gentleman who combines, in so high a degree as Mr. Watts, the scientific competence, the literary ability, and the practical experience requisite for its proper performance.

It is a necessary consequence not only of the rapid extension of chemical knowledge, but also of the revolutionary influence of fresh discoveries, that there is no possibility of a treatise on chemistry becoming a classical and standard work for any considerable length of time. The ephemeral character of such works is not only a most striking illustration of the wonderful progress of the science, it is also evidence of a less satisfactory truth—the absence of any sound and well-established basis for the systematic co-ordination of the facts, the principles, and the materials furnished in such overflowing abundance by the labour of chemists.

Little more than fifty or sixty years have elapsed since the period when chemical knowledge was for the first time sufficient to admit of the formation of any settled ideas as to the elementary composition of material substances, and no sooner was this the case than there began to grow up, and to develope with astounding celerity, a branch of chemical knowlege, constituting almost a science within a science—the chemistry of organic substances. This section of chemistry is now so extended, the number and importance of the facts and principles which it comprises, are so great, and there is such an absence of any unquestionable system of classification applicable to them as a whole, that there is a serious difficulty in treating this subject in works either of reference or for the use of students. The English edition of "Gmelin's Chemistry" shows, in a striking manner, the inconveniences resulting from the present chaotic condition of organic chemistry. But there is every reason to hope that progress is being made towards a more satisfactory state of things. The new direction which the cultivation of chemical science has of late years taken, is of a kind to justify this hope. It is no longer the chief aim of chemists to acquire a knowledge of new facts, merely as such; the importance and necessity of ascertaining the *relations* of facts are now becoming generally recognised, and this conviction is exercising an important influence.

But while this path is being explored, and before the hoped-for results of the investigation have been achieved, there is a need for works which will afford information as to the existing knowledge of the science, and during such a transition state there is, perhaps, no way of treating it more appropriate than a Dictionary. One of the earliest Dictionaries of Chemistry was apologised for by its author upon the ground that "the chemistry then known was scarcely entitled to the name of a science, being little more than a collection of facts, the causes of which, and their relations to each other, are so imperfectly understood, that it is not yet capable of either the synthetic or analytic modes of explanation." This remark, which, three-quarters of a century ago, was true with regard to chemistry generally, is almost as applicable at the present time to organic chemistry.

The "Dictionary of Chemistry" now being brought out by Mr. Watts will therefore prove of the greatest service, as a repository of information, both to those engaged in the cultivation of the science, and to those whose business is its practical application. As Editor of the Cavendish Society's edition of "Gmelin," he has, during the last ten years, acquired facilities for embodying in the work the best and most recent information, greater than would fall to the share of a less practised writer; and if the number just issued may be taken as representing the whole of the work, it is certain that it will be a most valuable addition to our scientific literature.

The treatment which many of the subjects receive in this work is of course widely different from that which they received in the last edition of "Dr. Ure's Dictionary." As examples of this may be mentioned the articles on Acids, Alkaloids, Alcohols, which are most excellent expositions of subjects that have not hitherto been treated of in such a comprehensive and systematic manner in any English work. These and similar articles will be invaluable to the student.

Having thus expressed the general favourable opinion which a perusal of the first number of the "Dictionary" has produced, it is necessary to remember that it is both the duty and the privilege of critics to find fault. It is not possible to be very hard upon Mr. Watts in the present case, but it is also impossible to overlook the fact that he has adopted in this work a new system of notation, and a new scale of atomic weights. He has done so "in order that the work may, as far as possible, truly represent the present state of scientific chemistry," than which no better reason can be given for so doing. There can be no doubt, however, that Mr. Watts is aware of the fact that some chemists of eminence do not admit that a truthful representation of chemical science requires any alteration of the atomic numbers hitherto in use; and that they even go so far as to question the propriety and utility of the alteration that has been adopted by some chemists in these numbers, and consequently in the formulæ of most substances.

It is quite certain that, consistently with the remarks above made, conservatism in chemistry at the present time would be quite an anomaly; but at the same time it is equally clear that any change introduced in its doctrines should at least have good ground for recognition, even though it may not be generally accepted. In short, mere innovation should be carefully avoided, and the more convinced the advocates of a new theory may be of its superiority to the prevailing one, the more incumbent it is on them to establish and demonstrate its title to supersede the one hitherto received.

That part of the "Dictionary of Chemistry" in which this subject would be treated of has not yet appeared, so that it will suffice, without expressing any further opinion on this subject, merely to point out the necessity for a thorough elucidation of the grounds on which the new system has been adopted, and to look forward to its appearance in a future part.

There is another innovation of a less radicle nature, which was suggested by Mr. Watts some time ago, in regard to nomenclature, and which it is to be regretted he has not adopted in the "Dictionary." It is the substitution for the terms corresponding to sulphate of copper, chloride of zinc, &c., of the terms copper sulphate, zinc chloride, &c., consistently with the terms ethyl alcohol, phenyl amine, &c. There would be convenience in this change, and the names of salts would be more in accordance with the principles of grammar.

It is also somewhat to be regretted that acids are treated of as a class, inasmuch as this course seems, to a certain extent, inconsistent with the modern views to which Mr. Watts evidently leans. The old application of the term "acid," as representing a class of substances, is no longer practicable. No definition can be given of acids that is applicable to all the substances which, for other reasons than their characters of acidity, would fall within this class. The use of the term as that of a class is, in fact, a remnant of the old doctrine of specific principles. Acids were united in a class because they were supposed to contain a principle of acidity. Now, that substances are known which, in their chemical relations, are analogous to acids, but which are not acid, the basis of the classification disappears in this and in other particulars; and those substances which, in the most marked degree, present the characters formerly attributed to the class, come under the class of salts, as salts of hydrogen.

Consequently, the definition of acids given at the head of the article Acids must be regarded as having a value more historical than actual; and, in like manner, the definition of alkalies, as being the antithesis of acids, must also be limited in its application to a past state of chemistry. In other respects the sketch of the historic chemistry of acids, alkalies, &c., at the commencement of these articles, is most excellent; and it is to be hoped that the plan of tracing in this way the development and origin of chemical ideas may be adhered to in subsequent parts of the "Dictionary."

In conclusion, it may be desirable to mention that Mr. Watts is assisted in the production of the "Dictionary" by a staff of very able contributors, that the printing and paper of the work are unexceptionable, and that there is no useless sacrifice of space to mere decorative illustrations.

NOTICES OF PATENTS.

Grants of Provisional Protection for Six Months.

399. John Cronin Jeffcott, Anglesey Street, Cork, "Improvements in the production and generation of gases, and also in apparatus connected therewith."—Petitions recorded February 14, 1863.

457. William Trustrum, Marlborough Road, Old Kent Road, Surrey, "Improvements in the manufacture of oiled silk."—Petitions recorded February 19, 1863.

475. Edward Thomas Hughes, Chancery Lane, London, "Improvements in the treatment of colouring matters derived from tar for the purpose of making them applicable for painting."—A communication from Barthélemy Dupuy and Antoine Vibert, Lyons, France.

477. Alfred Henry Remond, Moorgate Street, London, "Improvements in preserving provisions, and in the apparatus employed for such purpose."

479. William Wood, Monkhill, near Pontefract, Yorkshire, "Improvements in the process of manufacturing Pomfret or liquorice cakes, rolls, sticks, and pipes, and other similar articles of confectionery."

521. William Readman, Glasgow, Lanarkshire, N.B., "Improvements in the manufacture of carbonate of magnesia, and of iodine and kelp salt, and other products from kelp."

541. Astley Paston Price, Lincoln's Inn Fields, London, "Improvements in the production and manufacture of blue colours."—A communication from Augustus Eisenlohr, Heidelberg, Baden.

543. Peter Spence, Newton Heath, Manchester, "Improvements in the manufacture of potash, alum, and other salts of potash."

579. John Watson Burton, Leeds, "An improved mode of refining and purifying oils."

592. George Davies, Serle Street, Lincoln's Inn, London, "Improvements in polishing or giving a lustre to soap, and in the apparatus employed in such process."—A communication from Adolphe Dupuis, Paris.

606. Thomas Henry Morrell, Leyland, Lancashire, and Joseph Williamson, Willcross, Gisburn, Yorkshire, "A new or improved method of purifying the noxious vapours or gases given off from night soil or other similar substances during the heating, drying, or evaporating of such substances."

607. Edward Alfred Wunsch, Glasgow, Lanarkshire, N.B., "Improvements in treating sea-weed, and in apparatus therefor."

611. William Clark, Chancery Lane, London, "Improvements in the manufacture of sulphuric acid, and in apparatus for the same."—A communication from Louis Désiré Verstraet and Charles Schmid, Boulevart St. Martin, Paris.

Notices to Proceed.

3021. Edward Sonstadt, Stewart Place, Alfred Street South, Nottingham, "Improvements in the manufacture of the metal magnesium."

395. Jules Albert Schlumberger, Golden Square, London, "Improvements in treating coal tar dead oils, and for producing phenic or carbonic acid."—A communication from Jean Jaques Muller, Bale, Switzerland.—Petition recorded February 13, 1863.

3069. Samuel Roberts, Sheffield, "An improvement in frames for containing stoppered bottles and jars."

162. Richard Archibald Brooman, Fleet Street, London, "Improvements in the manufacture of sulphate of soda for conversion into soda and other uses."—A communication from Germain Adolphe Thibierge, Versailles, France. —Petition recorded January 19, 1863.

412. John Morgan, Stephen's Green North, Dublin, "Improvements in embalming and preserving from decay human bodies and bodies of other animals, also pickling, curing, and flavouring animal bodies."—Petitions recorded February 14, 1863.

517. Frederick Albert Gatty, Accrington, Lancashire, "Improvements in printing and dying cotton and other fabrics."

CORRESPONDENCE.

Deaths from Nitric Acid.

To the Editor of the Chemical News.

Sir,—My former letter having drawn a reply from another of your correspondents, I cannot let it pass without saying a few more words on the above subject. What struck me as being remarkably strange was, that the parties at Edinburgh experienced no difficulty of breathing until an hour or two after the occurrence of the accident. It was this that prompted me to write you. In every case that I have known of persons inhaling the fumes of acids, difficulty of breathing, when it did take place, occurred immediately afterwards. However, I think I can show that whatever accidents are caused by this and other acids result from gross carelessness, or otherwise absence of mind. It is passing strange that persons well acquainted with the nature of this gas should be the first to suffer from the effects of inhaling it.

Admitting the injurious effects of inhaling these fumes, how can one account for the fact that of all the cases I know of, not one has exhibited alarming symptoms? I could give singly cases without number which have come under my notice. I will, however, take the whole of them together. The men I have known to inhale the fumes of this and other acids must certainly have swallowed a quantity, for they have been confined for several minutes in a perfect atmosphere of gas. Perhaps, however, they did not gape and gulp down wholesale the noxious vapour, which appears to have been the case with several persons. I have noticed the men cough slightly on leaving the not-to-be-desired air. I also noticed that each generally took a draught of water or some other kind of drink, and not once did I hear of any of them being attacked by pains of any description. These are facts, and they were not of uncommon occurrence; indeed, they took place day by day for many years, and therefore I think I am warranted in saying what I have done. The idea of Mr. Stewart and the janitor undertaking to wipe the floor and to endeavour to save a portion of the fluid, is to me very preposterous. Stooping, as they must have done, they would catch most of the rising fumes, and if they used a rag or other such article matters would be ten times worse. They could not have been aware that the gas might prove fatal, or if so they were exceedingly thoughtless. Such accidents as these would never have taken place had a little forethought and care been exercised.

I did not mean to proclaim that the nitrous fumes were inert. There is moderation in this as well as in other things. It is to be hoped that the sad occurrence at Edinburgh will be the means of making persons so employed use a little more precaution over such matters.

Apologising for intruding upon your valuable space,
I am, &c. J. H. Swindells.

Meadows Bridge, Wigan.

To the Editor of the Chemical News.

Sir,—On reading in your Journal a notice of the deaths at Edinburgh of Mr. Stewart and his assistant, I was forcibly reminded of a similar lamentable case which occurred some years ago to a friend of mine, Mr. James Heywood, Lecturer on Chemistry at Sheffield. His death was occasioned by the breakage of a carboy of mixed acids, nitric and sulphuric, which he used for exciting some voltaic batteries. During the act of pouring out into a jug the acid, he slipped and fell, the glass vessel broke, cutting his hand severely. The pupil who assisted him had his clothes burnt; he was quickly taken to a pump; he did not die. But poor Heywood went home; in a short time his feet became very cold, he felt an increasing difficulty in breathing, and died in the night.

The medical witnesses found a congestion of the lungs, which they attributed to his having inhaled the vapours of the acid.

This explanation is, I think, not the right one, for the circumstance of the cut on the hand was quite overlooked; and yet in that accidental cut we have the real cause of the mischief. We know that nitric acid coagulates blood powerfully. In the present case the coagulation would be propagated by the natural circulation of the blood, and would finally pervade the whole mass of the blood, congestion of the lungs being one of the results of this coagulation.

A strange corroboration of the powerful effect of nitric acid upon the blood, even in minute quantities, happened a short time after, also at Sheffield.

The wife of a workman was suffering from toothache, her husband, with the view of curing or of alleviating her pain, placed two or three drops of nitric acid in the hollow of the tooth, thinking, as he expressed it, to kill the nerve; the consequence was the death of the woman.

Other similar cases, I make no doubt, will be found by medical men supporting the view here put forward, of the dangerous effects of nitric acid upon the mass of the blood when in contact with it.

Now, in the cases of Mr. Stewart and his companion, I feel assured that a wound must have been made during the breakage of the glass; if such wounds, thus produced, are or were found on the bodies, it will at once render the cause of death evident.

The experience of men employed in the manufacture of nitric acid is quite contrary to the idea that the mere breathing of the fumes of this acid for a short time could produce death. I am, &c.
F. Trachsel,
Analytical Chemist and Engineer.

Manchester.

To the Editor of the Chemical News.

Sir,—Some years ago, after experimenting for some time with Callan's battery, I was attacked with a sort of pleurisy, which the doctor (Mr. Nicholls, of Savile Row) attributed to the fumes from the battery. I am, &c.
John S. Blockey.

To the Editor of the Chemical News.

Sir,—I fully agree with your correspondent, Mr. Swindells, as to the lamentable characters of the accident at the Edinburgh Institution, by which the lives of Mr. Stewart and his assistant were sacrificed.

Sad experience, however, makes me differ entirely with him as to the occurrence being singular; and, if not more careful, it would not surprise me to hear of his being the next victim to the deadly power of nitrous acid gas.

The following analogous case in my own works may serve as a warning to him and others who might by his letter be thrown off their guard. Last October at my works at Goole, in Yorkshire, it became needful to clean out a vitriol chamber for repairs. It had been off work for ten days, and was thought to be well ventilated. Some men were sent in, and while cleaning out the accumulated sulphate of lead, nitrous fumes arose freely. The men naturally complained, and my manager, a most worthy and faithful man, whose only fault was a reckless determination always to do his duty whatever the consequences, went in along with them, and continued longest exposed. He came out apparently none the worse, sat down and wrote me his daily letter, in which he detailed the event, merely saying that it was not pleasant. He went home, took tea, dressed, and actually went to a meeting in the town, where he was seized with Mr. Swindells' symptoms of difficulty in breathing, and for twenty-four hours this continued with varying symptoms, followed by violent inflammatory action of the brain, and in about forty hours after the exposure he died. One of the men who had been exposed for a shorter time after a similar attack died the following day. No gas is I am convinced more dangerous, its worst feature being that an exposure deadly in its ultimate result may give its victim no particular uneasiness for some time after he has inhaled a fatal dose. As I write this as a warning to chemists, I hope you will excuse its length.

I am, &c. Peter Spence.

Pendleton Alum Works, Newton Heath, Manchester.

Notices of Patents.

To the Editor of the Chemical News.

Sir,—In reference to the patent of Mr. Gerland for producing sulphate, &c., of copper by dissolving malachite, the process is old. In 1858, Mr. John Dale, of Cornbrook, Manchester, suggested that it was a more profitable mode of manufacture than any he knew, and likely to prove more practical than an invention I introduced to him. I have myself made sulphate of copper by this plan in the same year; but the difficulty of obtaining a large and constant supply of the carbonate prevented my continuing the process.

As to the patent of Mr. Balmain, the economisation of spent bark, &c., is old as far as the pyroligneous compounds are concerned; and as to the obtaining potash therefrom, I have tried the experiment myself in connexion with a large wood-acid factory, and came to the following conclusion:—

That to wash the charcoal spoils it, as it is impracticable to dry it again at a reasonable cost; and that the potash cannot be effectually recovered from charcoal by washing. I have treated charcoal with water, and no potash was extracted; but on burning the charcoal to an ash, the ashes contain soluble potash or its salts.

I am, &c. John S. Blockey.

Soda in Coal.

To the Editor of the Chemical News.

Sir,—I have just seen in the number of the Chemical News for March 28, an article by Mr. E. S. Wayne on the presence of "Soda in Coal." Without in any way wishing to detract from the merit of that gentleman's discovery, I consider it due to my friend Mr. John Horsley, F.C.S., analyst for the county of Gloucester, to state that as I have long been engaged in palæontological studies, Mr. Horsley consulted me last summer on the various opinions extant regarding the locustime and marine character of the plants forming the carboniferous flora, and to know whether soda had yet been found in the ashes of coal.

Mr. Horsley at the same time showed me a specimen of that alkali which he had obtained by operating upon 500 grains of coal ash. The solution of this substance had a powerful reaction on test-paper, and did not produce a crystalline precipitate; on the addition of the bichloride of platinum the dry powder imparted to flame the usual yellow colour indicative of soda.

Mr. Horsley has therefore the merit of priority in the discovery of soda in coal, although he has not yet published his analysis of coal ashes.

I enclose a specimen of this alkaline powder given me by Mr. Horsley, and extracted by him from coal ashes, and which may contain other matters besides soda if it was submitted to further analysis.

I am, &c. Thomas Wright, M.D.

St. Margaret's Terrace, Cheltenham.

MISCELLANEOUS.

International Exhibition.—We are informed that the report on Class 2, Section A, is now ready, but that it has been decided not to issue it in a separate form like the other reports. We hope the Commissioners will be induced to reconsider this decision, the impropriety of which must be obvious to every one but themselves. Why should the chemical exhibitors be mulcted in the price of a volume any more than the exhibitors in other classes?

Royal Institution of Great Britain. — The following are the probable arrangements for the Friday evening meetings after Easter, to which members and their friends only are admitted:—

Friday, April 17.—Frank Buckland, Esq., M.A., "On the Culture of Fish."

Friday, April 24.—Alex. S. Herschel, Esq., F.C.S., "On Luminous Meteors."

Friday, May 1.—John Leighton, Esq., F.S.A., M.R.I., "On Japanese Art," with native illustrations.

Friday, May 8.—Professor Voelcker, Consulting Chemist of the Royal Agricultural Society of England, "On the Chemical Properties and Productive Powers of the Soils of England."

Friday, May 15.—W. Odling, Esq., M.B., F.R.S.

Friday, May 22.—Professor Roscoe, F.C.S., "On the direct Measurement of the Sun's Chemical Action."

Friday, May 29.—Professor Max Müller, "On the Vedas, or the Sacred Books of the Hindus."

Friday, June 5.—John Ruskin, Esq., "On the Forms of the Stratified Alps of Savoy."

Friday, June 12.—Professor Tyndall, F.R.S., M.R.I.

The following are the lecture arrangements after Easter, 1863:—

"On Animal Mechanics;" two concluding lectures. By Professor Marshall, F.R.S. On Tuesdays, April 14 and 21, at Three o'clock.

"On Sound;" seven lectures. By Professor Tyndall, F.R.S. On Tuesdays, commencing April 28, at Three o'clock.

"On the Relations of Geology with Allied Sciences;" nine lectures. By D. T. Ansted, Esq., F.R.S. On Thursdays, commencing April 16, at Three o'clock.

"On the Science of Language." (Second Series.) Six concluding lectures. By Professor Max Müller. On Saturdays, commencing April 18, at Three o'clock.

"On Electric Telegraphy;" three lectures. By Professor William Thomson, F.R.S. On Saturdays, commencing May 30, at Three o'clock.

SCIENTIFIC SOCIETIES.

MEETINGS FOR THE ENSUING WEEK.

April 6. *Monday.*

Entomological—12, Bedford Row. 7 p.m.
Medical—32a, George Street, Hanover Square. 8.30 p.m.
Royal Institution—Albemarle Street. 2 p.m. *General Monthly Meeting of the Members.*

7. *Tuesday.*

Civil Engineers—25, Great George Street, Westminster. 8 p.m.
Pathological—53, Berners Street, Oxford Street. 8 p.m.
Photographic—King's College, Strand. 8 p.m.

8. *Wednesday.*

Society of Arts—John Street, Adelphi. 8 p.m. Edwin P. Alexander, "*On the Sewing Machine: its History and Progress.*"
Graphic—Flaxman Hall, University College. 8 p.m.
Microscopical—King's College, Strand. 8 p.m.
Literary Fund—4, Adelphi Terrace. 3 p.m.
Archæological—32, Sackville Street. 8.30 p.m.

10. *Friday.*

Astronomical—Somerset House. 8 p.m.
Archæological Institute—26, Suffolk Street, Pall Mall. 4 p.m.

11. *Saturday.*

Royal Botanic—Inner Circle, Regent's Park. 3.45 p.m.

ANSWERS TO CORRESPONDENTS.

Barium wishes to know where he can get caustic baryta in quantity.

Received.—A Reader; Cavendish.

Norwood Earle.—See Chemical News, No. 167.

C.R.—1. Our time is too much occupied to allow us to make the examination. 2. We know of none. 3. If there were, they might be easily distinguished by the different reactions of chlorine and iodine. 4. We think so.

Erratum.—Article "Potash," Balmain's patent, page 143, *for* "Larberg" *read* "Lorberg."

THE CHEMICAL NEWS.

Vol. VII. No. 175.—April 11, 1863.

PREPARATION OF ALCOHOL FROM COAL GAS.

When we first saw in the newspapers the announcement that "a young French chemist had discovered a process by which alcohol could be made from coal gas with an economy of 60 per cent. over the methods now in use," we confess we felt proud of being connected, however humbly, with a science capable of such marvels.

Knowing what had been done for the chemistry of the alcohols by men bearing such names as Hennell, Liebig, Dumas, Gerhardt, Wurtz, and Berthelot, not to mention a host of others, our feelings gradually merged into surprise that it should be reserved for an unknown name to acquire such lustre at one leap. Nevertheless, so authoritative was the announcement that we did not question the fact, but patiently awaited the unveiling of the mystery. Our readers will guess our chagrin when we found that the process was not only not new, but that it was only an attempt to apply in practice the method foreshadowed by Hennell and realised by Berthelot. That it was, in fact, to hydrate olefiant gas by the agency of sulphuric acid.

That sulphuric acid is capable of fulfilling the paradoxical functions of hydrating olefiants, and yet of dehydrating alcohols, according to the conditions of the experiment, we at once admit; and we doubt not that by operating on a sufficiently large scale, alcohol might be prepared by a modification of Berthelot's process in quantity; but that it could be done with an economy of 60 per cent. over the methods at present in use we do not for an instant believe.

Unfortunately the process is not one the economy of which can be tested with the ordinary resources of a laboratory, and we can only therefore found our judgment upon the evidence afforded by the published specification.

M. Cotelle, the patentee, employs several modes of producing intimate contact of coal gas with the sulphuric acid. The first is by means of a pump to discharge the acid in the form of rain into a leaden receiver containing the gas, the second is a more complex method of operating, but on the same principle. The third is to procure absorption by pressure, as in preparing soda-water, and the fourth consists in placing the sulphuric acid in the form of vapour in contact with the gas. The details cannot be given without engravings, and we must therefore refer those of our readers who desire to study the process to the patent.*

The patentee assures us that "except losses which can never be prevented in great concerns, the sulphuric acid that is employed may serve almost indefinitely." To carry out this, it is proposed after diluting the acid which has combined with the olefiant gas, to distil it to obtain the alcohol, and then to concentrate the diluted acid to the strength required to enable it to act on fresh quantities of gas.

We do not for a moment deny that by proper management of the absorptive apparatus the olefiant may be dissolved in the acid, but we greatly doubt whether more than a very small portion of the dissolved gas becomes converted into alcohol on subsequent dilution. Moreover, we conceive that on concentrating the acid much loss will be incurred, owing to formation of sulphurous acid from the carbonaceous matters, which are, we think, sure to remain with the acid.

The cost of the fuel required to concentrate the highly diluted acid to a state fit for operating on fresh gas will also be considerable.

When we consider also that it will be necessary to have pumps unceasingly at work with sulphuric acid of the strength known as pan acid, and of a density therefore of about 1·767, it must be admitted that "wear and tear" of machinery will be a not unimportant item in the expenditure of the "Company Cotelle."

Taking the above objections only, and disregarding the prejudice which will certainly be felt against the employment of coal gas spirit for anything but manufacturing processes, we must admit that we have no faith in the practical utility of M. Cotelle's patent. The specification, moreover, shows no ingenuity; the modes of procuring contact between the acid and the gas are merely those which would suggest themselves to any one giving even a very small amount of attention to the subject, and they constitute the only part of the patent upon which M. Cotelle had the opportunity to exercise his talents, inasmuch as the chemical part of the process had been done previously by others.

Some of our friends have felt alarmed lest to their numerous delinquencies wine merchants should add yet another—the sophistication of wines and spirits with M. Berthelot's hydrated olefiant gas. Let them be tranquil, the chances of such an adulteration being profitable, at least for some years, are very remote.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Composition of Gas-refuse,

by Dr. T. L. Phipson, *F.C.S., &c.*

It has been estimated that one ton of Newcastle coal gives off in distillation as much cyanogen as is contained in five to eight pounds of Prussian blue. As sulphuretted hydrogen is disengaged at the same time in greater or smaller quantities, according to the nature of the coal, it is natural that sulphocyanides should form also. My attention having been lately directed to gas-refuse obtained after eliminating the sulphuretted hydrogen and carbonic acid by hydrated oxide of iron and lime purifiers, I found in this substance certain

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* 1862, July 19, No. 2062.

sulphocyanides, and it occurred to me that it might prove a source of sulphocyanide of ammonium for photographic purposes. But the samples I have hitherto examined, having been used as long as possible, with a view of obtaining the maximum amount of sulphur, have not promised so much sulphocyanide as I should expect to find in the refuse purifiers which had not been employed for so long a period. It is curious to note, however, that this substance, which is offered to manufacturers of sulphuric acid on account of the sulphur it contains, has also been recommended to makers of artificial manures and to agriculturists as a cheap source of nitrogen! It will be seen by the analysis I subjoin that the employment of this substance for agricultural purposes is not only useless, but may become highly injurious to any soil. Besides the cyanogen compounds I have found in it, and which must be considered as prejudicial to vegetation, the existence of tar-products, highly antiseptic, and therefore capable of preventing organic decomposition in the soil—a process indispensable to vegetation—is evident; it contains also a very large amount of free sulphur.

When this gas-refuse has been exposed for some time to the air it contains the following substances:—Free sulphur in considerable quantity, oxide of iron, carbonate of lime, cellulose in small quantity, some hydrocarbons soluble in alcohol, double cyanide of iron (green), ferrocyanide of iron (blue), sulphocyanide of calcium, sulphocyanide of ammonium, chloride of ammonium, sulphate of lime, ferrocyanhydric acid (to which the mass owes its acid reaction), and water. Some of these are present in small quantity only, but it is not difficult to put them all in evidence. A rough analysis of the whole has given me—

Water	14·0
Sulphur	60·0
Organic matters insoluble in alcohol . .	3·0
Organic matters soluble in alcohol: sulphocyanide of calcium, chloride of ammonium, hydrocarbons, &c.	1·5
Clay and sand	8·0
Carbonate of lime, oxide of iron, &c. . .	13·5
	100·0

Hot water extracts sulphocyanide of calcium and ammonium, sulphate of lime, and ferrocyanhydric acid. The solution takes a red colour with persalts of iron.

Hydrochloric acid dissolves a considerable amount of the substance, and the solution obtained is deep blood-red, almost opaque, from the presence of sulphocyanide of iron formed.

Alcohol extracts principally sulphocyanides of calcium and ammonium, chloride of ammonium, a small quantity of hydrocarbons, and ferrocyanhydric acid.

By evaporating the aqueous solution to dryness, after having added enough carbonate of potash to neutralise its acidity, and treating the residue by alcohol, the ferrocyanhydric acid is left behind as ferrocyanide of potassium, and the alcoholic solution contains only chloride of ammonium and sulphocyanides of calcium and ammonium. By evaporating in presence of an excess of carbonate of potash to complete dryness, and treating with alcohol, the latter takes up principally sulphocyanide of potassium, and by using carbonate of ammonia, in place of carbonate of potash, the alcoholic solution consists of sulphocyanide and chloride of ammonium, which would serve for fixing photographic proofs.

The green compound which forms when gas-refuse is exposed to the air, is no other than the double cyanide of iron, $FeCy + Fe_2Cy_3$, discovered by Pelouze, the composition of which corresponds to magnetic oxide. By prolonged oxidation in the air it becomes blue; cold acids have no action on it, but hot nitric acid decomposes it.

TECHNICAL CHEMISTRY.

The Utilisation of Seaweed.

When we consider the immense quantity of marine algæ continually thrown up on our shores, and the comparatively small extent to which these plants are utilised, we are compelled to admit that they can scarcely as yet lay claim to a higher title than that of "weeds;" but we hope to show that the time is not far distant when these plants will be as much esteemed for their utility as some of them are already admired for their beauty.

Not that the practical uses of seaweed are totally neglected, for these plants have furnished food to mankind in various parts of the world. In England the demand is small, and the only kind consumed in any notable quantity is the *Porphyra luciniata*, which is pickled with salt, and supplied to the London market under the name of "laver." Other kinds are eaten in China, Iceland, the islands of the Archipelago, Valparaiso, and Australia, and in some parts of Great Britain certain varieties are eaten as condiments. The Chinese, however, are the only people who can really be considered as consumers of seaweed. It might probably be introduced with considerable advantage as a substitute for lime-juice, as that remedy is supposed to owe its anti-scorbutic properties to the presence of potash, a substance which enters largely into the composition of all seaweeds. Cattle, also, are fed on seaweed in New Zealand, Van Dieman's Land, and the Hebrides. That apparently paradoxical Chinese delicacy, bird's-nest soup, owes its origin to the fact that a certain kind of swallow employs a seaweed in the construction of its nest. Seaweeds have been extensively employed as manure on several parts of the coast, but although they contain the essential ingredients of a most valuable class of manures, yet their great bulk so much increases the cost of transport as to render their use unprofitable at any considerable distance from the coast.

Although these applications of seaweed may appear somewhat varied and extensive, yet they sink into insignificance when compared with the possible *industrial* applications of these plants, or even with those already actually made.

The only industrial application (properly so called) that seaweed has as yet met with, consists in burning it to obtain "kelp," which is a fused vitreous mass with charcoal, sand, stones, and other impurities largely diffused through its substance; its principal constituent, and, indeed, its only valuable one, being the mixture of salts forming the ash and fused together by the heat evolved in burning the weed itself.

That well-known substance, soda, or, more properly carbonate of soda, was formerly obtained exclusively from barilla, the ash of a marine plant (a species of *salsola soda*), which was grown in Spain and other parts solely for the sake of its ash. About the middle of the last century it was discovered that soda and some other substances could be obtained from the ashes of many other marine plants; this discovery raised the price of kelp to as much as 22*l.* per ton, but the duty having been soon after taken off foreign barilla, the price of kelp fell to 3*l.* per ton. The final blow to the manufacture of soda from kelp was given by the discovery of a process for obtaining this valuable substance from common salt. The great uses of soda are in the manufactures of glass and of soap, besides other miscellaneous uses, for cleansing, &c., so that any process for reducing the cost of its production was a most valuable discovery.

Although soda could no longer be profitably extracted from seaweed, yet kelp was not destined to remain quite unused, and the discovery (by M. Courtois, in 1812) of iodine, and of a process for extracting that substance from kelp, gave a new impetus to this branch of manufacture. The use of the new body was for some time almost entirely confined to medicine, but the discovery of photography opened a new and extensive field to the applications of iodine; for in all the rapid photographic processes, iodine and silver are the most important agents, as it is to a combination of these substances that the photographer is indebted for his most sensitive preparations.

It is curious to remark, by the way, that long before iodine was applied in medicine, the natives of parts of South America were in the habit of chewing the stalks of some kind of seaweed for the prevention of goître, and that in our own country sponge was used in similar diseases. Both these specifics owe their efficacy to the presence of iodine in their composition, and it is in glandular affections that iodine finds its chief medicinal application. A well-known parallel case is that of Peruvian bark, which was used by the natives as a cure for ague long before scientific men had heard of the remedy, or had discovered its active principle—quinine.

Kelp yields other substances besides iodine, such as salts of potash, common salt, and sulphur; but iodine is at present the principal product obtained from kelp, and therefore from seaweed. This being the case, it seems almost incredible that the processes for obtaining this most important body should be conducted with such bad economy that, out of the whole quantity of iodine contained in a given weight of seaweed, rather more than one-half is entirely wasted, and that no less than eight other valuable products (so valuable as collectively to eclipse even iodine) are also entirely lost.

We owe this valuable discovery to the acumen of Mr. E. C. C. Stanford, F.C.S., a gentleman who has spent years of patient research into the properties of different varieties of seaweed. Mr. Stanford, however, does not confine himself to detecting the disease, but, like a skilful physician, points out, at the same time, the remedy; for, by a process he has invented, he extracts from the weed not only *all* the iodine, but also all the other more valuable products before referred to.

We gather from these researches the following table, showing all these products, and the approximate amount of each that can be obtained from 1000 tons of dry weeds:—

Volatile oil	1,810	gallons.
Paraffin oil	2,250	,,
Naphtha	1,020	,,
Gas	1,000,000	cubic feet.
Charcoal and ash	335	tons.
Acetate of lime	5	,,
Iodine	3,260	lbs.
Sal ammoniac	25	,,
Sulphate of potash	20	,,
Chloride of potassium	50	,,
Sulphate of soda	160	,,

Some idea may be formed of the importance of the additional products if we briefly notice the principal uses to which they are put.

The first on the list (volatile oil) is a great source of benzol, from which the new aniline dyes, magenta, &c., now so extensively used, are obtained; benzol also in some cases replaces turpentine, which latter article has greatly risen in price since the commencement of the American war. Benzol Collas is a popular name for benzol.

The rapidly increasing use of paraffin oil needs no comment; suffice it to say that the oil obtained from this source is perfectly safe, as all the more volatile parts will have been driven off.

A very important application of the third substance, naphtha, is as a solvent for shell-lac in the manufacture of hats.

The acetic acid evolved during the process is converted into acetate of lime, and is afterwards extracted as vinegar, and meets with the numerous applications to which that article is put. Sal-ammoniac is extensively used for soldering, and, after being converted into sesquicarbonate of ammonia, for smelling-salts, for mixing in cakes, extracting grease from cloth, &c. The sulphate of soda is particularly valuable for the manufacture of carbonate of soda, as it is one step nearer that product than is common salt, from which, as we have said, carbonate of soda is obtained. The salts of potash are extensively employed in the manufacture of alum and of saltpetre. Of the uses of iodine we have already spoken, and of those to which the gas, charcoal, and ash can be put we shall have occasion to speak hereafter.

The only substances at present obtained from seaweed are the last six given in the above list; and the most valuable of these, iodine, is, as we have already said, obtained in only one-half the quantity, so that the whole value of the products *at present* obtained from 1000 tons of weed is about one-half the value of the products obtained by the new process.

In order to explain how this enormous difference of results occurs, it will be necessary to describe briefly the present mode of procedure, and then to compare it with Mr. Stanford's process.

"Kelp-burning," as the process of burning seaweed is commonly called, is carried on in the western and northern isles of Scotland, in the north-west of Ireland, and in Guernsey. The weed, after being collected by dragging it from the surf, or gathering together the masses left by the tide, is more or less dried by exposure to the sun and air, and is then burnt on the beach in the most primitive manner. As may be expected from the highly inflammable nature of some of the constituents of seaweed, the heat evolved during this process is intense, and as the iodine salts are very volatile at so high a temperature, a large proportion of them is driven off and entirely wasted. All that remains after the combustion is completed is the kelp, a fused vitreous mass consisting of about half the iodine salts, nearly all the more stable salts (chlorides of potassium and of sodium), mixed with sand, stones, charcoal, and insoluble ash. In fact, it is no exaggeration to say that kelp-burning is a process in which four of the more valuable constituents of seaweed are used as fuel to produce such a temperature as to volatilise and entirely waste one-half of the most valuable product of all.

The kinds of kelp richest in iodine are, of course, those produced from weed burnt at a lower temperature, the Irish kelp being one of the best; the Guernsey kelp is more valuable still, but the peculiar laws of this island throw unusual difficulties in the way of the manufacturer. About 10,000 tons of kelp, produced by the burning of 120,000 tons of weed, and worth on an average 40,000*l*., are annually sent into the market, but this quantity of weed, large as it appears, sinks into insignificance when compared with the enormous masses thrown up on our coasts.

On leaving the shore the kelp passes into the hands of the manufacturer, and the soluble salts are extracted by placing the mass in tanks of water. The solution obtained by this process (called "lixiviation") is then heated, and, when a certain amount of water has been evaporated, the sulphate of potash and the common salt (chloride of sodium) are successively deposited in crystals and collected separately. Sulphuric acid is then added to the liquid, and by its agency the sulphides are decomposed, and sulphur is "thrown down," and is afterwards collected as a secondary product. The liquor decanted from the deposited sulphur now arrives at the last process, which consists in heating it with binoxide of manganese, by which the iodine is driven off in the form of vapour, and is received in earthen vessels.

Such is the process at present employed (with but slight variations) by all "kelp-lixiviators," and, as a process for extracting the required substances from the kelp, but little improvement could be introduced; the mistake lies, as the reader will have perceived, in burning the weed, the waste of the iodine and potash salts, and the total loss of the combustible constituents, of which we have before taken notice, are not the only evils resulting from this proceeding, other disadvantages of considerable importance also ensue.

First. The high temperature at which the combustion is conducted causes certain reactions between the different constituents of the seaweed, which necessitate a large expenditure of sulphuric acid to reduce the products to the required condition. This addition of sulphuric acid produces a copious evolution of that noxious gas, sulphuretted hydrogen, and is besides very costly in its application. In fact, it has been estimated that one-half the whole cost of working one ton of kelp is due to this expenditure of sulphuric acid.

Secondly. The extremely rough mode of burning the weed in a clay-pit or in heaps on the beach, involves a great admixture of clay, sand, or stones. Kelp has been found to be adulterated with such undesirable materials to the extent of 50 per cent.

Thirdly. Incredible quantities of weed of the richest description are allowed to go to waste in the winter; for it is precisely at that season that storms tear up the most valuable kinds of weed, but there being no mode of drying or storing, this valuable harvest remains ungathered.

Fourthly. Kelp-burning produces dense smoke and offensive odours, and is conducted on desolate shores necessarily far from the lixiviating factory; the means of transit are perhaps inadequate and expensive, and the kelp is deteriorated by exposure to the weather during its long journey. The kelpers, too, are but poorly remunerated for their severe and arduous toil.

Mr. Stanford's admirable process strikes at once at the root of all the evils we have referred to as being inseparable from the system of burning the weed. The principal feature in the process seems to be the distillation of the weed in closed vessels. By this mode of dealing with the raw material all the iodine can be retained, the carbon will not be allowed to act upon the sulphates of potash, soda, &c.; all waste of weed will be obviated; the charcoal left after distillation may be used (with other fuel if necessary) for heating the retorts; and, lastly, instead of sending to a distant inland manufactory a bulky, adulterated, and impoverished article, scarcely a remove above the state of raw material, Mr. Stanford has his manufactory close to the shore, works the raw material for numerous important products, hitherto not obtained from this source, and sends into the market finished products ready for immediate use.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

Friday, March 27, 1863.

"*On the Discovery of the Metal Thallium*,"

by William Crookes.

So many brilliant discoveries have been announced to the world in this theatre, that it was with some diffidence I acceded to the request of the learned secretary of this Institution to relate to you to-night the history of the discovery of thallium. The discovery of a new metal is no novelty in this century. Since its commencement our acquaintance with the material world has been enlarged by the discovery of no less than thirty-two of these elements, and the particular and especial interest with which the three latest additions are regarded attaches as much perhaps to the means by which their existence has been revealed to us as to the metals themselves.

Some of the bodies called metals have been known as such from the earliest times. We have no record nor knowledge of the first discovery of the seven ancient metals—gold, silver, iron, copper, mercury, lead, and tin. I may remark, however, that they are metals which are either found native, or such as are easily separated from their ores by the agency of heat alone, or by the simplest chemical means. Of the rest I may briefly say that a large majority were first discovered by the employment of what I may call exclusively chemical methods. The exceptions to this rule are those whose discovery makes the early part of this century a marked epoch in the history of chemistry and of this Institution. These are the alkaline and earthy metals first obtained by Sir Humphrey Davy by means of voltaic electricity. I need not detain you now by dilating on the great impulse which the employment of this force in the decomposition of suspected metallic compounds gave to the progress of chemistry, and the fruitful results which have attended its use. No means have been attended with such brilliant and useful results until the researches of Bunsen and Kirchhoff definitively applied spectrum analysis to the recognition of elementary bodies.

You have heard and seen so much of the spectrum this season, that it might almost require an apology for introducing the subject once more, were it not that the discovery of thallium is inseparably associated with this method of analysis.

I may here point out one peculiarity with regard to the metals discovered by this method. It is this: that whereas in the case of most bodies discovered in other ways some compound of them was known, and the metallic nature of the base was suspected; or some particular reaction had before convinced the discoverer that he had a body endowed with hitherto unrecognised properties in his hand; yet in the case of spectrum analysis, no knowledge or suspicion of the metals themselves, or of any compounds, existed until, as I may say literally, they were first brought to light. The fact of their existence then flashed upon us at once.

A curious historical parallel may be mentioned in connection with the discovery of thallium; and here I must ask to be excused a more frequent use of the pronoun I (which I make with reluctance, and to avoid periphrasis) than is generally heard in lectures of this kind.

The discovery of thallium is strictly analogous to that of selenium by the great Swedish chemist Berzelius.

In each case the original source was some residue from a sulphuric acid manufactory burning pyrites. From some unexplained reaction the presence of the rare element tellurium was suspected. Further examination led to the discovery, by Berzelius, of the element selenium, whilst in my own case the result of the investigation was the discovery of thallium.

Had spectrum analysis been known at the beginning of this century, there is no doubt that Berzelius would have added thallium to the other elements which he was the first to isolate; for I do not hesitate to say that without this powerful means of research thallium would have remained unknown at the present time. The residue was from a manufactory at Tilkerode, in the Harz, and had been placed at my disposal, in 1850, by Professor Hofmann, for the purpose of extracting the selenium from it. This was effected in the ordinary way, and the crude selenium was purified by distillation in hard glass retorts. A considerable residuum was left behind in this operation, and as a few tests seemed to show that tellurium was present in this residue, it was set aside for further examination. Early in the year 1861, happening to require some tellurium for experimental purposes, I sought for this residuum, and examined it more minutely for this element, but without getting very definite results. The chemical tests seemed to give contradictory evidence, when, not succeeding in meeting with tellurium where there were many chemical reasons for suspecting its presence, I had recourse to the spectroscope. I expected to see a system of fine, nearly equidistant, bands of light and shade traversing the spectrum, but instead of this a single brilliant green line flashed across the field of view. [The spectrum of thallium was here projected on the screen, its remarkable green band appearing like an incandescent bar upon a perfectly dark background. The metal being placed between the carbon poles of a forty-cell Grove's battery.]

The chemical treatment through which the substance examined had passed was such as to preclude the possibility of other than the elements antimony, arsenic, osmium, selenium, or tellurium being present, and I well knew that none of these bodies gave a spectrum in any way similar to this. I was therefore convinced that a hitherto unrecognised element was present, and at once devoted myself to the task of isolating it. It would weary you were I to recount the numerous experiments made with this object. The whole amount of the original seleniferous deposit which I had at my disposal was not more than three pounds in weight, and later analysis have proved that the total quantity did not contain more than three grains of thallium. The invaluable assistance afforded by the spectrum test at last enabled me to announce definitely [*] that I was dealing with a new element, and to give a sufficient number of its reactions to show that its chemical properties were well marked and perfectly distinct from those of all other known bodies.

At first I had doubts as to the chemical position of the new body, and, arguing from the reactions I had then ascertained, I was rather inclined to class it amongst the metalloids or semi-metals. After some months' labour, Dr. Thornthwaite, in the most generous manner, placed at my disposal two cwt. of sulphur, likewise containing about one grain of thallium per pound. With this I was enabled to prosecute my experiments, and before long had isolated the metal not only in the form of a black powder by the action of zinc, but in distinct metallic crystals by voltaic precipitation; all doubt as to the actual metallic character of the element now disappeared.

I had already decided upon a name for the new body.[†] With the assistance of a friend, a skilful linguist, the word thallium was chosen, from the Greek θαλλὸς (a green twig), which seemed to recall the green line of its spectrum, and to be adapted for the requirements of chemical nomenclature better than malachium, smaragdinum, viridium, and others which were thought of. The name thallium was at once adopted by the scientific world as appropriate.

Whilst speaking of the metallic nature of thallium, it becomes a duty no less than a pleasure to mention that M. Lamy, a very skilful Belgian chemist, who had been fortunate in meeting with a rich source of the new body in May last, has, independently of my own researches, determined its metallic nature, and obtained it in considerable quantity. M. Lamy's specimen of thallium was exhibited to a large meeting of scientific gentlemen of all countries soon after the opening of the International Exhibition, and naturally excited great interest among them. It was afterwards placed in the Exhibition building, where, however, amid the splendours with which it was surrounded, it scarcely attracted the notice it deserved.

I first obtained the metal in crystals by voltaic action in September, 1861,[‡] and, although I have since witnessed the operation many hundreds of times, I know of no other experiment of a similar kind which can at all compare with it in beauty.

I have arranged on the table a small apparatus in which the crystallisation of thallium is now proceeding under the influence of two or three cells of Grove's batteries. By the end of the lecture I have no doubt you will find that the bottom of the dish will have become almost covered with beautiful metallic crystals.

In order to render this visible to all my audience, I have placed some of the same solution of sulphate of thallium in a small glass cell containing the platinum terminals of two Grove's batteries. I will endeavour to project an image of these poles on the screen by means of the electric microscope, during electrolysis. In an experiment of this kind, in which I cannot detain you many minutes, some of the beauty and delicacy of the forms must necessarily be sacrificed to rapidity of crystallisation; but still I think you can all see the slender branches and fern-like vegetation in which the metallic crystals are shooting across the liquid.

Thallium comes down with such ease from its solutions under voltaic influence, that I now invariably adopt this plan for reducing it in the first instance, in preference to precipitating it with metallic zinc.

In order to obtain the metal in the coherent form, it is now only necessary to squeeze it together and fuse it under cyanide of potassium, when it is obtained in the form of an ingot.

Hitherto I have been working under great disadvantages, owing to my small stock of material; each experiment requiring nearly the whole of my scanty store, which had then to be worked up again into an available form for the next experiment, causing great and unavoidable sacrifice both of time and material. Thanks, however, to the munificence of the Royal Society, my investigation will no longer stop for want of material, and I am now in communication with Mr. Spence, one of the largest burners of thalliferous pyrites in this country, who has, in the most handsome manner, offered to alter his flues, so that the greater portion of the thallium, which is now lost in the sulphuric acid, may be saved.

And now, having finished the history of the discovery of thallium, I shall follow the plan usually adopted in describing metals. And first as to the source. Up to the present time, the only available one is iron pyrites, such as is used largely,

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[*] Chemical News, iii., 191.

[†] Chemical News, iii. 303.

[‡] At the Lecture I inadvertently said 1862 instead of 1861.—W. C.

both in this country and on the continent, for making sulphuric acid. I have here before me a few specimens of one of the richest thalliferous pyrites which I have yet met with. It is brought from Belgium, and is largely used, both here and abroad, in the manufacture of sulphuric acid. The mineral being burnt in properly-constructed furnaces, the volatile products of its combustion are passed into large leaden chambers, where the sulphurous acid is mixed with nitrous vapours and steam, and becomes converted into sulphuric acid. Now, when the sulphur of the iron pyrites is burnt the product is a gas, and when the thallium contained in the pyrites is burnt the product is a volatile solid. By reason, however, of the great excess of sulphurous acid, and its high temperature (owing to the proximity of the leaden chamber to the furnace), nearly the whole of the thallium is carried through into the lead chamber, where it dissolves in the dilute sulphuric acid, forming sulphate of thallium, a soluble salt. In this case the thallium is lost, for it would be a most tedious and expensive operation to extract it from the acid, which, indeed, is no richer in thallium than the original ore. Thallium may, however, be readily *detected* in many samples of crude oil of vitriol, as well as in commercial products and chemicals which are made from it. Thus, the very common yellow hydrochloric acid, worth about 1d. per lb., often contains it. I have here a bottle of this acid, obtained from Messrs. Chance, Brothers, of Birmingham, and in this tube [exhibiting a specimen tube] I have a lump of pure metallic thallium, which I prepared from 1 cwt. of the acid. Owing to the imperfect means adopted to isolate the metal much of it was lost, but the piece actually obtained weighs upwards of five grains.

Let me also draw your attention to some of the other thalliferous substances which are on the table before you. Here is a fine specimen of cake-sulphur, some sulphide of cadmium, metallic cadmium, and metallic zinc, all from Nouvelle Montagne; a large ingot of Spanish copper; some sulphur, also from Spain; specimens of bismuth ores, with precipitated carbonate of bismuth, also thalliferous, recently purchased at a druggist's; commercial sulphate of copper, some pieces of selenium, and a quantity of metallic tellurium, in which thallium is present rather largely. There is also a large collection of thalliferous pyrites from all parts of the world, and a small specimen of native sulphur from Lipari, said to contain selenium, but which I have found so rich in thallium that it deserves to be classed as a new mineral.*

I have given you a brief outline of the path followed by the thallium in most of our sulphuric acid works. Fortunately, in some manufactories a slight modification is adopted, which permits of the accumulation of this metal in the flues. Instead of the sulphurous acid, &c., going direct from the furnace into the lead chambers, it passes through a greater or less length of flue, where the temperature becomes lowered sufficiently to deposit much of the thallium and other solid volatile matter (*e.g.*, mercury and arsenic) which it may contain. In this *flue-dust*, as it is called, I have frequently detected thallium, and in some instances in sufficient quantity to render its extraction well worth undertaking. In M. Kuhlmann's works at Lille the products of combustion of the pyrites are passed first into a supplementary chamber, where they are cooled down, and deposit nearly the whole of the thallium, arsenic, &c., before entering the converting chambers. This accounts for the richness of the deposit employed by M. Lamy for his researches.

The richest pyrites which I have yet examined have yielded me no more than ten ounces of thallium to the ton of ore. This may seem too minute a proportion to be worth noticing; but in 1000 tons it amounts to nearly 6 cwt., and as 8 or 10 tons of ore are burnt daily in some of our largest vitriol works, I do not think I was exaggerating when I said, nearly a year ago, that thallium was in some works being thrown away by the hundredweight, the whole of which might be saved if manufacturers could be induced to modify their flues so as to effect its proper condensation.

Through the kindness of Professor Chandelon, of Liège, two tons of thalliferous pyrites from the Société Anonyme de Rocheux et d'Oneux, Theux, were placed at my disposal. After considerable delay, this arrived at my laboratory in September last, and from that time almost to the present I have been constantly engaged in extracting the thallium from it.

The first operation consists in breaking up the pyrites into lumps the size of a walnut, and distilling the sulphur from it. For this purpose hexagonal iron pipes, closed at one end, are arranged in a reverberatory furnace, five at a time, so that the flame can lick round and raise them to a bright red heat. Each retort contains 20 lbs. of mineral, and has an iron tube receiver luted on to the end of it. After some hours' heating the operation is terminated, and the receivers contain from 14 lbs. to 17 lbs. of dark green or grey-coloured sulphur from each cwt. of ore. Several large cakes of this are on the table. This appears highly thalliferous when examined by the spectrum test, but when treated chemically I have not succeeded in obtaining a larger yield from it than 10 grains of thallium per pound of sulphur.

The separation of the metal from this sulphur is a matter of some little difficulty to one unprovided with the resources of a manufacturing laboratory. Indeed, I should have been quite unable to show you such pieces of thallium as I have here to night, had not Messrs. Hopkin and Williams, the well-known manufacturing chemists, undertaken to perform the subsequent operations in their well-appointed works at Wandsworth. In less time than it used to take me in my private laboratory to work up ten pounds of sulphur, they have prepared for me the chloride of thallium from two cwt. The process adopted is briefly as follows:—The sulphur is powdered and boiled in a large iron pan with a solution of caustic soda until it has all dissolved, with the exception of a black precipitate suspended in the liquid. After cooling, it is filtered, washed, and the precipitate boiled in sulphuric acid. When all has dissolved that will, it is filtered, and hydrochloric acid is added to the filtrate, which determines the formation of the difficultly soluble protochloride of thallium. This is filtered off, washed once with dilute hydrochloric acid (in which it is less soluble than in pure water), and converted into sulphate. From the solution of this salt the metal is precipitated by voltaic electricity, as is now in operation on the table.

In order to obtain this spongy thallium in the coherent form it is not even necessary to fuse it. There is in this bottle of water a quantity of metallic sponge—the results of this day's precipitation in my laboratory—I merely have to knead it into a lump between my fingers, and then put it into a steel crushing mortar, without even taking the trouble to dry it. I now submit it to powerful pressure in this vice, and you see that I have produced a brilliant metallic-looking ingot, perfectly solid, as can be told by the sound when thrown on the table.

Well, now, having obtained the thallium, let me draw your attention to its chief characteristics, wherein it agrees with or differs from its fellows. It is a white, opaque metal, endowed with a perfect metallic lustre, as may be seen at the close of the lecture by examining the beautiful crystals now being deposited all over the bottom of this dish. Metals are usually divided into two classes, *light*

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* This was a small mineralogical specimen, obtained in 1861 from Mr. Sowerby, geologist, Strand. It was labelled "Sulphur coloured with selenium, from Lipari," and consisted of very dark-coloured crystallised sulphur mixed with chloride of ammonium. Analysis showed that selenium was absent, but that it was very rich in thallium. Notwithstanding my repeated endeavours, I have hitherto failed in obtaining a further quantity of this mineral; a fine specimen of native sulphur which Dr. Hofmann most obligingly obtained for me direct from Lipari having been found quite free from thallium.—W. C.

and *heavy;* thallium certainly belongs to the heavy metals, not only on account of its specific gravity, which is 11·9, a trifle higher than that of lead, but also for other reasons which I shall come to presently. It is very malleable, and may readily be rolled into leaves as thin as tissue-paper, as shown by the specimen of foil before you. It is not very ductile, and can only be drawn into wire with great difficulty. By employing pressure, however, thallium wire can be made with the utmost facility. I have here a small hollow steel cylinder, with a piston fitting tight into it; at one end is a very fine hole, and upon filling the cylinder with thallium, and forcing in the piston by means of the vice, the thallium issues out through the fine hole in the form of wire. I have arranged an apparatus here by which the wire as it issues from the cylinder is conducted along a glass tube into a specimen bottle, a current of dry carbonic acid passing through the apparatus all the time. By means of the electric light I project the image of the specimen bottle on to the screen, and upon giving one or two turns to the screw of the vice you perceive the wire issuing forth and curling up in folds like a thick rope. The bottle is now filled with the coil of wire, and if I cement the stopper in, the specimen will preserve its metallic lustre and brilliant surface unchanged.

Thallium is very soft, in fact it is the softest known heavy metal, being only exceeded in this respect by the alkali metals. A piece of lead scratches it, as you perceive, with the utmost facility, without itself receiving an appreciable impression. It also possesses the property of welding together in the cold by pressure. I can illustrate this by taking another steel cylinder and filling it with several pieces of metallic thallium. [Fourteen pieces were put in.] A turn of the vice forces it out, as you see, into one solid rod, which, upon examination, will be found just as continuous and coherent as that made from one lump.

Thallium marks paper like plumbago, forming a streak having a yellow reflection. The mark almost entirely fades out in a short time from oxidation. A week ago I wrote a word upon this sheet of paper; upon close examination I can just read it, but I do not think it is visible to any one else. I have, however merely to pass over it a sponge which has been dipped into sulphide of ammonium, when the word THALLIUM appears black, and visible to all.

The electrical conductibility of thallium has been recently examined by Dr. Matthiesson, to whose instruction I am indebted for the wire-making process just shown. He finds it near lead in this respect. His researches were communicated to the Royal Society a few weeks ago.

The magnetic relations of thallium are very interesting. Professor Faraday has been good enough to arrange an experiment by which its behaviour under the magnetic force can be rendered clearly visible to all in the room. A small sphere of thallium is fastened to the short end of a light wooden lever, a square index of white paper being at the other extremity. The lever is suspended horizontally by some long threads of cocoon silk in such a way that the sphere all but touches one of the conical poles of the large electro-magnet belonging to this Institution. I now pass the current of forty Grove's batteries round the electro-magnet, when you see, from the large arc travelled over by the paper index, how violently the thallium sphere is repelled from the pole of the magnet. It is, in fact, next to bismuth, the most diamagnetic metal known.

Thallium is easily fusible, melting at a temperature of 550° Fah.; at a full red heat it may be distilled, but it begins to evolve vapours at a lower point. If I heat a piece on charcoal before the blowpipe, you perceive the metallic vapours flying off copiously, colouring the flame a rich green. The colour which it communicates to flame is better shown by holding, in a colourless gas flame, two or three platinum wires having an alloy of thallium and platinum fused on to their ends. I will place by the side of this, other two flames coloured respectively with barium and copper, when you can judge of the extreme richness and purity of the thallium green by the manner in which it appears to kill the others.

Thallium burns brilliantly in oxygen, and small fragments of the metal also take fire when thrown into a gas flame, giving rise to an intense green light. Exposed to the air it tarnishes very quickly; indeed, with almost the rapidity of an alkali metal, becoming coated with a film of oxide, yellow at first, and gradually darkening. This oxide is tolerably soluble in water, forming a highly alkaline solution. It, however, differs radically from potash and soda, and closely approaches oxides of silver and lead (which are likewise soluble and alkaline) in having scarcely any affinity for water; it being rendered anhydrous even at the ordinary temperature in a vacuum. I can illustrate the alkaline character of its oxide by taking this ingot of thallium, and rubbing it across a piece of moistened turmeric paper on a white plate. Wherever the thallium has touched a brown mark is produced. If I apply the tarnished surface of the metal to the tongue it tastes very caustic and biting, and somewhat metallic. When I place a piece of tarnished thallium in water, the superficial film of oxide dissolves, and exposes a bright surface of metal. Hot water cleans it at once, and renders the surface crystalline, like tinplate washed with acids. If the thallium be quite pure, and the water free from air, no sensible action appears to be exerted on the metal, but when exposed to the joint action of air and water it is gradually oxidised. Curiously enough alcohol acts upon it more than water does. The soluble oxide (the protoxide) is also formed very rapidly when thallium is melted in the air; it then fuses like litharge, and is absorbed in the same manner into a bone-ash cupel. When thallium burns in oxygen a peroxide is formed.

Thallium dissolves in acids, its proper solvent being nitric acid; it forms two if not three basic oxides and an acid oxide. The salts of the protoxide are the only ones which have been much studied. They are a well-defined series, most of them being beautifully crystalline. Many of these compounds have been very skilfully investigated by MM. Lamy and Kuhlmann, jun. Some of the proto-, sesqui-, and per-salts are on the table, as also one or two thallates. Let me especially draw your attention to the beauty of the crystallised sulphate, nitrate, and chlorate of the protoxide, the yellow sesquichloride in glistening spangles, and this large bottle full of the protochloride, upwards of a pound in weight.

On the table before me I have arranged three series of test glasses, the upper row containing protosulphate of thallium, the middle row nitrate of silver, and the bottom acetate of lead. To these I will apply the ordinary tests used in chemical analysis, and I think there is no doubt that my chemical friends who are present will admit with me that the true position of thallium is by the side of these two metals, and not, as M. Dumas and other French chemists affirm, in the potassium and sodium group.

Gas light not being very well adapted for showing shades of colour, I have arranged to illuminate the table with electric light during these tests. I first add hydrochloric acid to the solutions, a white precipitate falls in each case: the protochloride of thallium being scarcely distinguishable from chloride of silver: it is, however, slightly soluble in water. Iodide of potassium gives a yellow precipitate with each metal. Bichromate of potash a yellow precipitate with thallium and lead, and a red one with silver. Sulphocyanide of potassium gives white, and sulphide of ammonium black precipitates in all three metals. Bichloride of platinum produces an insoluble double salt with thallium, and precipitates the silver and lead as chlorides. Ammonia produces no change in the thallium solution: gives a precipitate with silver which redissolves in excess, and permanently precipitates the lead salt. Sulphuretted hydrogen has likewise no action on the thallium salt, but precipitates the other two. When, however, I add ammonia to the thallium after addition of the sulphuretted hydrogen, a black precipitate is produced, and finally sulphuric acid produces (of course) no change in sulphate of thallium, a slight crystalline precipitate in nitrate of silver, and a dense white one in acetate of lead. From these tests you therefore perceive that whilst the similarities between the three metals are very great, there are, nevertheless, characteristic differences, though not greater than between most nearly allied metals.

I have only time to show two reactions with solution of sesquichloride of thallium: the first is the precipitation of the slightly soluble protochloride upon addition of sulphite of soda, and the second the formation of a brown peroxide and precipitation of protochloride in crystals upon addition of ammonia. This latter reaction is a very curious one.

Let me, in conclusion, say a few words respecting the position of thallium amongst elementary bodies. In classification observers generally err in regarding natural bodies as so many links in a perfect chain, and facts are frequently strained in order to make them agree with this preconceived opinion. In such a group as chlorine, bromine, and iodine, we have, doubtless, three consecutive links; but most frequently Nature should be looked upon more as a perfect net than a perfect chain. In seeking for the chemical relationships of thallium, it is found that this metal occupies a somewhat anomalous position, being well described by an eminent French chemist as the ornythorynchus of metals. At first sight it might appear to belong to the group of alkali metals, on account of its forming a readily soluble, highly alkaline oxide; it likewise forms an insoluble platino-chloride, which also renders it analogous to some of the alkali metals, although not to others; but, on the other hand, its physical characters, its chemical reactions, and its high atomic weight (about 203), prove incontestably that the true position of thallium in the scale of elements is close to lead and silver.

In these utilitarian days a discoverer must be prepared to give some kind of answer to the question,—"What is the use of it?" Now, the possible uses of a body depend chiefly on its abundance, and as soon as thallium is procured by the ton at no greater cost than it is now by the ounce, it will certainly be utilised in many directions. In the pure metallic state it probably tarnishes too readily, and is too quickly acted upon by atmospheric agencies for it to possess much practical value in this form. In the form of alloy, however, its uses are likely to be very great, as it readily mixes with many metals, and communicates to them valuable properties.

The magnificent green which it communicates to a flame at once suggests a valuable application of thallium for pyrotechnic purposes. At the ordinary temperature of flame, the thallium light is absolutely homogeneous, and even at the high temperature of the electric arc, the other lines, which Dr. Miller has shown are produced, and which I have attempted to copy on this diagram, fade before the brilliancy of the characteristic green line.

Perhaps the best way for me to show the magnificent green light evolved by incandescent thallium vapour is by projecting on the screen the highly magnified image of the thallium electric arc. I have scooped out the lower carbon of the electric lamp into the form of a small cup, and, upon placing ten or a dozen grains of thallium in it, and making contact, the upper pole can be separated for a space of an inch or more, voltaic connection being maintained by the bridge of thallium vapour rising from the cup. The image of this on the screen fills up a space of twelve inches or more with absolutely monochromatic green light, and if I introduce into this green space variously-coloured bodies, a bouquet of bright flowers, or even my own face, you will see the strange changes it produces in the apparent colours of bodies, everything being either green or black, like as in the sodium flame every object is yellow or black.

The reason why the homogeneous light given by sodium and thallium is so intense is owing to all the luminiferous energy of the element being concentrated into one ray, instead of being diffused over different portions of the spectrum.

These experiments show that thallium is pre-eminently the pyrotechnic element; but regarded for the present merely from a scientific point of view, its early history will always be looked upon with interest, as proving the beauty and accuracy of spectrum analysis, and the striking manner in which the deductions therefrom have been confirmed.

PHARMACEUTICAL SOCIETY.

Wednesday, April 1.

Mr. P. Squire, *President, in the Chair.*

Mr. Symonds exhibited and described a *New Standard Barometer*, an account of which we defer.

Professor Bentley then said that as there was no paper for the evening, he would call the attention of the meeting to several donations to the Museum of the Society which were on the table. The first he would notice were some specimens of Chinese birds' nests, and a manufactured article made to resemble them, which had been presented by Mr. Hills. There was not very much known about these articles in this country. They were of great value in China and Java, where they were used principally as articles of diet made into soups, but occasionally as remedial agents. In appearance they were very unlike birds' nests, but they were, in fact, constructed by a species of swallow. In Hartwig's "Tropical World" there was a very interesting account of the way of getting these nests from caves on the sea-side, where they are found glued to the rocks at the top. Several species of swallow—five at least—made nests of the same material, but the nests most commonly obtained are formed by those which have been named Hirundo Esculenta and H. Nidifica. Three kinds of these nests have been described, one of which is collected after the first laying of eggs, and two inferior sorts, darker in colour, and having feathers stuck about them. The Chinese sometimes cleaned the darker kinds from the feathers and other matters adhering, and sold them for the first quality. The origin of the material of which these nests are made is an interesting question. There were several notions about it. Some thought the birds obtained it from decaying animal matter; others thought it consisted of zoophytes; some believed it was the juice of a tree mixed with lichens, or a glutinous matter obtained from algæ with the juice of a tree. Mülder, however, had analysed the material, and had found 90 per cent. of it to be animal matter, which excluded the notion of its having been derived from vegetables. It was, in fact, a peculiar glutinous matter secreted by the bird at the period of nidification. There was no doubt that the swallow which visits this country secreted a similar matter to glue together the materials, and fix the nest in its place. As regards the chemistry of the substance, it had been found to be something analogous to gelatine or isinglass, or Payen's gelose.

Mr. D. Hanbury, jun., said there was no doubt that the salivary glands of the common swallow and martin were excited at the period of nidification, and secreted a matter similar to that of which these nests were made. It was difficult to conceive how the materials of which the martin made its nest could adhere together without some such matter to glue them. It would be interesting to examine some nests to see if it was present.

Mr. P. Simmonds said he had not much to add to the statement of Professor Bentley. These nests had rather a fanciful value in the estimation of the Chinese. It had been settled beyond a doubt that they were constructed of animal matter, but the assertion that they were composed of vegetable matter was an error continued in some recent works. The nests by themselves were very insipid, and the flavour of the soups made from them of course depended on the seasoning. Numerous varieties of the nests were found in the Chinese markets, but they were variations in quality and not of origin. Some of them were in a very foul condition, but they were cleaned up by the Chinese, and made to pass for the better sorts. With regard to gelose, he believed the exact source of that article was unknown. There was an enormous commerce in algæ in China and Japan, and it was no doubt obtained from these weeds.

Professor Bentley said he believed gelose was obtained from the gelidium corneum, and probably other algæ as well.

Mr. Hills said he bought the nests on the table of Messrs. Matthesons. They were rather expensive; he paid 140s. per lb. for them.

Professor Bentley observed that Mr. Hills had bought them very well; *8l.* per lb. was very commonly paid for them. The Professor then called attention to a number of specimens of drugs from Manilla, 150 of which had been presented to the museum. He also exhibited a fine specimen of Scammony root, which was in Mr. Ransome's case in the International Exhibition, and had been given to the Society by that gentleman. Professor Bentley remarked that in this specimen the resin of the root could be seen *in situ.* He then noticed some oil of worm seed also given to the museum by Mr. Ransome. This is obtained from the fruit of the Chenopodium anthelminticum, and has long been known as an excellent anthelmintic. It is largely used as a vermifuge in the United States, and the Professor hopes it will receive a trial here. The fruit itself is powdered, and sometimes administered to children, and the dose of the oil is from four to six drops taken on sugar, for three or four days, and then followed by a cathartic. So used it was a very successful evacuant of "round" worms.

The Chairman observed that no remedy had been found so successful for tape worms as the etherial extract of the male fern. It was in general made from the young rhizome, but an etherial extract of the young fronds had been found to answer just as well.

Mr. Hills wished, as this was the last meeting of the year, to ask the Chairman whether glycerine was to be introduced into the new Pharmacopœia? Some time ago he had a sore throat, and was prescribed a gargle of tannin, glycerine, and infusion of roses. They made a very nice gargle, and he had found it keep quite well for three months. He had also found that glycerine was a very good solvent for some alkaloids, such as morphia and atropia, and other matters, but he had had no time to work out the matter. Plasma or solid glycerine, too, was a useful compound, which kept well, and altogether he thought glycerine a very valuable substance, which deserved to be placed in the Pharmacopœia.

The Chairman doubted whether plasmata would keep well. In the pharmacopœia of the Royal Ophthalmic Hospital, there were several such compounds as Mr. Hills had suggested, and some he had seen had kept well, but in general his experience was against plasmata. It was very desirable to find something which would supply the place of lard in pharmacy, the tendency to rancidity of lard being a great objection to its use. He had made many experiments to find a substitute, but the only things which seemed to succeed were the mixture wax and oil called Linimentum Simplex in the Edinburgh Pharmacopœia, and a mixture of cacoa butter and oil, which was perhaps the best substitute.

Mr. Haselden said the solvent powers of glycerine, as well as its antiseptic properties, had long been known. He had himself made many experiments on the subject, and had contributed the results of some to the Society. He did not believe any compound of glycerine would answer for ointments, and he had found cacoa butter a very unmanageable substance. By itself it was too hard, and a very little oil made it perfectly fluid.

The Chairman said he had found some proportions of olive oil and cacoa butter produce a compound of a good consistence, which kept perfectly free from rancidity for two years.

Professor Bentley then noticed some specimens of the bread fruit, and made some observations thereon, which we defer; and Mr. Palmer exhibited a piece of flint from a gravel pit, which gave rise to some remarks on flint implements, and the means by which they were made. In announcing the adjournment of the meeting, the Chairman said that, as this would be the last in his year of office, he would take the opportunity of thanking the members for their attendance and courtesy during the period he had filled the chair.

NOTICES OF BOOKS.

First Outlines of a Dictionary of the Solubilities of Chemical Substances. By Frank H. Storer. Part I. London: Trübner and Co. Cambridge (U. S.): Sever and Francis. 1863.

Any one wishing to gain some notion of the immensity, as we may say, of chemistry, could hardly get a better idea than he would derive from this book. Here is the first part of what the compiler modestly terms the "First Outlines" of a work. It is devoted to but a single property of "chemical substances," and includes only those substances whose names come within the letters A to C, Abietic Acid to Convolvulinol; yet it occupies 232 pages of large octavo, printed in double columns, in type the same as this article.

In the practical applications of chemistry and in chemical analysis, the solubility of a substance is, perhaps, the most important quality to know, and the author of this work deserves the thanks of every chemist for this compilation, which will save much trouble of reference. The extent of the information conveyed in the book could hardly be ascertained without a direct reference to its pages, our limited space forbidding us to make such extracts as would fairly represent it. In the Preface the author thus explains the scope of the work:—

"The term 'solubility' in the title of the present publication is to be taken in its most comprehensive sense. I have no intention, at this time, of attempting a strict definition of the word, or of discussing the forces upon which solution may depend. In the present state of science, the collection of experimental data, and the study and comparison of well-authenticated special observations, seem to be of far greater importance than the disputes of the earlier chemists whether the phenomena in question should be referred to the domain of chemical affinity, or be studied as a purely physical problem. It need only be remarked that I am accustomed to class among phenomena of solubility all those reactions of liquids upon solids or gases, and those combinations of liquids with liquids, excluding for the present molten metals, and other substances in a state of igneous fusion, in which the chemical force as understood by Berzelius, for instance, is not the principal, and as it were the overwhelming force in action. We may have, perhaps, 'solution' depending upon merely physical forces, like adhesion or cohesion, and also upon these forces *plus* a certain amount of chemical force. It can indeed hardly admit of a doubt that the chemical force is exerted in many cases of solution, while at the same time other forces unquestionably come into play, in which connexion the old adage, that 'like dissolves like,' should be borne in mind. Hence, while the manifestations of chemical affinity proper, as evinced by the combination of bodies in simple and definite proportions, constitute the main subject of chemical text-books, many of the less clearly-defined phenomena of chemical science may fairly come within the scope of a treatise on solubilities. Thus, though in the term solubility of a substance we ordinarily include only the comportment of the substance towards water, alcohol, wood-spirit, ether, oil of turpentine, benzine, and analogous hydrocarbons, and the other neutral solvents, it is obviously sometimes proper to add observation on the action of acids and alkalies; for example, any account of the solubility of nitrate of baryta would be manifestly incomplete without a statement of the fact that the salt is taken up but sparingly by nitric acid. Again, in the solution of chloride of silver in ammonia, and that of various salts, sulphate of lime, for example, in acids, there are probably at work other forces than the usual solvent power, but until the whole theory of solution is better understood, we must be content to treat of these allied phenomena under the same general head of 'solubilities.' Pains have also been taken to bring forward facts known respecting those cases in which two or more salts acting upon each other in the presence of water, or the like are to a certain extent mutually decomposed and dissolved, as in the familiar instance of nitrate of potash and chloride of sodium which promote each other's solubility; or that of the reciprocal decomposition which ensues when sulphate of baryta is treated with a solution of an alkaline carbonate."

Our readers will see that a large ground is here laid out, and we can assure them that it is well filled up. We know of no other work on chemistry which exhibits so much industry in compilation as this, except the great work of Gmelin. The quotations our limited space allows us to make will give our readers but a poor idea of the extent of information they will find in these pages, but we may extract what relates to the chloro-platinates of cæsium, potassium, and rubidium, the different solubilities of these bodies affording the readiest means of separating them.

Chloro-platinate of Cæsium. $CsCl,PtCl_2$. Less soluble than chloro-platinate of potassium in water.

By experiment—

100 parts of water at	0°	dissolve	0·021	part of it
,,	11	,,	0·072	,,
,,	40	,,	0·118	,,
,,	68	,,	0·234	,,
,,	100	,,	0·382	,,

From these results the following table was obtained by interpolation :—

100 parts of water at	0°	dissolve	0·024	part of it
,,	10	,,	0·050	,,
,	20	,,	0·079	,,
,,	30	,,	0·110	,,
	40	,,	0·142	,,
,,	50	,,	0·177	,,
,,	60	,,	0·213	,,
,,	70	,,	0·251	,,
	80	,,	0·291	,,
,,	90	,,	0·332	,,
,,	100	,,	0·377	,,

Chloro-platinate of Potassium. $KClPtCl_2$. Permanent. Difficultly soluble in cold, more easily soluble in hot water. Scarcely soluble in alcohol; difficultly soluble in dilute spirit. Soluble in 200 parts of water.

By experiment—

100 parts of water at	0°	dissolve	0·724	part of it
,,	6·8	,,	0·873	,,
,,	13·8	,,	0·927	,,
,,	46·5	,,	1·776	,,
,,	71·0	,,	3·018	,,
,,	100·0	,,	5·199	,,

From these results the following table was obtained by interpolation :—

100 parts of water at	0°	dissolve	0·74	part of it
,,	10	,,	0·90	,,
,,	20	,,	1·12	,,
,,	30	,,	1·41	,,
,,	40	,,	1·76	,,
,,	50	,,	2·17	,,
,,	60	,,	2·64	,,
,,	70	,,	3·19	,,
,,	80	,,	3·79	,,
,,	90	,,	4·45	,,
,,	100	,,	5·18	,,

There then follow the solubilities in alcohol and various reagents.

Chloro-platinate of Rubidium. $Rb.Cl,PtCl_2$. Much less soluble than chloro-platinate of potassium in water. Completely insoluble in alcohol.

By experiment—

100 parts of water dissolve	0·193	part of it at	0·0°
,,	,,	0·135	,,	13·5
,,	,,	0·195	,,	48·0
,,	,,	0·263	,,	60·0
,,	,,	0·641	,,	100·0

From these results the following table was obtained :—

100 parts of water at	0°	dissolve	0·184	part of it
,,	10	,,	0·154	,,
,,	20	,,	0·141	,,
,,	30	,,	0·145	,,
,,	40	,,	0·166	,,
,,	50	,,	0·203	,,
,,	60	,,	0·258	,,
,,	70	,,	0·329	,,
,,	80	,,	0·417	,,
,,	90	,,	0·521	,,
,,	100	,,	0·634	,,

The above determinations are quoted from the researches of Bunsen and Kirchhoff, and of course the great bulk of the work is compiled from other writers; but sprinkled through the pages we see original experiments by the author and his friends.

In conclusion, we may direct especial attention to the articles alcohol, arsenious acid, and chloride of sodium, as exhaustive on the subject of solubilities, and of the greatest practical value; and we may add that this is emphatically a book which no chemist's library should be without.

CORRESPONDENCE.

Spectrum Analysis.

To the Editor of the Chemical News.

Sir,—I find by repeated examination that the potassium line Ka *α*, coincident with the Fraunhofer line A of the solar spectrum, is a double line slightly thicker than the sodium lines, and quite as distinctly separate. As I believe that it has always been mapped down as a single line, I shall feel much indebted by the insertion of this note in the Chemical News.—I am, &c.

Wm. Edwd. Kilburn.

Burleigh House, Bridge Road, St. John's Wood.

Deaths from Nitric Acid.

To the Editor of the Chemical News.

Sir,—It is not my desire to prolong the correspondence on this subject, but the letters in your last issue incite me to address you again.

Mr. Spencer's caution, so far as regards myself, is alto gether unnecessary, and I would remark that it is the caution I have always used that will prevent Mr. S. from hearing of myself being "the next victim to the deadly power of nitrous acid gas." Had a little caution been used in the case quoted by Mr. Spence, the life of the party named would have been saved, if, indeed, death did result from the causes set forth. I am no stranger to the action of the fumes of this and other acids when inhaled, and I agree with Mr. Tratchsel in saying "that the experience of men employed in the manufacture of nitric acid is quite contrary to the idea that the mere breathing of the fumes of this acid for a short time could produce death."

I concur in the remarks of this gentleman as to the powerful effects of this acid upon the blood, and his letter tends to throw more light on the subject of "deaths from nitric acid." It would be interesting to know whether any wounds were found on the hands or any other part of Mr. Stewart and his assistant, and in what condition the blood was noticed to be. I presume a post-mortem examination was made. This would, I think, help to prove the action of this acid upon the circulation of the blood when admitted into the body by a cut received from glass, &c. It is far from my wish to throw any one off their guard with respect to this acid, and I sincerely hope that we shall not hear of another accident like that at Edinburgh. Still, such occurrences should be inquired into. It certainly adds to our knowledge of things.

I am, &c.
J. H. Swindells.

Meadows Bridge, Wigan.

Action of Heat and Force upon Matter.

To the Editor of the Chemical News.

Sir,—I send you copies of a letter which I addressed to Mr. Dyer, and of his reply, which sufficiently explain themselves; will you be so good as to give them publicity.

I am, &c. Barnard S. Proctor.

"11, Grey Street, Newcastle, March 28, 1863.

"Dear Sir,—I have just been reading in the Chemical News the report of your paper 'On the Action of Heat and Force upon Matter.' My first impulse was to send a note to the Chemical News, pointing out an error into which you appear to have fallen, but second thought dictated my writing to yourself. What I allude to is this:—You say the heat required to convert one pound of ice into vapour would fall far short of melting five pounds of cast iron.

"In the first place Tyndall only says 'to its melting point.' So that the latent heat of melted iron is not required.

"In the second, as (according to your quotations) he does not say from what temperature the iron would be raised up to its melting point, we must understand it to be not intended as an accurate, but as an approximative statement.

"In the third, according to established data, Tyndall's statement is very near the truth; thus:—

"The heat required to melt 1 lb. of ice will raise 1 lb. of water . . . . . . 142°
"The heat required to evaporate 1 lb. of water at 32° will raise 1 lb. . . . . . 1123°
1265°

The specific heat of iron being about one-tenth that of water, the above heat would raise one pound of iron 12,650°, or five pounds of iron 2530°, to which add 60° for the temperature from which the iron is to be raised = 2590°, and take the melting point of cast iron as 2786°, we have a deficiency of 196, or only about 7 per cent. of the number required.

"Hoping this may induce you to send a note to the Chemical News, either to correct or substantiate your statement,

"I remain yours faithfully,
"To J. C. Dyer, Esq." "Barnard S. Proctor.

"Barnage, March 31, 1863.

"Dear Sir,—In reply to your letter of the 28th, I beg to thank you for pointing out the slight error in the published abstract of the paper read by me at the Literary and Philosophical Society. I stated the 'heat for *melting* the cast iron' in place of saying 'for *raising* the cast iron to the melting point.' Now, as this latter expression is *literally* quoted a few lines above, the reader could hardly be misled by my words, especially as no reference is made to the heat that becomes latent or specific in the iron when it passes from the solid to the liquid state. It must therefore be seen that I took into view only the two sums of latent heat, as given by you about 1262°, so transferred and entering the iron to raise its *temperature* to the melting point. I should be extremely sorry to afford the slightest ground of complaint to Professor Tyndall, and I fully trust that the above explanation will be satisfactory to that gentleman and his friends. I am averse to public controversy, and would fain avoid giving occasion for any; and I leave with you to communicate my reply to your letter, or its substance, either to Professor Tyndall direct, or through the journal you mention, as you may think best.

"With respect to the second point you adduce against my view of the action of heat in raising the temperature of the iron, as this involves unsettled questions relating to the *line*, if any, to be drawn between the *heat latent* or the *heat specific* in bodies. The subject is too wide for discussion on this form, yet I must respectfully dissent from the conclusion at which you arrive—in making the *tenfold* amount of *sensible heat* in the iron at the melting point. I therefore must leave this matter for future inquiry by those who can make it clear either way.

"I am, Sir, yours faithfully,
"To Barnard S. Proctor, Esq." "J. C. Dyer.

Chemical Notices from Foreign Sources.

TECHNICAL AND PHARMACEUTICAL CHEMISTRY.

Antidote for Arsenic.—The hydrated peroxide of iron requiring to be freshly precipitated to be useful as antidote for arsenic, and the preparation in the ordinary way requiring some time when every moment is of importance, it has been proposed to keep a solution of the persulphate iron, and to add when the oxide is wanted calcined magnesia, the mixture being administered, which saves the trouble of washing the precipitated oxide, and at the same time gives a purgative to the patient. We may suggest that an excess of the calcined magnesia should always be used, and then the mixture would be unobjectionable and perhaps useful.

Estimation of Glucose in Beer.—Vogel (*Chem. Centralblatt*, 1862, p. 1244), suggests the use of the dialyser for separating saccharine from gummy matter in the examination of beer.

Preparation of Nitric Ether.— M. Persoz (*Comptes-Rendus*) uses two parts of the strongest and purest nitric acid and one part of absolute alcohol, both cooled by the application of ice. The alcohol is slowly added to the acid, and the two are continually shaken. The ether is produced immediately. He operates on small quantities at a time, and when the reaction is finished adds a small piece of ice. The ether is purified in the ordinary way.

Process for the Decortication of Seeds.—M. Lemoine suggests the use of moderately strong sulphuric acid, which he agitates with the barley, for instance, for fifteen or twenty minutes, and then washes away, using in the last washing a little carbonate of soda. In the case of barley he assists the operation by the application of a gentle heat. After the grain is dried the husk is easily separated.

English Green.—We read that a pigment called English green is made by first colouring sulphate of baryta diffused in water by successive additions of acetate of lead and bichromate of potash, and then, after washing, adding to the yellow prussian blue suspended in water. Vogel proposes to shorten this process by using the bichromate and prussian blue at the same time, dissolved in oxalic acid.

MISCELLANEOUS.

Appointment of Mr. Squire as Chemist on the Household of the Prince of Wales.—Our readers will be gratified to learn that Mr. Squire, the President of the Pharmaceutical Society of Great Britain, and who has held during the present reign the appointment of Chemist on her Majesty's establishment, has recently received a similar appointment on the household of his Royal Highness the Prince of Wales.

Soda in Coal.—We have received another communication on this alleged new discovery. We beg to assure our correspondents that there is nothing new in it. Dr. Percy has proclaimed the fact to his class during the last ten years; and soda is, moreover, mentioned in his "Metallurgy" as one of the constituents in the ashes of coal.—Ed. *C. N.*]

Royal Institution of Great Britain.—General monthly meeting, Monday, April 6, 1863. The Rev. J. Barlow, M.A., F.R.S., Vice-President, in the Chair. Leveson Francis Vernon Harcourt, Esq., B.A., William Harvey, Esq., F.R.C.S.L., Joseph Norman Lockyer, Esq., F.R.A.S., Paul Julius Reuter, Esq., Octavius Sturges, Esq., Frederick Thompson, Esq., and Robert Wigram, Esq., were elected members of the Royal Institution. Col. Dickens, Abraham Pope, Esq., John Rutherford Russell, M.D., and John Rivington, Esq., were admitted members of the Royal Institution. The presents received since the last meeting were laid on the table, and the thanks of the members returned for the same.

Austrian Gun Cotton.—Take cotton yarn and twist it into strands of suitable size to answer the same purpose as grains in gunpowder. (The size of these strands can only be ascertained by experiments.) It is then steeped for a few minutes in nitric acid contained in a stoneware vessel, squeezed, and thoroughly washed by water, which is permitted to fall upon it from a pipe set at a height of several feet. After this it is squeezed, and dried in a room heated to 130° Fahr., when it is ready to be treated with a mixture of nitric acid of 1.52 specific gravity, and sulphuric acid of 1.14 specific gravity. These acids, in equal quantities, are mixed together in a glass or stoneware vessel, and allowed to stand for twenty-four hours, then the prepared yarn is immersed in it for forty-eight hours, with occasional stirring; the vessels being covered; then it is squeezed, washed for several hours in running water, and dried again. After this it is soaked for a short period in dilute silicate of potash, squeezed, washed again, dried, and is fit for use. This gun cotton is manufactured by M. Reny, of Vienna. It emits but little smoke, and is not subject, like common gun cotton, to explode by percussion.

Modern Inventions not always New.—You are, I believe, at all times willing to give publicity to facts which may tend to promote the manufactures of this country, and, assuredly, in many cases, none will contribute more to this important result than the communication of evidence in proof of the invalidity of troublesome patents. This horrible patent incubus has become intensely oppressive and annoying; and it is a great satisfaction to be able to do anything which may, even in the least degree, abate the nuisance. An account of a peculiar alloy of brass with small quantities of tin and iron appeared in your columns some time ago; and there is reason to believe that it has not been in vain. But this, or a very similar alloy, I find, is secured by a patent in England, of which the specification is now before me; it is dated 1860 (No. 278), and at the end the words are as follow:—"I claim the exclusive manufacture and use of the metal, alloy, or amalgam produced by the fusion of copper, zinc, and iron, as hereinbefore described or set forth." The patentee does not confine himself to exact proportions of the component metals; but he states that the alloy is best made of 60 lbs. by weight of copper, 38 lbs. 2 oz. of zinc, and 1 lb. 8 oz. of iron. Taking 60 lbs. of copper as the standard, the proportion of zinc may be increased, even as far as 44 lbs., and that of iron diminished to ½ lb., or increased to 3 lbs. Now, Sir, permit me to present you with the following extract from the specification of a patent granted in 1779 (Dec. 10, No. 1240), to J. Keir, and invite you to compare it with the claim set forth in the specification of the preceding patent. The title of Keir's invention is "for a compound metal capable of being forged when red-hot or when cold, more fit for the making of bolts, nails, and sheathing for ships, than any metals heretofore used or applied for those purposes, and also for various other purposes where other metals have been used or applied." The alloy is made by combining together 100 lbs. by weight of copper, 75 lbs. of zinc, and 10 lbs. of iron, and its percentage composition would be nearly 54 of copper, 40·5 of zinc, and 5·5 of iron. I think, Sir, you will be able to agree with me that at least there is a remarkable similarity between the old and the new patents, and a knowledge of this fact will, I am sure, be important to many of our artificers, who otherwise might be deterred from using the alloy in question.—Y.—*Times.*

SCIENTIFIC SOCIETIES.

MEETINGS FOR THE ENSUING WEEK.

April 13. *Monday.*

Geographical—15, Whitehall Place. 8.30 p.m.
Medical—32a, George Street, Hanover Square. 8.30 p.m.

14. *Tuesday.*

Royal Institution—Albemarle Street. 3 p.m. Professor Marshall, "*On Animal Mechanics.*"
Medical and Chirurgical—53, Berners Street. 8.30 p.m.
Civil Engineers—25, Great George Street, Westminster. 8 p.m.
Syro-Egyptian—22, Hart Street, Bloomsbury. 7 p.m. *Anniversary.*
Ethnological—4, St. Martin's Place. 8 p.m.

15. *Wednesday.*

Society of Arts—John Street, Adelphi. 8 p.m. George Wallis, Esq., "*On the New Art of Auto-typography.*"
Royal Society of Literature—4, St. Martin's Place. 8.30 p.m.

16. *Thursday.*

Royal Institution—Albemarle Street. 3 p.m. Prof. Ansted, "*On Geology.*"
Chemical—Burlington House. 8 p.m. Messrs. W. H. Perkin and A. H. Church, "*On Derivatives of Naphthylamine.*"
Royal—Burlington House. 8.30 p.m.
Linnæan—Burlington House. 8 p.m.
Antiquaries—Somerset House. 8.30 p.m.
Numismatic—13, Gate Street, Lincoln's Inn Fields. 7 p.m.
Royal Society Club—St. James's Hall. 6 p.m.
Zoological—11, Hanover Square. 4 p.m.

17. *Friday.*

Philological—Somerset House. 8 p.m.
Royal Institution—Albemarle Street. 8 p.m. Frank Buckland, Esq., "*On the Culture of Fish.*"

18. *Saturday.*

Royal Institution—Albemarle Street. 3 p.m. Prof. Max Müller, "*On Language.*"

THE CHEMICAL NEWS.

VOL. VII. No. 176.—April 18, 1863.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Remarks on the Proposed Application of the Soap-test to the Quantitative Analysis of Water, *by* B. H. PAUL.

THE soap-test, in the form in which it was brought into use by Dr. Clark, is well known to be exceedingly useful in determining certain characters of water which are of importance as regards its economic applications; but Dr. Clark has not proposed to make any further application of the soap-test than the determination of these characters in water.

Some years ago—about 1855—MM. Boudet and Boutron brought before the Académie des Sciences in Paris, an account of a method by which they proposed to effect, to some extent, a quantitative analysis of water, and the means by which they proposed to do this was essentially the same as that employed previously by Dr. Clark, viz., an alcoholic solution of soap. They received a prize for having brought forward this method; but neither MM. Boudet and Boutron, nor the members of the Academy who awarded to them this prize, made any mention of the fact of the soap-test having been previously in use for testing the quality of water, or of its having been introduced by Dr. Clark.

This was in itself sufficiently remarkable; but it is much more remarkable to find a proposal now made before the Chemical Society to adopt the method of MM. Boudet and Boutron, in a somewhat modified form, as an improvement upon the plan introduced by Dr. Clark, and now generally used and established as a practical means of determining the relative qualities of water from different sources.

The reason given for thus proposing to supersede the recognised mode of using the soap-test is, "the want of a process by which surgeons and other scientific men interested in hygienic matters could easily make an analysis of water."

This proposed method consists:—

1. In estimating the hardness of the water in its normal condition;—the hardness thus found is considered to represent calcareous, magnesian, and iron salts, and half of the carbonic acid existing in the bicarbonates of lime, magnesia, and iron, or that existing as the solvent of the corresponding carbonates.

2. In estimating the hardness of the water, after it has been evaporated to dryness with sulphuric acid, so as to convert the bases into sulphates, then igniting the residue, and dissolving it in distilled water to the original bulk. The hardness thus found is supposed to represent the earthy salts alone, inasmuch as the iron would, by the ignition, be converted into oxide, and remain insoluble, and the carbonic acid that held the carbonates in solution would be expelled, together with that existing as part of those carbonates. The amount of carbonic acid and oxide of iron is therefore considered to be represented by the difference between these two results, viz., the hardness of the water in its normal condition, and the hardness of the water when its salts are converted into sulphates.

3. In adding to the water a known volume of solution of nitrate of baryta of known hardness, and estimating the hardness of the mixture. This hardness is considered to afford a means of ascertaining the quantity of sulphates in the water, on the supposition that any sulphuric acid in the water will be precipitated as sulphate of baryta, and that, in this way, the hardness of the solution of baryta added to that of the water will be reduced in a degree equivalent to the amount of sulphates in the water. In the absence of sulphates, the hardness of the water mixed with baryta solution would equal the hardness of both added together; while, if sulphates were present, the reduction of that hardness would represent the quantity of sulphuric acid.

There are other details in this proposed method of analysing water which it is unnecessary for me to refer to now, since those above mentioned will serve my present purpose of pointing out the fact that this method is quite untrustworthy.

In the first place, it is a necessary consequence of the data on which the application of the soap-test is based by Dr. Clark, that the carbonic acid in water cannot be estimated as proposed by Mr. Nicholson. The hardness communicated to water by a given amount of earthy base or metal is the same whether it exists as carbonate, bicarbonate, sulphate, or chloride. It is true that the presence of carbonic acid in distilled water interferes with the indications of the soap-test to some extent, and it may do so in ordinary water; but carbonic acid does not act in the same manner as earthy salts do; that is to say, a quantity of carbonic acid equivalent to a grain of carbonate of lime does not, when combined with carbonate of lime as bicarbonate, produce the same amount of hardness in water as the carbonate of lime does.

In the second place, there is not an equal degree of hardness produced by equivalent quantities of calcareous and magnesian salts, as has long since been shown by Mr. Campbell. This fact, which is probably due to the tendency of magnesia to form double salts, renders it impossible to estimate the amount of magnesian salts in water by means of the soap-test.

In the third place, the proposed method of estimating sulphuric acid cannot give correct results, because sulphate of baryta is not formed in the presence of a large mass of water containing but minute proportions of sulphates and nitrate of baryta. This, among other conditions preventing the formation of sulphate of baryta, has long since been shown to be of importance in analysis.

These facts, then, I consider to be sufficient to show that the opinion I have expressed, as to the untrustworthy nature of this proposed method, is well-founded.

But I may refer also to the results given by Mr. Nicholson to represent the composition of the water supplied to Fort Pitt by the Chatham Water Company as illustrative of the defects of his method. He represents this water as containing, per gallon, 16·905 grains of carbonate of lime, besides carbonate of magnesia and oxide of iron in small amount; but, for the solution of these carbonates, he states, as the result of analysis by his method, that there is only 2·52 cubic inches, or 1·169 grains of carbonic acid per gallon. This is a totally impossible case. There is much discrepancy in the statements as to the solubility of carbonate of lime in water; Mr. Graham giving it, on the authority of Bucholz, as dissolving to the extent of from 2·9 to 4·5 grains in the gallon; Fresenius giving it as 6·6 grains per gallon, while Lassaigne represents it as being much less soluble. Some few experiments which I have made on this subject seem to show that the maximum solubility of carbonate of lime in pure water is not more than 2 grains per gallon; but, according to Mr. Nicholson's analysis of the Chatham water, it would contain 14 grains of carbonates dissolved without carbonic acid.

In order to obtain some positive evidence of the untrustworthy nature of the method represented as so unequalled in exactness for the estimation of carbonic acid in water, I made some comparative trials with water very similar to that supplied to Fort Pitt—the New River water. Its hardness, at the present time, is about 15°, according to Dr. Clark's scale, and of this 11° 5 are due to carbonate of lime held in solution by carbonic acid, the water being softened to that extent by boiling. By evaporation to dryness with sulphuric acid, igniting and dissolving the residue in distilled water to the original bulk, the hardness was, within less than one degree, as much as that of the normal water, the reduction of hardness in this case being due to the separation of some little oxide of iron by the ignition. But if carbonic acid, holding carbonates in solution, could be estimated by this means, there should have been a difference between the hardness of the water in its normal condition and that of the water with its salts converted into sulphates, equal to more than 5°, to represent the carbonic acid holding 11·5 grains of carbonate of lime in solution.

In order to meet an objection that might be raised to this result, viz., that the carbonate of lime might have been held in solution by a very small proportion of carbonic acid, I prepared an artificial water with a solution containing chloride of calcium, equivalent to 16 grains carbonate of lime per gallon, and added to it bicarbonate of soda equivalent to one-half that quantity. There was no precipitate produced; the water remained perfectly clear, and might then be regarded as containing, per gallon, chloride of calcium and bicarbonate of lime each equivalent to 8 grains of carbonate of lime. The hardness of this water was exactly the same, after the addition of the bicarbonate of soda, as it was without it; and when evaporated to dryness with sulphuric acid, as above described, the hardness was not any different. I am therefore satisfied that the calcium hardness of water is invariable, whatever may be the kind of salt present.

This artificial water also afforded a means of ascertaining the solubility of carbonate of lime; for if carbonate of lime were absolutely insoluble in water, the boiling of this water, so as to expel all the carbonic acid, should reduce its hardness to an extent equivalent to the quantity of bicarbonate of soda added; while, on the other hand, whatever part of the hardness originally due to bicarbonate, remained after boiling the water, would represent the carbonate of lime dissolved in water. To obtain a precise result in this way for the solubility of carbonate of lime, of course numerous experiments would be requisite; but I consider the results I have obtained sufficient to justify the opinion that it does not exceed 2 grains per gallon.

In endeavouring to ascertain the amount of sulphates in the New River water I found that the results obtained by Mr. Nicholson's method indicated the absence of sulphates, although this water is stated by Messrs. Graham, Miller, and Hofmann to contain upwards of 3 grains per gallon of sulphuric acid, and although in the ordinary way of testing it gave a marked indication of sulphuric acid. On searching for the cause of this anomaly, I found that a pint of this water, mixed with the baryta solution prescribed by Mr. Nicholson, gave no precipitate of sulphate of baryta.

These simple facts are sufficient to show that whatever want there may have been of a method of analysing water easily, it is still as great as ever.

Note.—I find it necessary to state, in reference to the above paper, that, since bringing this subject under the notice of the Chemical Society, I have with much surprise learnt that Mr. Dugald Campbell lays claim to the contents of it, either wholly or in part, as embodying "discoveries made by him." As Mr. Campbell has not complied with my request that he should particularise what it is which he imagines to belong to him, I am wholly at a loss to understand what can be the meaning of so extraordinary a claim, and I can only observe, in regard to the matter, that I am ignorant of any "discovery" whatever being made known in the above paper, or of its containing any sort of novelty. In my estimation, it is simply a statement of self-evident conclusions from well-known facts, which it was competent for any chemist to use. So far as the experiments referred to in the paper are concerned, I may add that Mr. Campbell did not know of their being made until after they were completed, and therefore that he cannot claim whatever infinitesimal credit might be considered to attach to the making of them, or to the statement of their results.—B. H. Paul.

TECHNICAL CHEMISTRY.

Aluminum.

It is a remarkable indication of the vitality of chemical science, as it is cultivated now-a-days, that the novelties and discoveries which, in such rapid succession, mark its onward progress, no sooner become currently known than they reappear in another sphere of activity, in the form of useful applications in some branch of arts, manufactures, or industry. This circumstance is evidence of a healthy appreciation on the part of those connected with the arts, of the value of results arrived at by chemical investigation conducted with a purely scientific aim. It is evidence, if not of the overthrow, at least of the decline of that monstrous fallacy, which assumes a necessary antithesis and antagonism between the scientific man and the practical man. It is evidence, on the one hand, that practical men are beginning to recognise the necessity of a knowledge more extensive than that which can be acquired by the mere routine practice of their art, whatever it may be; and, on the other hand, it is evidence that scientific men are beginning to appreciate the industrial value of the results obtained by their labours to a greater extent than was formerly the case. It is true we still have the old-fashioned practical man, at times manifesting in a most comical manner his superiority to anything like a recognition of scientific principles, and we still have scientific men persisting in that esoteric bigotry, which has contributed to render science less esteemed than it might have been; some, who unable to disentangle themselves from the mist they have gathered round them, continue to regard the work and the results of the chemist's laboratory as they might the contents of a Wardour Street curiosity-shop. Not very long since paraffin figured in our courts of law, under such patronage as this, as a "chemical curiosity." It is now to be bought in the shape of candles at a price, which, but a few years ago, would have been moderate for mutton dips. But happily the practical and scientific men of this description are but the fossil remains of a past epoch. Before the title of chemical facts and discoveries to be regarded as curiosities, can be established they must first be shown to be useless. The industry which has grown up out of the conversion of aniline into colouring substances is sufficient to show that this subject has not been treated as a "chemical curiosity." Numerous similar instances might be mentioned of the eagerness with which the new results of investigation are seized hold of and made serviceable, and one among these is the progress that has been made in the manufacture of aluminum, and its applications to useful purposes.

In the hands of Messrs. Bell, of Newcastle, the results obtained by Wöhler, Deville, and others, have been brought into a practical shape, and the manufacture of the metal may now be said to be well started.

Unquestionably there are difficulties still to be overcome, but with the start that has been made they are in a fair way of being met. The softness, the dull appearance, and the fragile nature of the metal are objections to its use when compared either with silver or with plated goods. But there are many applications in which these characters would be of far less importance than its cardinal merit of lightness, and, as compared with inexpensive metals, its lesser liability to discolour, tarnish, or oxidise by exposure to the atmosphere.

The applications that have hitherto been made of aluminum have been most in the way of ornamental purposes; but, nevertheless, its price has been brought down to about sixty shillings the pound, whereas three or four years ago it cost as much an ounce.

As regards aluminum itself, one of its most likely applications is probably as a material for statuettes and small works of art of this description, especially if a means could be found of giving to it a richer colour and appearance, either by a kind of bronzing or by the addition of some alloy. It requires a much less intense heat than silver for melting, and, when melted, it solidifies much more slowly. Consequently, it is particularly well adapted for castings that require to be executed with great delicacy.

The sonorous character of aluminum is very peculiar, far exceeding that of silver as regards clearness, and this, together with its lightness, may become serviceable in the construction of musical instruments.

The alloys of aluminum have been less minutely studied than they deserve to be, but the alloy of copper with 10 per cent. of aluminum is one which by its beautiful appearance and other characters will no doubt be of importance. This aluminum bronze has, like aluminum itself, been chiefly applied for ornamental purposes, and its beautiful yellow colour and lustre render it well adapted for such purposes, not because it is like gold, without being it, but because it is nearly as beautiful in itself, and combines with this character an intrinsic value so much less than that of gold, that it may be applied to purposes for which gold could not possibly be used at all. It is very strong, tenacious, and malleable, and remarkably hard, this character being in fact so marked that it constitutes one of the greatest present difficulties in the working of the alloy.

The alloy of aluminum with silver seems likely to prove more useful as a material for articles of domestic use than the bronze, for notwithstanding the beauty of the latter, the fact of its containing 90 per cent. of copper would tend to limit its applicability more to articles of ornament than of utility. The silver alloy would not be open to this objection, but little seems yet to have been done with it.

A very interesting collection of articles manufactured in aluminum and aluminum bronze have been exhibited for some few days by Messrs. Mappin, of Regent Street, who have taken up the working of this metal and its alloys in earnest at their Sheffield works.

The Utilisation of Seaweed.

(Continued from page 172.)

We have already remarked on the saving of weed that Mr. Stanford proposes to effect. He accomplishes this by drying and storing the plants in sheds after they have been allowed to drain in heaps or layers on suitable floors. It appears that it is easy to dry seaweed in this way, and, when dry, that there is no difficulty in keeping it. After drying, the weed may be compressed into cakes by hydraulic or other pressure, thus facilitating transport, stowage, &c., and rendering the residual charcoal denser and more suitable for burning. The dry seaweed, whether unpressed or in cakes, is now placed in retorts, arranged in a suitable furnace, and exposed to a dull red heat. By this proceeding the weed is separated into charcoal, tar, aqueous solutions, and gas.

The gas (about 1200 cubic feet per ton of dry weed) may be employed for heating the rectifying stills or drying-sheds, or for lighting the factory, or it might even be employed for lighting a district, should there be one within available distance.

The retorts that Mr. Stanford recommends would be in the form of cylinders of wrought iron, placed vertically and with proper mechanical appliances for supplying the weed, conducting and collecting the products of distillation, and withdrawing the charcoal. The charcoal may be allowed to fall either immediately into the lixiviating water, or into iron boxes so constructed as to protect it from the air. In the latter case, the boxes are wheeled into the drying-shed, so as to assist the operation of drying by the heat they give out. The charcoal presents a great advantage over kelp in lixiviation, as it floats on the water, so that as the water saturated with salts sinks to the bottom, the lighter portion rises, and a continual current is kept up, obviating all necessity for agitation, &c. Larger tanks are, however, required, as the charcoal is more bulky than kelp.

The solution thus obtained is evaporated down, and treated by similar processes to those before described for extracting the required salts from kelp solutions. The salts obtained by the process have the advantage of being pure and colourless.

After all the soluble salts have been extracted from the charcoal, and the latter has been dried in the air, it may be used for heating the retorts and evaporating the solutions. Peat may also be used as fuel, should it be abundant and easily obtained, or we may suggest seaweed itself, if thrown up in sufficient quantity, may be so employed. The charcoal may also (after the extraction of the salts) be treated with ammoniacal liquid, and, together with the insoluble ash, be used as manure. The ash is most valuable for this purpose, as it usually contains above 20 per cent. of earthy phosphates, which proportion is about the same as that in which these substances exist in Peruvian guano. The addition of crude ammoniacal salt obtained in the course of the process would form a manure worth about 10*l.* or 12*l.* per ton.

The residue left in the retort being thus disposed of, we will consider the "distillate," being that portion containing volatile oil, paraffin oil, naphtha, ammonia, acetic acid, and colouring matter, none of which substances have hitherto been obtained from this source.

The first operation is to remove the tar by means of a syphon, and to distil it with its own bulk of water in an appropriate apparatus. The light volatile oil is thus separated, and floats on the surface of the water that has passed over. The oil is then decanted, and picoline and other oily bases are removed by the addition of sulphuric acid, which also causes the deposition of a red colouring matter. The oil is again distilled after agitation with sulphuric acid and successive washings with water and caustic soda.

The tar from which the oil has been removed is redistilled at a stronger heat, by which means the paraffin oil is obtained, and is purified by sulphuric acid, caustic soda, and redistillation.

The residual pitch may be either used on the spot for fuel, or, by being exposed to a red heat, still more paraffin may be extracted, the residue being a sort of coke particularly valuable on account of its freedom from sulphur.

To return to the liquid left in the condensers after the original distillation was followed by the removal of the tar. Distillation of this liquid with an excess of lime liberates the ammonia and naphtha, which are conducted into muriatic acid, leaving in the retort a crude solution of acetate of lime, which is evaporated to dryness and purified by recrystallisation, &c. The distillate collected in muriatic acid is of course a mixture of sal-ammoniac and naphtha. As the latter of these is the most volatile, it passes over first in the next distillation (conducted by means of steam), and is purified by redistillation over quicklime. The sal-ammoniac is obtained in the usual manner from its solution left in the retort.

This completes the process, which may appear somewhat complicated on paper, but is sufficiently simple in practice, and yields products so valuable as amply to compensate for the trouble and expense of extracting them.

We have before dwelt upon the advantages that the charcoal produced by this process possesses over kelp, both in ease of manipulation and in purity and richness of products. We have now to notice a few other advantages offered by the process:—

1. The factory will, more or less, supply its own light and heat, both being obtained from the weed.
2. It can be erected in the immediate neighbourhood of the source of supply.
3. The manufacture will be continuous, and, of course, independent of weather, as the weed will be stored when most plentiful.
4. Other kinds of weed not containing iodine may be advantageously worked.

It will no doubt be found that, in practice, the process is capable of many improvements; it is also susceptible of modifications. For instance, the charcoal might be allowed to consume at a gentle heat, and the ashes conveyed inland, and treated as kelp is now. But we cannot help thinking that it will be found more advantageous to preserve the process entire, and to send into the market nothing but purified products.

Doubts might have been entertained of the ultimate commercial success of a manufacture necessarily involving considerable expenditure in the first instance, did it not appear, from comparisons that have been made with a manufacture now actually carried on, that the real value of the products is such as will amply remunerate the projectors for their outlay, and soon yield them a large net profit.

The manufacture to which we allude is for the extraction of various substances from peat. The two manufactures are peculiarly suitable for comparison with each other, as in each manufacture similar, and in some cases identical, products are obtained. This comparison has in both cases been based upon averages, and in the case of the seaweeds, fine varieties have been examined.

In the first place, three of the most valuable products from seaweed are absent from the list of peat products. These three are iodine and the chlorides of potassium and sodium, and are of more value than any of the peat products.

Secondly, from 1000 tons of seaweed are obtained about three times as much sulphate of ammonia, considerably more than twice as much paraffin oil and naphtha, and one-half as much again of acetate of lime and insoluble ash as are obtained from an equal weight of peat. When we add nearly 1½ ton of iodine and 50 tons of chloride of potassium, we have a very large excess of valuable products over those obtained from peat. We must, however, make allowance, on the other hand, for nearly 1½ ton of paraffin, 190 gallons of volatile oil, 70 tons of charcoal, and between 31,000 and 32,000 cubic feet of gas.

The analyses from which these results have been calculated were conducted by Mr. Stanford on a somewhat large scale, not merely as minute chemical analyses.

The process has been discussed at the Society of Arts and the Pharmaceutical Society, on both of which occasions the process, as described by Mr. Stanford, met with the warm approval of the numerous scientific and commercial gentlemen present. Mr. Stanford has received the silver medal of the first-named Society for his paper, and another from the International Exhibition for his case of seaweed products exhibited in the Eastern Annex, and, since the closing of the Exhibition, removed to the Polytechnic Institution.

In conclusion, we may remark that it rests with capitalists, kelp manufacturers, and the lairds of kelp districts, to avail themselves of the advantages offered by this manufacture. In due course of time this will be doubtless done, and the resulting benefits will prove that the marine algæ need no longer be called weeds in the generally accepted sense of "noxious and *useless* plants."

Nickel.

The following very interesting and useful information, respecting the metal *nickel*, was communicated by Mr. Lewis Thompson, M.R.C.S., to *Newton's London Journal of Arts*:—

"There is every reason to suppose that metallic nickel is an alloy of that metal with cobalt, in greater or smaller proportion—that, in fact, absolutely pure nickel has not hitherto been obtained. Pure nickel is, however, much more easily made than pure cobalt, for its affinity for oxygen is much less. Taking advantage of this fact, I made up a quantity of pure oxide of nickel into a paste by means of a little water, and forced this paste through a perforated earthenware plate, so as to form it into a granulated mass; when this mass had been thoroughly dried, I introduced it into a porcelain tube, and, after heating it red-hot, I passed a current of pure hydrogen gas over it, and continued this until it had become cold. The grey metallic sponge thus produced was fused with a little borax, in a crucible lined with pure alumina, and yielded a beautiful white silvery-looking button, of the weight of 620 grains; its specific gravity was 8·575, and it was almost as soft as copper. Its malleability seemed very great indeed, for a piece of it was rolled out nearly to the thinness of tinfoil; it showed, however, a disposition to tarnish after a few days' exposure to the air, and became then of a pale yellow colour—a kind of green-sickness tinge. Its magnetic properties were less decided than those of either cobalt or iron; and, judging by the globular form and other evidences of perfect fusion in the button, I believe that nickel is much more fusible than the two metals just mentioned. When portions of it were melted with copper and zinc, in the quantities usually adopted to form albata, it produced a compound vastly superior in appearance to any of the miserable make-shifts that now disgrace our markets. Indeed, I am quite convinced that it would well repay any respectable person to commence the manufacture of pure nickel, and it would not surprise me if a compound of aluminium and nickel could be formed, which, for beauty of appearance, might equal silver, and surpass it in durability and freedom from sulphurous deterioration.

"Whilst alluding to the advantages of an improvement in the manufacture of nickel, it may not be amiss for me to notice two points of some importance in the way of improvement. At present the extraction of nickel from the ore is made to depend very much upon the affinity of arsenic for that metal, so as to form with it an arseniuret of easy fusibility and sufficient specific gravity to separate freely from the melted slag or gangue; and for this purpose large quantities of arsenic are employed by the workmen, not only to the detriment of their own health, but also to the injury of their neighbours. This pernicious practice is quite unnecessary, as I have myself proved by experiments upon a large scale; for example, after carefully roasting six hundred-weight of the common ore of nickel, which is an arsenio-sulphuret, I mixed it with half its weight of chalk, and threw the mixture into a cubilo furnace in full blast; the result was, that the lime of the chalk formed, with the quartz and oxide of iron in the ore, a perfect flux, whilst the oxide of nickel, being easily reduced to the metallic state, fell, in that condition, into the well of the cubilo, from whence it was run out in a melted form, and readily separated from the slag. There was no appreciable loss of nickel in this operation, and the rough metal was found to contain 88 per cent. of pure nickel, the rest being cobalt and iron, with a little sulphur, but no arsenic could be detected in it; moreover, this rough metal might, from the cheapness of the process, have been profitably sold at 3s. per lb., and was decidedly more pure than the ordinary commercial nickel.

"The other point to which I have alluded is applicable to the wet mode of separating nickel, and depends upon a fact hitherto, I believe, unnoticed by chemists. If we have in solution a mixture of the sulphates of nickel, cobalt, zinc, manganese, iron, and copper, we have only to add to this solution, in a warm state, as much sulphate of ammonia as it will dissolve, and then set it aside to cool. Almost every particle of the nickel and cobalt will separate as a green crystallised powder, and leave the other metals in solution. The explanation is very simple. The sulphates of nickel and cobalt form triple salts or alums with the sulphate of ammonia, and these salts are absolutely insoluble in a cold saturated solution of sulphate of ammonia, particularly when this solution is slightly acidulous. I shall conclude these remarks upon nickel by stating that this metal appears to possess the property of 'welding' like iron. At my request, a workman heated two small bars of nickel, which had been previously powdered over with borax, the bars were heated in a forge, and the two hot ends 'jumped' together, that is to say, the white-hot ends were forcibly driven one against the other by gentle blows with a hammer, applied to the other ends, the symmetry of the bar being preserved by blows applied laterally. Although the point of junction was afterwards subjected to much twisting, straining, and so forth, with a view to test its cohesive power, yet it showed no signs of weakness, even after much cold hammering."

PHYSICAL SCIENCE.

*On the Long Spectrum of Electric Light,** *by* G. G. Stokes, *M.A., D.C.L., Sec. R.S., Lucasian Professor of Mathematics in the University of Cambridge.*

(*Continued from page* 164.)

Minerals.—I have examined a large number of minerals by the rays from the induction spark, both as to their transparency and as to their fluorescence. The transparency of those crystals which were of such a form as to permit it, was examined by holding them in front of a pure spectrum formed on a fluorescent screen. The fluorescence was sought for by forming an image of the spark, for which aluminium electrodes were employed, by the 2·5-inch lens, holding the mineral first at the focus of the visible rays, and then moving it up towards the lens, and watching for any image which might be formed by the rays of higher refrangibility. Should such be observed, its nature was further demonstrated by interposing in the path of the rays a very thin piece of mica. This cut off the image by intercepting the invisible rays, with respect to which, except a small portion of the lowest refrangibility, mica is intensely opaque.

Carbonate of lime, the sulphates of lime, baryta, and strontia, and colourless fluor-spar were found transparent (sulphate of strontia less so), at least in the qualified sense above mentioned, thus demonstrating the transparency of carbonic, sulphuric, and probably hydrofluoric acid, and of the bases, lime, baryta, strontia. But this subject would be better followed out by salts artificially prepared, and has been investigated by Dr. Miller. In two cases results of considerable interest were obtained with reference to fluorescence.

At the time of writing my first paper, on the "Change of Refrangibility of Light," I had found but one mineral, yellow uranite, to the essential constituents of which the property of fluorescence plainly belongs.† In many other cases both before and since that time, I have

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* Abstract from the *Proceedings of the Royal Society.*

† *Philosophical Transactions* for 1852, p. 524.

observed with solar light fluorescence in minerals, but always apparently having reference to unknown impurities, and therefore to my mind of much inferior interest. By means of the induction spark, employed as above described, I have found one more fluorescent mineral.[‡]

On receiving the image on adularia, and focusing it for the rays of highest refrangibility, a pair of bluish dots were seen, which were the images of the tips of the electrodes exhibited by fluorescence. As the appearance was everywhere the same, on natural faces and cleavage planes alike, and the same was observed with colourless felspars generally from different localities, it is doubtless a property of the silicate of alumina and potash constituting the crystal. Some specimens, it is true, did not show the effect so strongly as adularia or moonstone, but this is easily explained by the greater purity of the latter varieties. For the fluorescence extended to a very sensible though small depth within the crystal, and yet the rays producing it were cut off by a film of mica much thinner than paper. The intense opacity of mica is doubtless due to peroxide of iron, which nevertheless forms no more than perhaps 5 per cent. of the mineral. Hence a very small percentage of peroxide of iron, or any other impurity having a similar absorbing action, would suffice greatly to reduce the quantity of fluorescent light emitted.

In a concentrated solar beam passed through a suitable absorbing medium, adularia did not show the least sign of fluorescence, in which respect it notably differs from common glass, such as window-glass.

The other case of interest relates to a particular variety of fluor-spar found at Alston Moor, in Cumberland. This variety is very pale by transmitted light, being in part of a brownish purple colour, shows a strong blue fluorescence, and is eminently phosphorescent on exposure to the electric spark. On presenting such a crystal to the spark passing between aluminium electrodes, besides the usual blue fluorescence there is seen another of a reddish colour, extending not near so far into the crystal. On receiving on the crystal the image of the spark, and moving the crystal from the focus of the invisible rays towards the lens, it was soon in best focus for the rays producing the blue fluorescence.

It had to be moved much nearer to the lens before it came into focus for the rays producing the reddish fluorescence, and was then at the distance at which a well-defined image of the tips of the electrodes is formed on the uranium salt; which proves that the reddish fluorescence was produced by the rays belonging to the bright lines (considered as a whole) of aluminium of extreme refrangibility.

The crystal which showed this effect best was externally colourless for about the $\frac{1}{20}$th of an inch, which stratum showed no fluorescence when examined in this way. Then came one or two strata, parallel to the faces of the cube, showing the ruddy fluorescence, and exhausting apparently the rays capable of producing that effect. The blue fluorescence extended much deeper, and presented a stratified appearance, as Sir David Brewster long ago observed.

On admitting a pencil concentrated by a quartz lens parallel to and almost grazing a face of the cube, so that the rays traversed the colourless stratum, the reddish fluorescence was observed in the stratum which produced it to a long distance from the face by which the rays were admitted, which demonstrates the transparency of fluoride of calcium for the rays of very high refrangibility.

The property of exhibiting such a well-marked effect under the exclusive influence of rays of extreme refrangibility, renders such a crystal a useful instrument of research. Several other metals besides aluminium show the reddish fluorescence; but none of those examined showed it so well, partly because it is evidently produced more copiously by aluminium electrodes, and partly because it is less masked by the blue fluorescence, the spectrum of aluminium being rather wanting in brightness until the region of extreme refrangibility is reached.

If the crystal be held near the electrodes, and observed while their distance changes, it will be found that on passing from the greatest striking distance the reddish fluorescence decidedly improves. On still further diminishing the distance between the electrodes, the reddish fluorescence appears still to increase; though whether this is a real absolute increase, or only an increasing preponderance over the blue, it is not easy in this way to say for certain. Hence the copiousness of rays of high refrangibility increases at first, and continues to increase relatively if not absolutely. It is supposed that the jar is sufficiently large to prevent the discharge from degenerating into what will be presently described as the arc discharge.

If the crystal be held close to the contact-breaker when the secondary terminals are separated, and the effect be compared with that of the secondary discharge (a jar being in connection, as has been supposed all along), the electrodes being of platinum for fairness of comparison, it will be found that the proportion of rays of extremely high refrangibility is decidedly greater for the spark at the contact-breaker than for the secondary discharge.

On forming by the 2·5-inch lens an image of the spark from aluminium electrodes, and placing a crystal, such as that above mentioned, in the focus of the rays producing the reddish fluorescence, it is easy to determine the transparency or opacity of substances for those rays, the alteration of the focus by the introduction of a thick plate being of course borne in mind, and the crystal moved accordingly. The rays forming the image have had to pass only through air, and through a very small thickness of quartz, before reaching the crystal. In this way I have found that even quartz itself in very moderate thickness is opaque for these rays; but different specimens, or different parts of the same specimen, vary in this respect. I possess a large plate, 0·42-inch thick, cut perpendicular to the axis of the crystal, which is generally transparent, but is slightly brownish on one side, to the distance of about half an inch from the face of the hexagonal prism. The colourless part of this plate, beyond a little distance from the brownish part, is opaque for the rays in question,[§] while the brownish part is nearly transparent. It may be inferred that the colourless part contains a minute quantity of some impurity capable of absorbing these rays, which does not exist, at least to the same extent,

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[‡] The method by which M. Edmond Becquerel has examined the fluorescence of minerals (*Annales de Chimie*, ser. iii., tome lvii., p. 43) does not permit of distinct vision of the specimen from the distance of a few inches, which seems to me necessary to allow the observer to judge whether the fluorescence which may be observed is due to the essential constituents of the crystal or to accidental impurities.

[§] It should be mentioned that this part contains those delicate, definitely directed, elongated laminæ or crystals, hardly visible except in a beam of sunlight, which are called by practical opticians "blue shoots." An examination of a number of cut pieces of quartz lent me by Mr. Darker confirms me in the suspicion that such crystals are more defective in transparency than other colourless specimens for the rays of extreme refrangibility.—July, 1862.

in the brownish part, although the latter is not perfectly pure silica, as is shown by its colour. On the whole, I am disposed to think that quartz, if it were rigorously pure, would be transparent. We see at any rate how difficult it is to draw certain conclusions respecting the transparency or opacity of a substance which, in the state of purity in which it may be obtained, shows only a slight defect of transparency.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, March 24, 1863.

E. W. Binney, *F.R.S., F.G.S., President, in the Chair.*

Among other donations for which the thanks of the Society were voted, was a framed photograph of the Executive Committee of the British Association in Manchester, by Mr. Alfred Brothers.

The following letter was read :—

"Sir,—1. I have to call attention to some errors in Mr. Dyer's representation of the nature and contents of a short Essay of mine, which was read to the British Association, in 1854, and reprinted, with a few abridgments and emendations, in the *Engineer* for January 23, 1863, and which Mr. Dyer criticises in his 'Notes on the Action of Heat and Force upon Matter,' read to the Literary and Philosophical Society of Manchester on March 11.

"2. Those errors consist, in the first place, of a complete misconception of the object of my Essay; and, in the second place, of misquotations, in which, by the alteration of some words and the omission of others, the meaning of my statements is perverted or reversed.

"3. I have no doubt that those errors arose merely from inadvertency on the part of Mr. Dyer; and I trust that, when they are pointed out, his own sense of justice will make him see the propriety of having them corrected before his paper is published at length in the *Transactions.*

"4. With regard to the object of my Essay, Mr. Dyer says (at page 78 of the abstract of his paper), 'Yet we have seen two elaborate essays, by Dr. W. J. M. Rankine and Mr. Tyndall, each based on the *force heat* assumption, but without affording any solid proofs of its application to the phenomena adduced by them.'

"5. This passage is calculated to convey the idea that my Essay *professes* to give proofs of the mechanical theory of heat. But that Essay does not profess to give a single proof of that theory. It is entitled '*Outline* of the Theory of the Mechanical Action of Heat:' it gives a sketch of the general principles and results of that theory only; and for the *proofs*, it refers to published detailed accounts, by various authors, of experimental researches, and calculations founded on the results of those researches, the whole of which published accounts are in the Library of this Society.

"6. The proofs of this, as of every other physical theory, consists in a mass of experiments and calculations, which cannot be condensed within the limits of a brief popular Essay; and in order to examine and test those proofs, it is necessary to go to the fountain-head of information; that is, to the detailed accounts of calculation and experiment.

"7. As to defective and erroneous quotations, I have in the first place to point out the following :—The words in the genuine passage, whose omission in Mr. Dyer's quotation has the effect of materially altering the meaning of the passage, are enclosed in square brackets, thus : [ ].

"The Genuine Passage.	"The Quotation.
"5. *Threefold Effect of Heat.*—The communication of heat to a substance produces [in general, three kinds of effects (setting aside chemical, electrical, and magnetic phenomena, as being foreign to the subject of the present paper) :—] "1st, An increase of temperature and expansive pressure; that is to say, an increased tendency to the communication of heat to other bodies, and to the development of mechanical power by expansion. "[2ndly, A change of volume; which, under a constant pressure, is an *increase* for every substance, except some liquids near their freezing points.] "3rdly, A change of molecular condition; as from the solid to the liquid state, or from the liquid or solid to the gaseous state, or any imperceptible change of molecular arrangement; the change to the gaseous state being always accompanied by an increase of volume. "The heat which produces [the first of] those effects is known by the name of *sensible heat,* as retaining the form of heat, and, in short, *making the body hotter.* "[The heat which produces the second and third of those effects is called *latent heat,* as having disappeared in developing a mechanical effect, and being capable of reproduction by reversing the change which caused it to disappear.]	"Mr. Rankine states 'that heat produces an increase of temperature and of expansive force in bodies; that the change from the solid and liquid states to the gaseous state is accompanied by an increase in volume; that heat which produces those effects is known by the name of sensible heat, as retaining the form of heat, and in short making the body hotter.'

"8. It is evident that the omission of the words in brackets has the effect of precisely reversing the meaning of the definition which I have given of sensible as distinguished from latent heat.

"9. I have next to point out that Mr. Dyer, in criticising the 4th article of my Essay, represents me as making a certain statement, commencing as follows—'It is natural to suppose that the motion, during this phenomenon, has not been really destroyed,' &c.—in order to account for what he calls the '*non-production* of heat by the moving force exerted in waterfalls.'

"10. Now there does not occur in my Essay one word about any such '*non-production* of heat,' nor one phrase or allusion which by any conceivable amount of ingenuity can be construed to bear any such meaning. The 'phenomenon' really referred to in the passage above cited, and distinctly described in the paragraph preceding it, is the well-established fact of the *production* of heat by the fall of water; and that not as a thing to be accounted for, but as a fact to be reasoned from. In this instance my meaning has again been exactly reversed in Mr. Dyer's account of my Essay.

"11. I shall not enter into any controversy respecting the theory of heat; for it appears to me that physical theories are questions to be settled, not by argument, but by experiment and calculation; and I have nothing at present to add to the experiments and calculations already published.

"12. My only object in writing this letter is that my brief sketch of the mechanical theory of heat, which Mr. Dyer has criticised, may be judged according to its real contents, and may not be erroneously quoted in a publication so important as the *Transactions of the Literary and Philosophical Society of Manchester*.*

"I am, Sir,
"Your most obedient Servant,
"W. J. Macquorn Rankine.

"Glasgow, March 21, 1863."

Mr. Sidebotham exhibited a large sheet of photo-lithographs, executed by Mr. Dean, of Douglas, Isle of Man. They consisted of copies of wood engravings, copper-plate, pen drawings, writing, music, and letter-press, all copied by the camera and then transferred to stone, and printed in the ordinary manner with printing ink. Mr. Sidebotham said he considered this by far the best specimen yet exhibited, from the extreme sharpness of the lines, and thought as this was a branch of art likely to be much used, it was of great interest to see its present state of perfection. He also called attention to the great facility this plan offered to the forger of Bank-notes, &c. Hitherto, although there was no difficulty in getting a perfect *fac-simile* of a note or cheque, the fact of its being printed on paper by silver or iron salts made its detection easy; now that printing ink could be used a perfect *fac-simile* in every respect could be made, so far as the printing is concerned. Mr. Sidebotham thought some steps should be taken in the matter, and suggested, as the most simple mode of defeating the photographer, to use yellow paper and print in blue. So far as at present known, there would be no possibility of copying such a document by any process of photography.

Mr. Sidebotham read a Paper "*On the Planet Mars*," by James Nasmyth, Esq., and exhibited at the same time a large drawing of the planet, as seen by Mr. Nasmyth on the evenings of September 25 and October 11, 1862, by the aid of his fine reflecting telescope of twenty inches diameter. Mr. Nasmyth called special attention to the patch of snow, as it was considered to be, at the south pole of the planet. This was unusually bright and round, like a white wafer on the pole of a globe; it had also a cliff-like edge. This supposed patch of snow does not coincide with the true pole of the planet, but, like the arctic poles of the earth, or poles of minimum temperature, is slightly eccentric.

Mr. Nasmyth also called attention to the different shades of the planet as exhibited in the drawing—which was a faithful representation of the appearance of the planet—and the probability of their being land and water. One ruddy spot in the blue band of the supposed sea he could not help thinking to be an island, and, as its first discoverer, he claims it to be called Nasmyth's Island. He also made some remarks on the superiority of the reflecting over the achromatic telescope, in giving the true colours of the heavenly bodies, and gave his explanation of the supposed cause.

PHARMACEUTICAL SOCIETY.

Wednesday, April 1.

Mr. P. Squire, *President, in the Chair.*

(*Continued from page 176.*)

The *New Standard Barometer*, contrived and described to the meeting by Mr. Symons, is designed to overcome the practical objections to Gay Lussac's syphon-barometer, which would probably be much more used but for two reasons—the absence of any arrangement for making it effectually portable, and the use of a vernier and scales to both limbs of the syphon, necessitating two observations, which must be added together to obtain a correct reading. The instrument exhibited by Mr. Symons has a simple but effectual arrangement for making it portable, and, instead of two verniers, there is a continuous inside brass tube, to be adjusted by a rack to the level of the mercury in the lower limb of the syphon-tube. The barometer is read off by the vernier attached to the top of this internal and moveable tube in the same way as Fortin's, or other standard barometers. No cistern is required, and therefore the instrument is much lighter, simpler, and of course of cheaper construction than Fortin's or other similar barometers, while it appears to possess all the desiderata of a standard barometer, and can be accurately adjusted more quickly than an instrument with an ivory point dipping in the mercurial cistern.

NOTICES OF BOOKS.

Chemical Technology. By F. Richardson, M.A., Ph.D., and H. Watts, B.A., F.C.S. Second Edition. Vol. I. Part III. No. I. Pp. 384. London: H. Baillière, 1863.

The value of this book is very great. Not only are the illustrations of apparatus clearly drawn, but they are adequately explained in the accompanying letterpress,—a point not always sufficiently attended to in similar works. Another merit must be noted—the illustrations are for the most part representations of forms of apparatus in present and actual use, not of antiquated and disused contrivances. Besides the illustrations so necessary in a work of this character, and which are here given profusely (there are 232 in the present number of the work), tabular statements of the results of analyses of ores, products, &c., are scattered through the pages, together with accounts of the working cost of many of the methods of manufacture, and descriptions of many analytical processes. The chemical reactions, on which depend the various processes described, are intelligibly given, with equal regard to easy comprehension and scientific accuracy. The dates of important inventions are duly recorded, while care is taken to give a fair share of praise and appreciation to those inventors who have proposed plans approaching perfection in theory, but which in practice have not as yet been rewarded by success.

The matters discussed in the present part are as follow:—The manufacture, sources, consumption, and uses of sulphur. An amusing figure (No. 20) on page 28, represents the therapeutic application of sulphurous acid, while further on we find many valuable details concerning the manufacture of this acid and its salts, their uses, and processes for ascertaining their value. The next sixty pages are occupied with copious and accurate information regarding sulphuric acid. The recent applications of bisulphide of carbon in the extraction of bitumen from minerals, and oils and essences from seeds, &c., are fully explained.

It would be impossible to do justice to the admirable skill displayed in the selection and arrangement of material in the present volume, except by quoting whole pages of text and many extensive woodcuts; for knowledge regarding salt, brine, brine-springs, English and foreign, soda in all its states, we must refer our readers to pp. 142 to 328 of the work before us. A good engraving of "soda crystals," from a specimen in the International Exhibition, will be found on p. 292. On analysis, this specimen gave results leading to the formula—

$$NaO,CO_2 + 10\ aq.$$

With an excellent account of chlorine and its bleaching compounds the present part of *Chemical Technology* closes. We shall be glad to welcome, before long, another instalment of this valuable work.

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* Mr. Dyer states that in making the quotations from the *Engineer* he had studied brevity, and had no thought of conveying any other than Mr. Rankine's meaning.—Ed.

The Utilisation of Town Sewage. By J. B. Lawes, Esq., F.R.S., F.C.S. Reprinted from the *Journal of the Royal Agricultural Society.* Clowes and Sons.

This little pamphlet of twenty-eight pages contains an account of the experiments carried out under the direction of the Royal Sewage Commission (of which the author was a member) for the purpose of arriving at some practical results bearing upon the interests of agriculture. The Royal Commissioners appointed some years ago "to inquire into the best mode of distributing the sewage of towns, and applying it to beneficial and profitable uses," commenced their inquiry by visiting almost every locality in this country and on the Continent where sewage was applied in any way for the purposes of agriculture, and the evidence they collected on the subject was in every respect very conflicting. Though relating to the practical application of sewage as manure, it consisted almost entirely of mere opinions more or less favourable and unfavourable, and therefore the Commissioners, feeling the importance of the question they were called upon to investigate, and the necessity of obtaining more exact and trustworthy data on which to base their opinion in regard to a subject of such vast sanitary and economic interest, determined upon instituting a series of experiments. These experiments were made on a farm at Rugby, which had previously been manured with sewage, and the above-named pamphlet contains an account of the mode in which they were conducted, and the results arrived at.

The quantity of sewage applied per acre varied from 3000 to 9000 tons per acre per annum, and the quantities of grass obtained per acre were from 14 to 33 tons per acre, as compared with about 9 tons per acre obtained from the land without sewage, the increase in the quantity of green grass per acre being from 3 to 5 tons, according to the quality of sewage applied and the situation of the land. As an average of all the results, this increase was equal to about three-fourths of a ton of hay per acre for each 1000 tons of sewage applied.

The grass thus obtained was consumed in feeding oxen and milch cows in sheds. Neither the oxen nor cows were found to do well on the grass alone, without a mixture of oil-cake, which was supplied to both during the latter part of the experiments. In the case of oxen, the produce of grass per acre proved to be capable of feeding one ox from 45 to 99 weeks, according to the quantity of sewage applied to the land, and as compared with the produce of unsewaged land, which yielded grass sufficient to keep an ox only 33 weeks; but in the latter case the grass was of better quality than the grass from the sewaged land. The increase in live weight of cattle fed varied from 134 lbs. to 297 lbs. per acre, as compared with 88 lbs. in the case of unsewaged land. Taking the average of the oxen, those fed on unsewaged grass gave scarcely $2\frac{1}{2}$ lbs. increase per week on 1000 lbs. live weight, and those fed on sewage grass scarcely $2\frac{3}{4}$ lbs. increase; whereas, with good fattening food, such oxen should give an increase per week of from 9 to 10 lbs. per 1000 lbs. of live weight. The result of this application of the large quantity of grass obtainable by the use of sewage was therefore far from favourable.

In the case of cows, more favourable results were obtained. The average yield of milk per head was less in the case of sewaged grass than in the case of unsewaged grass, being in the one case 20 lbs., and in the other nearly 25 lbs. daily; but the consumption of unsewaged grass per head was greater than the consumption of sewaged grass, in the proportion of 150 lbs. to 124 lbs. The produce of the unsewaged land would keep one cow 19 weeks per acre, while the produce of the sewaged land kept one cow from 41 to 69 weeks per acre, according to the quantity of sewage applied. The total yield of milk per acre was 321 gallons in the case of unsewaged land, and from 570 to 961 gallons for the sewaged land; the quantity of milk obtainable being dependent very much on the quantity of sewage applied; the increase in the quantity of milk per 1000 tons sewage used varying from 5*l.* to 6*l.* in value per acre.

There was apparently but little difference in the average composition of the milk in any case.

The amount of dry substance in the grass varied very considerably—from 7 to 30 per cent.—according as it was of an earlier or later crop, to the state of the weather, and the condition in which it was cut. The composition of the dry substance of the sewaged and unsewaged grass presented considerable differences. That of the more luxuriant sewaged grass contained a much larger proportion of nitrogenous substance than the unsewaged grass; it also contained more fatty and mineral substances. It would seem, however, to be the greater succulence of sewaged grass that is chiefly conducive to the production of the greater yield of milk. The cows on the unsewaged grass consumed more fresh food, containing a larger amount of dry substance, than those on the sewaged grass; they at the same time gave more milk per head than the latter, almost in proportion to the larger quantity of food consumed. But as the sewaged grass contained much less dry substance than an equal weight of unsewaged grass, the dry substance of the sewaged grass produced, weight for weight, a much larger proportion of milk than the dry substance of the unsewaged grass.

The composition of the sewage used in these experiments varied very much at different periods, according to the weather, but the average composition was almost the same in the two fields. It contained on the average 1 part in 1000 of dry substance, suspended and in solution, or about 78 grains per gallon. The average amount of ammonia was nearly 7 grains per gallon, and in 1000 tons of the sewage it was 204 lbs., equal to the nitrogen of the annual mixed excreta of 21 or 22 persons of a mixed population, and also to that in 11 cwts. of Peruvian guano, and the average amount of solid substance was rather more than 1 ton in 1000 tons. Comparing this with the average increase in produce resulting from the application of this quantity of manure, and amounting to only three-fourths of a ton of hay per acre, it will be seen that the manure constituents of dilute sewage cannot be valued at the same rates as those in manures such as guano. This is a most important result as regards the value of sewage as manure, and notwithstanding the striking effect produced in the proportionate increase of the quantity of grass by the increased application of sewage, it requires considerable attention. The experiments made at Rugby are the first in which this circumstance has been at all brought out in a tangible form, and when it is more fully developed it will doubtless constitute a very appreciable correction to be applied to some of the extraordinary statements and exaggerated calculations that have been made public in reference to the value of sewage for agricultural purposes.

NOTICES OF PATENTS.

3150. *Treatment of Pyrites for the Manufacture of Iron.* E. Cajot, St. Servais, Belgium. Dated December 14, 1861.

After the pyrites have been roasted for the expulsion and utilisation of the greater portion of its sulphur, the inventor treats the residue for the manufacture of cast iron. For this purpose it is smelted in an ordinary blast furnace, with suitable proportions of clay and limestone, or chalk.

The mode of production indicated in this patent is not likely to be carried out in this country, where natural sources of iron of vastly superior quality are so plentiful. The large amount of sulphur retained by the pyrites, even after thorough roasting, must involve great difficulties in the process of refining the pig iron.

3116. *Manufacture of Iron and Puddled Steel.* Robert Mushet, Coleford, Gloucestershire. Dated December 12, 1861. (Not proceeded with.)

With the object of introducing small quantities of titanium into steel and bar iron the inventor adds a quantity of titanic pig metal (the preparation of which is described in a former patent*) either to melted pig iron, to refined metal, or any other quality of iron intended to be used in the production of puddled steel or malleable iron.

3151. *The Preparation of Materials applicable to the Manufacture of Paper.* J. Willis, junior. Dated December 16, 1861.

This invention refers to a mode of treating sawdust, oatmeal, and various seeds, for the purpose of adapting them to the manufacture of paper. The sawdust is dried in a kiln, mixed with salt, and ground to a fine powder in an ordinary flour-mill, then sifted, and the finer portion added direct to the pulp for paper-making. The admixture of salt is necessary to prevent the sawdust taking fire by the heat generated during the grinding. The preparation of seeds and oatmeal is very similar, these being reduced to coarse powder under fluted grindstones. The use of a set of brushes, affixed to an endless belt, is recommended, for the purpose of assisting the operation of the sieve.

3208. *Treating Coal and other Bituminous Minerals and Peat, in order to obtain Solid and Liquid Hydrocarbons therefrom.* M. M. Williams, Handsworth. Dated December 21, 1861.

In the distillation of coal, peat, and the other substances named in the title the inventor employs a construction of retort which permits of being heated from the top, so that the vapours are driven downwards through the charge, and make their escape from the bottom of the retort; an excessive exposure to heat is thus avoided, and a more abundant yield of volatile products is the result.

3185. *Improvements in the Manufacture of Paper and Cardboard, with the Object of Preventing Forgery and Alterations in Bankers' Cheques and other Documents, and which Improvements are also Applicable to the Manufacture of Playing Cards, and Railway and other Tickets.* A. Trenille and F. X. Traxler, Paris. (A communication.) Dated December 19, 1861.

The security offered by this kind of paper is based upon the circumstance of its being formed of several individual films superposed, each layer having a different colour and texture, and capable of bearing a distinct water-mark.

3199. *An Improved Composition for Cleaning and Reviving Woollen Cloths and other Fabrics, and the Colours thereof.* E. E. Pereau, Moorgate Street, London. (A communication.) Dated December 20, 1861.

This liquid, which is very efficacious in the removal of dirt and stains from cloth, is prepared from citric acid, alum, carbonate of potash, water, and alcohol. To these ingredients any suitable colouring matter may be added, cochineal being preferred for the treatment of scarlet cloth, and in this case the inventor designates the material "Eau E'carlate."

Grants of Provisional Protection for Six Months.

3072. Christopher Binks, Parliament Street, London, "Improved methods of, and apparatus for, treating linseed and other oils and fats."—Petition recorded November 15, 1862.

586. William Clark, Chancery Lane, London, "Improvements in preparing and obtaining photographic impressions, and in the application of such impressions."—A communication from Alphonse Louis Poitevin, Boulevart St. Martin, Paris.—Petition recorded March 2, 1863.

605. Joseph De Keyser, Molenbeek St. Jean, Belgium, "Improvements in treating petroleum oil, and in the combination thereof with other oils for lubricating and other like purposes."—Petition recorded March 4, 1863.

660. Robert Thomson Monteith, St. Malo, France, and Robert Monteith, Crystal Terrace, Cecil Street, Greenheys, Manchester, "Improvements in making dyes from aniline and its analogues."—A communication from Georges Auguste Jules Delvaux, Rue Corneille, Paris.—Petition recorded March 11, 1863.

698. Richard Moreland, jun., Old Street, St. Luke's, London, "Improvements in apparatus for making extracts of hops, and for selecting or separating the seeds and pollen from hops."

Notices to Proceed.

579. John Watson Burton, Leeds, "An improved mode of refining and purifying oils."—Petition recorded March 2, 1863.

606. Thomas Henry Morrell, Leyland, Lancashire, and Joseph Williamson, Willcross Gisburn, Yorkshire, "A new or improved method of purifying the noxious vapours or gases given off from night-soil or other similar substances during the heating, drying, or evaporating of such substances."

3085. Christopher Binks, Parliament Street, London, "Improved methods of obtaining oxygen and chlorine gases."

3134. Robert Walter Swinburne, South Shields, Durham, "Improvements in the manufacture of soda."

CORRESPONDENCE.

Deaths from Nitric Acid.

To the Editor of the Chemical News.

Sir,—Being a medical student, I was struck with what seemed to me a novel fact in pathology, emanating from a correspondent who signs himself "F. Trachsell." This gentleman, denying the accuracy of the medical opinions as to the cause of death of poor Mr. Stewart, suggests a theory of his own, which is certainly at variance with all surgical experience of the effect of nitric acid when applied in substance to the living tissues.

I would venture to ask him the following question:—If the coagulation of the blood by nitric acid applied to a recent wound is in his estimation to be propagated through the system, and thus be the cause of remote congestions, why does not the application of the same acid in its most concentrated form to phagedænic sores, infantile nævi, and internal hæmorrhoids, &c., produce the same results? Such being the ordinary treatment for those affections, without ever being followed by the awful consequences which Mr. Trachsell alleges. I am, &c.

Llewellyn Powell.

Coal in Holland.

To the Editor of the Chemical News.

Sir,—I expect you duly received my small parcel sent from here, and containing a small lump of the Gildenhuis cannel coal. I fear I have made a blunder in what I wrote, in so far as regards the quantity. In looking over Mr. Eekhout's letter, I read 80,000 Berliner schefftel at 50 kilogrammes each, would make the presumed quantity in the first discovered seam—as far as such has been possible to ascertain, viz., as far as exploration has been proceeded with, which is only very superficially done—about 4000

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* No. 2609. *Vide* Chemical News, vol. vi., p. 35.

tons. I must add, however, that there is some difference of opinion on this subject between practical and scientific men, the latter of whom do not exactly believe that any quantity really worth the trouble and expense of working will be found. However this may be, I shall feel obliged to you to correct any inaccuracy I might have made in referring to the supply, of this, at all events, remarkable mineral found on the very frontier of what, geologically speaking, is a very recent formation; beside the purity of the mineral, *i.e.*, its freedom from great quantities of ash, sulphur compounds, &c., is remarkable.

I am, &c.,

Dr. A. Adriani.

P.S.—My further note, dated this day (March 25), settles the point of supply more satisfactorily.

March 25.—I had written the preceding lines, and was about to dispatch them, when I received from H. P. Gelderman, Esq., at Oldenzaal, who is one-third owner of the concession of the locality where this coal is met with, and a full copy of Dr. Vohl's, at Bonn, analysis, and a roughly sketched map of the concession. I give you hereby the abstract of the results obtained by that gentleman.

One hundred parts of the coal carefully distilled, especially with the view of obtaining the largest quantity of tar, gave:—

Tar	45·000
Ammoniacal water	0·340
Coke	40·000
Gas	14·660
	100·000

The tar obtained was of a light brown colour, and solidified at a temperature of + 10° Reaumur, owing to the paraffine it contained; its specific gravity was ascertained to be 0·845, water being 1·000; this tar, when submitted to a carefully conducted fractional distillation, yielded in 100 parts by weight—

Photogene, or paraffin oil	38·888
Gas, or lubricating oil	30·763
Paraffin	2·369
Creosote	16·869
Residue of distillation	11·111
	100·000

One hundred parts of the coal by weight will therefore yield, when treated most suitably for oils,—

Paraffin oil	17·49960
Lubricating oil	13·84335
Paraffin	1·06605
Creosote	7·59105
Coke	40·00000
Gas	14·66000
Ammoniacal liquor	0·34000
Residue of distillation	4·99995
	100·00000

The paraffin oil obtained is quite colourless and clear, gives off an ethereal, not unpleasant, smell; is very fluid, and has a density of 0·825, water being 1·000; it burns in suitable lamps with great brilliancy, without charring the cotton of the wick, nor giving any unpleasant smell. It may be safely applied to remove, as benzol does, dirty fatty marks from woven tissues.

The lubricating oil burns with ease and without causing inconvenience in such lamps as are constructed for the purpose of burning huile de schiste, or mineral oil; it is extremely fit for lubricating machinery, and, being a neutral oil, does not cause the oxidation of metals. This oil may be applied with great advantage for the manufacture of gas. The paraffin obtained by Dr. Vohl from this coal is of very good quality, and may be readily purified and applied for the manufacture of candles. The creosote is fit, in all aspects, to be used as a means of preserving timber, &c.

As the coke left only contains 0·637 per cent. of ash, it is an excellent fuel.

As a gas-producing coal, Dr. Vohl found that, treated in the most advantageous manner for that purpose, 1 lb. of coal yields 12·5 cubic feet of gas—(at the gasworks of the town of Munster the coal was found to yield 22,000 cubic feet of gas per ton, the gas having a specific gravity of 0·745, air being 1·0)—of an illuminating power twice as great as that of the ordinary coal gas; as the crude gas contains only a very small quantity of sulphuretted hydrogen and other impurities, it is readily purified. According to Dr. Vohl, a coal identical with this Gildenhuis coal is met with near the river Ruhr, and in France on the left bank of the Rhine. With the exception only of the fossil of Argshalt, in Galicia, Vohl states he is not acquainted with any more fit, or valuable for making paraffin oil and similar produce.

As the present owners are too much engaged in other business matters, they feel inclined to sell their rights to the concession. In order more correctly to learn what the prospects of the supply are, I took the trouble to write on the subject to Mr. Gelderman, at Oldenzaal, whose reply, quite reliable, comes to this—The quantity estimated to be present in the seam, now worked again, is from 80 to 100 thousand scheffel, each containing 50 kilogrammes.* It must be, however, understood that no thorough exploration has been made, owing to dissension between the owners; but this much has been ascertained, that a full supply may be relied on, and that the coal does not change its nature, but remains of the same quality.

The terrain granted for concession to work for coal is from three and a-half to four English miles long, and about one broad.

I enclose the diagram of the seams of coal now, or at least lately worked, and the rather rough map of the district under concession; these you perhaps will do well, and be kind enough to deposit at your office for the inspection of parties interested. Neither the map nor diagram sent to me by Mr. Gelderman were accompanied by any written explanation.

It appears the coal is surrounded by the sandstone of the district. As a railway is in course of construction from Almelo to Salzbergen, which line is contiguous to the locality where the coal is met with, there is no difficulty for transport, as at Almelo means of transport by water begin and proceed uninterruptedly to sea.

Dr. Adriani.

Rotterdam.

On Bathvillite, a New Inflammable Mineral.

To the Editor of the Chemical News.

Sir,—This new mineral, it strikes me, is what geologists call a coprolite—no very uncommon thing either in coals or shales. That the organic matter has the composition of the mineral in which it is found, is also a thing Mr. Williams will find common enough, and, indeed, generally the case with all fossils found either in coals or shales. If all these are to be regarded as new minerals, and provided with distinct names, may Providence furnish us, poor students, with the very best of memories.

Mr. George Miller it seems informs Mr. Williams that his new mineral has not been found in the strata at Torbanehill, but only at Bathville. These are regarded as the same deposit by geologists, and both gentlemen may feel assured that all new minerals found in the coal from the one locality will be found, if looked for, in the other. The mineral at Bathville, I understand, is darker than Torbanehill, not paler, as Mr. Williams states.

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* 80,000 × 50 = 4,000,000 kilos., and, taking the ton at 1000 kilos., comes to 4000 tons.

Mr. Williams, in his paper, repeatedly calls Boghead coal, or Torbanehill mineral, "torbanite." Does he not know that the greater number of British chemists and geologists call it cannel coal? When we find "new inflammable minerals," what specific characters must they have in order to be "torbanite?" Mr. Williams did not, I am aware, invent the word, but why should scientific writers take up the use of every new useless word until they can give a definition of it. "Bathvillite, a crumbly nodule of torbanite." By-and-by we shall have a name for every particular nodule found in the rocks.

Mr. Williams remarks that "torbanite is employed chiefly as a source of photogen." When Mr. James Young discovered that it yielded what he took the liberty of naming "paraffin oil," he thought he had produced this fluid from coal; it appears, however, now, that he had produced photogen from torbanite. Mr. Williams' friends, one may suspect, would require to

"Streak him canny wi' the hair,"

if one of his names were to undergo alteration. In his announcement to chemists in a late number of your Journal, that he is about to give birth to a paper, he takes care to correct Dr. Hofmann in the name of his blue dye.

When Mr. Williams describes or figures a glass tube, drawn out to form a retort or receiver, does he think no chemist ever before imagined such a piece of apparatus? We suspect, however, we give ourselves too much trouble; he must have intended his paper as a satire on some production we are unacquainted with. I am, &c.

Ein Untersucher.

Phwilhelli, April 6.

MISCELLANEOUS.

Chemical Society. — Arrangements have been made for the delivery of the following lectures respectively at the next three meetings of the Chemical Society:—May 7, Dr. Lyon Playfair, C.B., F.R.S., "On the Constitution of Salts;" May 21, W. R. Grove, Esq., Q.C., F.R.S., "On Certain Effects of Intense Heat on Fluids;" June 4, M. Marcellin Berthelot, "On Synthetic Methods in Organic Chemistry."

Chinchona Plants in India.—The advantages which the important and beneficial measure of introducing chinchona cultivation are likely to confer upon India may be considered under three heads:—1st. In their bearings in relation to the State, by providing an abundant and certain supply of bark for the use of hospitals and troops, and effecting a saving of many thousands of pounds. 2nd. As a commercial speculation, and a means of adding to the resources of the country. And, 3rd, though not least, as a boon to the people, by bringing the remedy within the reach of frequenters of jungles, and of the native population generally. The Government, by working their own plantations, will eventually save at least 20,000*l.* a-year, while they will secure a large and unfailing source of quinine supply for their own servants, the importance of which it is impossible to exaggerate. Dr. Macpherson, of Calcutta, tells us that since quinine has been extensively used among the troops in India, there has been a steady diminution of mortality; and whereas, in 1830, the average percentage of deaths to cases of fever treated was 3·66, in 1856 it was only 1 per cent. in a body of 18,000 men, scattered from Peshawur to Pegu. We next come to the consideration of chinchona cultivation in its commercial aspect, and as a good investment for capital. Private enterprise will be supplied with plants from the Government nurseries on the Neilgherries; 20,000 are to be sold this year, at four annas (sixpence) a piece, and next year a much larger quantity will be available. As many as 35,000 have already been ordered by companies and private planters, and there can be no doubt that, as soon as land can be had, the demand will be almost unlimited. When it is considered that it will not cost more than two annas (threepence) to produce a pound of red bark, which is now selling at from half-a-crown to eight shillings in the London market, there is every reason to think that men of business will not overlook so promising an investment, and that the great difference between the cost of production and the value of the produce will give chinchona cultivation a good name as a safe speculation, in combination with coffee, the former beginning at the upper limit of the latter in the same clearing.—*Social Science Review.*

SCIENTIFIC SOCIETIES.

MEETINGS FOR THE ENSUING WEEK.

April 20. *Monday.*

British Architects—9, Conduit Street. 8 p.m.

Medical—32a, George Street, Hanover Square. 8.30 p.m.

Asiatic—5, New Burlington Street. 8 p.m.

21. *Tuesday.*

Royal Institution—Albemarle Street. 3 p.m. Professor Marshall, "*On Animal Mechanics.*"

Civil Engineers—25, Great George Street, Westminster. 8 p.m.

Zoological—11, Hanover Square. 9 p.m.

Statistical—12, St. James's Square. 8 p.m.

Pathological—53, Berners Street, Oxford Street. 8 p.m.

22. *Wednesday.*

Society of Arts—John Street, Adelphi. 8 p.m. Captain T. E. Symonds, R.N., "*On the Construction of Twin Screw Steam-ships.*"

Geological—Burlington House. 8 p.m.

Royal Society of Literature—4, St. Martin's Place. 4 p.m. *Anniversary.*

Archæological Association—32, Sackville Street. 8.30 p.m.

23. *Thursday.*

Royal Institution—Albemarle Street. 3 p.m. Prof. Ansted, "*On Geology.*"

Royal—Burlington House. 8.30 p.m.

Antiquaries—Somerset House. 2 p.m. *Anniversary.*

Royal Society Club—St. James's Hall. 6 p.m.

24. *Friday.*

Royal Institution—Albemarle Street. 8 p.m. Alex. F. Herschel, Esq., "*On Luminous Meteors.*"

25. *Saturday.*

Royal Institution—Albemarle Street. 3 p.m. Prof. Max Müller, "*On the Science of Language.*"

Royal Botanic—Inner Circle, Regent's Park. 3.45 p.m.

ANSWERS TO CORRESPONDENTS.

C. V. Babin.—Van Voorst, Paternoster-row. Price 12s. 6d. we believe.

J. H. M.—You will probably obtain it of Mr. Gregory, the mineralogist.

J. W. Y. (*Glasgow*).—We do not imagine the mixed precipitate has been described.

—, (*Bristol*).—1. Weights from a grain to 7000 grains can be obtained of the balance makers. 2. At our Office, Wine Office Court, Fleet Street.

B. S.c.—There is an attempt made every year to form an evening class at the Birkbeck Laboratory, University College, but it generally fails for want of pupils. We know of no other at present. The Secretary of the College will give you information of the time.

W. S. S. (*St. Austell*).—1. We are not acquainted with the process at present. 2. Liebig's solution for silvering glass is made as follows:—Dissolve 154·3 grains of fused nitrate of silver in $6\frac{3}{4}$ fluid ounces of water, and add enough ammonia to dissolve the precipitate formed at first. Then add gradually $15\frac{1}{4}$ fluid ounces of liquor potassa, sp. gr. 1·05, and if any precipitate forms add more ammonia to re-dissolve it. Now put enough water to make $49\frac{1}{4}$ ounces, and then drop in a dilute solution of nitrate of silver until a decided grey precipitate falls, after which dilute the solution to 51 ounces. When required for use the clear solution is mixed with one-eighth to one-tenth of its bulk of a solution of one part sugar of milk in ten parts water.

THE CHEMICAL NEWS.

Vol. VII. No. 177.—April 25, 1863.

UTILISATION OF SEWAGE.

From a chemical point of view the sewage question has long been definitively settled. There is no doubt of the immense intrinsic value of the manure constituents of sewage which are annually wasted under the present system of disposing of it. Nor is there any less doubt that, under this system, those valuable constituents are distributed through such a disproportionately vast mass of water, that it is entirely out of the question to think of turning them to any profitable account in agriculture. The case is precisely analogous to that of the gold-bearing minerals in Wales, or the auriferous Rhine sand. The gold is undoubtedly there in immense quantity in the aggregate; but it is so disseminated throughout a preponderating mass of worthless material, that it is practically inaccessible. No chemist acquainted with the subject; no engineer, or farmer at all capable of appreciating the chemical facts relating to it, has, or can have, but one opinion as to the utilisation of sewage, viz., that it is a thorough delusion to suppose that it can be carried into effect, so as to admit of the sewage of London being disposed of, and made a source of profit.

This opinion, however, is far from being accepted or acquiesced in generally. A great number of persons believe—for it is only belief with them—that the sewage of London, representing an aggregate value of perhaps not less than a million sterling per annum, can not only be utilised in agriculture, but even made a source of profit to those whose business it is to get rid of it somehow. There are, indeed, a few who endeavour to bring forward evidence of such a possibility, and there is no doubt that many of the facts which they rely upon are unquestionable. There is no doubt that sewage, when put upon cultivated land, does act as a powerful manure, and produce very excellent results in augmenting the produce of the land. This has long been proved and admitted by every one; and this is precisely the circumstance which leads people astray in their opinions as to the utilisation of sewage. If, they argue, this can be done here or there on this or that patch of ground, why should it not be done with the whole of the sewage of London, and why should not the value of the immense quantity of material now wasted be realised? The reasons why it cannot be done have been given over and over again—any time these six years past—but they have not been heeded. Those reasons are of precisely the same kind as the reasons which operate against the extraction of gold from Welsh minerals, and are probably much more forcible. It is all very well to say there is a bar of gold weighing forty or fifty ounces, and to appeal to that as a conclusive proof that it can be got; or to say that such and such results have been obtained by applying sewage to land. This kind of evidence and argument will have great weight with many, but it is not conclusive, nor is it to the point. If the gold that has been extracted, and which is worth some 4*l.* an ounce, has cost 6*l.* an ounce to get, and if this fact can once be perceived, there is an end to the chimerical opinions as to its value and the possibility of extracting it. To all intents and purposes it might as well not be there.

Now this is just the case with regard to the agricultural application of sewage, with the additional difficulties, in the case of London, that the quantity of the sewage is so immense, and subject to such large increase, as to render it probably impossible to find a sufficient area of land to receive it within a reasonable distance. Further difficulties then arise from the situation and level of the land round London, and, above all, from the fact that it is only to grass land that the sewage could be applied so as to meet the absolutely necessary requirement of disposing of it continuously every day throughout the year. No doubt a larger proportion of the land immediately round London is under grass than is the case in some other districts, but still it is only a fraction of the land that is so situated, and precisely that land is in no want of sewage, being abundantly supplied with manure produced by the consumption of its own hay in London, and carried to the land by the carts bringing up the hay.

There was, some years ago, a bare prospect of the utilisation in some degree of the sewage of London being effected. The inhabitants of London having determined upon incurring a vast expenditure for the purpose of getting rid of the sewage, might have applied that expenditure in such a manner as to render the sewage available to farmers round London, instead of devoting it to a means of throwing it away into the sea, as is to be done now. This was a prospect that was certainly worth investigation at that time; but, it may safely be said that even with regard to it, there are no data which would in any way justify the opinion that such a mode of disposing of the London sewage would have been attended with advantage, or have been at all practicable.

So far as the expenditure on sewage disposition, originally contemplated, is concerned, this opportunity is past. It may be that it will recur again, if, as some are disposed to consider, the means that have been adopted for getting rid of the sewage should prove to be insufficient to meet the requirements of the case. If the discharge of the whole bulk of the sewage into the Thames at one place should be attended with the disadvantages of reflux up the river, and of pestilential exhalations in the neighbourhood of its discharge, the question as to the disposition of sewage will revert to the position in which it stood some six years ago.

The obstinacy with which, from time to time, schemes for the application of sewage in agriculture are urged upon the notice of the public, without any adequate foundation and in the face of overwhelming antagonistic evidence, is simply a revival of the spirit manifested years ago in the attempts to manufacture solid manure from sewage, attempts that were, if anything, more visionary and absurd than the advocated utilisation of sewage under present circumstances. In Mr. Lawes' pamphlet it is stated that an expenditure of some 60,000*l.* was required to satisfy those who insisted upon the very high agricultural value of solid manure obtained from sewage by lime, that the value assigned to it by himself and others was correct. And yet this was a fact that was almost self-evident, and which required only a consideration of the simplest chemical data to be perceived. But years after the utter worthlessness of this method of treating sewage had been laboriously demonstrated *ad nauseam*, ignorant and plausible adventurers succeeded in persuading the authorities of provincial towns to renew the attempt under some novel disguise, and for a time outweighed the sound and trustworthy opinions and advice of men competent to pronounce a judgment on the subject.

The notion now most in favour is to distribute the sewage of London in small quantities over an enormous area, applying it to all kinds of crops; but, if there be any evidence as to the utility of sewage as manure, it is that it depends upon the copious application of it on a comparatively small area, in regard to the actual amount of manure substance it contains. This is the course indicated, not only by practice but also by considerations as to the cost of distribution, and various other important particulars. Hence, it is not surprising to find it stated, as the result of investigation, that an attempt to apply the sewage of London, in its present state of dilution, to crops generally, in quantities of a few hundred tons per acre annually, would result in great pecuniary loss and signal failure, and that it is clearly quite fallacious to assume the general applicability to all crops, of manure so diluted, on the ground of any considerations as to the value and applicability of the same constituents in the undiluted state. The practical manure value of sewage cannot with any justice or reason be calculated according to the amount of manure substances it contains, since it is not known how far they can become available when the sewage has been applied to land.

Over and above all these uncertainties there is the engineering question as to the cost of putting it on the land, of which, perhaps, nothing more than a mere approximative guess has been made.

It would be a strange result if it should be found that the water-carriage system of disposing of town refuse, which has of late years been a source of so much satisfaction to sanitary reformers, has been altogether a mistake, and a retrogression instead of an improvement. Such a result is not without its probabilities, when considered from more than one point of view, nor is it at all improbable that means might be devised for inoffensively disposing of the excreta of a large population without the aid of water-carriage, and that by retaining them in a comparatively portable form they might be rendered available in augmenting and maintaining the fertility of the land in the place of the materials now imported at such considerable cost for the purposes of agriculture. Whether such a change is ever likely to be effected is, however, far beyond the range even of conjecture.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Analytical Notes on Thallium—On the Presence of Thallium in Commercial Hydrochloric Acid—Separation of Thallium from Iron, by WILLIAM CROOKES.

ABOUT a year ago I was engaged in some chemical operations in which many gallons of common hydrochloric acid were employed. In one part of the subsequent process a precipitated sulphide was obtained, which, upon appeal to the spectroscope, appeared rich in thallium. It became of interest to know the source of this metal, especially as the original body under treatment showed no signs of the green line. An assiduous hunt for some weeks at last proved that the source of the thallium was the hydrochloric acid employed, experiments revealing the unexpected fact that many samples of the crude acid contained enough thallium to be easily detected. The particular acid used in the above experiments was obtained from Messrs. Hopkin and Williams, being supplied to them by Messrs. Chance, Brothers, of Birmingham. Upon inquiry, I was informed by the latter firm that the hydrochloric acid is produced in the ordinary way, by the decomposition of common salt in cast-iron pans and fire-brick furnaces. The acid is condensed in high stone towers or chambers filled with coke, and is afterwards collected in gutta-percha cisterns, and bottled or drawn off. The sulphuric acid used for the decomposition of the common salt is obtained from iron pyrites burnt in kilns.

The source of thallium is evidently the pyrites; indeed, a subsequent examination of some of the ore actually employed in this process revealed the presence of thallium. A few experiments clearly pointed out the path followed by the metal:—Being oxidised along with the sulphur of the pyrites, it passed into the leaden chambers, where it dissolved in the acid in the form of sulphate. Upon the subsequent admixture with chloride of sodium, and heating, chloride of thallium was formed, which volatilised with the hydrochloric acid, to be subsequently condensed in the water trickling through the coke towers.

In the endeavour to render the hydrochloric acid free from thallium, a portion of it was distilled, but upon testing the distillate traces of the metal were still perceptible. It was then redistilled, this time at a temperature lower than its boiling-point: the acid now appeared free from thallium.

The volatility of chloride of thallium with aqueous or acid vapour appeared sufficiently interesting to render its more accurate determination advisable. Ten grains of pure metallic thallium were dissolved in a considerable excess of nitro-hydrochloric acid, and the solution gently boiled down in a retort. Upon testing the acid distillate, a considerable precipitate of sulphide of thallium was obtained. Nitro-hydrochloric acid was then added to the residue of sesquichloride of thallium remaining in the retort, and the distillation was repeated over a water-bath, care being taken that the evaporation in this case was conducted below the boiling point of the liquid. Upon now testing the distillate, evident traces of thallium were found in it; the metal in this case could not have been carried over mechanically, as the liquid in the retort had not once entered into ebullition.

Having for upwards of a year had considerable quantities of liquid containing thallium evaporated in open dishes in my laboratory, it was natural to anticipate, after the above experiments, that some quantities of the metal had been thus volatilised along with the aqueous vapour, and would be found adhering to the walls, and deposited with the dust on the upper shelves of the room. A small portion of dust was accordingly removed from a shelf at a height of above eleven feet from the floor, and tested for thallium. A brilliant green line in the spectroscope showed me that this metal was present in more than minute traces.

The analytical detection of thallium in hydrochloric (or sulphuric) acid is not difficult. Dilute ten or twelve ounces of the acid, and render it slightly alkaline with ammonia, now add sulphide of ammonium, and digest at a gentle heat for half-an-hour. A black or dark brown precipitate will gradually collect together, which, when collected on a filter and washed, will generally show the green thallium line in the spectroscope. If thallium is present in sufficient quantity, it may be separated from the iron which constitutes the great bulk of the precipitate in the following manner:—Dissolve in hydrochloric acid, with the addition, if necessary, of a drop of nitric acid. Dilute with water, and filter. To the filtrate add sulphite of soda until the iron and thallium are reduced to the state of proto-salts, then add a drop or two of solution of iodide of potassium. Upon standing for some time a bright yellow precipitate of iodide of thallium will separate.

This will be found a very delicate process for detecting thallium in the presence of iron. By its means I have shown the presence of thallium in thalliferous iron pyrites when working no more than 10 grains of ore, although analysis has showed that this ore only contains one part of thallium in three thousand.

Analytical Notices—Estimation of Nitric Acid,
by M. H. ROSE.

NITRIC ACID dissolved in water is most easily estimated by acidimetric processes. It has been proposed to evaporate the acid liquid with a given weight of oxide of lead; but oxide of lead forms with nitric acid various insoluble basic combinations, which retain a certain quantity of water at 160°, and attract atmospheric carbonic acid. Oxide of lead may be advantageously replaced by baryta water, and carbonate of baryta by carbonate of lime.

Volumetric Estimation of Nitric Acid contained in its own Salts.—M. Pelouze's method consists, as is well known, in making protochloride of iron and free hydrochloric acid act on the nitrate; nitric acid is thus transformed into binoxide of nitrogen, a corresponding quantity of perchloride of iron being produced at the same time. The binoxide of nitrogen is expelled by boiling, and the excess of protochloride of iron estimated by the volumetric process. To obtain exact results the operation must be conducted in a hydrogen or carbonic acid atmosphere.

MM. Lauger and Wawnikiewicz suggest another method (*Annalen der Chemie und Pharmacie*, vol. cxvii., p. 230). They dissolve a given weight of nitrate in water, and then add a quantity of a standard solution of soda, potash, or carbonate of soda sufficient to render the liquid strongly alkaline. The base being precipitated, the excess of alkali is determined in the supernatant liquid. This process is not adapted to the nitrates of fixed alkalis, nor to mixtures containing another acid together with the nitric acid. Otherwise, its results are generally satisfactory.

Estimation of Nitric Acid in the State of Binoxide of Nitrogen.—Mr. Walter Crum estimates nitric acid by transforming it into binoxide of nitrogen. He introduces the salt, dissolved in water, into a graduated gas jar conversely over mercury; he adds sulphuric acid to the extent of three times the volume of the water used. After a few hours the binoxide is disengaged, and the volume of it is measured in taking into account the column of mercury and that of sulphuric acid, as well as the temperature and the pressure. Unsuccessful have hitherto been all attempts to estimate nitric acid by heating the nitrate, apart from the air, with metallic copper and hydrochloric acid, or with silver and weak sulphuric acid in a carbonic acid atmosphere, or by trying to absorb binoxide of nitrogen in a concentrated solution of ferrous sulphate, and by determining the increase of the weight of this salt.

Estimation of Nitric Acid by Distillation with Sulphuric Acid.—A very good method consists in distilling nitrates with sulphuric acid diluted with twice its volume of water, at a temperature not above 160° or 175°. The operation is conducted in a retort with the neck drawn out and bent, so that by means of an india-rubber tube it may be connected with a little receiver with three bulbs (similar to that used for collecting ammonia in the estimations of nitrogen by MM. Will and Varrentrapp) containing a known volume of a standard solution of soda or potash. The distillation must be continued for three or four hours to obtain one or two grammes of nitrate.

The distillation may be effected by a water bath in a vacuum either by means of an air-pump, or by expelling the air from the apparatus by boiling, and then closing hermetically. If the nitrate is mixed with chloride, a solution of sulphate of silver or moist oxide of silver is added previous to distillation.

Estimation of Nitric Acid in its own Salts after Precipitation of the Bases by Alkaline Earths.—When the nitrate is soluble its solution is decomposed by hydrate of baryta, carbonate of baryta, or carbonate of lime. The base is easily estimated in the precipitate, and the nitric acid in the filtered liquid freed from excess of baryta. The liquid is merely to be evaporated to dryness, as described above.

Estimation of Nitric Acid after the Precipitation of the Bases by Sulphuretted Hydrogen or by Soluble Sulphides.—The nitrates of metals precipitable by sulphuretted hydrogen being first decomposed by this reagent, the nitric acid can be estimated in the liquid by baryta, the excess of sulphuretted hydrogen being of course previously eliminated by sulphate of copper. Hydrosulphate of ammonia, or sulphide of barium does not well take the place of sulphuretted hydrogen; there is risk of the formation of hyposulphites, which might considerably interfere with the estimation.

Estimation of Nitric Acid by the Elimination of this Acid.—Nitric acid may be estimated by difference in some salts decomposable by calcination, unless the production of a superior oxide of the base may occur. Nitric acid may be driven off by sulphuric acid, the weight of the sulphate being deducted from that of the nitrate.

Nitrates with energetic bases are transformed into chlorides by calcination with chloride of ammonium.

Nitrates may also be decomposed by fusion with borax, as proposed by M. Schaffgotsch, or, better still, by bichromate of potash, as suggested by M. Persoz

(*Répertoire de Chimie Appliquée*, 1860, vol. ii., p. 254). The crucible is weighed with the alkaline nitrate, heated, and melted sufficiently to melt the nitrate; bichromate added after cooling (2·25 of bichromate for 1 nitrate); the whole is gently heated and weighed. The crucible is very gradually heated to dull red heat, and weighed after cooling. The difference gives the weight of nitric acid. Neither chlorides nor sulphates are decomposed under these conditions.—*Poggendorff's Annalen der Physik und Chemie*, vol. cxvi., p. 112.

Analysis of Tin Used for Domestic Purposes,

by MM. Millon *and* Morin.

The chief drawback to the analytical process used in investigating the composition either of utensils of massive tin, or of thin layers of tin covering other metals, especially copper and iron, is the excessive weight of the metal to be operated on recommended in special treatises on the subject.

Fifteen or 20 grammes of the metal are not always easily obtained by scraping a tinned surface. Now, with a tenth of this weight the analysis may be quite as satisfactory, if not more so.

We have been led to reduce the weight of the metal in experimenting with the nitric acid process and tin. Before describing another method it will not be out of place to specify the modifications of the old method of analysis, which, while these greatly improve it, yet fail to remove the principal objections to which it is open.

Introduce about 1 gramme of the tin to be analysed into a glass balloon of a capacity from 150 to 200 c.c.; dilute pure concentrated nitric acid with its volume of water, and pour it in portions on the metal. When the metal has completely disappeared, empty the balloon into a porcelain capsule, and evaporate to dryness, without exceeding the heat necessary to vaporise the excess of acid.

Pour from 120 to 125 c. c. of hot distilled water, with from 15 to 20 drops of nitric acid on the residuum; throw the matter thus detached from the capsule on a filter and wash; in the filtered liquid are to be sought the metals soluble in nitric acid.

This process gives perfectly exact results when tin is associated with lead or zinc, or with the two together. Lead is precipitated by sulphuric acid, and zinc by carbonate of soda, without altering the usual mode of estimation.

Iron, always present when the tin coating of this metal is operated upon, already imports some difficulties into the analysis; copper has the same effect, and it is easy to show that the nitric liquid with which the insoluble stannic acid is washed does not eliminate from it all the iron and copper.

But if arsenic and antimony still further complicate the composition of the alloy, the simple qualitative investigation of these two elements would prove embarrassing, and their estimation conformable to the directions of analytical treatises would give no satisfactory result. This supposititious case occurred to Alger in recasting considerable quantities of old tin utensils. The antimony oxidised and remained mingled with the stannic acid; the same took place with arsenic acid, no trace of which is to be found in the nitric liquid.

At a later stage in the experiments arsenic and antimony have been found frequently in the products of the scraping, while, in the old analytical process, their presence was not even suspected; they were estimated by the process further on described without the least difficulty.

Into a small balloon from 80 to 100 c. c., furnished with a disengaging tube, put about 1 gramme and a-half of the tin, taking care to reduce the metal to fine grains, if it be not divided by the scraping; nearly fill the balloon with pure fuming hydrochloric acid; adapt to the disengaging tube a bulb tube containing a solution of chloride of gold, and supplement this little apparatus with another bent tube plunged into mercury.

The reaction is evidenced by a slight effervescence; it commences while cold, goes on spontaneously, and ceases in about twenty-four hours.

The presence of antimony or arsenic greatly favours the gaseous disengagement; scraped tin is usually more easily attacked than granulated tin.

The gas directed across the bulb tube is composed of pure and arseniuretted hydrogen; all the arsenic escapes in the latter form; unmixed with antimoniuretted hydrogen, it remains in the chloride of gold, where the arsenic acid is to be sought for and estimated, in the form of ammoniaco-magnesian arseniate.*

When the action of the hydrochloric acid ceases, and no more gaseous bubbles are given off, a more or less abundant black powder is seen at the bottom of the balloon containing all the antimony existing in the tin —antimony free from arsenic, but retaining the copper. There also should be found the bismuth, so often mentioned by former analysts, but which we have never once been able to detect.

The copper remains in the black powder, except when it exists in large proportion in the alloy; above 20 per cent. the copper would dissolve; but this has never occurred in any of our numerous analyses.

The black powder also occasionally contains traces of iron, but so slight that its presence may pass unnoticed.

The supernatant acid liquid is decanted, the black powder washed with distilled water, and the lixivium and acid liquid mixed. After being well washed the powder is attacked by weak nitric acid; with concentrated acid the reaction is so energetic as sometimes to ignite the powder. The excess of nitric acid is got rid of by boiling, and the residuum, containing antimony and copper, is evaporated to dryness, and slightly calcined by a spirit lamp.

The residuum is dissolved in water acidulated with nitric acid, which dissolves the oxide of copper, but takes with it a little antimony. If calcined with too quick a flame the oxide of copper cannot be separated from the antimonic acid.

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* When the proportion of arsenic is considerable, the gaseous disengagement becomes so rapid, that one bulb tube is insufficient to arrest the arseniuretted hydrogen; but this never happens in the analysis of tin for domestic purposes; traces only of arsenic are found in this tin, at the most a few thousandths. However, if in an exceptional instance the amount of arsenic exceeds these limits, the difficulty is overcome by operating on a much less weight of alloy, and using hydrochloric acid diluted with about a fourth of its volume of water.

It should be borne in mind that this method is not absolute, and is not in every case applicable to the analysis of the metals contained in tin used for domestic purposes. It is simply *relative* to the ordinary composition of the alloy used in tinning or manufacturing utensils; we recommend it with confidence, because it has never been found to fail; but we do not give it as a general method of analysis. It is certain, for instance, that hydrochloric acid does not perfectly separate arsenic in the form of arseniuretted gas when the proportion reaches 1·5 per cent.

Antimony and tin alloy resists fuming hydrochloric acid when the antimony is in the proportion of 25 to 30 per cent. The alloy must then be melted (Chaudet's process) with a given quantity of fine tin (Gay-Lussac's process), or to facilitate the action of the hydrochloric acid nitric acid is added to it drop by drop; the antimony is then precipitated by tin, in presence of a large excess of hydrochloric acid. This large proportion of antimony is found in English, or imitation English utensils, but never in those destined for the use of the army.

Any bismuth existing in the alloy would be found with the copper in the nitric solution.

It is evident that this process for separating antimony and copper is not quite perfect; but its drawbacks are easily overcome by pouring pure fuming hydrochloric acid on the black powder, then boiling, and afterwards adding carefully, drop by drop, a saturated solution of chlorate of potash; the powder dissolves, and the copper and antimony are then separated by the aid of sulphide of potassium in excess, in which the sulphide of antimony alone dissolves, while the sulphide of copper remains insoluble.

We have now to examine the hydrochloric liquid decanted from the black powder, and mixed with the washing-water. This liquid holds tin in solution with lead, iron, and zinc; and in it we must also look for the exceptional presence of cadmium, cobalt, and nickel.

The hydrochloric liquid is diluted with several times its volume of water, and a current of sulphuretted hydrogen is directed through it; the lead and tin are precipitated, the iron and zinc remaining in solution. Were the other metals present, the cadmium would be precipitated with the lead and tin, while the nickel and cobalt would be found with the iron and zinc. But the three exceptional metals, cadmium, nickel, and cobalt, must not be taken into account, their presence has only once been shown, and then in insignificant proportion.

The lead and tin precipitated as sulphides are moistened, at a moderate temperature, by hrdrosulphate of ammonia in excess; the sulphide of lead remains insoluble; is washed, oxidised by nitric acid, and weighed in the state of sulphate. The sulphide of tin, completely dissolved in hydrosulphate of ammonia, is precipitated by weak hydrochloric acid, then collected, dried, calcined in the air, moistened with nitric acid, and reheated before being weighed.

After the separation of lead and tin, iron and zinc are searched for in the liquid, through which a sulphuretted hydrogen current has been passed. This liquid is evaporated to dryness and dissolved in hydrochloric acid; the iron is peroxidised by a little nitric acid, again evaporated, redissolved by hydrochloric acid, and to the hot liquid ammonia in excess is added, to precipitate the oxide of iron. But more exact results are obtained by precipitating the iron by succinate of ammonia.

We have now only to examine the zinc in the filtered liquid, which is brought near the boiling point, and carbonate of soda in excess is added by drops, following, in this instance as in the foregoing, the rules laid down in analytical treatises.

In analysing tin-plating or tin utensil, the intervention of hydrochloric acid has the effect of forming three groups of perfectly distinct principles, each containing all the metals which it is necessary to investigate and estimate.

The first analytical results obtained by this method induced the managers of the military tin manufactory of Algiers to use more fine tin, and they found it successful with iron utensils, for which a large proportion of lead alloy was considered indispensable. The work is not inferior, and lasts longer, and there is no question of its superiority on the score of health; and this point suffices to decide the question. However this may be, it is by the aid of new analytical means we arrive at a more exact estimate of the facts relating to the domestic use of tin, and thus this manufacture is sure to be better understood and more real progress made in it.—*Journal de Pharmacie et de Chimie.*

TECHNICAL CHEMISTRY.

Chemistry of American Petroleum and the Products of Destructive Distillation.

MM. J. **Pelouze** and Aug. Cahours have published* the results of their examination of the more volatile portion of American petroleum, that boiling under 200° C. They have succeeded in isolating seven distinct hydrocarbons, homologous with marsh gas, viz.:—

		Density.	Boiling Point.	Vapour Density.
Butyl hydride.........	C_8H_{10}	—	—	—
Amyl hydride.........	$C_{10}H_{12}$	0·628	30°C	2.577
Caproyl hydride	$C_{12}H_{14}$	0·669	—	—
Œnanthyl hydride...	$C_{14}H_{16}$	0·699	92° 94°C	3·616
Capryl hydride	$C_{16}H_{18}$	0·726	116° 118°C	4·009
Pelargonyl hydride	$C_{18}H_{20}$	0·741	136° 138°C	4·541
Rutyl hydride.........	$C_{20}H_{22}$	0·757	160° 162°C	5·040
,, hydride.........	$C_{22}H_{24}$	0·766	180° 184°C	5·458

The amount of amyl hydride in commercial samples of the more volatile portion of petroleum, used as a substitute for turpentine, was found to be about one-sixth to one-seventh by weight.

MM. Pelouze and Cahours express the opinion that these substances appear to be related to paraffin, which is always associated with them in American petroleum, and which, like these hydrides, is characterised by great chemical stability. The same conclusion has already been arrived at by Mr. Schlorlemmer† with regard to the hydrocarbon products of bogheal cannel, and is one that appears to have a high degree of probability. They also consider it possible that there are several solid hydrocarbons constituting distinct varieties of paraffin. This has actually been found to be the case with the solid hydrocarbons obtained from boghead cannel and from peat, those hydrocarbon products being separable into several portions, differing in melting point. In Reichenbach's original memoir on paraffin, the melting point is stated to be 43°·75 C., and this melting point is given in most chemical works. But the paraffin now manufactured for candle making has a melting point of 54° C.

The presence of benzol and toluol in American petroleum, pointed out by Mr. Schlorlemmer, indicates an interesting relation, on the one hand, between that substance and the oil obtained from cannel coal by distillation at a low temperature, and, on the other hand, between the oil obtained in this way and that obtained by distillation at a high temperature. The coal tar of gasworks, and especially that obtained from cannel coal, contains chiefly benzol and its homologues,‡ the only substance belonging to the hydride series yet known to be present in it being paraffin. The oil obtained from cannel coal at a low temperature contains chiefly hydrocarbons belonging to the hydride series, together with some benzol and toluol, as shown by both Greville Williams§ and Schlorlemmer.‖ The proportion of these substances is much smaller than in the coal tar of gasworks, but at the same time it is much larger than in petroleum.

These facts seem to show that so far as the influence of temperature is concerned in the production of these hydrocarbons, the consequent difference in the oily product of destructive distillation is one of degree more than of kind. That is to say, the hydrides are produced

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* *Comptes-Rendus*, lvi. 505.

† Chemical News, vii. 157.

‡ Mansfield.

§ *Phil. Trans.*, 1857.

‖ *Journal of the Chemical Society*, Oct., 1862.

in larger amount at a low temperature, or, more correctly speaking, they escape decomposition to a greater extent at a low temperature, while at a high temperature the hydrocarbon products belong chiefly to the benzol series. This is certainly the fact as regards the oily products obtainable from coal by distillation, and if petroleum has originated from coal in a manner analogous to distillation, which appears to be highly probable from every point of view, the preponderance of the hydrides over substances of the benzol series in it would be strong additional evidence in support of the above view.

It has, indeed, been very positively asserted that the chemical nature of the hydrocarbons obtained from coal by distillation depends entirely upon the temperature, but this assertion has never been supported by any kind of chemical evidence. Altogether, the chemistry of destructive distillation is exceedingly scanty and defective; very little is known as to the conditions which really determine the production of substances of different series, beyond the vaguest and most empirical generalities. There are no facts known which justify the opinion that the chemical nature of the hydrocarbons produced depends upon the temperature of distillation, and what is known with regard to this subject tends to lead towards an entirely opposite conclusion, viz., that the difference which is found to exist in tars obtained at high and low temperatures, as to the relative amount of hydrocarbons belonging to the hydride and benzol series, results from the influence of heat in decomposing substances of the former series, and not from its influence as determining the production of hydrocarbons belonging to one or other series.

The well-known facts observed in gas-making, that the ratio of hydrogen to carbon in the gas is greater in proportion as the temperature is higher, and, on the contrary, that the ratio of carbon to hydrogen is greater in proportion as the temperature is lower, necessarily involve the existence of converse ratios in the oily hydrocarbons simultaneously produced. Hence we find in the oily products obtained from coal at a low temperature a preponderance of substances homologous with œnanthyl hydride $C_{12}H_{14}$, and in the oily products obtained from it at a high temperature a preponderance of substances homologous with benzol $C_{12}H_{6}$.

In the absence of any evidence as to the existence of a similar relation between the products obtainable from other materials than coal by distillation at high and low temperatures, it is only possible to point out the probability that such a relation would be found to obtain generally in the products of destructive distillation. But there are other circumstances to be taken into account of equal importance, and probably of much greater importance as regards the nature of the hydrocarbons obtained by destructive distillation. Chief among these is the nature of the material from which they are obtained. Even in the case of coal, the differences in the nature and proportions of the individual substances obtainable under the same conditions from different varieties of coal are exceedingly great, far surpassing the differences in the products obtainable from the same coal under different conditions of temperature, &c.

The natural products analogous to the oily products of destructive distillation afford an illustration of the differences of this kind that may exist. In all probability native petroleum has in all cases originated in a similar manner, but it is very likely that the materials from which it has originated have been various. Consistently with this probability we find the petroleum of Rangoon[¶] containing, in addition to a considerable amount of liquid hydrides, benzol, toluol, and other members of this series, while the petroleum of Galicia[**] contains also substances belonging to the carbolic acid series.

Altogether the products and general phenomena of destructive distillation present features of the highest interest in a chemical point of view as well as in a technical sense, and it is matter for surprise that they have not received a greater amount of attention and study, especially since the production of the substances originating from this process has become of such immense importance as it now is in so many different branches of industry.

Method of Preparing Colours from Carbolic Acid.

M. Guinon-Marnas gives the following directions for the preparation of peonine:[*]—Twenty-three parts carbolic acid, from ten to twenty parts oxalic acid, and from seven to fourteen parts of sulphuric acid are mixed and heated till the mass becomes sticky and dark-coloured. It is then washed with boiling water to remove excess of acid, and the pitchy residue, which has the appearance of cantharides, is dried and powdered. This substance is then mixed with twice its weight of solution of ammonia, and the mixture heated in a closed metallic vessel to a temperature of about 270° F. for about three hours. By this treatment the pitchy substance is dissolved, yielding a solution of considerable colouring power, which with acids gives a deep red precipitate, consisting of the colour substance.

It is stated that other oxidising substances may be used, such as salts of mercury, arsenic acid, &c., with carbolic acid or its homologues, and ammoniacal salts in the place of ammonia, either with or without pressure.

Azuline[†] is prepared by heating together a mixture of six or eight parts aniline with about five parts of peonine, to a temperature near 212° F. for some hours. This method may be applied to other alkaline bases, such as toluidine, cumidine, and naphthylamine.

PHYSICAL SCIENCE.

On the Long Spectrum of Electric Light,[‡] *by* G. G. Stokes, *M.A., D.C.L., Sec. R.S., Lucasian Professor of Mathematics in the University of Cambridge.*

(*Continued from page* 187.)

I tried reflecting the rays from the spark by a fine Munich grating, but the light was far too faint to be of any use. Possibly a large and very closely ruled plane speculum, with a concave speculum instead of a lens, might give light which it would be possible to observe. But at present I have not found any sufficiently marked effects referable to rays of still higher refrangibility to make it worth trying.

The same crystal which showed the reddish fluorescence was eminently phosphorescent, with a blue colour. The phosphorescence, like the fluorescence, was arranged in strata parallel to the faces of the cube, and, like the reddish, but unlike the blue fluorescence, was not perceptible beyond a moderate distance from the

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[¶] Warren De la Rue and Hugo Müller.

[**] Pebal and Freund. *Annalen der Chem.*

[*] Specification, No. 2130.

[†] 1862. *Ibid.*, No. 2133.

[‡] Abstract from the *Proceedings of the Royal Society.*

surface at which the exciting rays had entered. On forming an image of the discharge by the 2·5-inch lens, focusing the crystal for the rays producing the reddish fluorescence, fixing it there, and breaking the circuit after the induction coil had worked for a little while, a dart of blue phosphorescent light was seen in the crystal at the focus of the lens. On focusing for the rays most efficient in producing the blue fluorescence, the reddish was diffused over a broad portion of the strata producing it; and on repeating the above experiment in this position of the crystal, the blue phosphorescence was seen similarly diffused. This shows that the rays of extremely high refrangibility are those most efficient in producing the blue phosphorescence.

[We may suppose that the blue fluorescence, the reddish fluorescence, and the blue phosphorescence are due to the action of the assemblage of heterogeneous exciting rays on the same substance (doubtless some impurity taken up during crystallisation), or on two or three distinct substances. The blue fluorescence is produced abundantly at a depth within the crystal at which the two other effects are invisible; but this alone is no proof of a diversity in the nature of the substance acted on, because the rays producing the two latter effects would have been absorbed before arriving at such a depth. Hence it is among the early strata, in crossing which rays capable of producing each of the three effects are still vigorous, that evidence must be sought, in the coincidence or non-coincidence of the strata in which the three effects are respectively perceived, of the probable identity or certain diversity of nature of the substance acted on. At the time when this paper was read I fancied I had observed slight discrepancies as to coincidence in the strata. But a renewed examination, in which a larger number of specimens were observed, leads me to regard the fancied discrepancies as too doubtful to rely upon, and to overpower the increasing weight of evidence on the other side.

The blue fluorescence may be observed in the early strata (which ordinarily, at least with electrodes of aluminium and several other metals, show a red) by absorbing the more refrangible of the exciting rays by a suitable plate of quartz, or else by substituting for aluminium some metal, such as magnesium, which is poor in rays of extreme refrangibility. On the other hand, the red fluorescence really existing in the early strata, when it is overpowered by the blue, may be seen by viewing the crystal through a solution of chromate of potash, which greatly enfeebles the blue fluorescence, while at the same time it transmits enough of the spectrum to allow the unabsorbed residue to be at once distinguishable by its colour (green) from the red fluorescence. In this way the red fluorescence may be readily perceived even with electrodes of magnesium. Again, a particular stratum which showed a blue fluorescence when acted on by rays which entered by a face of the cube, and before reaching it had to traverse some other strata showing fluorescence, exhibited a red fluorescence when acted on by rays which fell on it directly, having been admitted through an octahedral face.

It is more difficult to decide as to the identity or diversity of the strata showing respectively red fluorescence and blue phosphorescence, because the two effects are observed in a different way; but as far as I could decide, the strata appeared to correspond.

On the whole, then, I am disposed to think it probable that it is the same substance which, in consequence of the action of rays beginning with a part of the violet and extending from thence onwards, exhibits a blue fluorescence, which, in consequence of the action of rays of extreme refrangibility, exhibits a red fluorescence, and which, in consequence of the action of rays of a similar refrangibility, exhibits a powerful blue phosphorescence. At least, if the substances be different they would appear to have co-existed in solution, and so to have been taken up together in the crystallisation of the mineral. I should mention, however, that it is contrary to all my experience that the fluorescence of a single substance (*i.e.*, not a mixture) should thus, as it were, take a fresh start *with a totally different colour* on proceeding onwards in the spectrum; but then my experience is derived mainly from the examination of substances in the comparatively short solar spectrum.—July, 1862.]

I have said that the phosphorescence was produced in certain strata within the crystal. These strata were in some places sharply terminated, so as to be foreshortened into well-defined lines. On watching the phosphorescence, there was nothing to be seen at all like conduction; the strata remained sharply defined as long as the light was strong enough to enable one to judge. This is at variance with one of the two results which, on the authority of others, I formerly mentioned as indicating a distinction between phosphorescence and fluorescence.§ On trying shortly afterwards along with Mr. Faraday, I could not obtain either of these results. One of them, that relating to apparent conduction, which was obtained by MM. Biot and A. C. Becquerel, has since been explained by M. Edmond Becquerel as an illusion of observation.‖ The other, that relating to the production of phosphorescence in Canton's phosphorus by rays which had traversed a strong solution of bichromate of potash, I am, after a conversation with Dr. Draper, still unable to explain.

Advantage of Broad Electrodes.—At first I employed by preference wires or sharp pieces of metal for electrodes, in consequence of the greater facility with which the discharge passed, and the larger quantity of light given out by the spark. Certain considerations, however, led me to try broad electrodes; and I accordingly procured electrodes of the common metals shaped like small watch-glasses, about an inch in diameter. These showed in some cases a most marked superiority over thin wires, exhibiting the invisible metallic lines in far greater strength, while with some metals there was not much difference. With copper, for example, the superiority was very great, with iron it was comparatively small.

Instead of electrodes of this shape, it is sufficient to take two pieces of thick foil, make them slightly cylindrical by means of a round ruler, or a pencil, and mount them with their convexities opposed and the axes of the cylinders crossed.

Besides, copper, silver, tin, and aluminium show a great advantage of flat electrodes, and lead a moderate advantage; while with zinc, as with iron, sharp electrodes are nearly as good. Brass agrees in this respect with zinc, and not with copper, though it shows the copper lines very strongly.

With such electrodes, however, the spark dances about; and its unsteadiness is objectionable in some experiments. A good part of the advantage of flat electrodes is however retained if one only be flat, especially if this be negative, and the spark is now steadier. Instead of using the end of a wire to com-

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§ *Philosophical Transactions* for 1852, p. 547.

‖ *Annales de Chimie*, tom. lv. (1859) p. 112.

bine with a flat electrode, it seems rather better, according to a plan suggested to me by Dr. Miller, to bend a wire to a gentle curve lying in a vertical plane passing through the prism; or the edge of a flat piece of metal may be similarly employed.

On forming an image of the spark between a sharp and a flat electrode of copper, and receiving it on a fluorescent screen, the flat electrode gave the brighter of the two images already mentioned, and that, whether the electrode was positive or negative.

On similarly forming an image of the spark between two very broad electrodes, and focusing for the rays of highest refrangibility, the image did not, as usual, consist of two separate dots; but whether it was that, from the shortness of the spark, the two ran into one, or that the rays belonging to the metallic lines of high refrangibility were emitted throughout the whole length of the spark, I am not quite certain; but I incline to the latter opinion, as a separation of the discharge into two portions, corresponding to the immediate neighbourhood of the two electrodes respectively, could hardly have escaped detection had it existed.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, April 2, 1863.

Professor A. W. Hofmann, *LL.D., F.R.S., Vice-President, in the Chair.*

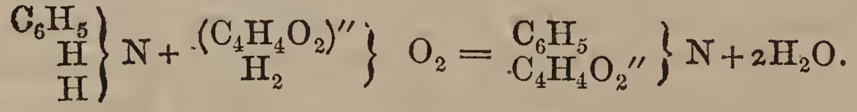
The minutes of the ordinary meeting on March 19, and of the anniversary meeting on March 30, were read and confirmed.

Mr. James Mungo Brown was duly elected a Fellow of the Society, and Mr. Hall and Mr. Madden were admitted, and signed the statute book.

A paper, by **Eugene Sell**, Esq., was read by the **Secretary**, "*On the Substitution Products of Toluidine.*" The author referred to the interesting investigations which had already been made by Dr. Hofmann upon the dye-residues, or "*queue d'aniline*" of the French manufacturers, and stated that he was indebted to that gentleman for a supply of this material, which had served for the preparation of the toluidine employed as the starting-point in his researches. The experiments had resulted in the production of several new compounds, which occupied similar positions in the toluyl series to others already described in the phenyl series. The action of various acids, sulphuric, succinic, &c., had been studied, and the toluyl-urea prepared and analysed.

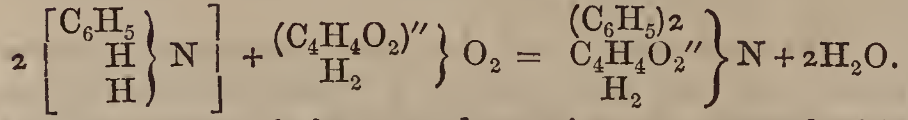
Dr. A. W. **Hofmann** had recently undertaken researches in a direction somewhat parallel to those just now described. By the action of succinic acid upon one and two equivalents of aniline respectively, two bodies were produced, the formation of which might be explained as follows :—

I.



$$\left.\begin{matrix}C_6H_5\\H\\H\end{matrix}\right\}N+\left.\begin{matrix}(C_4H_4O_2)''\\H_2\end{matrix}\right\}O_2=\left.\begin{matrix}C_6H_5\\C_4H_4O_2''\end{matrix}\right\}N+2H_2O.$$

II.



$$2\left[\left.\begin{matrix}C_6H_5\\H\\H\end{matrix}\right\}N\right]+\left.\begin{matrix}(C_4H_4O_2)''\\H_2\end{matrix}\right\}O_2=\left.\begin{matrix}(C_6H_5)2\\C_4H_4O_2''\\H_2\end{matrix}\right\}N+2H_2O.$$

If the product of the second reaction were treated with more succinic acid, it was transformed into the first compound; but, on the other hand, the phenyl-disuccinylamine once formed did not undergo any change on fusion with a larger proportion of aniline.

Dr. **Odling** asked, with reference to the second compound, whether the replacement of the two remaining equivalents of hydrogen by the diatomic radical, $C_4H_4O_2''$, would not necessitate the whole formula being divided by two, and its composition then be represented by the simpler formula already written on the board?

Dr. A. W. **Hofmann** replied that he was unable at the present moment to answer this inquiry, inasmuch as the boiling point and vapour-density had not yet been determined. In allusion to the replacement of two equivalents of hydrogen by a corresponding amount of a compound radical, he had formerly made an attempt to obtain pure diethylamine by acting upon acetamide with the iodide of ethyl. All were aware of the difficulty in obtaining definite products by the action of ammonia upon the iodide of ethyl, the several compound ammonias being always generated at the same time, and presenting a mixture from which the individual constituents were not easily isolated. He had hoped, by the aid of the reaction already described, to obtain one of these ethyl-ammonias in a state of purity; the result was not, however, successful.

In adjourning the meeting until the 16th inst., the Chairman announced that a paper by Messrs. Perkin and Church, "*On the Coloured Products Derived from Naphthaline,*" would then be read.

The Officers elected on March 30 to serve on the Council of the Society during the current year are the following :—*President*—A. W. Williamson, Ph.D., F.R.S. *Vice-Presidents*—W. T. Brande, F.R.S. ; Sir B. C. Brodie, F.R.S. ; C. G. B. Daubeny, M.D., F.R.S. ; Thomas Graham, F.R.S. ; A. W. Hofmann, Ph.D., LL.D., F.R.S.; W. A. Miller, M.D., F.R.S. ; Lyon Playfair, Ph.D., C.B., F.R.S. : Colonel Philip Yorke, F.R.S. ; Walter Crum, F.R.S. ; Henry Bence Jones, M.D., F.R.S. ; Alfred Smee, F.R.S. ; and Robert Warrington. *Secretaries*—Theophilus Redwood, Ph.D. ; and William Odling, M.B., F.R.S; *Foreign Secretary*—Edward Frankland, Ph.D., F.R.S. *Treasurer*—Warren De la Rue, Ph.D., F.R.S. *Other Members of the Council*—F. A. Abel, F.R.S. ; Thomas Andrews, M.D., F.R.S. ; H. Debus, Ph.D., F.R.S. ; J. B. Lawes, F.R.S. ; George H. Makins ; A. Matthiessen, Ph.D., F.R.S. ; Hugo Müller, Ph.D. ; A. R. L. M. Normandy; David S. Price, Ph.D. ; H. E. Roscoe, Ph.D. ; W. J. Russell, Ph.D. ; and John Stenhouse, LL.D., F.R.S.

SOCIETY OF ARTS.

April 8, 1863.

On Wednesday, the 8th inst., a very valuable and interesting paper was read at the Society of Arts, "*On the Supply of Quinine, and the Cultivation of Chinchona Plants in India,*" by Mr. **Clement R. Markham**, the gentleman specially charged by the British Government with the introduction of these valuable plants into that country. The original paper is too long and detailed to be transferred to our pages in its entirety, but we shall give an abstract of the most important items of information contained in it. Mr. Markham commenced by adverting to the inestimable benefits accruing to the inhabitants of almost every portion of the civilised world from the use of the chinchona alkaloids as febrifuges. The lecturer then gave a detailed account of the districts especially fitted by Nature for producing this genus of plants. It extends along the line of the Cordilleras of the Andes for a distance of 1740 miles of latitude on each side of the equator, and passes through four of the South American Republics, namely, New Granada, Ecuador, Peru, and Bolivia, beyond which regions the chinchona plant has not as yet been found growing naturally. The result of the certainty of possessing so close a monopoly has been most disastrous. The inhabitants of these countries appear to have rivalled each other in their reckless destruction of the trees for the sake of their bark with a view to immediate profit only, no system of cultivation or plantation having ever been so much as thought of. Commencing with the northernmost portion of the bark region in New Granada, Mr. Markham states that wild work has been going on there for many years past, the destruction of the trees of the most valuable kinds having become so general, that in 1855 the supplies began to fall off, and although, on account of the last civil war, the forests have had a respite, and small cargoes of bark are beginning to be exported once more from Carthagena, still in a few years the supplies will once more fail. The once invaluable forests of Ecuador on the western slopes of Chimborazo, and on the Loxa mountains, are actually extirpated, fulfilling the prophecies of Condamine and Ulloa, who warned the bark collectors that they were killing the goose with the golden eggs. Even in Humboldt's time the work of destruction had so far progressed that the original variety of the plant first discovered by Condamine, and sent by him to Linnæus, had almost disappeared. The once large forests of the *Chinchona succirubra*, or "red bark" of commerce, are almost entirely devastated, and, on the authority of Mr. Spruce, the botanist, who accompanied Mr. Markham to that district, it is stated that little or no red bark will be exported from that country during the present year. Peru, the very country which gave the name "Peruvian bark" to this invaluable medicament, has almost ceased to supply any of it; fifteen or twenty years of ruthless tree slaughter having resulted in the extermination of the most valuable of the tribe, the *Chinchona Calisaya*. Mr. Markham struggled for days and days through the forest regions of Caravaya in search of this once common, though precious variety, without meeting with one specimen higher than six feet, the beautiful crests of brilliant foliage and sweet-smelling flowers that formerly overtopped the highest trees of the forests having long since disappeared. Bolivia appears to have been only slightly behind her neighbours in this suicidal work. The supplies of Calisaya bark from this country are rapidly being worked out. The Bolivian legislature has, however, from time to time endeavoured to check the extinction of this branch of commerce by stringent legislative enactments, but the decrees have either not been rigidly enforced, or else they have been abrogated. Dr. Weddell tells us that at the centre of what was formerly the chief bark-collecting district, the surrounding forests are now quite cleared of chinchona trees, and that it is necessary to seek them at a distance of ten or twelve days' journey from any inhabited locality. These facts are sufficient to show the amount of danger threatening the bark trade of South America. It must, however, be distinctly understood that the great peril does not consist so much in the total extirpation of the chinchona genus (for this would be beyond the power of even the most reckless of the *cascarilleros*) but in the indiscriminate destruction of all trees above the size of a small sapling. The number and size of the trees being thus diminished, the result will be the stoppage of the supplies during several years and at frequent intervals, rendering quinine a still more costly luxury than it is at present, and quite beyond the reach of thousands, whose lives would be sacrificed from inability to secure it. To an ordinary observer the remedy for this state of things would seem to be stringent forest laws, backed by European capital and enterprise. But the unsettled state of these countries, and the impossibility of procuring adequate labour when required, present insurmountable difficulties. The consequences of a failure or even a diminution in the supply of quinine would result in the most disastrous consequences to the tropical possessions of the British Crown. It is not too much to say that our rule in India is built on chinchona bark, and a quinine famine in that country would be as destructive to Europeans as the food famine was to the natives a short time since. Hence the incalculable importance of introducing the chinchona plant into India. For more than twenty years the late Dr. Royle and a host of other scientific men of eminence endeavoured to render the British and Indian Governments alive to this great fact, but it was not until the matter was laid before that enlightened young statesman, Lord Stanley, in 1859, that any decisive steps were taken. An expedition was formed under the superintendence of Mr. Markham for the collection of chinchona plants and seeds of all the valuable species in South America, and for their introduction into India. The difficulties in the way of accomplishing this great work were simply almost insuperable; but the energy and determination of Mr. Markham and his companions, Messrs. Spruce, Pritchett, Cross, and Weir, overcame every obstacle, and there are now growing in the Neilgherry Hills and in other parts of India some 130,000 flourishing young chinchona plants of the most valuable species. The greatest possible discrimination had to be exercised with regard to the species to be collected, all the valuable kinds having to be obtained, and the worthless sorts having to be rejected. It must also be remembered that the chinchona had never been cultivated, that its conditions of growth were undiscovered, and that it grows in forests scarcely ever visited by any European. These forests are approached by paths which skirt the edges of perpendicular precipices, and pass through the regions of perpetual snow and bleak wilderness, destitute of all the resources needed by the traveller. To add to all this, the forests themselves are close masses of tangled fever-haunted vegetation, so closely matted that every foot of way had to be hewn out. These obstacles would surely have deterred less courageous men than Mr. Markham and his companions from prosecuting the objects of their search, but they endured more than these, for the inhabitants of the country put every conceivable difficulty in their way of finding chinchona localities. When once reached, the forests were found to be utterly denuded of chinchona-trees, nothing being left but a few inconspicuous shrubs, as easily discoverable as a needle in a bundle of hay. By March, 1862, the objects of the expedition were gained, and the whole of the plants and seeds were deposited in the Neilgherry Hills in Southern India, under the charge of Mr. McIvor and his able assistants. In addition to the above-mentioned plants and seeds, they also received six fine plants of *C. Calisaya* from Sir W. Hooker, a specimen of *C. officinalis* from Mr. J. E. Howard, the eminent quinologist, besides several other specimens, including one of the species yielding the Carthagena bark of commerce from the Dutch plantations in Java. The kinds now growing in the Neilgherry Hills embrace all that are considered valuable in commerce. The success and well-being of the plantations in India are due entirely to Mr. McIvor, aided by the invaluable advice and information rendered by Mr. Markham and his coadjutors as to the nature of the habitat of the plants. The spots selected for the plantations were fixed on only after much study and careful examination of soil, elevation, temperature, and general meteorological conditions. The sites chosen resemble as closely as possible in these respects those in South America in which the different species are found, the only fear at first being the amount of moisture natural to the district might not be sufficient for the requirements of the plants; but as they have weathered two very dry seasons all anxiety on that score is at an end. During the first year the operations were confined to the propagation of the plants under glass; and owing to the great talent shown by Mr. McIvor in this branch of scientific botany, he has been able to increase his stock of plants from 8613, in January, 1862, to nearly 13,000 in January, 1863. The plants planted out in the open air are in a very flourishing condition, and cover 745 acres. The Government intend planting 150 acres annually for ten years with the plants grown in the propagating houses, besides which numbers have been sent to Darjeeling to form a nursery, to Travancore, to Ceylon, and to other parts of India. Mr. Markham then drew the grand distinction between mere transplantation and true cultivation, which consists not merely in imitating the surrounding circumstances of the plant in its wild condition, but also in removing the adverse influences which also act upon it in its native forests. The Dutch, in establishing chinchona plantations in Java, appear to have somewhat erred in this particular, by placing it in a forest under dense shade, forgetting that all forest plants necessarily suffer from want of light and air. The successful rearing and transplantation of the chinchonas having been attained, it remained to determine the way in which the harvests of bark should be secured; whether, in fact, they should be reared as bushy shrubs or tall trees. The former alternative was decided on; the chinchonas will therefore be treated as shrubs, the yearly harvest of bark being obtained by simply lopping and pruning, by which they will be rather benefited than injured. Mr. McIvor calculates that the plants will begin to yield quill bark when about six years old until their twelfth year, when a large proportion will be *plancha*, or flat bark. With regard to the amount of alkaloids yielded by the wild bark it is stated to be from three to five per cent. From the cultivated bark, when grown under every advantage that science and practical experience can suggest, the yield ought to be greater. A microscopic portion of bark grown in the Neilgherries was lately submitted to Mr. J. E. Howard for analysis, and as far as so small a piece would enable him to do so, he obtained a satisfactory result. Mr. Markham then entered largely into the commercial advantages to be gained by the cultivation of the chinchona plants by private individuals, who can be supplied from the Government plantations at a merely nominal price. When it is considered that a pound of red bark may be produced for 3d. which will sell in the London market at from 2s. 6d. to 8s., there is every reason to think that men of business in India will not neglect so promising an investment. The lecturer concluded by enumerating the numerous advantages certain to result from the successful introduction of chinchona plants into India. It is well known that intermittent fever now makes fearful havoc amongst the natives of certain districts who are quite beyond the reach of the healing bark. There will be shortly no reason why the poorest ryot should not have a chinchona tree behind his hut, the green bark of which is, according to Dr. Poeppig, as excellent a remedy for fever as the extracted alkaloid. As an additional article of commerce it will of course be a valuable addition in a trade point of view, but from the benefits which the growth of the bark trees will confer on the native population, Mr. Markham prophesies that before many years they will be regarded as the sacred dwelling-places of the Indian gods of healing, Rishi and Aghastya.

During the discussion several interesting facts were mentioned by the various speakers.

The Chairman, Dr. Chambers, suggested that under proper cultivation many of the chinchonas now worthless might be made to yield as large a quantity of alkaloid as the best varieties. He also mentioned that many hospitals and dispensaries were unable to give quinine to their outpatients on account of its high price, so that a large portion of the poorer population were deprived of the use of this excellent drug.

Mr. J. E. Howard considered the Indian experiment to be a decided success, but warned commercial speculators against entering too rashly into schemes for chinchona cultivation. The very first thing to be considered was to procure the proper species of trees, the best, in his opinion, being the *Calisaya* varieties. He then entered into particulars of the influences of soil and locality on the production of different amounts of quinine by the same species, and concluded by passing a high eulogium on Mr. Markham and his companions.

Mr. S. L. Howard reminded the meeting that the salts of chinchonine, an alkaloid at one time thought to be as valuable as quinine, had been made the subject of experiment as febrifuges by several eminent medical men with great success, although many others were prejudiced against them. He thought it highly important that they should be tried as tonics and febrifuges both at home and in India, being much cheaper than the corresponding quinine compounds. He urged the necessity of proving absolutely their value or worthlessness at once, as the bark containing them could be readily procured while they were waiting for the Indian plantations to arrive at maturity.

Mr. P. L. Simmonds gave some interesting statistics of the bark and quinine trade. He also mentioned that the chinchona was in experimental cultivation in the Botanic Gardens, Jamaica, and expressed his surprise that France, Spain, and other countries having colonial possessions with suitable climate and elevations of country, had not turned their attention to its introduction.

Mr. Daniel Hanbury stated that the *C. condaminea* yielded as a shrub a Loxa bark exceedingly poor in alkaloids, while the broad tree bark was so rich as to give the almost worthless shrub bark now obtained a kind of hereditary reputation.

Mr. J. E. Howard, in reply, said that although this was the case with the bark of the *C. condaminea* it was almost the direct contrary with the *C. calisaya*, the species he so strongly recommended for cultivation in India, the small quills often yielding a higher percentage than the flat bark of the larger trees. He severely criticised the neglect of the Government in allowing Mr. Markham, with his valuable cargo, to find his way from Peru to India the best way he could, instead of sending a steamer to convey him from shore to shore. Many of the most valuable plants were irrecoverably lost to India through having perished on their way thither through the cold climate of Europe.

Dr. Seeman said that, knowing the chinchona districts as well as he did, he could better appreciate than describe the enormous difficulties Mr. Markham had surmounted, to whom the greatest honour was due for carrying out so well the noble purposes of the expedition.

A unanimous vote of thanks to Mr. Markham was carried with acclamation.*

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

MICROSCOPICAL SECTION.

March 16, 1863.

Mr. Joseph Sidebotham, *Vice-President of the Section, in the Chair.*

Mr. Watson presented specimens of Jungermannia tomentella and asplenoides, collected on Baguley Moor.

Mr. Sidebotham presented specimens of the following mosses, in fruit:—Fissidens exilis, Fissidens adiantoides, Grimmia pulvinata, Weissia controversa, Bryum atropurpureum, &c., in a good state for microscopical examination.

Mr. J. G. Dale, F.C.S., presented a specimen of crystallised film of picrate of aniline; and in a note to the Secretary explained his method of preparation from picric acid and aniline. The equivalent of picric acid is 229; that of aniline is 93; and when dissolved in strong alcohol in those proportions by weight, mixed and set aside, the picrate of aniline will crystallise in yellow needles. The film for the microscope is formed from a solution of these needles in absolute alcohol, a drop of which being spread over a clean hot glass slide, the crystallised film is at once

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* In his paper, Mr. Markham adopted the innovation proposed by Mr. J. E. Howard, of calling the quinine plants *chinchona* (*tshintshona*), instead of *cinchona* (*sinkona*), the name having been originally derived from that of the Countess of Chinchon (*tshintshon*), the first European patient upon whom the curative properties of the bark were successfully tried. Custom is, however, no respecter of etymology, and it is difficult to eradicate false orthography and pronunciation which have obtained for any length of time.—Ed. C. N.

produced by the rapid evaporation of the alcohol, if the slide be at the proper degree of heat, which can only be found by repeated trials. If too hot, the salt will melt and become partially decomposed; if not hot enough, it will be crystallised in needles, or be deposited as an amorphous film. When properly crystallised, circular radiated discs will appear with more or less regularity, showing with the polariscope very brilliant colours, and a black cross in the centre. The crystallised films may be mounted in *new soft* balsam; but a mixture of chloroform and balsam dissolves them immediately.

The Natural History Society presented for distribution amongst the members a number of beetles not required for the Museum.

Mr. Nevill reported upon the fossil Foraminiferous shells found in the Montreal deposit presented by Mr. R. D. Darbishire at the last meeting. They were mostly in a fine state of preservation, and many were as perfect as recent shells. He found—

Polystomella,
Nonionina umbilicatula,
Polymorphina lactea,
Miliolina seminulum,
Entoselenia squamosa, var. scalariformis,
Ditto of a peculiar form and rare,
Entoselenia marginata,
Ditto globosa, very fine,
Patalina corrugata,
Textularia,
Dentalina,
Lagena vulgaris.

The Polystomella and Nonionina were in great profusion, the other kinds were scarce; but Mr. Nevill was of opinion that remarkably fine specimens might be found of all the various kinds, if there were a larger quantity of material to operate upon. Mr. Nevill was indebted to the worthy President of the Section, Professor Williamson, for verifying the names, and he presented to the Section mounted and named slides for the cabinet. No diatomaceæ were found amongst the material.

Dr. Alcock exhibited a young living salmon, about fourteen days old, attached to part of the ovum. Dr. Alcock particularly called attention to the form of the vertebral column, which, whilst young, is similar to that of the lower grade of cartilaginous fishes when fully grown; the skeleton of the salmon, however, becomes gradually changed, until at maturity it is that of the higher class of osseous fishes.

Dr. Alcock also exhibited a lingual riband of the Patella athletica, from Bray, in Ireland; he compared it with that of the common limpet, Patella vulgata, and pointed out the differences in the form of the teeth.

Dr. Roberts exhibited some mounted specimens of blood-corpuscles from an albuminous urine, which showed an appearance as if the contents of the cells had separated from the cell-wall, and become aggregated round the centre like a nucleus. When these corpuscles were treated with magenta, the central portion was either not coloured at all, or only faintly so, whereas the circumferential portions became deeply tinted. By treating fresh blood with an excess of a solution of carbolic acid this appearance could be produced at will. In the blood-corpuscles of the fowl a similar effect was produced by the carbolic acid solution; the cell contents appeared to detach themselves from the cell-wall and to collect round the nucleus. The appearances presented strongly suggested the idea that the cell envelope of the blood-disc was a double membrane; that the inner separated under certain circumstances from the outer membrane, and shrank in toward the centre. Dr. Hensen, of Kiel,[†] seems to have convinced himself that such is the case in the blood-disc of the frog, and he compares the inner membrane to the primordial utricle of the vegetable cell. Of the prolongations described by Dr. Hensen as stretching radially between the shrunk inner membrane and the outer one, Dr. Roberts saw nothing. If the said view of the structure of the blood-cells were substantiated, it would greatly facilitate the explanation produced on these cells by magenta and tannin.

Mr. Charles O'Neill, F.C.S., exhibited a mounted fibre of Orleans cotton, torn by a gradually increasing weight suspended to its extremity. It had sustained a weight (gradually increased) of 162 grains for many minutes. Mr. O'Neill stated that there were 143 such fibres in ·01 grains of cotton, each fibre therefore weighing less than the ten thousandth part of a grain. The strongest fibres were capable of supporting more than two million times their own weight. He is engaged in making experiments upon the tensile strengths of various fibres by a special apparatus, but they are not yet completed.

Mr. Brothers exhibited a number of fresh water insects, larva, &c.

Mr. Parry exhibited the transverse section of a Fossil Palm from the Island of Antigua.

NOTICES OF PATENTS.

3225. *Manufacture of Colouring Matters.* F. Laurent and J. Casthelaz, Paris. Dated December 24, 1861.

For the production of red colouring matters the patentees act upon nitro-benzol or nitro-toluol in the usual manner with a reducing agent, preferring for that purpose a mixture of iron filings and hydrochloric acid, afterwards converting the aniline or toluidine so formed into colouring matters by treatment with metallic salts, those of iron being particularly specified. The dye is purified by dissolving it in water, and re-precipitating by the addition of salt.

3241. *Treating Fatty and Resinous Bodies either in a Neutral or Acid State.* P. D. L. De Moreau, South Street, Finsbury, London. A communication. Dated December 27, 1861.

The inventor employs "nitramidine" (xyloidine) as obtained by the action of nitric acid upon flour or potato starch, for the purpose of disinfecting crude fatty and resinous matters, this agent at the same time favouring the conversion of the oleic acid into elaidic acid, which has a higher fusing-point, and is consequently better adapted for candle-making and other applications.

3247. *Composition suitable for Painting, Varnishing, and Coating.* J. J. H. Fajole and P. A. Agostini, Courbevoie, France. Dated December 27, 1861. (Not proceeded with.)

The ingredients composing this paint are gum-copal, caoutchouc, oil of turpentine, and sulphate of zinc.

3265. *Manure.* T. Pickford, Fenchurch Street, London. Dated December 31, 1861. (Not proceeded with.)

It is well known that the manure imported into this country under the name of "Sombrero Island Guano," although rich in phosphates, is remarkably deficient in nitrogen, and the object of this invention is to manipulate the natural substance in such a manner that by the introduction of nitrogenous animal matters and ammoniacal salts an effective mixed manure is produced. A certain proportion of ground bones is first incorporated with the Sombrerite, and the whole is converted into superphosphate of lime by digesting with sulphuric acid; a quantity of sea-shells ground to powder and common salt are then introduced, and to every ton of the above mixture is added a hundredweight each of Peruvian guano saturated with urine, magnesian limestone, and sulphate of ammonia prepared from gas-liquor.

There is nothing particularly novel in this combination, nor in its mode of manufacture; if the excellence of a manure is dependent upon the number and variety of its constituents, then, indeed, would this article offer special advantages.

[†] "Siebold and Kölliker's Zeitschrift, for 1861," p. 263.

CORRESPONDENCE.

Deaths from Nitric Acid.

To the Editor of the Chemical News.

Sir,—Allow me to trespass on your valuable space, in replying to Mr. Llewellyn Powell, and to say that, not having read the Medical evidence in the case of Stewart and his fellow-sufferer, I could not have denied its accuracy. But as the case was one of those that fortunately happen rarely, and of great interest to practical chemists, I took the opportunity of instancing the only parallel case that I have ever known, viz., that of my friend Mr. James Heywood, and since in both cases the explanation of the immediate cause of death does not appear sufficient to many persons who are in the habit of using large quantities of nitric acid, and whose testimony agrees with mine, I considered it a subject worth a dispassionate inquiry in all its bearings. The question involved is one pertaining more to physiology than to pathology; and I do not think even Mr. L. Powell would go so far as to recommend the dressing of a fresh wound with applications of nitric acid at 80° Twaddel, whatever he might do in the case of cancerous sores, to which he refers.

Not being desirous to enter into a polemic upon subjects which ought to be decided by actual experiment, I may add that I would have greater faith in charred linen than in nitric acid for the umbilic; and, that since the blood is alkaline, the contact of acids generally, and of nitric acid especially, should be avoided.

If I am offending therapeutics in the hypothesis I advanced, I have at least the consolation of a logical induction in the support of my opinion. I am, &c.

F. Trachsel.

Manchester, April 20.

MISCELLANEOUS.

MR. GLAISHER'S TENTH SCIENTIFIC BALLOON ASCENT.

Mr. Glaisher made his tenth scientific balloon ascent from the Crystal Palace on Saturday last at 1.17 p.m. The descent was accomplished at Newhaven at 2.50 p.m. amidst circumstances attended with some considerable danger. The wind, which at starting was blowing steadily from the north at an estimated velocity of forty miles an hour, would have carried the balloon and its occupants out to sea, but for the intrepid conduct of Mr. Coxwell, who effected their descent, by using the valve so freely as to convert the balloon into a monster parachute. They descended the last two miles in four minutes; had they done so less rapidly they must have missed the land, the place where they descended being less than half a mile from the sea. The wind at the time was blowing at the rate of nearly a mile per minute directly out to sea; it is clear therefore that a stay in the air of half a minute more would have placed them in great peril.

Mr. Glaisher made a number of observations of the thermometer, and obtained a series of results which tend to confirm his previous experiments, and conclusively establish the necessity of rejecting the theory of uniform decrease of temperature with increase of elevation. From 2.15 to 2.31 numerous observations were made of the lines of the solar spectrum. When the direct light of the sun entered the slit the lines were, of course, present in great numbers, the spectrum being considerably lengthened at the violet end, and the nebulous lines H being distinctly visible. At the red end A was seen very clearly. The light from the sky in the immediate neighbourhood of the sun gave a shorter spectrum, the lines from B to G only being visible. From this point the spectrum shortened considerably, until the spot opposite the sun was reached, which gave no light at all.

The photographic observations made by Mr. Glaisher are interesting. He took with him slips of sensitized paper, having arranged that similar slips made at the same time should be exposed at the Royal Observatory, Greenwich, and the amount of coloration per minute noted at simultaneous intervals. On comparing results, it was found that, when about three miles high, the paper did not colour in half-an-hour so much as it did in the grounds of the Royal Observatory in one minute. This fact is so singular as to require confirmation before any explanation is sought. The temperature at three miles was 21° F., but this great degree of cold could hardly account for so great a diminution of sensitiveness.

The registrations of temperature were 61½° before starting; 41° at one mile; 32° at two miles; 21° at three miles; 16° at four miles; and 12° at four miles and a-half, the highest point reached. The air was extremely dry, both at starting and during the whole ascent. Clouds were reached at the height of one mile, and, on passing them, the sea of shining white clouds extending to the horizon was exceedingly fine.

SCIENTIFIC SOCIETIES.

MEETINGS FOR THE ENSUING WEEK.

April 27. Monday.

Royal Geographical—Burlington House. 8.30 p.m.

Institute of Actuaries—12, St. James's Square. 7 p.m.

Medical—32a, George Street, Hanover Square. 8.30 p.m.

Philosophical Club—6 p.m.

28. *Tuesday.*

Royal Institution—Albemarle Street. 3 p.m. Professor Tyndall, "*On Sound.*"

Medical and Chirurgical—53, Berners Street. 8.30 p.m.

Civil Engineers—25, Great George Street, Westminster. 8 p.m.

29. *Wednesday.*

Zoological—11, Hanover Square. *Anniversary.* 1 p.m.

Society of Arts—John Street, Adelphi. 8 p.m. D. T. Ansted, F.R.S., "*On the Varieties of Minerals Used Economically, Considered in Reference to their Geological Position and relative Value for certain Purposes.*"

London Institution—Finsbury Circus. 12 p.m.

30. *Thursday.*

Royal Institution—Albemarle Street. 3 p.m. Prof. Ansted, "*On Geology.*"

Royal—Burlington House. 8.30 p.m.

Antiquaries—Somerset House. 8.30 p.m.

Royal Society Club—St. James's Hall. 6 p.m.

May 1. Friday.

Royal Institution—Albemarle Street. 8 p.m. John Leighton, Esq., "*On Japanese Art.*" *Annual Meeting* at 2.

Philological—Somerset House. 8 p.m.

Archæological Institute—26, Suffolk Street, Pall Mall. 4 p.m.

2. *Saturday.*

Royal Institution—Albemarle Street. 3 p.m. Prof. Max Müller, "*On the Science of Language.*"

ANSWERS TO CORRESPONDENTS.

⁂ In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

Ein Untersucher.—A reply to the strictures of this correspondent relative to Mr. C. G. Williams is unavoidably postponed until our next Number.

THE CHEMICAL NEWS.

Vol. VII. No. 178.—May 2, 1863.

QUININE SUBSTITUTES.

In our last Number we gave an abstract of the very interesting paper read by Mr. Markham before the Society of Arts, on the "Introduction of Chinchona Plants into India." The facts contained in it lead us to suppose that the anticipated quinine famine will not be so severe as most people have appeared to think. At the same time, there is a period of at least ten years to pass through before we can hope to obtain chinchona barks in any quantity from their new country. Bark merchants shake their heads and sigh dolefully when they are questioned about the prospects of the bark trade during the next decade. The mischief that would result from a bark famine, not merely to our colonies, but to our hospitals and dispensaries at home, is fearful to think of. As it is, Dr. Chambers assures us that many institutions have been compelled to discontinue the use of quinine altogether, on account of its dearness. At the same time, the Registrar-General tells us that febrile maladies are on the increase. What, then, is to be done? Looking at the matter from a scientific point of view, there appear to be three remedies applicable to the case:—1. The use of the salts of chinchonine. 2. The discovery of the method of making quinine artificially. 3. The successful substitution of this febrifuge by some other organic compound, natural or artificial.

The first remedy is perfectly applicable for the present only. Our largest chinchona alkaloid manufacturers, Messrs. Howards, introduced the hydrochlorate of chinchonine to the medical profession at the late International Exhibition at a comparatively low price, and, in spite of the well-known conservative principles of certain members of that body, they have since manufactured and distributed large quantities of this alkaloid and its salts over all parts of the world. But the low price of chinchonine cannot be expected to continue. Some years since, we recollect that the same firm introduced quinidine into the market at a low rate, but the demand soon rose above the supply, and quinidine went up nearly to the price of its brother, quinine. We fear greatly that the same thing will occur with the last valuable addition to our stock of febrifuge alkaloids. It is, however, a remedy for the present, and should be well tried and reported on by those who are competent to do so. The second remedy—the discovery of the method of forming quinine by synthesis—should be again sought for. In 1856, Mr. W. H. Perkin endeavoured to form this alkaloid artificially by oxidising allyl-toluidine with bichromate of potash; but instead of discovering quinine, he discovered aniline purple, the magnificent golden reflection of which so dazzled his eyes, that he naturally gave up his search for the healing alkaloid. Since that period neither the talented chemist just named, nor any other of our vast army of workers in organic chemistry, appears to have taken up the subject. The discovery of the third remedy seems to be closer at hand. Amongst the vast number of natural organic bases, how few are there that have received proper trials as to their therapeutic qualities! Phloridzin, esculin, salicin, and some half-dozen more, have been partially tried at various times, and are said, in certain pharmacological works, to be febrifuges; but we have little information about them that is to be relied on. All of them are, however, to be easily obtained and experimented upon. But the largest field open to the ambitious, would-be discoverer of *the* substitute for quinine, seems to us to be amongst the innumerable artificial organic bases, acids, and neutral products that are being discovered daily in almost every laboratory in the world. The members of this vast group of substances that have received attention at the hands of therapeutists may be reckoned on the fingers of one hand. Propylamine has been experimented on by Avenarius, in Russia, and by Procter, in America, who state that it is a valuable curative agent in cases of chronic rheumatism. Picric acid has been tried as a febrifuge by Braconnot, but it dyed the unfortunate patients a brilliant yellow, and was consequently objected to by them. There is but little doubt that pharmacy generally would receive important benefits from the systematic study of some of these compounds, but our physicians are but seldom chemists, and our chemists are too fond of either purely scientific investigations, or of making researches into artificial dyeing materials. Surely we have dyes enough to go on with for a few years; and as to new compounds, we have not yet discovered the properties of a thousandth part of those whose names we glibly use every day. Far be it from us to take a utilitarian view of chemical research. But we must never forget, while admiring as we do the magnificent discoveries in pure chemistry that are taking place daily, that one of the noblest aims of our glorious science is the discovery of substances that may administer to the wants and enjoyments of our fellows, that may bring health and ease to the writhing sufferer, or that may restore the dying patient to life and strength.

The experience of the last ten years has shown that therapeutists are fully alive to the importance of practising their art on scientific principles; let them now, therefore, show their proper appreciation of the truths of chemical science by aiding their chemical brethren in testing to the utmost some few, at least, of the organic products formed in the laboratory. At first, no doubt, the work would be purely experimental, but gradually the experience gained would soon show from what class of products successful results might be expected. While botanists are ransacking the fields and forests of the world in search of new remedies, it will be surely a disgrace to the scientific chemist if no results are obtained from the use of substances of more definite composition which may easily replace the thousand and one medicaments with pseudo-scientific names, so puffed and advertised in every direction.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On Bathvillite, a New Inflammable Mineral.

In a recent number of our Journal Mr. Greville Williams gave an interesting account of a soft, pale brownish yellow substance occasionally found at Bathville in the strata of torbanite. We use this term because it does not involve a decision of the question whether the mineral of Torbanehill be a shale or a coal, and because it is now generally employed by mineralogists to designate the substance in question. A correspondent, signing himself "Ein Untersucher," forwarded some strictures on Mr. Greville Williams's paper. These remarks appeared in No. 176, page 191, and we feel it due to Mr. Greville Williams and our readers to say that we do not approve of their tone or their substance. We think that even "Ein Untersucher" would be unable to pronounce the pale brownish yellow Bathvillite analysed so carefully by Mr. Williams cannel coal; neither would he be able to call it a coprolite had he studied its characters—physical and chemical—more closely. We say more closely, but, for all we can learn from his remarks, "Ein Untersucher" appears to be totally unacquainted with the mineral under review, which we have carefully examined, and which appears to deserve a distinctive appellation as much as dysodile, retinite, or copaline. He writes as a partisan, not as a man of science; and the objectionable introduction of a fragment of verse increases the distance between the style of his remarks and that of the true dignity of discussion. Mr. Greville Williams is well known as an indefatigable and successful scientific chemist, and his results and opinions are entitled to respectful treatment. As the author of a standard work on "Chemical Manipulation," his exact modes of conducting an experiment always possess an interest for the working chemist.

Magnetic Ferric Oxide.

At a meeting of the Académie des Sciences, on the 16th March, M. Malaguti disclaimed having appropriated the honour of the discovery of magnetic ferric oxide obtained by peroxidising ferroso-ferric oxide.* He admitted that Mr. Robbins had a right to accuse him of having ignored his labours, but not of having appropriated the discovery which he claims. He explained that the reference in his former communications, to the magnetic ferric oxide resulting from the peroxidation of ferroso-ferric oxide, was only incidental, since he was already aware M. Pelouze had observed that iron peroxidised under the influence of air had the property of being attracted by the magnet. Consequently, he considered it extremely probable that, under these circumstances, iron did not become peroxidised without passing through the intermediate condition of ferroso-ferric oxide, in part at least, and he then mentioned that the magnetic-ferric oxide was obtainable in three ways, viz.:—

1. Slight but long-continued calcination of ferrous salts of organic acids.
2. Calcination of rust, previously purified from any magnetic particles.
3. Calcination of the ferric oxide resulting from the spontaneous oxidation of hydrated ferrous oxide by exposure to the air.

In the first paper by M. Malaguti on this subject,† he referred at the commencement to the observation made by M. Delesse, in the course of his investigation of the magnetic power of minerals, that even the pure ferric oxide is magnetic, this character being more marked in proportion to the crystalline texture of the oxide. Taking the magnetic power of steel as 100, that of specular iron of Vesuvius was 25, while that of fibrous hematite was only 6·4.

M. Malaguti then pointed out the connection between these facts and the magnetic character of the ferric oxide produced by calcining pure iron in powder, which had been observed by M. Pelouze, and communicated by him to M. Becquerel.

He then showed that amorphous hydrated ferric oxide and salts of iron yield, by slight calcination, ferric oxide that is highly magnetic, and described the conditions requisite for obtaining it in this condition.

The ferric oxide obtained by these means was in all cases found to be free from ferrous oxide, and they were found to be magnetic even after deflagration with chlorate of potash. The magnetic character was, however, destroyed by the long-continued application of a high heat.

The hydrated ferric oxide obtained by precipitation from ferric salts is not magnetic, nor does it become so by calcination. This is also the case with ferric oxide resulting from the peroxidation of ferrous oxide combined with a mineral acid. Thus the ochrey deposit from a solution of ferrous sulphate or chloride, consisting almost entirely of hydrated ferric oxide, cannot be rendered magnetic.

These facts are considered by M. Malaguti to be in some way connected with the production of steel, inasmuch as the amorphous ferric oxide is magnetic only when it is produced from ferrous oxide that has been combined with carbon or with nitrogen.

From the fact that ferroso-ferric oxide does not lose its magnetic character when it is converted into ferric oxide by calcination, it may be conjectured that, in the calcination of an organic salt of ferrous oxide, ferroso-ferric oxide is first produced, which, in passing to the state of ferric oxide, retains its magnetic character. On the other hand, in those cases where the hydrated ferric oxide, containing only traces of ammonia, is converted, as in the case of rust, into magnetic ferric oxide by a slight torrefication, it is difficult to imagine how sufficient ferroso-ferric oxide could be produced by the reducing action of the trace of ammonia to determine in that way the highly magnetic character of the whole mass.

A committee having been appointed to report on this subject, M. Malaguti sent a series of specimens to the Academy,‡ among which was a specimen of ferroso ferric oxide artificially prepared, which he mentioned as being the most certain and least questionable source of magnetic ferric oxide, the method of preparing this being to deflagrate the ferroso-ferric oxide with chlorate of potash. He also adds that in preparing the magnetic ferric oxide from organic ferrous salts by calcination, a moderate heat must be employed, otherwise the magnetic character of the oxide is destroyed. The deflagration with chlorate of potash is not necessary in this case unless the oxide is found to contain ferrous oxide.

By repeating the deflagration with chlorate of potash a second time, a diminution of the magnetic character

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* See Chemical News, vol. vii., p. 100.

† *Comptes-Rendus*, lv., 350.

‡ *Comptes-Rendus*, lv., 714.

of the oxide was observed, probably in consequence of the influence of heat.

The ferroso-ferric oxide may also be converted into magnetic ferric oxide by simple torrefication, but the quickest way of effecting the change is deflagration with chlorate of potash.

In every case the magnetic ferric oxide loses its magnetic character when dissolved by an acid and reprecipitated.

TECHNICAL CHEMISTRY.

On the Production of Aniline Red.

At a meeting of the Académie des Sciences on September 20, 1858, a paper by Dr. Hofmann, "On Organic Bases,"* was read, in which he describes as follows the reaction of bichloride of carbon and aniline :—

When a mixture of anhydrous bichloride of carbon, with three times its weight of anhydrous aniline, is exposed to a temperature of 170° or 180° C. for thirty hours the liquid is converted into a dark coloured mass, which is either soft and tenacious, or hard and brittle, according to the length of time and the temperature the action has been maintained.

The dark mass is a mixture of several substances; it partially dissolves in water, leaving a more or less solid resin. The water solution gives with potash an oily precipitate, containing a considerable portion of the aniline unaltered. When this precipitate is boiled with weak solution of potash, the aniline distils over, while a viscid oil remains, which gradually solidifies with a crystalline structure. By washing this residue with alcohol, and crystallising it several times from boiling alcohol, it is rendered perfectly white, and there remains in solution a very soluble substance, of a magnificent crimson colour.

That portion of the dark mass which is insoluble in water dissolves very readily in hydrochloric acid, and is reprecipitated by alkalies in the state of an amorphous powder of a red colour, soluble in alcohol, which it colours a rich crimson. The greater part of this colouring substance is the same as that accompanying the crystalline substance.

In the early part of the following year, Messrs. Renard, Frères, of Lyons, applied for and obtained a patent for improvements in the "Preparation of Red Dyes,"† consisting in the production of a new dye (which they call "fuschiacine," from the resemblance of the colour to that of the fuschia), by heating together aniline and anhydrous bichloride of tin to the boiling point, and boiling the mixture for fifteen or twenty minutes.

In the place of bichloride of tin may be substituted bichloride of mercury, perchloride of iron, or protochloride of copper, but no proportions are given as proper to be used in any case.

The remainder of the specification relates to the separation and purification of the colouring substance.

Messrs. Renard have lately taken proceedings against other manufacturers of this dye, and have claimed the sole right of producing it. They also obtained a judgment in their favour last August, the effect of which was to give them a monopoly of this manufacture.

In the trial of this case it was argued that the prior publication by Dr. Hofmann of the fact that a red colour was obtainable from aniline did not affect the claim of Messrs. Renard, inasmuch as that fact was merely an accidental result of laboratory experiment, and also because the mode of production adopted by him could not be employed in practice in consequence of its being extremely dangerous to the operator.

The "Société Industrielle" of Mulhausen have taken up this subject, with the view of inquiring into the reality of these objections to the production of aniline red by means of bichloride of carbon, and, as the result of experiments, they have come to the conclusion that this mode of producing aniline red is neither dangerous nor impracticable.

They found that pressure is not a necessary condition of the reaction, but is requisite only for the purpose of preventing the escape of the bichloride of carbon or aniline, and, that in place of applying pressure to the mixture while it is being heated, an equally good result may be obtained by using a condenser, which will admit of the condensed vapour flowing back again into the vessel in which the mixture is heated. In conducting this experiment the flask containing the mixture of bichloride of carbon with three times its weight of aniline was kept at a temperature between 130° and 135° C. for one and a-half to two hours, during which time there was a slight reaction perceptible, and the volatilisation of the bichloride ceased. The flask was then heated to 170 or 180° C. for one and a-half to two hours, and during this time the liquid became thicker. After cooling, a solid blackish brown mass remained. This was treated with boiling water, and yielded a very acid solution, containing much hydrochloric acid and hydrochlorate of aniline, and some of the colourless crystalline base described by Dr. Hofmann.

Further treatment of the mass with boiling water containing alkali gave a nearly colourless solution, which, after being filtered, evaporated, and neutralised with an acid, was converted into a deep red coloured liquid that dyed floss silk deep crimson.

The resinous residue remaining after this treatment with alkaline solution contained but very little colouring substance.

In consequence of these results, the decision in favour of Messrs. Renard has been appealed against, and the case will be tried again. The question raised is one of especial interest to patentees. Messrs. Renard appear to claim, not merely a method or methods of producing a red dye from aniline, but also the conversion of aniline into a red dye, by bringing about a certain change by any means whatever. Such a claim as this naturally shuts the door to any possibility of obtaining patents for improvements in the mode of manufacturing the red dye, which could be worked independently of Messrs. Renard's patent. Any such improvements could only be practised under licences for the production of the red dye from aniline. In very many cases it would only be justice to an original inventor that his patent should have such an application, nor would it be any injustice to the inventors of improvements in the details or methods of manufacture; for to take the case of the production of red dyes from aniline, any improvements in the mode of effecting this derives its value in great measure from the original discovery that such a production of red dye from aniline is possible, and the practice of those improvements should be subject to a royalty to the original discoverer of that fact.

But before such a title can be justly established, it must be satisfactorily shown that the original discovery was protected by a patent, and was made by the patentee, who claims to exercise a prior right over the inventor of

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* *Comptes-Rendus*, xlvii.

† Specification, No. 921, April 12, 1859.

improvements, and to participate in the profits arising from those improvements. In the case of the production of red dye from aniline, it does not appear that Messrs. Renard are in a position to establish this title. The discovery of the fact that a red substance was obtainable from aniline was not made by Messrs. Renard, but was made prior to the date of their patent by Dr. Hofmann. Moreover, it was not made the subject of a patent, but was publicly announced. It is true the announcement of this fact by Dr. Hofmann was not accompanied by any reference to the application of the colour-substance to dyeing; but even in this case the fact of such a substance being obtainable was so far made public property, that it was placed beyond the power of any one who afterwards applied the substance to dyeing to claim a monopoly in that application other than that which might belong to the mode of producing the colour.

Messrs. Renard appear, indeed, to have admitted this view of the matter to be correct by their endeavour to prove that the production of the red substance from aniline by Dr. Hofmann was not capable of being carried out practically according to the method he adopted. There can be no doubt, however, that if Dr. Hofmann had taken out a patent for the production of a red colour from aniline by means of bichloride of carbon, that he would have been fairly entitled to a monopoly in the production of this colour, and to have claimed a royalty from other inventors of improved methods of producing this colour by means of reagents analogous in their nature and mode of action to bichloride of carbon.

In the presence of such a patent, too, the use of bichloride of tin as a substitute for bichloride of carbon might even have been regarded, not as an actual improvement, but as a colourable imitation. The objection that bichloride of carbon is not an article of commerce, or sufficiently easy to obtain for the purpose of manufacturing dyes, would not be of much weight, for the value of the results obtainable by its use must be taken into account before this objection could be maintained. The same objection might have been made to the use of aniline for the production of dyes, on account of its being a substance known only in the chemist's laboratory as an object of scientific research. The fact of any substance being only so known is, however, no argument against its being serviceable in manufacturing industry. It is the application, or the use of which it is susceptible, and the demand created by its use, that determines whether or not such a substance is to be regarded as belonging to science or to industry.

PHYSICAL SCIENCE.

*On the Long Spectrum of Electric Light,** *by* G. G. Stokes, *M.A., D.C.L., Sec. R.S., Lucasian Professor of Mathematics in the University of Cambridge.*

(Continued from page 200.)

Arc Discharge, and Lines of Blue Negative Light.—On diminishing the distance between the electrodes, formed suppose of copper wires, the brightness of the metallic lines at first improves, and afterwards changes but little, or, if anything, rather falls off. On still further diminishing the distance, so that the electrodes almost touch, and the discharge passes with little noise, a new set of strong lines make their appearance in the invisible region of moderate refrangibility. In this mode of discharge, in which the negative electrode, if at all thin, quickly becomes red-hot and fuses, the jar has not much influence, and the lines in question are still better seen when it is suppressed altogether. To show them to perfection, it is best to take a flat negative electrode, so as to carry off the heat, and not to hide from the prism any part of the blue negative light, and a sharp positive electrode almost touching the former. In this way the visible discharge is reduced almost wholly to an insignificant-looking star of blue light; but it is wonderful how strong an effect it is capable of producing in the invisible region. The most striking part of the invisible spectrum consists of four bright lines situated not far from the visible spectrum. These are followed, after a nearly dark interval, by light arranged in masses resembling in its general aspect the groups of copper lines (from which, however, it differs), but not strong enough to be resolved or accurately measured. There are also a couple of blue bands seen by projection. These are not seen on looking at the blue light directly with a flint-glass prism of 60°, because everything is seen in too great detail. Most of the air-lines in the invisible spectrum, especially the bands beyond line 4, have an ill-defined look, and would probably be resolved did the intensity of the light permit.

The appearance just described is independent of the nature of the electrodes, and therefore is to be referred to the air, and not to the metal. On viewing in a moving mirror the star of light producing this effect, it is found to have a considerable duration.

On slightly separating the electrodes, forming an image of the discharge with the 2·5-inch lens, and receiving it on a cake of the uranium salt, a very strong fluorescence was seen over the image of the blue disc when the lens was focussed for a point a little beyond the visible spectrum. On moving the lens onwards, the fluorescence produced by the rays belonging to this image spread out into a ring; and on moving still further, a tolerably well-defined image of the whole discharge was perceived. Of this the part belonging to the blue disc was the brightest, and was surrounded concentrically by the ring before mentioned, now still further widened. The image of the remainder of the discharge was brightest where it was most contracted at the positive electrode. The discharge generally was perhaps of slightly higher refrangibility than the blue disc, even excluding from the latter the rays belonging to the ring. It thus appears that the four bright lines figured were produced mainly by the blue negative light.

The mode of transition of the discharge may be studied by placing the electrodes at the greatest striking-distance and making them gradually approach. At first there passes a clean bright spark making a sharp report, and not resolved by a revolving mirror. The invisible spectrum which this shows is too faint for precise observation; the visible spectrum shows chiefly air-lines. As the electrodes approach, the spark becomes clothed by the well-known yellowish envelope capable of being blown aside, and the blue negative light begins to appear. A moving mirror, as M. Lissajous has already observed,† shows an instantaneous spark at the commencement, in point of time, of the envelope and blue negative light, both which are drawn out, indicating a very appreciable duration. On making the electrodes approach somewhat nearer, the spark diminishes, and the envelope is formed in perfection, especially with

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* Abstract from the *Proceedings of the Royal Society.*

† See Du Moncel, "Récherches sur la non-homogénéité de l'étincelle d'induction," p. 107.

broad electrodes. The air-lines now begin to show themselves well, but are brightest on the side of the spectrum answering to the blue negative light.

It might be supposed at first sight that the permanence of the yellowish and of the blue light only indicated a glow of appreciable duration left by a sensibly instantaneous discharge; but several circumstances indicate that the discharge itself lasts, and that it is under its action that the glow takes place.‡ The action, I am persuaded, is this: a spark first passes; and this enables a continuous discharge to pass, which due, in part at least, to the inductive action of the still falling magnetism, just as a voltaic arc may be started in a powerful battery by passing an electric spark between the slightly separated electrodes; and the glowing of the air under the action of this discharge produces the yellowish envelope and blue negative light. Thus, when the electrodes are nearly at the greatest distance at which this sort of discharge takes place, the blue negative light is seen pretty sharply terminated in a moving mirror. Were it a dying glow, it ought to fade away; but if produced under a discharge, it ought to cease almost abruptly, inasmuch as at this distance of the electrodes a continuous discharge is unable to pass when the tension has sunk much below that under which it was first produced.

The same conclusion may be drawn from an effect which I once obtained, the exact conditions for the production of which it is not easy to hit off. With a jar in connection, each discharge due to a single breach of contact appeared in a moving mirror as a bright spark joined to a spark less bright by the blue negative light, and also by the yellowish or reddish light, brightest close to the positive electrode. Were the blue light due to a glow, it ought to be reinforced instead of being put out by the second spark, whereas the explanation of the result is easy on the supposition of a continuous discharge. The first spark started a continuous discharge, which emptied the jar less fast than it was filled by the secondary coil; so that presently another discharge took place which emptied the jar, so that a continuous discharge could no longer pass.

On viewing the broad discharge formed without a jar when the electrodes are at a moderate distance, through a revolving disc of black paper with a single hole near the circumference, while the envelope was being blown aside, so as to get a succession of momentary views of the discharge, the envelope was seen extravagantly bent, as a flexible conductor might have been—not torn across, as a column might have been which was heated by a previous spark. The central spark, of course, was usually missing, as it is sensibly instantaneous.

I have spoken of the arc, and especially the blue negative light, as exhibiting air-lines. The arc, however, is liable to be coloured not only by casual dust (as when it passes partly through the flame of a spirit-lamp with a salted wick, when it is coloured yellow by sodium), but also by matter torn from the positive electrode. This is well seen with electrodes of aluminium, when the arc or a portion of it is frequently coloured green. This green light has a very sensible duration, and a distinctive prismatic composition, and is brighter towards the positive than towards the negative electrode, but is not confined to the immediate neighbourhood of that electrode (extending, indeed, sometimes over almost the whole length of the arc), in which respect, and in its duration, it differs from the light of the spark proper.§ With aluminium opposed to another metal, as copper or iron, the green light is seen only when the aluminium is positive. Even with aluminium this light may generally be got rid of by making the electrodes approach.

On the Cause of the Advantage of Broad Electrodes; and on the Heating of the Negative Electrode.—Although the spark appears instantaneous when viewed in a moving mirror, it must yet occupy a certain time; so that we have, in fact, a brief electric current, to which we may apply Ohm's laws. The electromotive force is here the difference of tensions of the coatings of the jar. As to the resistance, the short metallic part of the circuit may be neglected, and we need only attend to the place of the discharge. The resistance here may be divided into that due to the air, and that due to the parts of the electrodes close to the points of discharge. That the latter is by no means insignificant, may be inferred from the enormous temperature to which minute portions of the electrodes are raised, as indicated by the excessively high refrangibility of the rays emitted by the metals, in the state doubtless of vapour. By the use of flat electrodes the striking-distance is materially diminished, without any change in the difference of tension of the coatings of the jar. Hence the electricity which it contains passes at a higher velocity, and therefore produces a more powerful effect on the metals.

The injurious effect of the introduction of a small resistance was very strikingly shown with broad, slightly curved copper electrodes, three inches in diameter, by leading wires from a coating of the jar into a tumbler of water, and from thence to the corresponding electrode, when the spark became quite insignificant in comparison to what it had been.

With one sharp and one flat electrode placed near together, bright sparks passed when the connection was metallic, and the invisible spectrum then showed the copper lines with one or two air-lines not conspicuous; but when water was interposed the spark was greatly reduced, and the invisible spectrum showed the air-lines. In both cases the spark was followed by an arc discharge, as might be seen in a moving mirror; and in the latter case the arc discharge was increased in consequence of the diminution of the spark, which, though necessary to start it, was formed at its expense; and as in the arc discharge the jar was idle, the increase of resistance in a circuit already comprising the secondary coil was unimportant.

The fact that the blue negative light which appears when the arc discharge is formed shows air-lines, points to the air as the seat of the intense action which there takes place; and the very high refrangibility of some of the rays emitted, and the copiousness of those rays, indicate how intense that action is. The heating of the negative electrode seems to be a secondary effect, not due to the direct passage of the electricity through the metal (for the section through which it passes is not by any means small), but to the heat communicated from the film of air investing it. Small as is the mass of the film compared with that of the portion of the electrode adjacent to it, the rate at which heat is communicated is enormous. Thus with a positive point nearly touching underneath a negative electrode of platinum foil containing water, the foil is kept red-hot under the water, though the mere passage of electricity through

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‡ Although this view may be considered already established (see the work by the Vicomte Du Moncel just quoted), the observations here mentioned will not, I hope, be altogether useless.

§ The outer part of the jar-spark between aluminium electrodes has the same green colour and prismatic composition, though in this case the green light is sensibly instantaneous.—July, 1862.

the metal would be quite inadequate to produce that effect. Corresponding to the heating of the electrode by the air is the cooling of the air by the electrode; and such a powerful abstraction of heat can hardly take place without altering the state of the film of air in relation to its power of conducting electricity. This would seem to be the reason why the film of air in contact with the negative electrode behaves so differently from any arbitrary section of the column along which the discharge takes place, and from offering greater resistance becomes the seat of a more intense emission of highly refrangible rays. At the positive electrode, at which, for whatever reason, the issue of electricity is confined almost to a point, nothing of this kind takes place; but, from the contraction of the section through which the electricity has to pass in the electrode, a minute portion of the metal of which it is composed is so highly acted on that matter belonging to the electrode is liable to appear in the arc.

These views lead to curious speculations respecting the negative light in highly exhausted tubes, and respecting the remarkable reversion of heating-effect which Mr. Gassiot has obtained according as the discharge is intermittent or continuous,|| but I forbear to speculate further.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, April 16, 1863.

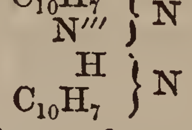
Professor A. W. Williamson, *Ph.D., F.R.S., President, in the Chair.*

The minutes of the preceding meeting were read and confirmed, after which the following gentlemen signed the statute-book, and were formally admitted Fellows of the Society:—Robert G. West; A. Kemshead; and George Warington. Henry Salt, 29, Gordon-square, London; and Levisson F. Vernon Harcourt, of Oxford, were balloted for, and duly elected Fellows of the Society.

The first paper read was by Messrs. W. H. Perkin and A. H. Church, "*On some Derivatives of Naphthylamine.*" The authors have submitted to a more searching inquiry a colouring matter discovered and described by them in 1856. It is made by the action of one equivalent each of hydrate and nitrite of potassium upon two equivalents of the hydrochlorate of naphthylamine, thus:—

$$2\,(C_{10}H_9N,\ HCl) + KHO + KNO_2 = C_{20}H_{15}N_3 + 2KCl + 3H_2O.$$

A bright red substance is precipitated, which crystallises from boiling alcohol in long crimson needles, having a green metallic reflection. These crystals melt at 136°; they dissolve in alcohol with a rich orange red colour, which acids change to violet. The formula of this substance, deduced from very numerous analyses, is, as already stated, $C_{20}H_{15}N_3$, arranged as follows:—



$$\left.\begin{matrix}C_{10}H_7\\N'''\end{matrix}\right\}N \qquad \left.\begin{matrix}H\\C_{10}H_7\end{matrix}\right\}N$$

It is made up, therefore, of two equivalents of naphthylamine, conjoined by the substitution of one equivalent of nitrogen for three of hydrogen. Messrs. Perkin and Church have succeeded in replacing the remaining equivalent of hydrogen with benzoyl, and the original base being termed azodinaphthyldiamine, this new body may be named benzoyl-azodinaphthyldiamine. The authors have likewise obtained an amidic acid—"azodinaphthylcitraconamic acid"—by heating the base with anhydrous citraconic acid. The salts of azodinaphthyldiamine are in some respects rather anomalous; the authors describe three hydrochlorates:—

$$(C_{20}H_{15}N_3)_4,\ HCl;$$
$$C_{20}H_{15}N_3,\ HCl;$$
$$C_{20}H_{15}N_3,\ 2\,HCl:$$

and a sulphate—

$$(C_{20}H_{15}N_3)_2,\ H_2SO_4.$$

When reduced by nascent hydrogen, azodinaphthyldiamine yields a new base, remarkable especially for the brilliant green colour which it shows when oxidised by ferricyanide of potassium and similar agents. The authors are at present engaged in the investigation of this substance.

Dr. A. W. Hofmann, in allusion to the formula of the new base described by Messrs. Perkin and Church, remarked that it was very closely related to the several colours obtained from aniline—the new naphthaline product stood, indeed, at the head of the series; thus,—

$C_{20}H_{15}N_3$—Azodinaphthyldiamine.
$C_{20}H_{17}N_3$—Yellow chrysaniline.
$C_{20}H_{19}N_3$—Rosaniline.
$C_{20}H_{21}N_3$—Leucaniline (colourless).

This analogy, true of the bases, held good also in regard to several of their combinations with acids, and to their colorific properties particularly.

A paper, by Dr. T. L. Phipson, "*On Vanadium Ochre,*" was read by the Secretary. The author referred to statements regarding the occurrence of vanadium in certain clays and minerals, and to the modes of detection and separation of this element from the general constituents introduced by M. Beauvallais and others. Vanadium was said to be present in small quantity in the clay of Gentilly, near Paris, but the author had not succeeded in detecting more than the minutest trace of vanadic acid, although ·32 per cent. of rutile was contained in the flower-pots made of this clay for the Jardin des Plantes. A larger proportion was, however, found in the clays of Belgium. Vanadic acid was usually found in nature associated with titanic and tantalic acids. Mr. Edward Riley had recently pointed out new sources of titanium, and it was probable that vanadium had often escaped detection. Dr. Phipson had discovered the existence of nearly 2 per cent. of vanadic acid in a description of bog iron ore from Saxony, which he had obtained from Mr. Gregory, mineralogist, of London. This ore consisted of small rounded grains or aggregations of hydrated peroxide of iron, of a pisolitic character, and about the size of millet seeds; the colour varied from reddish-brown to purple; these granules were cemented together by an argillaceous material, which probably contained the bulk of the vanadic and titanic acids. The ore gave off small quantities of chlorine and carbonic acid on digesting with hydrochloric acid, and left insoluble a residue of silica. The author did not consider that the vanadic acid in this ore existed, like the rutile, in a free state, but was probably combined chemically with alumina, oxide of iron, and other bases. Analysis indicated 1·9 and 1·6 per cent. respectively, as the amounts of vanadic acid in two different samples of the mineral. Peroxide of iron amounted to 59 per cent.; alumina, 5; and titanic acid 2·3; besides these, the proportions of water and phosphoric acid were considerable. The latter constituent appeared to stand in relation to the vanadic acid as 3 : 2, and the author ventured to believe that these acids would generally be found in that ratio, and probably, as is the case with molybdenum, would unite in the formation of a definite compound. In support of this opinion, he had already detected vanadium in samples of British ironstone wherein much phosphoric acid was present. With regard to the mode of examination adopted, which was equally applicable to clays, ironstone, pitchblende, and a variety of other minerals, fifteen grammes of the finely-powdered ore were fused with half

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|| *Proceedings of the Royal Society*, vol. xi., p. 329.

that weight of nitre in a platinum crucible, at a dull red heat for about fifteen minutes. The mass dissolved out in water, a small quantity of caustic soda added, and the solution boiled, sulphuretted hydrogen gas then passed through the liquid, and nitric acid and chloride of barium added to precipitate the sulphovanadiate of baryta. This compound collected, and subsequently decomposed by dilute sulphuric acid, and to the filtered solution ammonia and solid chloride of ammonium to saturation are added, when, after standing for a day or two, the whole of the metal became precipitated as vanadiate of ammonia. The colour of this product is subject to modification by exposure to air and light; it varies consequently from white to yellow or brown, but in any case by merely drying and heating this substance loses ammonia, passing immediately to the state of anhydrous vanadic acid. This process may in some cases be shortened by adding carbonate of soda to the materials employed in the first fusion, and precipitating the ammonia salt directly by the addition of solid chloride of ammonium to the aqueous solution. Other methods were described as having been employed by the French chemists. Beauvallais merely boils the baked clay with 3 per cent. of carbonate of soda dissolved in water, adds to the solution successively sulphuric acid, ammonia, and sulphide of ammonium, when the sulphovanadiate of ammonia is precipitated, this salt being afterwards decomposed by boiling with acetic acid. Deville recommends the use of caustic soda in preference to the carbonate, hydrosulphuric acid being then passed, in order to effect the conversion of the metal into sulphovanadiate of soda, which is decomposed by acetic acid, and the precipitate calcined to yield pure vanadic acid. The employment of tannin as a means of aiding the precipitation of vanadic acid was enumerated as one of the processes tried, but did not appear to offer any decided advantage. The interference exerted by titanic acid in the course of testing before the blowpipe for vanadium was referred to, and regarding which the author stated that whenever these metallic oxides were associated it was impossible to perceive the green colour which the last-named substance alone imparted to a bead of microcosmic salt in the inner flame.

The amounts of vanadic acid occurring in the several samples of clay and other minerals examined were as follows:—

London clay	·023	per cent.
Do (another specimen) .	·056	,,
Gault clay, Sussex . . .	.06	,,
Do. (another specimen) .	·07	,,
White clay, Belgium . .	·03	,,
Vanadium ochre, Saxony . .	1·60	,,
Do. do. . .	1·90	,,
Brown Hæmatite . . .	·03	,,
Iron slags (average amount) .	·37	,,

The **President** considered the facts announced in Dr. Phipson's paper very encouraging. Vanadium must still be classed among the rare elements, and the discovery of new sources of this metal was highly important as a preliminary to its fuller examination, which would very likely lead to its more frequent identification.

The next communication was read by Mr. W. H. **Perkin**, and entitled, "*On Chloro-maleic Acid obtained from Tartaric Acid.*" In former researches undertaken by the author in conjunction with Mr. Duppa,* it had been shown that there existed a close relationship between succinic, malic, and tartaric acids; and that from the bibromo-succinic acid it was possible to prepare common tartaric acid.† The action of pentachloride of phosphorus upon tartaric acid gave rise to the formation of chloro-maleic acid, the identity of which was proved by its being converted into maleic acid, and ultimately into succinic acid, by treatment with sodium amalgam; the product last-named exhibited all the properties of ordinary succinic acid; it gave off on heating the irritating vapours so characteristic of that substance, and furnished the usual precipitate with solution of perchloride of iron. The formulæ expressing these changes were written on the board, thus—

$$\underbrace{C_4H_3ClO_4}_{\text{Chloro-maleic acid.}} + HH = \underbrace{C_4H_4O_4}_{\text{Maleic acid.}} + HCl.$$

$$\underbrace{C_4H_4O_4}_{\text{Maleic acid.}} + HH = \underbrace{C_4H_6O_4}_{\text{Succinic acid.}}.$$

The **President** remarked that our knowledge in regard to the conversion of the several organic acids was fast progressing, and every new instance was important in a theoretical point of view.

Dr. A. W. Hofmann begged leave to submit to the notice of the Society a reaction which formed part of a series of researches upon which he was at present engaged. Starting from the hydro-carbon benzol, all were acquainted with the body named azo-benzol, which was one of the products obtained by the action of alcoholic potash upon nitro-benzol. The constitution of this substance was still rather doubtful, and although the simplest formula for azo-benzol would be C_6H_5N, there were reasons, founded chiefly upon the vapour-density, for doubling this expression, and writing it $C_{12}H_{10}N_2$. One of the most interesting derivatives of this body was the base benzidine $C_{12}H_{12}N_2$, and it was the action of heat upon this last-named substance which he was prepared to demonstrate by an experiment. (The solid benzidine being then introduced into a small glass retort, was heated over a spirit lamp, when the substance fused, boiled, and quickly furnished a dark-red liquid which distilled over. The product in the receiver consisted of aniline mixed with azo-benzol, and on adding an acid the latter was left insoluble, whilst the base was of course dissolved.) The primary object in making this experiment would become intelligible by an examination of the formulæ—if from two equivalents of benzidine one atom of azo-benzol were subtracted, there remained the elements of paraniline, $C_{12}H_{14}N_2$, and the author was anxious to know whether this method would actually serve for the production of a substance which had presented much difficulty in its preparation by other means. The result proved, however, in this respect unsuccessful, for the secondary product split up at the moment of liberation into two equivalents of the ordinary aniline, so that the following equation represented the change:—$2(C_{12}H_{12}N_2) = C_{12}H_{10}N_2 + 2(C_6H_7N)$.

In answer to an inquiry on the part of the President, Dr. Hofmann stated that paraniline was usually obtained as a bye-product in the manufacture of aniline, and that this constituted in fact the most eligible source.

After passing a vote of thanks to the authors of the several communications, the President announced that Dr. Lyon Playfair would deliver a discourse "*On the Constitution of Salts*" on the 7th May next, until which evening the meeting stood adjourned.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, April 7, 1863.

E. W. **Binney**, *F.R.S.*, *F.G.S.*, *President*, *in the Chair*.

Mr. Edmund Salis Schwabe, B.A., was elected an ordinary Member of the Society.

Messrs. Robert Worthington and G. V. Vernon were appointed Auditors of the Society's accounts for the present session.

Mr. **Brothers** exhibited and described an apparatus for determining the magnitudes of stars.

Professor **Roscoe** communicated the following extract from a letter he had just received from Professor Bunsen respecting the atomic weight of cæsium. Professor Bunsen formerly found the atomic weight of this metal to

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* *Philosophical Magazine*, April, 1859.

† *Journal of the Chemical Society*, vol. xiii., p. 102.

be 123; since then, Messrs. Johnson and Allen, Yale College, United States, found, by operating on large quantities, the number to be 133. He writes: "The atomic weight Cs.=133 is quite right. The difference is caused by an error of about one per cent. on the chlorine contained in the chloride of cæsium, owing to an amount of impurity which cannot be separated according to the method I first used for purification. I have employed all the three grammes of Cl Cs, with which I was obliged to make the whole of the investigation of the cæsium compounds for another determination of the atomic weight of the metal, and I find the same number (133) as the Americans found, but by quite a different method. The alteration from 123 to 133 makes a difference in the analysis of most of the salts of from a quarter to one per cent. in the percentage composition."

Mr. G. C. Lowe exhibited part of a mercurial seconds pendulum, which, from its mode of construction, bears a strong resemblance to a horse-shoe magnet, and possesses considerable magnetic power. It is what is called the stirrup, and has attached to its lower extremities a circular brass disc, upon which the jar of mercury is placed. The stirrup is attached to the lower end of the pendulum rod, and has two steel rods, about ten inches in length, connected at their upper ends by a short steel bar, so that when magnetised the lower ends form one a north and the other a south pole. It had been suggested by Mr. Baxendell that some irregularities which are observed in the rates of clocks may be explained by referring them to variations in the intensity of the earth's magnetism influencing a pendulum which has thus become magnetised. He had noticed in two clocks which had mercurial pendulums sudden changes of rate, amounting to nearly, or in some instances quite, one second per day. There were no considerable changes of temperature or of the atmospheric pressure at the time, and the compensation is so nearly perfect that so great a deviation from the rate could not be due to change of temperature. A very brilliant display of aurora borealis was observed by Mr. Baxendell on the evening when the last remarkable change of rate took place, which favours the view now suggested, viz., that the lower part of the pendulum having become magnetised, an increase or diminution of the intensity of the earth's magnetism may have the effect of accelerating or retarding the motion of the pendulum. Mr. Lowe suggested that if the rods forming the stirrup were connected with brass at the top instead of steel, they would have much less force as magnets. In a pendulum also under Mr. Baxendell's observation, the disturbance of rate was much smaller in amount, scarcely exceeding one-third of the change that was found in the two clocks already mentioned. This pendulum consists of a long steel rod and two shorter ones, and has a very correct compensation effected by a zinc tube. This pendulum has another advantage, viz., that the steel rods and the zinc tube are so nearly the same in size that sudden changes of temperature affect all the parts nearly simultaneously, whereas in the mercurial pendulum the steel rod is much more quickly affected than the large mass of mercury; the compensation therefore does not take place soon enough to prevent a small error in the clock's rate.

A letter by Mr. Dyer was read, relative to Dr. Rankine's reply, inserted in the *Proceedings* of March 24. Mr. Dyer disavows all intention of unfairness towards Dr. Rankine, and states that although the brevity which it was necessary to observe in drawing up his abstract compelled him to give only detached passages, he conceived that the sense could not be considered to be perverted by reason of the omissions. He had always felt and expressed the highest regard for Dr. Rankine's genius and learned researches.

A paper was read entitled "*Note as to Two Events in the History of Steam Navigation*," by W. J. Macquorn Rankine, C.E., LL.D., F.R.S., hon. member of the Society. (1.) An interesting paper was lately read to this Society by Mr. Dyer, containing a history of a series of important events in the progress of steam navigation. (2.) It is to be regretted, however, that the author has noted either very slightly, or not at all, what appears to have been an event of paramount importance in the first adaptation of the double-acting cranked steam-engine to drive a paddle-wheel. Before that adaptation was made, the success of all attempts at steam navigation, such as those of Jouffray, Rumsey, Titch, Miller, and Taylor, &c, had been only temporary, because of the rudeness of the machinery for communicating motion from the piston to the shaft. (3.) That first adaptation was unquestionably accomplished by William Symington in 1801, as is proved by authentic documents which have been published by Mr. Woodcroft in his "Origin and Progress of Steam Navigation." Symington, instructed by the failure of the ratchet-work engine which he had made for Miller's boat, fitted up the *Charlotte Dundas* in 1801, with a double acting horizontal cranked engine, and this made her what Mr. Woodcroft has justly called "the first practical steamboat." Her speed, when running alone, and not towing other boats was six miles an hour. (4.) The use of this vessel was abandoned, not from any fault in her construction or working, but because the Directors of the Forth and Clyde Canal feared that she would damage its banks. Yet the man in all Britain who possessed, at that time, the greatest practical experience of the working of canals —the Duke of Bridgewater—was not deterred by any such apprehension from ordering, in 1802, eight similar vessels from Symington, to be used on his canal. (5.) The death of the Duke of Bridgewater, early in the following year, prevented the execution of that order. But Symington had evidently done all that lay in his power, and all that was necessary, to convert the steamboat from an awkward piece of experimental apparatus to a practically useful machine; and the honour paid to his memory ought not to be lessened because the career of his invention was cut short by a misfortune. (6.) There is nothing in this to detract from the honour which is justly paid to Foulton, as having been the first to practise steam navigation on a great scale, as a commercially profitable art. (7.) Another event passed over in the paper to which I have referred, is the first introduction of commercial steam navigation into Europe, which was effected on the river Clyde, in 1812, by Henry Bell, as is proved by documents cited in Mr. Woodcroft's work already referred to.

Dr. R. Angus Smith said that he had been using an amalgam of sodium for the production of hydrogen. When doing this, it was found that the amalgam had become one mass of crystals. These crystals are fine needles or prisms of considerable length; some may be traced in the tangled mass for about half an inch; they present hollow ends, although it was not possible to trace this hollowness through the whole length of the crystals. They are hexagons, and contain 9·47 per cent. of sodium; an excess of mercury remains; 10·3 per cent. would be equal to 2 at. of mercury and 1 of sodium. He had not seen any notice of an amalgam of similar composition. It can be examined only through glass, as it rapidly absorbs oxygen from the moisture of the air, and cannot be retained in the air many seconds without change being perceived. If this change be examined under a microscope, bubbles of hydrogen are seen to rise from every corner, extremely minute certainly, so that many millions would be required to cover a square inch, but affording one of the most convincing and direct proofs of the existence of moisture. This property may in many cases be found to be of value. The inclination to combine with oxygen when dry seems comparatively slight.

Dr. Smith mentioned also a method of separating lime from phosphoric acid, which, as far as he was aware, was new. Many very elaborate methods had been given. After adding tartaric acid to a solution of phosphate of lime in muriatic acid, the lime may be precipitated by oxalate of ammonia and ammonia as in other solutions. Carbonate of lime has been obtained in this way free from every trace of phosphate, and the solution has been freed entirely from lime. Tartaric acid prevents the precipitation of phosphate of lime in weak solutions by ammonia, but has no influence over the oxalate of lime. The true action of tartaric and similar acids in preventing precipitation deserves a more careful inquiry than has yet been made.

PHYSICAL AND MATHEMATICAL SECTION.

April 2, 1863.

Joseph Baxendell, *F.R.A.S.*, *President of the Section, in the Chair.*

A paper was read, entitled, "*Notes of Observations of the Speed and Order of Succession in Magnitude of Waves observed in Gales of Wind off the Cape of Good Hope*," by Mr. Thomas Heelis, F.R.A.S. The highest waves measured were about 35 feet in height from the trough, no broken crests having been measured. Their speed varied (the force of the wind being 8 according to the Board of Trade scale, and equal to weather in which a ship on a wind can just carry treble-reefed topsails) from twenty to twenty-three miles per hour, the breadth of trough being 300 to 350 feet. The observations show that usually the succession of magnitudes (or heights) returns in series of twelve waves, the first and second of each series being very large, the sixth or seventh being also large, but inferior in magnitude to the first and second, and the intermediate ones being small. The observations show that waves are limited in length, measured along their bases, the crest being apparently at the middle point of the base, and the length varying with the altitude of the crest, and that the order of succession of magnitudes depends upon their being arranged so that the crest of one wave follows on the same line as the lower flanks of a preceding wave. The speeds also of waves appear to vary, so that a following wave often coalesces with, and is increased in size by, absorbing one immediately preceding. When a wave is first formed, it is small, and increases in size in its progress, until the crest topples over in foam, after which the height decreases rapidly; and there seems reason for thinking that if ordinates were drawn so as to represent the height of any wave at different periods of its existence, its height would be found to coincide with Mr. Scott Russell's wave-line curve. The length of a wave in open water, measured along its base, seems also to depend upon and bear a definite relation to the width of the trough between two successive waves. The speed of the waves is not so much affected as would naturally be imagined by the force of the wind. In a moderate gale they run as fast as in a heavy one. It is otherwise with their height.

Mr. Alfred Brothers read the following paper, "*On an Improved Diaphragm for Determining the Magnitudes of Stars.*" At a recent meeting of this Society, when Mr. Baxendell made a communication respecting one of the variable stars of small magnitude, it occurred to me that the system used by him for determining the variability of the smaller stars, must be not only tedious but often unsatisfactory, owing to the necessity for leaving the position of observation for the purpose of changing the diaphragms. When apertures of various diameters are used as diaphragms, placed at the end of the dew-cap of large telescopes, and particularly refractors, it must at all times be difficult to make these changes, even when the advantage of clock-work motion is available; but, with telescopes whose motions must be controlled by hand, the possibility of determining with accuracy such results as the one-tenth of a magnitude must be very uncertain; as, unless an assistant be at hand to change the diaphragms during the observation, the observer must be constantly liable to lose the star at the moment it becomes necessary that it should be in the centre of the field of view, and the eye in position to note the effect produced by the decrease of light on the change of diaphragm being effected. Several contrivances have been adopted to contract the aperture of the telescope, such as square and hexagonal forms, opened and closed mechanically; but hitherto, it is believed, no attempt has been made to retain the circular form of aperture. In order to facilitate observations of this character, and to enable the observer to dispense with an assistant, I have recently adapted to my telescope (of 5 in. aperture) an apparatus which may be termed the "Iris Diaphragm," as its action resembles to some extent the expansion and contraction of the pupil of the eye. The invention was, I believe, patented by Mr. Jennings, of London, as a capsule for closing jars, bottles, &c., but has been superseded by a more simple method for the same object. If found to be practically successful for the purpose I have now applied it to, it will, I think, be extremely useful in the Observatory, and form a desideratum long required by astronomers. A great saving of time will be effected, and the results will be regarded with greater confidence. The following is a description of the invention, and the method I propose for adapting it for astronomical purposes. The invention consists of two rings of metal, one working within the other, of nearly the same diameter, but differing in width, to the outer edges of which is attached a piece of sheet india-rubber joined at the ends, and so placed that when the rings are moved in opposite directions the india-rubber is stretched, and closes the aperture of the rings when they have made about one-half of a revolution, the aperture remaining circular or nearly so, its perfect circularity depending on the proper attachment of the india-rubber. The adaptation I have effected in the following manner:—A flat ring of tin with a flange is made to fit accurately the end of the dew-cap of the telescope. To the flange the diaphragm is soldered; to the broader ring of the diaphragm is fixed a circle of brass, having teeth cut rather more than one-half round its circumference. Fixed to the ring, which is attached to the dew-cap, is a bearing for a small pinion, which gives motion to the half wheel by means of a slender rod (of brass tubing) the length of the telescope, having at the eye-piece end a handle of convenient form, and worked by the left hand. The rod is supported at the eye piece end of the telescope, on a bearing attached to a metal collar made to clip the telescope by a screw, and attached to this bearing is a ratchet-wheel, which prevents the too rapid reverse motion of the india-rubber diaphragm. The ratchet is not absolutely necessary with the present apparatus, owing to the friction of the pinion against the wheel preventing the india-rubber contracting too rapidly. It is not considered necessary to give the dimensions of the parts of the apparatus, as in every case these must vary with the diameter of the object-glass of the telescope. The principal point to be attended to is the width of the broader ring, which is covered inside by the india-rubber, and this must be sufficiently wide to close the aperture when in use. In order to prevent the over-winding of the diaphragm, stops are placed at each end of the teeth on the half wheel. An index of some kind will be necessary to make the apparatus complete. A pointer might be fixed on the end of the dew-cap, and a scale might project from the wheel. The diameter of the various openings could also be determined by the ear, the number of teeth in the wheel and pinion being known, the ratchet-wheel could be so adapted, that a certain number of *clicks* would indicate that a certain aperture is then in use. This method, however, might be tedious to the observer, owing to the necessity for counting. The index and scale will perhaps be more convenient, and if the light from the sky should not be sufficient to show the reading of the scale, the micrometer lamp could be placed so as to illuminate the scale and index only. The additional weight at the end of the telescope renders a counterpoise necessary, and this is effected on my telescope by a slight addition to the counterpoise already attached. It may perhaps be necessary to say, that I have as yet tried the apparatus merely for the mechanical working, but am very sanguine that it will answer the purpose intended more effectively than any other method I am acquainted with.

Mr. G. V. Vernon, F.R.A.S., communicated returns of the rainfall for 1862.

NOTICES OF BOOKS.

The Retrospect of Medicine: being a Half-Yearly Journal Containing a Retrospective View of Every Discovery and Practical Improvement in the Medical Sciences. Edited by W. Braithwaite, M.D., and James Braithwaite, M.D. July to December, 1862. London: Simpkin, Marshall, and Co.

We have already on several occasions remarked the punctuality with which this half-yearly compilation from the medical journals makes its appearance, and the general care shown in the selection of the matter. The same observations may be made with respect to this volume; and another, which also might have been made on the past volumes—namely, that the Editors would do well sometimes to attach to their quotations some critical notices. The Editor of a weekly journal can, of course, do nothing of this sort. He must allow his contributors and correspondents to speak for themselves, and, is often under the necessity of publishing a good deal of what he knows to be nonsense. No such necessity, however, exists in the case of the half-yearly journal compiled from published sources. The Editors of such a journal have plenty of time to think over their selections, and a little judicious criticism would be as useful to writers as readers.

We, of course, can only notice such articles as relate to or involve chemical and pharmaceutical matters, and in such in this volume we find something to astonish, and sometimes to amuse us. As every chemist can appreciate a good joke we shall extract a story, premising, however, that it is told seriously by the relater. There is a paper headed "The Chlorine and Milk Treatment of Scarlet Fever," &c., the author of which became convinced that chlorine was a valuable remedy for scarlet fever, by the rapid recovery of a child who had swallowed a little of a gargle composed of "a solution of chloride of lime, alum ustum, compound tincture of iodine, tincture of capsicum and water." He adopted a chlorine treatment commonly afterwards, and found that his patients recovered, and he subsequently read a paper on the subject at a meeting of the British Medical Association, which was afterwards published. We now quote:—"A week after its publication I was agreeably surprised at receiving a polite note from Mr. Martin Ricketts, of Droitwich, stating that he had perused the paper with great interest and pleasure, and could confirm all that I had written on the good effects of chlorine in scarlet fever; and that, strange to relate, a similar accident to a child using a chlorine gargle led to the adoption of chlorine in his practice; lately, however, he had used a preparation made by the Apothecaries' Company at Liverpool, and termed *chloriform*, which appeared to him to possess all the *antiseptic* powers of chlorine, with a sedative influence peculiarly its own. The dose was from five to thirty minims. He requested my acceptance of a small quantity! In this preparation lay hidden a grand secret—a glorious gem—*sparking*, and beckoning to any scientific mind to free it from its prison-house. Its advances were unheeded until Dr. Simpson *first recognised in its component parts something more than the composition of ether*—a something which possessed all the anæsthetic properties of that fluid without its nauseous ones. What is its position now? It is the first on the list of alleviators of human suffering; and the discovery of it should have won for its discoverer a world-wide testimonial. I soon recognised the preparation, made public towards the termination of 1844, to be the same as that sent me by Mr. Ricketts. On the re-breaking out of scarlet fever, and a typhoid fever of the usual form, I prescribed chloroform, but as it did not remove the effluvia so effectually as chlorine, I administered the latter in combination with it." And of course the patients got well. But stop, ye who are patiently endeavouring to collect a few firm supports for the great edifice of a true philosophy of therapeutics, and think that in this fact of the successful treatment of scarlet fever by chlorine, and what we suppose the author would call the particular preparation of it termed chloroform, there is something to build upon! Our author continues:—"It must not be imagined from what has been advanced that these fevers were treated solely with chlorine, chloroform, and milk. The antiphlogistic treatment was *invariably* practised. The commencement of the attack involving sometimes *abstraction of blood, counter-irritation, chloride of mercury, potassio-tartrate of antimony, and purgatives, with gargles of capsicum, alum, iodine, and chloride of lime solution*;" and lastly, "if the stomach could bear it, gruel made thick as good cream, with two-thirds milk!!"

The publication of the paper from which we have quoted above brought forth another, in which we find valuable information on pathological chemistry. The author states that from his observation "the chlorine acids are to scarlatina what the nitric and nitrous acids are to typhoid fever," *i.e.*, chemical antidotes—facts, as our readers will see, of the utmost importance. We cannot quote much more of this kind; but as we have many medical readers who might perhaps prescribe nitric acid in typhoid fever, we may extract a caution:—

"When nitric acid has been given to excess, the tongue becomes very red, and this is an indication for suspending its use, reducing the dose, or administering an alkali, such as carbonate of soda, to which should be added a little pure ammonia or sal volatile, *to absorb the carbonic acid which may be disengaged.*"

Nitric acid, our author states, decomposes the morbid matter of the blood in typhoid fever, as is clearly proved by a dusky countenance becoming bright under its use; while, on the contrary, ammonia is shown to have an affinity for the morbid matter, as the dusky hue of the face is increased by it.

There is another paper on sulphurous acid and sulphites as a means of neutralising morbid poisons in the blood, from which we learn that dogs injected with morbid poisons generally died when a sulphite was not administered, but usually recovered when it was given.

But we must hasten to matters which will be of more interest to the generality of our readers. Rennet wine is an article now in some demand; and Dr. G. Ellis recommends the following as the best mode of preparing it:—

"Take the stomach of a calf fresh from the butcher. Slit up the stomach longitudinally; wipe it gently with a dry napkin, taking care to remove as little of the clean mucus as possible. Then cut it into small pieces (the smaller the better) and put all into a common wine bottle. Fill up the bottle with good sherry, and let it remain corked for three weeks; at the end of this time it is fit for use."

Mr. W. White Cooper recommends cacao butter as a basis for ophthalmic ointments; if found too hard, two parts of the butter may be mixed with three of olive oil. Pitting after small-pox can be prevented by the early application of plasma containing iodide of potassium. Lastly, a mixture of creosote and collodion is said to be a valuable application for the toothache.

With these extracts we must conclude our notice of this volume of a very valuable publication.

NOTICES OF PATENTS.

2. *Manufacture of Leather Cloth.* N. C. SZERELMEY, Brixton. Dated January 1, 1862.

FOR the purpose of rendering linen, woollen, and other fabrics waterproof, and for the manufacture of a kind of imitation leather, or leather cloth, the patentee claims the employment of a material known under the name of "zopissa."

The substance used by Mr. Szerelmey in his experiments upon the decayed stone of the Houses of Parliament is likewise called "zopissa," but its nature never having been disclosed by the inventor, it becomes questionable whether the declaration contained in the foregoing announcement is sufficiently explicit to permit of a valid claim being based thereon.

The imitation leather shown by Mr. Szerelmey at the International Exhibition was, however, a very beautiful article, and appeared to be generally admired.

25. *Artificial Fuel.* G. STRACEY, Rackheath Hall, near Norwich. Dated January 3, 1862. (Not proceeded with.)

THE inventor mixes together small coal, sawdust, and cut straw, and makes these into blocks of fuel by the help of clay and chalk under powerful pressure.

It seems hardly politic to increase the proportion of mineral matter in a fuel whilst abundant supplies of low quality coal can be obtained at a trifling cost from natural sources.

37. *Purifying Coal Gas.* A. WARNER, Threadneedle Street, London. Dated January 4, 1862. (Not proceeded with.)

THE inventor proposes to employ in the purification of gas a material prepared from the cinders, hammer-slag, and other waste oxides of iron produced in iron works. These substances are reduced to powder and levigated in water in order to fit them for successful employment in the dry gas purifiers.

The litigation respecting the earlier and rival claims of Hills *v.* Evans, for the application of oxides of iron in the removal of sulphuretted hydrogen from coal gas, disposes at once of this suggestion.

81. *Coke.* T. RAMSAY, Newcastle-upon-Tyne. Dated January 11, 1862.

FOR the production of a superior quality of coke, especially free from sulphur, the inventor reduces the coal to a very fine state of division by grinding under edge runners, or between horizontal stones like those in a flour mill, before submitting the same to the action of heat in the coking oven. A highly bituminous or caking coal is preferred for this purpose.

90. *Artificial Fuel.* F. C. WARLICH, New Cross, Kent. Dated January 13, 1862.

THE patentee economises the volatile products, both liquid and gaseous, which are given off during the distillation of coal, by employing them in the direct production of block fuel; the tar being itself mixed with the small coal, and the evolved gases by their combustion aiding the operation of heating the ovens.

113. *Treating and Utilising certain Materials Used, and Products Obtained, in the Manufacture of Gas.* W. CLELAND, Everton, Liverpool. Dated January 15, 1862.

THIS specification enumerates as many as eleven distinct claims for different modes of treatment to which the spent oxides of iron which have been employed in the purification of coal gas may be submitted. These bear reference to the collection of the sulphur by distillation in retorts, or by simple fusion and pressure; to the production of sulphurous acid and sulphites by combustion in a current of air, at the same time saving the ammonia; to the extraction of sulphate or sulphide of iron from the metallic residues; and by secondary reactions to make alum, sulphate of soda, ferruginous pigments, &c. In some of these processes the material becomes again available for use as a gas purifying agent.

123. *Composition for Preventing Rust on Bright Steel, Iron, Brass, or Metal Surfaces.* T. MYERS and E. MYERS, Millbank Street, Westminster. Dated January 17, 1862.

FOR this purpose the inventors take—

Gutta Percha . . .	10 lbs.
Mutton Suet . . .	20 ,,
Beef Suet	30 ,,
Neatsfoot Oil . . .	2 gallons.
Rape Oil	1 ,,

These materials are melted together until thoroughly dissolved, and then coloured with a small proportion of rose pink, oil of thyme or other perfuming matter being at the same time added. When cold the composition is ready to be applied by rubbing upon the metallic surfaces which require protection.

Notices to Proceed.

3147. James Webster, Birmingham, "Improvements in the construction of burners and blow-pipes.—Petitions recorded November 22, 1862.

3159. Albert Lewis Woolf, Birmingham, "A new or improved metallic alloy."—Petition recorded November 25, 1862.

3184. William Clark, Chancery Lane, London, "Improvements in the preservation of animal and vegetable substances."—A communication from Henri De Lapparent, Boulevart St. Martin, Paris.—Petitions recorded November 27, 1862.

MISCELLANEOUS.

THE ALKALI WORKS REGULATION BILL.

THE bill introduced by Lord Derby has been read a third time in the House of Lords; any opposition, if necessary, must therefore be made in the House of Commons. The title and provisions of the bill may be shortly stated as follows:—It is to be cited as the "Alkali Act, 1863." It is to come into operation on January 1, 1864. The term "alkali work" is to mean *every work in which muriatic acid is evolved.* Every alkali work is to be carried on so as to ensure, to the satisfaction of the inspector, the condensation of not less than ninety-five per cent. of the muriatic acid evolved. Any less condensation than this will make the owner liable to a penalty of fifty pounds for a first offence, and for every subsequent offence to a penalty not exceeding twenty pounds, nor less than two pounds, for every day the offence continues. All alkali works must be registered, and every change of ownership must be registered. The Board of Trade is to appoint inspectors to assigned districts. The next clause is important, as showing the wish of Lord Derby to ensure fairness in carrying out the Act. No land agent, nor any one engaged in any manufacture, or interested in any patent in or according to which the decomposition of salt or the condensation of muriatic acid may be effected, shall act as inspector under this Act. The inspectors may enter works at all reasonable times, day and night, without giving notice, but so as not to interrupt the process of the manufacture, to see that the provisions of the Act are carried out. The owners of the works, upon demand, are to supply the inspector with plans of those parts of the works (to be kept secret by the inspector) in which the salt is decomposed and the muriatic acid condensed. The inspector may make any experiments to ascertain the efficiency of the condensing apparatus, and the owner of the works is to give him all necessary facilities for the testing. All persons obstructing an inspector or refusing to give him the facilities incurs a penalty of 10*l.* for every offence. The inspectors are to report to Parliament every year. Owners of works may, with the sanction of the Board of Trade, make special rules for the workmen attending to the condensing apparatus, and may annex penalties to the violation of such rules. The remainder of the Act refers to the recovery of penalties.

The Oil Springs of America and Canada.—Mr. A. S. Macrae, petroleum oil broker, Liverpool, writes: "The demand for all descriptions of petroleum has been very large this week, and prices to the close are well maintained. Owing to the bracing nature of the weather, the temperature in the various stores is as it ought to be, and, so far as leakage and evaporation are concerned, there is nothing at all to intimidate; while it seems probable that the expenses of holding will be considerably less than has generally been anticipated. Last year it was stowed miscellaneously in sheds, and even amongst cotton; whereas now it is in vaults and level ground, where everything contiguous is cool and drafty, and where leakage would be immediately apparent. Crude.—1000 casks of American at 11*l.* 15s. to 12*l.* for 810° and under. Much that is coming over now is 830° and upwards, and the condition of it very bad. Water and sand are being repeatedly found in the casks, and a searching scrutiny has to be made on the quay here, all of which adds seriously to the expense. Such, of course, only fetches a proportionate value, while it creates great distrust and much disappointment amongst buyers. In Canadian no sales. Refined.—8000 casks have changed hands this week at 1s. 8d. up to 1s. 10d. for present delivery, 2s. for September—October, and 2s. 1d. for November—December delivery. Benzine is in more demand. 400 casks sold at 1s. 6d., and 250 cases at 1s. 9d."

The Effects of Sulphuret of Carbon on Health.—The symptoms caused by sulphuret of carbon are peculiarly severe, and the situation of the workers really most miserable. Few of the workers marry, as it is well known that those who habitually work among these fumes rarely or never have children, and the malady is one which will only succumb to good air and rest, and total exclusion from the work-sheds. The head is much affected, and partial insanity is far from uncommon. The sight is troubled frequently. The taste is vitiated, so that most things appear to taste of sulphur. The hearing is likewise affected, and the digestive functions are sometimes strangely excited, though this latter symptom is less frequent than the others. Appetite is increased to the verge of gluttony, and nausea is almost invariable. The breathing and the circulation are injured, and most, if not all, of the secretive functions are disordered in a very alarming manner. In fact, the wretched people—we can give them no other name—who work with this agent are miserable to themselves and the world; they live in pain and trouble of both body and mind, with no hope or expectation of cure so long as they continue at their particular occupation. As to the remedies to be taken for alleviation of these misfortunes, baths, good air, a few simple medicines, and a country life—of course necessitating total abstinence from the poisonous work—have been found most efficacious. The power and density of the fumes may be better appreciated, when we state that in many cases the sheds are merely roofs supported by poles, and open to every breeze or gale that blows. M. Delpech does not appear to propose any method for ameliorating the condition of the workpeople. He mentions that the injury is due entirely to sulphuret of carbon; not to the chloride of sulphur; and says that phosphorus has had a beneficial effect in many cases of "depression." In fact, it would, we suppose, be almost impossible to do anything except abolish the process, and as this cannot be effected, it only remains for scientific men to discover some agent which will produce the same results in the arts, without any of the great and certain dangers of sulphuret of carbon. There are many minds at work on this project, but as yet no practical result has been secured.—*Social Science Review.*

SCIENTIFIC SOCIETIES.

MEETINGS FOR THE ENSUING WEEK.

May 4. Monday.

Entomological—12, Bedford Row. 7 p.m.
British Architects—9, Conduit Street. *Anniversary.* 8 p.m.
Asiatic—5, New Burlington Street. 3 p.m.
Royal Institution—Albemarle Street. 2 p.m. *General Monthly Meeting.*

5. *Tuesday.*

Civil Engineers—25, Great George Street, Westminster. 8 p.m.
Pathological—53, Berners Street, Oxford Street. 8 p.m.
Photographic—King's College, Strand. 8 p.m.
Ethnological—4, St. Martin's Place. 8 p.m.
Royal Institution—Albemarle Street. 3 p.m. Professor Tyndall, "*On Sound.*"

6. *Wednesday.*

Society of Arts—John Street, Adelphi. 8 p.m. Dr. A Wynter, "*On Bread-making, particularly with Reference to the Condition of those Employed in its Manufacture.*"
Geological—Burlington House. 8 p.m.
Royal Society of Literature—4, St. Martin's Place. 8.30 p.m.

7. *Thursday.*

Royal—Burlington House. 8.30 p.m.
Chemical—Burlington House. 8 p.m. Dr. Lyon Playfair, C.B., F.R.S., "*On the Constitution of Salts.*"
Linnæan—Burlington House. 8 p.m.
Antiquaries—Somerset House. 8.30 p.m.
Royal Society Club—St. James's Hall. 6 p.m.
Artists and Amateurs—Willis's Rooms, St. James's. 8 p.m.
Royal Institution—Albemarle Street. 3 p.m. Prof. Ansted, "*On Geology.*"

8. *Friday.*

Astronomical—Somerset House. 8 p.m.
Royal Institution—Albemarle Street. 8 p.m. Prof. Voelcker, "*On the Soils of England.*"

9. *Saturday.*

Royal Botanic—Inner Circle, Regent's Park. 3.45 p.m.
Royal Institution—Albemarle Street. 3 p.m. Prof. Max Müller, "*On the Science of Language.*"

ANSWERS TO CORRESPONDENTS.

⁂ In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

H. G.—The idea is very good, and, as far as we know, original. Send a description for publication, and we shall have much pleasure in inserting it in the Chemical News.

J. T. T.—No curriculum is prescribed. The title F.C.S. is not a *degree*, it simply expresses that the person is a Fellow of the Chemical Society; and it is open to any qualified person to become so. Write to the secretary, Burlington House.

THE CHEMICAL NEWS.

Vol. VII. No. 179.—May 9, 1863.

PATENTS FOR INVENTIONS IN CHEMICAL ARTS.

The defects, anomalies, and vices of the present patent laws are now becoming very generally recognised, not only in this country, but also on the Continent, and proposals for their amendment, or even total abolition, are not unfrequent topics of discussion at scientific and technical societies.

Among the evils of the present system in this country, the indiscriminate granting of patents is perhaps the worst. The host of patents that are annually granted for alleged inventions—which are in reality nothing more than repetitions of prior patented methods, or applications of facts that are well known, and have in some cases been already used, or for sweeping and comprehensive claims devised for the purpose of including manufacturing operations that are likely to be carried out, and making them subject to claims for royalty, or for the most absurd projects—constitute in themselves a growing incubus on invention, and a source of impediment to enterprise, which is the very opposite of the object that the grant of a limited monopoly in the form of a patent is intended to conduce to.

Those who are in favour of the continuance of the patent system, either as it exists or in some amended form, have one argument which is entitled to consideration—viz., that the advantages conferred by a patent constitute a legitimate reward for the publication of valuable inventions, and that by this means the industrial progress of the nation is encouraged. But this argument raises the prior question, whether the grant of a patent is a reward for real invention? There is much reason for the opinion that it is not so. It is true that the reduction of the charges for a patent has made them more easily obtainable by the real inventor; but this is a facility that is equally afforded to the spurious inventor, who is thus enabled to enter the field with the true man, and either deprive him of his legitimate right, or oblige him to defend it by a costly legal process. The same necessity of costly defence against infringement is involved by the holding of a patent for any really valuable manufacturing operation.

The inventor pays for his patent, and gives his invention to the public. He receives nothing in return but a qualification to maintain a suppositious privilege, with the understanding that the State affords him no further protection than he can obtain by his own means, and it does not even guarantee the validity of the qualification.

The payment of fees for a patent is only to be defended under the admission that patents are granted indiscriminately to those who have a claim to them and to those who have no claim. The possible propriety of the system in the latter case is no excuse for the injustice in the former case. If an invention is wo thy the grant of a limited monopoly as a reward to the inventor, he renders his equivalent for that reward by giving to the public the advantages of his invention after the expiration of a certain time. This should be sufficient without the imposition of preliminary and periodical fees, however small. It is reasonable enough, in its way, that the man who takes out a patent with no idea of ever working it, but merely for the sake of turning a penny, honestly or otherwise, by means of it, should pay for the privilege he thus obtains. But it is unreasonable to treat these men in the same manner.

The only reason why these two classes of men are, as regards patents, placed on the same footing, is the difficulty, or, it may be said, the impossibility of discriminating as to the value, the novelty, or the utility of an alleged invention. The system of preliminary inquiry into these points has broken down everywhere. Its adoption is surrounded with all sorts of objectionable features, and one of the most serious is the fact that it is precisely with regard to great inventions that there is the greatest chance of its being impossible to appreciate their utility.

Would it not, therefore, be a lesser disadvantage to the real inventor to abolish the patent system, and to leave him to exercise his energies in carrying his invention into practice, or in submitting it to trial, instead of conferring upon him a semblance of protection which involves the exercise of those energies in supporting the title it seems to confer? This is now an almost invariable result of the establishment of any new industry under the ægis of the patent laws. Whether the invention be a steam-plough, an iron ship, a new dye, or a new manufacture of any kind, its value may be estimated most correctly by its more or less early appearance as a bone of contention in our courts of law. There the real advantages of patents are reaped, not, unfortunately, by the inventors, if by any of the contending parties.

But, it may be asked, do real inventors look to the patent laws as affording them a reward for the communication of their inventions, and protection in the enjoyment of such reward? A fact that was mentioned the other evening at the Society of Arts would seem to indicate that this is not the case. It was there stated that notwithstanding the existence of a vast number of British patents relating to sewing-machines, few, if any, of those patents are worked; and while the patents that are worked are chiefly American, the manufacture of those machines is almost altogether carried on in America.

The reason assigned for these conditions was the influence of the patent laws, which, instead of fostering invention and promoting manufacturing industry, encouraged piracy of ideas.

The same view of the case is indicated by the fact that manufacturers, who professedly work under patents, nevertheless maintain a profound secresy as to their operations, and trust to the honesty of their workmen rather than the protection of their patents.

If any feasible means should be devised for amending the patent law, and removing the serious evils it now presents, some provision should be made for limiting the barrier that old, and sometimes utterly forgotten, patents present to the patenting of what may be, to all intents and purposes, a new invention at the present time, but which cannot be made the subject of a patent by reason of a prior patent existing for an invention which has either never been brought into use, or which was at the time impracticable. It would seem that in such cases as this the absence of novelty, as regards patents, in a really valuable invention should not bar the validity of a second patent for that invention at a later period.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Analytical Notes on Thallium—Separation of Thallium from Copper, by William Crookes.

In the Chemical News for March 21 for this year (vol. vii., p. 133), a process was given for separating these two metals. Continuing my researches upon this point, I have found another method more delicate than the former one in its results, and requiring different reagents; it may therefore be found more serviceable in certain cases than the former process, and for this reason I consider that no apology is necessary for recording it. Moreover, in the present state of our knowledge respecting this new metal, any contributions to its history are of value, even though they be nothing more than different ways of effecting the same result. The plan formerly recommended consists in adding cyanide of potassium to the mixed ammoniacal solutions, and then sulphide of ammonium, the thallium being precipitated and the copper remaining in solution. This, as I have already said, is an exceedingly delicate process, but it is, I think, inferior to the following:—To the acid solution of thallium and copper add sulphurous acid in excess, and iodide of potassium. A dirty white precipitate will fall, consisting of subiodide of copper and iodide of thallium; if the precipitate is dark-coloured, and smells of free iodine, more sulphurous acid or sulphite of soda must be added, until the liquid, after agitation, smells of sulphurous acid. Now filter and wash well. Next, place the precipitate in a small flask with a wide neck, and pour pure ammonia over it; the iodide of copper will rapidly dissolve, with absorption of atmospheric oxygen, to a deep blue liquid, whilst the iodide of thallium will be left behind as an insoluble yellow powder, resembling iodide of lead, but not quite so dark; this may be filtered off and washed from copper.

Having constantly had occasion to decompose large quantities of iodide and chloride of thallium, it may be of some service to give the plan I found easiest to reduce these compounds to the metallic state. I formerly advised their conversion into sulphate, by heating with excess of oil of vitriol, and this is the plan now generally adopted. It is a troublesome process, as it requires the heat to be continued until the greater portion of the free sulphuric acid is volatilised, or a residue of chloride or iodide will be left behind on dissolving the mass in water. Another objection is, that the great heat required to effect complete decomposition volatilises some of the thallium. I now invariably adopt the following plan:—Boil the chloride or iodide of thallium in sulphide of ammonium for some minutes; decomposition takes place readily. Filter and wash with hot sulphuretted water (see note, page 133) until no chlorine or iodine is detected in the filtrate, and then dissolve in hot dilute sulphuric acid. If the operation has been conducted properly, complete solution will take place almost instantly. The metal can now be precipitated from this liquid by two or three Groves's batteries, or the sulphate can be obtained in the crystalline form by evaporation. It forms brilliant colourless prisms, permanent in the air, anhydrous, and fusible without decomposition below a red heat. The crystals are soluble in about five times their weight of boiling water, and twenty times their weight of cold water.

On the Reciprocal Action of Subsalts of Copper and Salts of Silver, by MM. E. Millon *and* A. Commaille.

On pouring a solution of ammoniacal subchloride of copper into a solution of nitrate of silver, also containing a little ammonia, perfectly pure metallic silver is immediately precipitated. At the same time, the following particulars may be observed:—

The silver precipitate is amorphous, and in so minute a state of division, that the diameter of each grain does not exceed 0·0025 of a millimetre. We know that silver obtained by an electric current, or by the action of metals is often brilliant, and always crystalline. The amorphous silver obtained by us is of a dull grey colour, but sometimes nearly white; but in each case it assumes when burnished the highest metallic lustre, and, by reason of its divided condition, it can be easily applied to a variety of substances, such as wood, stone, leather, and different tissues. Thus, by one process, silver, both pure and divided, is at once obtained. These conditions are doubtless favourable to its application to many purposes.

In order to arrive at a due appreciation of the usefulness of this reaction, whether to extract, purify, or estimate silver, or to analyse exactly the compounds of copper, it ought to be understood that the reaction takes place among the principal reagents in the proportion of their chemical equivalents.

Thus, by the weight of silver precipitated, the quantity of suboxide of copper engaged in the reaction is exactly determined, it matters little whether the subsalt is pure or mixed with protosalt. This is a new and exact method of analysing a mixture of subsalt and protosalt of copper, and suitable for the study of cupreous compounds, and is free from all the uncertainty which formerly attended this study.

If the cupreous compound is used in sufficient quantity in relation to the salt of silver, all the metal of the silver salt will be precipitated. This, in fact, is what takes place, which we have verified by operations with a known quantity of silver dissolved in nitric acid, which we recovered with no appreciable difference of weight by the action of ammoniacal subchloride of copper.

Thus, 1·115 grm. of fine silver dissolved in nitric acid, and the liquid rendered strongly ammoniacal, we added to it some equally ammoniacal subchloride of copper. The precipitated silver, well washed and dried, weighed 1·114 grm. or 99·99 per cent.

0·588 grm. of silver treated in the same way was reduced to 0·5855 grm., or 99·57 per cent.

0·9827 grm. of same metal dissolved in the same way, then precipitated by ammoniacal cupreous chloride, weighed 0·983; 100·03, instead of 100 of recovered silver.

This process, which is exact, gives the silver in a condition so easy to be collected and estimated, that the analysis of silver compounds by this method is very simple, and, above all, is speedily effected by it.

Passing over the preceding facts relating to the purification and extraction of silver, we were desirous of determining the solubility of chloride of silver in different liquids. For this purpose we used as solvent for the precipitated or melted chloride of silver, sometimes pure ammonia at different degrees of concentration, occasionally ammonia containing a solution of potassic or ammonic chloride, &c.; we have besides investigated the solubility of chloride of silver in chlorides, without free ammonia.

We used very ammoniacal subchloride of copper for the precipitation of metallic silver with the result shown in the following table, which indicates the proportion of metallic silver to one litre of each liquid:—

Solvent of Chloride of Silver.	Amount of Silver Dissolved.
Ammonia at 18° Cartier	51·6
Ammonia at 18° Cartier, with its own volume of water	23·8
Ammonia at 22° Cartier	58·0
Ammonia at 26° Cartier	49·6
Ammonia at 18° diluted with its volume of a saturated solution of sea salt	20·8
Ammonia at 18° diluted with its volume of a saturated solution of chloride of potassium .	20·4
Ammonia at 18° diluted with its volume of chloride of ammonium	22·4

Chloride of silver is insoluble in chlorides of calcium and zinc.

These numbers were obtained with precipitated chloride of silver, but the solubility of melted chloride seems much the same; thus, the solubility of precipitated chloride being represented by 49·6 of metal, 48·4 would represent the melted chloride. It is, however, necessary to prolong the contact by occasional shaking the melted chloride reduced to small fragments.

The above table proves that it is easy to dissolve in a litre of commercial ammonia (the strength most easily obtainable) as much as 58 grammes of metallic silver in the state of chloride. This degree of solubility seems to us sufficient to render it possible that silver ores converted into chloride may have an industrial application, and it may supplant for this purpose mercury, so dangerous and expensive in the operation of extracting, which may be effected with peculiar simplicity.*

A litre of ammonia, saturated with chloride of silver, is precipitated by 230 cubic centimetres of an ammoniacal solution of subchloride of copper at its maximum of concentration; the precipitant must always be in excess, and the same quantity of copper serves any number of times; the protochloride of copper when formed being reduced by zinc, which is most energetically affected in the ammoniacal liquid. By this means the metallic copper required for the formation of subchloride is constantly reproduced; and, on the other hand, the ammonia disengaged by lime, and brought to the desired degree of concentration may be constantly re-employed. There is no need to show how much by this means the process of purifying silver is simplified.—*Comptes-Rendus*, February, 1863.

Analytical Notices on Arsenic, by M. H. Rose.

Arsenic may be estimated by difference in many of its combinations with metallic oxides, by calcination of the substance with sulphur; the arsenic volatilises as sulphide, the base remaining under the form of fixed sulphide. The operation is easily effected in a hydrogen current, but is especially successful with arseniates of manganese, iron, zinc, lead, and copper.

Calcination with sulphur in a porcelain crucible completely eliminates the arsenic of arseniates of nickel and cobalt; but the weight of nickel and cobalt cannot be calculated from the residual sulphide.

Arseniate of silver, heated in a hydrogen current, or with sulphur, leaves silver which obstinately retains a certain quantity of arsenic.

The alumina resulting from the calcination of arseniate of alumina in a hydrogen current, whether singly or with sulphur, also invariably retains arsenic. It is the same with the magnesia resulting from ammoniaco-magnesian arseniate.

The above process applies, of course, to many metallic arsenides, particularly to arsenical iron, to mispickel (arsenio-sulphide of iron), to arsenical nickel, and to grey cobalt (arsenio-sulphide of cobalt), though with greater difficulty. Grey cobalt must be previously oxidised by nitric acid, and the mixture of oxides then treated by sulphur; and this must be repeated several times.

In many instances the best way to expel arsenic acid from arseniates is to calcine them with hydrochlorate of ammonia. Alkaline arseniates are perfectly transformed into chlorides by a single calcination; arseniates of alkaline earths offer more resistance, and the magnesia of the arseniate always retains a certain proportion of arsenic.

Many oxides of true metals are reduced to a metallic state by calcination with sal ammoniac; but the metal will contain arsenic.

Bisulphate of ammonia, as shown by M. H. Finkener, can often advantageously replace the hydrochlorate. However, the fused bisulphate attacks the porcelain crucible, which must be used on account of the arsenic, and the sulphates obtained are mingled with those produced by this attack. The arsenic is generally entirely expelled. The author has ascertained this to be the case with ammonio-magnesian arseniate, with arseniates of soda, lime, and lead. Arsenides of nickel and cobalt are more easily freed from arsenic by this method than by fusion with sulphur.

Some few metallic arseniates may be decomposed by boiling with a solution of a hydrated or carbonated alkali. The author in this way has been successful with arseniates of peroxide of iron and copper; arseniates of zinc and protoxide of manganese yield oxides retaining notable quantities of arsenic.—*Poggendorff's Annalen der Physik und Chemie*, vol. cxvi., p. 453.

On the Electro-Chemical Decomposition of Insoluble Substances, by M. Becquerel.

Whilst seeking to oxidise silicium at the positive pole, in distilled water, with a pile of eighty elements of sulphate of copper, I found that this metalloid is not, as has hitherto been supposed, a non-conductor, but that it possesses, when traversed by an electric current, sufficient conductility to produce remarkable caloric effects, by reason of its great resisting powers. By putting small cylindroid crystals of silicium, prepared by M. Deville's process, into a porcelain, or better still a platinum capsule, in communication with one of the poles of the pile, and closing the circuit with a platinum-wire, at least one millimetre in diameter, then by simply touching with this wire one only of the crystals, the adjacent crystals become incandescent. All the crystals follow

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* The silver residuums in laboratories are so quickly recovered by this process, that probably it will soon be exclusively used.

the wire when it is raised, forming a small chain at reddish white heat; at the same time a white smoke arises more or less visible, according to the force of the pile, and possessing an odour rather like that produced by breaking a piece of flint.

The intense heat is really produced by the resistance offered to the electricity when traversing the silicium; for this result is obtained by using a pile of such force, that by touching the platinum capsule with a wire of the same metal only a feeble spark is emitted.

In experimenting with a nitric acid pile of twenty elements the heat is so intense that the platinum vessel is perforated, being melted where it comes in contact with the substance, as well as the end of the platinum-wire, and, at the same time, a white smoke is disengaged, with formation of silica, deposited as powder on the melted platinum, and also some silicide of this metal.

With charcoal electrodes complex effects are obtained, resulting from their combustion and the effects above described. The light is then too brilliant to be borne by the naked eye. In this and the preceding instance it is necessary to operate on a plate of rock crystal, the surface covered with silica; under the microscope this silica appears to be in a vitreous condition.—*Comptes-Rendus*, February, 1863.

Experiment on the Separation of Sal-Ammoniac into Hydrochloric Acid and Ammonia at the moment of Vaporisation, by M. L. Pébal.

It is known that MM. Cannizzaro and H. Kopp explain the abnormal condensation of the vapour of certain bodies, such as sal-ammoniac, hydriodate of phosphoretted hydrogen, perchloride of phosphorus, monohydrated sulphuric acid, &c., by supposing that bodies during vaporisation are in a state of dissociation, as M. H. Deville happily expresses it.

If sal-ammoniac gives 8 volumes of vapour ($H_2O_2=4$ vols.), it is owing, according to these authors, to the fact that the vapour of sal-ammoniac is a mixture of hydrochloric acid and ammoniacal gas. M. Kekulé entertains the same idea.

But there has hitherto been no direct experimental proof of this theory, though with respect to sal-ammoniac the author has furnished this proof.

The method he employed is new only in its application to this special research. M. Bunsen has shown that to decide whether a gas consists of a single or compound substance there are only two experimental solutions,—namely, by absorption and diffusion. In the case in point, it would be difficult to apply the method of absorption; by experimenting by the method of diffusion the solution of the question might be determined.

In fact, if sal-ammoniac vapour is really formed of a mixture of ammonia and hydrochloric acid, it is evident the least dense gas, ammonia, ought to pass by diffusion more rapidly than hydrochloric gas into a hydrogen atmosphere. Supposing such a diffusion effected, the hydrogen atmosphere, after a certain time, should contain free ammoniacal gas, while the free hydrochloric gas should appear in the atmosphere of the sal-ammoniac vapour.

This theoretical assumption has been proved to be correct by the following experiment:—

Place some fragments of sal-ammoniac on a plug of amianthus in a tube drawn out at one end, and fixed inside a larger one. The tubes are traversed by a current of hydrogen, while, by means of a furnace of incandescent charcoal, their temperature is raised sufficiently to volatilise the sal-ammoniac.

The sal-ammoniac vapour being formed above the amianthus plug, ammoniacal gas penetrates through the plug in quantity sufficient to blue litmus-paper placed in the course of the hydrogen which sweeps through the inner tube, while the hydrogen traversing the annular space between the two tubes, the atmosphere of which is impregnated with excess of hydrochloric acid, reddens blue litmus-paper placed across its passage. It is thus made evident that the sal-ammoniac contains free hydrochloric acid and free ammonia, for these two gases passed through the diaphragm in unequal proportions.

The use of such metals as mercury and platinum has been purposely avoided, to escape the objection that might be urged by intervention of decomposing or catalytic forces.—*Annalen der Chemie und Pharmacie*, vol. cxxiv. p. 199.

TECHNICAL CHEMISTRY.

Metallurgy.

On Aluminum Bronze.—Lieut.-Colonel Strange has communicated to the Royal Astronomical Society some interesting observations on the use of aluminum bronze as a material for the construction of astronomical and other philosophical instruments. Colonel Strange remarks that, "the qualities of most importance in instrument making are, (1) tensile trength; (2) resistance to compression; (3) malleability; (4) transverse strength or rigidity; (5) expansive ratio; (6) founding qualities; (7) behaviour under files, cutting tools, &c.; (8) resistance to atmospheric influences; (9) fitness to receive graduation; (10) elasticity; (11) fitness for being made into tubes; (12) specific gravity."

Tensile Strength.—The mean of experiments made by Mr. Anderson at the Royal Gun Factory, Woolwich, shows that the average breaking tensile strength of aluminum bronze is 73,185 lbs. per square inch, while that of gun metal is 35,040 lbs., the ratio being rather more than two to one in favour of the aluminum bronze.

Resistance to Compression.—Experiments made by Mr. Anderson show that no effect was perceptible until 9 tons 2 cwt. per square inch was applied, when the specimon gave ·006 of an inch; on removing the weight an elasticity of ·001 was observed, giving the first permanent compression as ·005 of an inch. The ultimate amount of compression applied was 59 tons 2 cwt. 1 qr. 4 lbs. (132,416 lbs.), under which the specimen became too much distorted to permit of more weight being applied with any true result.

Malleability.—Mr. Anderson states that, "the qualities of this metal for forging purposes would appear to be excellent; with the exception of the part heated to a red heat in the shade, all show that it is a good workable material under the hammer almost up to the melting point." Colonel Strange adds, that there were specimens exhibited in the Industrial Exhibition at London, which showed that the alloy could be drawn out under the hammer almost to a needle point.

Transverse Strength, &c.—Messrs. Simms found by experiment that aluminum bronze was three times more rigid than gun-metal, and upwards of forty-four times more rigid than brass; and, in regard to its *expansive ratio*, they found this alloy less affected by change of temperature than either gun-metal or brass—a little less than gun-metal, and much less than brass.

Its *founding qualities* are such that it produces admirable castings of any size. It does not clog the file, and in the lathe and planing-machine the tool removes long elastic shavings, leaving a bright, smooth surface. It can be worked with much less difficulty than steel, and, notwithstanding its greater cost, the Messrs. Simms think that screws made of it would in the end prove less expensive than steel. It tarnishes less readily than any metal usually employed for astronomical instruments. It is remarkably well fitted to receive graduation, as it takes a fine division, which is pure and equable, surpassing any other *cast* metal in this respect. Colonel Strange remarks that in its elasticity it is said to surpass even steel, and it would therefore appear to be the most proper material for the suspension springs of clock pendulums. Regarding *its fitness for being made into tubes*, it can be soldered with either brass or silver solder; it can be rolled into sheet metal, and it can be hammered and drawn. Gun-metal does not admit of being rolled, so that hitherto the tubular portions of telescopes and other instruments have been made almost exclusively of yellow brass, an alloy very deficient in rigidity. The *specific gravity* of the alloy containing 90 copper and 10 aluminum is, according to Messrs. Bell, 7·689, very nearly that of wrought iron.

Colonel Strange adds, "it appears, from these experiments, and from the concurrent testimony of those who have given it a fair trial, that the 10 per cent. aluminum bronze is far superior, not in one or some, but in every respect, to any metal hitherto used for the construction of philosophical apparatus, and that for such purposes it may be employed in the dimensions that would be proper in the case of cast steel. All parts which would otherwise be made of steel may with perfect safety, and even with advantage, be made of the new alloy, particularly such parts as bolts, and fixing, tangent, and micrometer screws. Its hardness and comparative inoxydisability point it out as peculiarly adapted for pivots, axes, and bearings. If employed for receiving the graduation of circles, the necessity for inlaying another metal will be obviated, by which two advantages will be gained; the hammering which forms part of the operation of inlaying, and which, more or less, must cause unequal density and tension in the circle subjected to such treatment will be dispensed with; and the effect of inequality of expansion, in the circle and the inlaid strip, will no longer be a cause of apprehension. With respect to the due visibility of divisions cut on this metal, opinions will perhaps differ. I can only say that I should be well content to observe with them."

This alloy has been selected by Colonel Strange as the most appropriate metal for the construction of the large theodolite for the use of the Trigonometrical Survey of India. The horizontal circle of this theodolite is three feet in diameter, and the effect of using this alloy will be to keep the weight of the instrument within reasonable limits, notwithstanding its possession of means and appliances not hitherto bestowed on such instruments. In the manufacture of the alloy, Colonel Strange says that extremely pure copper must be used; electrotype copper is best, and Lake Superior copper stands next, giving an alloy of excellent quality. The ordinary coppers of commerce generally fail, owing, it is said, to the presence of iron, which appears to be specially prejudicial. Further, the alloy must be melted two or three times, as that obtained from the first melting is excessively brittle. "Each successive melting, up to a certain point determined by the working, and particularly the forging properties of the metal, improves its tenacity and strength. It is probable that after several meltings there will remain in combination with the copper a somewhat smaller proportion of aluminum than 10 per cent. The present price of English-made 10 per cent. aluminum bronze is 6s. 6d. per lb. This is four or five times that of gun-metal, but a much smaller quantity of the new alloy than of gun-metal will give the same strength; and when it is considered how small a ratio the cost of material bears to the cost of workmanship in refined apparatus, it will be found that even at the present price of the new alloy, its cost is not prohibitory, whilst the advantages attending its use promise to outweigh the increased expenditure."—*L. E. and D. Phil. Mag.*, [2], xxiv., p. 508.

C. Tissier, Director of the Aluminum Works at Rouen, shows that 1 per cent. of aluminum in copper makes the latter more fusible, giving it the property of filling the mould in casting, at the same time preventing it from rising in the mould. The action of chemical agents upon it is also weakened, and the copper gains in hardness and tenacity without losing its malleability, thus producing an alloy which has the malleability of brass, with the hardness of bronze.

In transverse strength, this alloy was found to be more than twice as rigid as either brass or copper. Tissier also finds that one part of aluminum, added to bronze consisting of 96 copper and 4 tin, gives an alloy of a fine colour, of remarkable homogeneity, of great hardness and malleability. During casting, this alloy does not oxidise at all, and it is therefore free from the oxide coating with which ordinary bronze castings are covered. The transverse strength of the castings of this alloy Tissier finds to be two and a-half times that of the original bronze, and that of the hammered alloy is four times as great as that of bronze. Ordinary cannon bronze, 89 parts copper and 11 tin, has the same transverse strength as castings of the new alloy. In reference to the hardness, tenacity, and malleability, it is equal in these respects to aluminum-bronze made of 90 parts copper and 10 parts aluminum, and, as it is considerably cheaper, it can with advantage be substituted for this more expensive alloy.—*Polytechnisches Journal*, clxvi., p. 430. G. J. B.

On the Quantity of Air Indispensable for Respiration During Sleep, by M. J. Delbruck.

The author has addressed to the Academy a very interesting note, calling attention to the fact of the very small amount of air consumed by animals during sleep. The air is probably more quickly exhausted in the sleeping than in the waking state, and that the predominance of nitrogen aids the drowsiness. The experiment is easily tried.

M. Delbruck's short note would suffer by abridgment, so must be given textually:—

"What amount of air is necessary for respiration during sleep? This question I would submit to the serious attention of the Academy.

"Discussions periodically arise in the journals as to the quantity of air indispensable during the hours of sleep, and many learned men have decided upon a number of cubic metres for each sleeper, which is far from reassuring.

"Now, here are a series of facts which every one has observed, or may observe, and which seem to lead to an entirely opposite conclusion.

"In the first place, as to animals who possess lungs like ours, and breathe as we do, what takes place? What does a savage beast (lion, tiger, bear, &c.) do at bedtime? It leaves the open air, retires to the very end of its cave, and, as much as possible, cuts off its supply of air.

"The domestic dog: what does he do? He seeks his dog-house, or some corner, and hides himself with his muzzle under his flank.

"Birds, living constantly in the air, which succumb so readily to asphyxia in laboratory experiments, how do they behave at bed-time? They retire to their shelter, and, hiding their heads in the fine down under their wings, limit the supply of air as much as they can.

"There is the marmot, and other hibernating animals, hiding themselves before taking their prolonged sleep, far from the danger of air. Similar instances might be indefinitely multiplied.

"And what does man, when following his natural instinct? The voluminous bed-curtains formerly used are the first answer. But see the child, the school-boy who sleeps in a large dormitory generally well ventilated. If sleep does not come readily, he buries his head in the bed-clothes, much like the bird, or pulls his night-cap down to his chin.

"Finally, for these remarks must be brought to an end, the soldier, when obliged to sleep in the open air, with many cubic metres of air at his disposal, is obliged, if he would sleep well, to cover his head.

"Do not these facts suggest food for reflection? Plants during the day exhale the oxygen which they absorb by night. Does not analogy lead us to conclude that animals during sleep should breathe some of the gas which they exhale while awake?"—*Répertoire de Chimie Appliquée.*

On the Solubility of Sulphate of Lime in Chlorhydric Acid, by S. W. Johnson, *New Haven, U.S.*

In this laboratory it has long been the custom to bring into solution for analytical purposes gypsum, so-called super-phosphate of lime, and other substances containing much sulphate of lime, by treatment with hot dilute chlorhydric acid. The action is rapid, and the analysis may be carried on with more convenience than when decomposition is effected by carbonate of soda. The sulphate of lime is not taken up by very concentrated chlorhydric acid to nearly the same extent as when the acid is dilute, and therefore a saturated solution of the salt in the latter is copiously precipitated by the addition of fuming chlorhydric acid as well as by that of water.

PHYSICAL SCIENCE.

M. de Littrow's Reflecting Spectrometer.

We received last December from M. de Littrow, director of the Observatory at Vienna, a photograph of a remarkable spectrum apparatus invented by his son, M. Othon de Littrow, constructed under his supervision at the Polytechnic Institution of Vienna. In this apparatus the bundle of rays proceeding from the collimator carrying the slit, meets first a series of prisms, which it traverses consecutively, and then falls perpendicularly on a mirror, reflecting it so as to make it retraverse the system of prisms. This reflection of the dispersed band is already present in M. Dubosq's spectroscope, where it is applied to a single prism. M. Jansen has also employed it for several prisms in an apparatus invented simultaneously by him and M. Littrow. It is, however, immaterial who first introduced this reflection.

The return band gives an image of the spectrum close to the slit, observable by means of a small reflecting prism and a lateral eye-piece. The collimator, with the slit and eye-piece, is invariably fixed on the table supporting the prisms. These prisms are connected together on the tripods so as to form a chain; at each point of contact rack-work is disposed in the centre of the circle formed by the prisms. This rack and pinion movement is maintained in a perpendicular position to the table by a plate soldered to its base; it can run the length of the rack of the first prism, which, with its rack, is fixed in one position. By turning the pinion, the distance between the points of contact and the centre of the circle is lengthened or shortened, and it follows that if the prisms are originally arranged so as to give the minimum of deviation for a certain colour, the other colours passing before the eye-piece will also be at their minimum of deviation. The distances of the rays are measured by a micrometer.

By this arrangement one lens is dispensed with, and a double effect is obtained by the same number of prisms, which are regulated by simply turning the rack-work; moreover, the proximity of the eye-piece to the slit simplifies the manipulation. The apparatus made by M. Littrow, jun., has four flint prisms at 60 degrees; it is contained in a dark chamber 30 centimetres square and 12 high. It can be made for 400 francs. According to M. Littrow, many more lines are visible in the solar spectrum with this spectroscope than with that of M. Kirchhoff.—*Moniteur Scientific.*

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

Annual Meeting, Friday, May 1, 1863.

The Duke of Northumberland, *K.G., F.R.S. in the Chair.*

The Annual Report of the Committee of Visitors for the year 1862 was read and adopted.

The amount of contributions from members and subscribers in 1862 amounted to 3079*l.* 13s.; the receipts for subscriptions to lectures were 560*l.* 14s.; the total income for the year amounted to 4630*l.* 8s. 1d.

On December 31, 1862, the funded property was 29,341*l.* 2s. 2d.; and the balance at the bankers, 804*l.* 3s. 4d., with six Exchequer bills of 100*l.* each.

A list of books presented accompanies the Report, amounting in number to 161 volumes; making, with those purchased by the managers and patrons, a total of 558 volumes (including periodicals) added to the library in the year.

Sixty-two lectures and twenty-one evening discourses were delivered during the year 1862.

Thanks were voted to the President, Treasurer, and Secretary, to the Committees of Managers and Visitors, and to Professor Faraday, and the other Professors, for their services to the Institution during the past year.

The following gentlemen were unanimously elected as officers for the ensuing year:—*President*—The Duke of Northumberland, K.G., F.R.S. *Treasurer*—William Pole, Esq., M.A., F.R.S. *Secretary*—Henry Bence Jones, M.A., M.D., F.R.S. *Managers*—Sir William George Armstrong, F.R.S.; the Rev. John Barlow, M.A., F.R.S.; Sir John Peter Boileau, Bart., F.R.S.; George Busk, Esq., F.R.C.S., F.R.S.; George Dodd, Esq., F.S.A.; Sir George Everest, C.B, F.R.S.; John Peter Gassiot, Esq, F.R.S.; Sir Henry Holland, Bart., M.D., D.C.L., F.R.S.; Sir Roderick I. Murchison, K.C.B., D.C.L., F.R.S.; James Nasmyth, Esq.; William Frederick Pollock, Esq., M.A.;

Robert P. Roupell, Esq., M.A., Q.C.; the Lord Wensleydale; Charles Wheatstone, Esq., D.C.L., F.R.S.; Colonel Philip James Yorke, F.R.S. *Visitors*—Hon. and Rev. Samuel Best; George J. Bosanquet, Esq.; Archibald Boyd, Esq.; John Watkins Brett, Esq.; Bernard Edward Brodhurst, Esq.; John Charles Burgoyne, Esq.; Montague Chambers, Esq., Q.C.; George Frederick Chambers, Esq.; Christopher Darby Griffith, Esq., M.P.; Captain Frederick Gaussen; Kenneth Macaulay, Esq., M.P., Q.C.; Edmund Packe, Esq.; the Earl of Rosse, F.R.S., Chancellor Univ. Dublin; the Earl Stanhope, D.C.L., F.R.S., Pres. Soc. Antiq.; George Tomline, Esq., M.P.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Annual Meeting, April 21, 1863.

E. W. Binney, *F.R.S., F.G.S., President, in the Chair.*

Mr. Leopold Hartley Grindon was elected an ordinary member of the Society.

The following reply to Dr. Rankine's paper, read at the last meeting, was communicated by Mr. Dyer :—

In Dr. Rankine's paper, entitled "*Note on Two Events in the History of Steam Navigation*," he calls attention to my paper "*On the Introduction of Steam Navigation*," and expresses regret that "the author has noted either very slightly, or not at all, an event of paramount importance—the first adaptation of the double-acting cranked steam-engine to drive a paddle-wheel." Now, I own to have taken no notice at all of this double cranked motion, simply because I did not consider it of much importance in attaining success in steam navigation. I had before me Mr. Woodcroft's interesting treatise, and I fully appreciated his advocacy of the claims of Mr. Symington for having "fitted up the *Charlotte Dundas*, as the first practical steamboat, in 1801." This fact is fairly stated in my paper. I aimed to place before the Society the several inventions and discoveries relating to the use of steam power to supersede that of wind to navigate vessels, and to prove that the final success was due to the invention of Watt's steam-engine. No great stress need be laid on the double cranked action, or on any of the other methods used for transmitting the power to the paddle-wheels. The principles involved in overcoming the resisting forces by steam power in navigation are entirely apart from those relating to the mechanical means of converting rectilinear into circular motion. It seems strange that such able engineers as Dr. Rankine and Mr. Woodcroft should have given this prominence to the said double crank action. Whatever may have been the degree of success in the case of the *Charlotte Dundas* in 1801, and whatever may be said about the bar to further progress of Symington's inventions by the Duke's death, the fact remains clear that his schemes died out with the Duke, as no more was heard of them after the *Charlotte Dundas*, and from some cause she was discontinued; and it was in fact fifteen years after her advent before the *Margery* came out from the same waters to enter the Thames, in 1816. What shall be said of the "enlightened" of Glasgow, if "Symington's practical steamboat" was suffered to rot and be forgotten, leaving no successor, and the inventor himself to remain unrewarded and unnoticed for fifteen long years? Through the limited space allowed for the abstract of my paper, Dr. Rankine was misled respecting the other event, also cited by him from Mr. Woodcroft's book, namely, that I had "passed over the first introduction of steam navigation into Europe, by Henry Bell, in 1812;" for in my paper due notice is taken of this well-known experiment of Mr. Bell. Although his trial boat proved a failure, on account of its being a very small one, and of his want of pecuniary means for continuing or extending the experiment, and the lack of any aid or encouragement afforded him by others, yet his trial boat served as the model for constructing the *Margery*, three years after, which, as I have before said, was a success, and the first steamboat on English waters. I therefore think that due honour should be paid to the name of Henry Bell, for his spirit and enterprise, and a due stigma cast upon the capitalists who allowed him to sink under pecuniary pressure, from which he ought to have been relieved by them in so important an enterprise.

Report of the Council.

The following Report of the Council was read by one of the Secretaries :—"At the commencement of the present session the number of ordinary members was 204; during the course of the session 18 members have resigned; 2 have been struck off the list as defaulters; 1 member, Mr. G. D. Fleming, has died; and 4 new members have been elected. The present number of ordinary members is therefore 187. No alteration has taken place in the list of honorary or corresponding members. Your Council are glad again to be able to refer to the very satisfactory financial statement of the Treasurer, from which it appears that the balance in hand this year amounts to 320*l.* 18s. 7d., against 248*l.* 6s. 7d. in hand last year. Your Council are likewise glad to remark the increasing estimation in which the *Proceedings* of the Society are held by the scientific and reading public, as evidenced by their being regularly printed in full in several literary and scientific journals. The valuable library of literary and scientific reference which the Society now possesses has during the past year been enriched by the additions of the memoirs and *Transactions* of the numerous academies and scientific societies throughout the world with which the Society is now connected. These additions are noted in the Librarian's Report. Amongst numerous gifts of literary and scientific value which have been made to the Society during the past year, your Council desire to mention the donation by Dr. W. C. Henry, of an extensive collection of letters, papers, and other documents belonging to the late Dr. Dalton; and also several manuscripts, diagrams, &c., belonging to the late Professor Hodgkinson, and presented by his widow. These papers, many of which have a high scientific value, are preserved in the archives of the Society."

MICROSCOPICAL SECTION.

April 20, 1863.

Professor Williamson, *F.R.S., President of the Section, in the Chair.*

Mr. Charles O'Neill, F.C.S., and Mr. John Shae Perring, M.Inst.C.E., were elected members of the section.

Mr. John Slagg, jun., and Mr. H. A. Hurst, were elected auditors of the Treasurer's accounts.

Mr. Alfred Fryer presented for distribution amongst the members a number of impressions of an engraving of the Acarus sacchari found in raw grocery sugar, from Mauritius.

Mr. Brothers stated that he had made some observations upon the circulation in plants, and he found that a degree of heat which would cause free circulation in Vallisneria, entirely destroyed it in Chara vulgata. Mr. Brothers also described the appearances presented by the cilia of Melicerta ringens, which he had the unusual opportunity of observing whilst the animal was outside its case in a dying state. As the motion of the cilia gradually became fitful and then ceased, it was apparent that the cilia of the inner row are much longer than those of the outer row, over which the former appear to bend and to brush off whatever may be adhering to them into the channel between the two rows. Thus are produced the wavy lines and apparent onward progression of the cilia, which render this, under suitable illumination, so brilliant and interesting a microscopical object.

Mr. Charles O'Neill, F.C.S., made a communication upon the "*Appearances of Cotton Fibre during Solution and Disintegration.*" These experiments referred to the application of Schweizer's solvent. Two strengths were used; the weaker contained oxide of copper, equal to 4·3 grs. metal per 1000, and 47 grs. dry ammonia; the stronger contained 15·4 grains metal and 77 grs. dry ammonia per 1000. The latter is about the most concentrated solution which can be made. Referring to the researches of Payen, Fresny, Peligot, Schlossberger, and others who have employed this solvent, the author said the only experimenter who seemed to have worked in the same direction with himself, and that apparently only to a small extent, was Dr. Cramer, whose paper he had only been able to see in a translation appended as a note to a memoir of M. Payen in *Comptes-Rendus*, p. 319, vol. xlviii. Mr. O'Neill considers that cotton exhibits, under the action of this solvent (1), an external membrane distinct from the true cell wall or cellulose matter; (2), spiral vessels situated either in or outside the external membrane; (3), the true cell wall or cellulose; and (4), an inner medullary matter. The external membrane is insoluble in the solvent, and may be obtained in short hollow cylinders by first acting upon the cotton with the dilute solvent so as to gradually remove the cellulose, and then dissolve all soluble matters by the strong solvent. If the strong solution is first applied, the extraordinary dilation of the cellulose bursts the external membrane, and reduces it to such a state of tenuity that it is invisible. This membrane is very elastic, appears to be quite impermeable to the solvent, and when free from fissures protects the enclosed matter from its action. It is not seen in cotton which has been submitted to the action of alkalies, acids; and bleaching powder, being either chemically altered, or, what is most probable, entirely removed. The spiral vessels are unmistakably apparent, running round the fibre in more or less close spirals, sometimes single, sometimes double and parallel, and at other times double and in opposite directions, or again seemingly wound close and tight round the cylinder. They are well seen in the spherical swellings or beads, but are prominent at the points of strangulations of long ovals formed when the ends of the fibres are held tightly. They collect in a close mass, forming a ligature, and are frequently ruptured, the ends projecting from the side of the fibre. The cellulose is enormously dilated by the weaker solvent, and expands the external membrane into beautiful beads, which are doubtless the result of the spiral vessels acting as ligatures at the points of strangulation; at the open end of a fibre it can be seen oozing out as a mucilaginous substance. The stronger solution bursts the beads, or dissolves all the cellulose into a homogeneous mass, amidst which the empty cuticular membrane and the spiral vessels remain nearly unacted upon. The substance called medullary matter is seen occupying the axes of the fibres; it is nearly insoluble in the solvents. It may be well seen projecting from the open end of a fibre where the cellulose is exuding, and often remains *in situ* when the fibre has quite disappeared. It has many appearances of being a distinct body, but the author in some cases thought it might be only the thickened or modified inner cell wall; in others it looked like a shrunk membrane, probably the dried up primordial utricle. It is generally absent or indistinct in old cotton, or cotton which has been submitted to bleaching agents. Mr. O'Neill intends to submit further details when his investigations are more advanced.

Mr. Hepworth stated that he had observed spiral markings in Sea Island Cotton not subjected to chemical action, and that he had calculated there would be about 50,000 spirals to an inch of fibre.

NOTICES OF BOOKS.

A Chemical Review. By a B. London: Taylor and Francis.

A pamphlet of twenty-five pages, small 8vo, entitled "A Chemical Review. By a B," has recently emanated from the printing-office of Messrs. Taylor and Francis, Red Lion Court, Fleet Street, E.C. It is done up in glaring magenta red covers, with gilt lettering, and sells at a shilling to the public and ninepence to the trade. It consists of forty-seven stanzas of doggrel rhyme, supposed to be descriptive of the characters and habits of British chemists; and may possibly be amusing to the "swarm" at the "Cheshire Cheese," who revel in potations, tobacco, and sawdust. However, the propriety of publishing such an effusion as a mercantile speculation is more than questionable. Although it is for the most part inoffensive, and hardly rises to the level of respectable insipidity, yet there are a few hits which good taste would certainly have forbidden to appear in type. The author should remember that "B" is a highly suggestive initial; it may stand for bee, bluebottle, bear, bungler, and various other words which it is not necessary to enumerate. The field of verse is a new region for the exercise of the chemical intellect; but the quality of poor B's versification justifies us in believing that he has sadly mistaken his vocation.

External appearances are sometimes very deceptive. Brass has been mistaken for gold; but one thing it is not possible to mistake, that is, a copper field for a gold field. *Verbum sap.*

Heat Considered as a Mode of Motion: being a Course of Twelve Lectures Delivered at the Royal Institution of Great Britain in the Season of 1862. By John Tyndall, F.R.S., &c., Professor of Natural Philosophy in the Royal Institution. London: Longmans and Co.

The recognition of the facts on which modern chemical doctrines are based, took place with such rapidity, and produced such a vast alteration in the mode of regarding chemical phenomena, that it cannot be much wondered at that in the transition from that condition of chemistry in which qualitative changes only were considered, to that condition in which these were looked upon only as consequences of quantitative changes, the attention of chemists immediately succeeding Lavoisier should have been somewhat too exclusively directed to the study of the material aspect of chemical phenomena, or that there was too great a tendency to regard them all as instances of combination and separation of substantive elements, without considering their causes. Some few chemists of that period did protest against the general disregard of the evolution of heat and light in chemical action; but, as they were chiefly adherents of the discarded doctrine of phlogiston, their influence was but slight. It was not until the results of Volta's experiments had been made known, and so successfully rendered of service to chemistry, that the causes of the changes taking place under various circumstances came to be more minutely and carefully studied. Still it has been chiefly by physicists that the phenomena of heat, electricity, and light have been studied, and the ideas of the causes to which these phenomena are referable are still expressed with much vagueness and uncertainty in chemical works.

This is the case not only with regard to what Gmelin treats of as the "imponderable elements," heat, electricity, light, &c., but also in regard even to the cause of chemical combination, aggregation, and the structural peculiarities of solids. In the study of chemical phenomena, details of composition and constitution have been more regarded than the phenomena of force, or the exercise of energy as the cause and the effect of chemical change. Beyond a general recognition of the inherent activity of material substances, and the adoption of terms to represent various modifications of that activity, little has been done by chemists towards acquiring a more adequate knowledge of the relations and connection between the material and dynamic features of chemical phenomena.

The necessity for prosecuting this branch of investigation is daily becoming more apparent. The known phenomena of isomerism, allotropy, phosphorescence, and those obscure phenomena named catalytic. are now so numerous, that it is evident mere substantial differences are not by any means the only ones of importance for the consideration of the chemist. Substantial differences or resemblance no longer afford a means of predicating diversity or identity; the means of doing so must be sought elsewhere than in the mere elementary composition of substances, perhaps even elsewhere than in their constitution.

Chief among the subjects which call for the chemist's study are the nature of the cause of chemical action and the phenomena of heat. The co-relation of those modes of energy which give rise to phenomena of chemical change, of heat, electricity, light, and magnetism—at present but somewhat vaguely indicated—presents a fertile field of research to the chemist. The doctrines hitherto current among chemists with regard to these forces, or as they may perhaps be more properly defined, modes of energy, are now effete, or require remodelling to bring them into accordance with the knowledge of the present time. This has become especially necessary as regards the phenomena of heat, in consequence of the intimate connexion that has been shown to exist between these phenomena and those of mechanical force. Whether heat really consists in a vibratory or other motion of the molecules of substances, or not, is probably still a question, but it is one well worthy of being examined. In some form or other, this view of heat has long been entertained. Without going back so far as the time of Aristotle, it will be sufficient to mention the names of Bacon, Locke, Newton, Boerhaave, Boyle, Rumford, and Davy, as supporters of such a view, in order to show that it has claims on the consideration of both chemists and physicists.

Professor Tyndall, adopting in the broadest sense the theory that heat is motion, gives, in the lectures he has just published, an exposition of the physical phenomena of heat in accordance with this theory. Taking as the basis of his argument Mr. Joule's determination of the "mechanical value of heat," and the demonstration of the mutual convertibility of heat and motion, or mechanical force, he illustrates the applicability of the dynamic theory of heat in the forcible manner which is peculiar to him, by the production of heat by friction, by the phenomena of expansion, specific and latent heat, conduction, and radiation. In all cases heat is supposed to consist in the vibratory motion of the atoms of molecules of bodies, and these are supposed to swing in an ether of extreme tenuity which permeates all space and all substances.

This assumption of the existence of an ether, whatever may be its advantages as a theory, appears to be open to the same objection that was so reasonably urged against the doctrine of phlogiston or against the hypothesis of heat being an imponderable substance. It is not quite easy to see why, if heat be a vibratory motion of molecules, that vibration should not be regarded as taking place without the intervention of this hypothetical ether; or why it should not be admitted that, according to the view put forward by Newton in his "Optics," the particles of bodies act at a distance upon one another in virtue of their attraction.

The opinion that heat consists in motion, or that it results from the percussion of the atoms or molecules of substances, is very clearly put forward by Newton. He refers the heat evolved on mixing sulphuric acid and water to a great motion on the parts of the liquids, which, in mixing, coalesce with violence, and rush towards one another with an accelerated motion, and clash with great force.

Black strongly dissents from Bacon's view, that heat is motion, a rapid tremor or vibration of the particles of solid bodies. He says:—"I cannot form to myself a conception of this internal tremor that has any tendency to explain even the more simple effects of heat, or those phenomena which indicate its presence in a body."

In accordance with this objection, it is perhaps worth mentioning that those substances in which vibratory motion is most readily produced, such as steel, are less susceptible of being heated by a blow than such substances as lead, in which the parts are mechanically displaced by percussion. In fluids, also, where the mobility of particles is greater than in solids, the thermal effects of friction or mechanical force are less than in solids, where the particles are apparently less susceptible of being set in motion among themselves. It would seem that these facts point rather to the inference that heat, or at least sensible thermometric heat, is a result of the stoppage of motion rather than motion itself; in fact, a mode of energy rather than a mode of motion.

Professor Tyndall insists strongly upon the necessity of clearness of conceptions in regard to the phenomena of heat and their cause, and there can be no question as to the vast importance of such a character of the ideas formed with regard to this or any other class of physical phenomena. He also maintains that conceptions of the invisible part of physical phenomena must be obtained by means of proper images derived from the visible. But it may be asked,—What are "proper images" of the invisible part of physical phenomena, which are also unknown? If imagination, as Professor Tyndall contends, is to be allowed to furnish those images, our theoretical views will be likely to be better representations of fancy than of fact, and that is a result certainly not to be desired. The use of such comparisons as those between the waves of the sea breaking on the shore and waves of light and heat breaking on the atoms of bodies, or the description of an electric current breaking impetuously upon the molecules of a platinum wire, is liable to produce distorted and erroneous ideas rather than strict conceptions of physical facts; and as these illustrations are applied to an hypothesis, it is doubtful whether the fixity of ideas they are calculated to confer is not a greater disadvantage than their effective nature is an advantage. Goethe has well remarked that the disadvantages of hypotheses do not result necessarily from their truth or falsehood, but from their becoming established as articles of faith which no one ventures to entertain a doubt of; and that this is really the misfortune from which centuries suffer.

In the definition of energy, at p. 137, there appears to be some omission of a due consideration of those effects which, though not consisting in motion, are the results of the same activity or energy that under other circumstances may produce motion. Potential energy is defined as the power of motion which a suspended weight possesses, but which has not yet been exercised by falling. The antithesis of this is actual energy, or the force acquired at the end of the fall. Thus, in the case of a mass of lead suspended above the ground, or resting upon the ground, it is represented to be in both instances "dead and motionless," having "no energy," but that by raising it; there is, then, "an action possible to it, which was not possible when it rested on the earth: it can fall;" . . . there is "conferred upon it a motion-producing power." The accuracy of this representation may be questioned. It is true there is a difference between the mass when resting upon the ground and when raised above the ground, as regards the possibility of its motion; but, as regards energy, is the condition of the mass entirely changed? To hold the mass suspended, a continuous exercise of energy is requisite, proportionate to the magnitude of the mass. Between it and the earth the energy of attraction is exercised equally while it is suspended and when resting on the ground. In the latter case it is manifested as pressure against the support, which presents a continuous and uniform resistance to the motion of the mass, and on which the mass of lead rests without the possibility of falling. In both instances energy is exerted equally, but while it is so opposed there can be no dynamic effect. In falling the same energy of attraction is exerted, and it is then unopposed, and accumulates, producing accelerated motion. The term potential energy can only be applied in reference to motion, and in this sense a body resting on the ground has no potential energy, because it cannot fall. But the raising of the mass of lead above the ground does not confer upon it a motion-producing power; all that it does is to place the mass in a condition admitting of its energy being exercised accumulatively in producing motion, instead of being expended momentarily as pressure. The only difference between the supported mass and the falling mass is in regard to the possibility of motion and the capability of performing work. If the term "energy" is used with this limitation, as denoting merely a capacity to effect changes, it will not involve any physical inconsistencies, as it would if applied to the result of the mutual activity of masses under all conditions.

A Manual of Elementary Chemistry, Theoretical and Practical. By the late George Fownes, F.R.S. Ninth edition, revised and corrected. Pp. 820. London: Churchill and Sons, 1863.

In the very numerous alterations and additions effected by the editors of this admirable class-book from time to time, the original design of Professor Fownes has not been obscured. Each successive edition now appearing at intervals of two years, although containing concise reports of chemical progress in the various departments of the science, yet does not show any marked increase in the bulk of the volume. A typical process having been once described with sufficient fulness, the editors content themselves in the case of new discoveries of analogous character with condensed descriptions, which we may aptly designate by borrowing a chemical expression, and calling them "well-crystallised."

The general scope and plan of Fownes' "Chemistry" is so thoroughly known and appreciated, that it is quite unnecessary to review these points here. We can only say that this ninth edition is still better than the eighth; that the chief recently ascertained facts of chemistry are duly incorporated in the work; that the essay on the unitary system and notation of Gerhardt appended to the manual has been rendered more complete; and that it is with justice that the editors say in their preface—

"Each part has received important additions, and the whole work has throughout been carefully revised.

"In inorganic chemistry an outline of spectrum analysis, and of the great discoveries made by it, and an abstract of the researches of Mr. Graham on diffusion and dialysis, are inserted.

"In organic chemistry, an account of the recent extension of the theory of acids and polyatomic alcohols, and a chapter on the new colouring matters derived from coal-tar, form the chief additions. Throughout the whole of this portion of the work the results of the latest researches of importance are recorded.

"In animal chemistry a notice of the remarkable experiments of Pettenkofer on respiration has been added."

NOTICES OF PATENTS.

Grants of Provisional Protection for Six Months.

641. Henry Revell Spicer, Clement's Lane, Lombard Street, London, "Improvements in protecting and preserving the bottoms and sides of ships and other submerged surfaces from oxidation or fouling by incrustation, the attachment of barnacles, the action of animalculæ, or from any other like causes of injury."—Petition recorded March 6, 1863.

771. Samuel Healey, Elizabeth Street, Hackney Road, London, "Improvements in the manufacture of zinc, and in the apparatus employed therein."

783. John Henry Johnson, Lincoln's Inn Fields, London, "Improvements in the manufacture of zinc, and in the apparatus employed therein."—A communication from Adrien Muller, Paris.

450. John Gray and Joseph Hudson, Botolph Lane, London, "Improvements in the treatment of steatite, and in its application to certain purposes."—Petition recorded February 18, 1863.

667. William Wood, Monkhill, near Pontefract, Yorkshire, "Improvements in the manufacture and ornamentation of Pomfret or liquorice cakes, rolls, sticks, and pipes, and other similar articles of confectionery."—Petition recorded March 11, 1863.

956. Isham Baggs, Cambridge Terrace, London, and William Simpson, Tovill Upper Mills, Kent, "Improvements in purifying and treating coal gas, sulphuretted hydrogen, and other gases containing sulphuretted hydrogen, and in obtaining sulphur, sulphuric, and other acids in such treatment."

551. Henry Fehr, Fenchurch Street, London, "Improvements in the treatment of mineral oils."—Petition recorded February 27, 1863.

663. John Cassell, La Belle Sauvage Yard, London, "Improvements in moderator lamps to adapt them to the burning of petroleum and other mineral oils and hydrocarbons."—A communication from Louis Martin, Paris.

679. James Polkinghorne, Redruth, Cornwall, "Improvements in treating tin ores and in apparatus for treating ores and matters containing arsenic."—Petitions recorded March 12, 1863.

714. William Henry Emett, St. George's Terrace, South Kensington, London, "Improvements in processes for facilitating and combining the art of writing with engraving together on stone, applicable to maps, plans, specifications, and other lithography, which improvements are also available for re-transfers to zinc or stone, or printing from original."

717. Georges de Laire, Imperial Mint, Paris, "Improvements in the manufacture of brown colouring matters."—Partly a communication from Charles Girard, Lyons, France."—Petitions recorded March 17, 1863.

732. Auguste Morel, Rue du Marché, Brussels, "Improvements in apparatus for generating carbonic acid."

735. Ellis Lever, Manchester, "An improved composition for the coating and preservation of canvas and other materials to make them waterproof and non-inflammable."

752. Fedor de Wylde, Trinity Square, Tower Hill, London, "Improvements in the manufacture of cement from gypsum."—Petitions recorded March 20, 1863.

755. Charles de Groote, Brussels, "Improvements in the construction of lamps with circular burners or wicks for the combustion of petroleum, schist, and other volatile oils, parts of said improvements being applicable to gas-burners, night lights, and lamps burning spirits and animal oils."

788. Robert Mushet, Coleford, Gloucestershire, "An improvement or improvements in treating steel and iron prepared by the pneumatic process."

Notices to Proceed.

521. William Readman, Glasgow, N.B., "Improvements in the manufacture of carbonate of magnesia, and of iodine and kelp salt, and other products from kelp."—Petition recorded February 25, 1863.

3299 Richard Archibald Brooman, Fleet Street, London, "Improvements in treating liquorice root to obtain liquid and solid extracts therefrom."—A communication from Frumence Nicolas Frezon, Paris.

3312. Astley Paston Price, Lincoln's Inn Fields, London, "Improvements in the manufacture or production of blue colours."—A communication from Augustus Eisenlour, Heidelberg, Grand Duchy of Baden.—Petitions recorded December 10, 1862.

3383. Edmond Lepainteur, Paris, "Improvements in the fabrication of a salt for dyeing textile materials."

3353. John McInnes, Liverpool, and Elijah Freeman Prentiss, Birkenhead, Cheshire "Improvements in the distillation and treatment of petroleum and other like oils to obtain products therefrom, and in the apparatus to be used therefor, parts of which can be applied for distilling other liquids."

3355. George Cockburn Warden, Islington, London, "Improvements in ornamenting textile fabrics, leather, and other surfaces, in a cement employed therein, which is also applicable to the waterproofing of fabrics and materials, and in apparatus for applying and spreading such waterproof cement."—A communication from Adolph Baldamus, Berlin, Prussia.

3361. John Louis William Thudichum, Kensington, London, "Improvements in collecting human excreta, and in the apparatus and means employed therein."

3363. Rudolph Schomburg, Onslow Terrace, and Adolph Baldamus, Surrey Terrace, Lorrimore Road, Surrey, "Improvements applicable to all kinds of oil used for illuminating purposes, whereby combustion thereof is rendered more perfect, smoke prevented, and the purity of the light increased."

3390. John Savory, Bond Street, London, "A new or improved apparatus for the inhalation of medicinal powders or vapours for the treatment of diseases of the throat and lungs."—A communication from Dr. John Roberts, Paris.—Petition recorded December 18, 1862.

3430. Thomas Callender Hinde, Cardiff, Glamorganshire, "Improvements in furnaces or apparatus for generating carbonic oxide."

CORRESPONDENCE.

Preservation of Stone from Decay.

To the Editor of the Chemical News.

Sir,—A statement appeared in the *Times* the other day in reference to this subject, which is not only incorrect, but unfair to some of those who have made experiments with a view to the preservation of the stone of the Houses of Parliament. It is said that "Mr. Szerelmey, a gentleman well known for his most curious chemical discoveries in hardening stone . . . . is, up to the present time, the most successful of all the many competitors for preserving the Houses of Parliament from further decay, by indurating the surface of the stone with a fluid silica, which, it is asserted, renders the stone beneath perfectly indestructible."

It is impossible to say what curiosity may attach to Mr. Szerelmey's method of treating stone with a view to its preservation, since that method is professed to be kept by him a profound secret.

As to the success which he has achieved in comparison with other competitors, there is no doubt, however. In whatever way his connection with the Houses of Parliament may have been a success to him, it is certain that there has been no successful result in the preservation of the stone from decay. There is every reason to believe his method of treatment to be as worthless as those tried by others, and in confirmation of this opinion the report of the committee appointed to inquire into the subject speaks unmistakably:—

"With regard to the processes which have actually been applied, whether experimentally or extensively, your Committee are decidedly of opinion that the discovery of a proper mode of treating stones in a state of decay has not yet been made, and there is no evidence whatever on the building itself to induce them to believe that the decay, where decay has arisen, has been arrested, or that permanently the decay has been prevented by any of the processes yet applied."

The chemical sub-committee appointed to inquire into the proposed method of treatment as to whether any of them are likely to be successful in effecting preservation, report:—"There is not one which we at present feel justified in proposing that the Committee should definitely recommend as a preservative, either for general or local application."

"They allude to secret processes, regarding which they say they can offer no opinion, but they express a doubt of the applicability of any suggestion which would demand the veil of secrecy for protection. Concurring in this view, it may be further noted that even if such applications were found successful in sample or experiment, no security would be afforded for a corresponding success in any subsequent large operations. They recommend that a series of experiments should be conducted under chemical supervision for a considerable period of time, and the Committee are most reluctantly compelled to coincide with them, and to urge upon the Government the adoption of such a course."

I am, &c. B. H. Paul.

Bathvillite.

To the Editor of the Chemical News.

Sir,—"Ein Untersucher," a correspondent in the Chemical News of April 18, makes some remarks on a short paper by Mr. Greville Williams which appeared in the News of March 21. These remarks seem to me to be needlessly personal, and not to be written in the tone which ought to be adopted when writers take different views on any scientific topic. I am an occasional reader of your excellent journal, and, on the whole, am inclined to take the opposite view to Mr. Williams when he says that the Torbanehill mineral is not a coal; at the same time, I can see no harm in that substance being designated "Torbanite," even though it be a coal. Mr. Williams is as much entitled to call it "Torbanite" as any one else to call "blind coal" "anthracite." In this way the name may be used to designate a variety of coal: it does not necessarily imply that the substance is not coal.

A similar remark applies to the objection of "Untersucher" to the word "Photogene." He must admit that the term "Photogene," when applied to an oil used for the light it gives, is a more elegant and terse appellation than Paraffin-oil.

Surely "Untersucher" cannot believe that Mr. Williams would not know a coprolite when he sees one. A mere reader of Mr. Williams' paper, like myself, can see in the analytical details given abundance of reasons for rejecting the statement of "Untersucher," that the mineral analysed was a coprolite.

Again, what is the meaning of the closing remark of "Untersucher's" letter when he says, does Mr. Williams think that no chemist ever imagined such an apparatus as that Mr. Williams figures in his paper. Doubtless many chemists may imagine such things, but it remains for the busy workers amongst them to make and to use them.

Will any one say that it was out of place for the author of the best "Handbook of Chemical Manipulation" to describe the means whereby he so successfully analysed a mineral, operating on such a small quantity as three decigrammes (about five English grains)? Mr. Williams' object in describing the apparatus was evidently to bring this prominently forward. I am, &c.

Boghead Coal without Choler.

Dumfries.

MISCELLANEOUS.

Adulteration of Linseed.—At the adjourned meeting of the linseed trade on the question of the prevention of adulteration, and the proposal to sell by weight instead of measure, held at the Baltic Coffee-house on Tuesday morning, the following report was unanimously received and adopted, and a permanent committee was nominated to manage the proposed association:—

"The committee, after seeking for information from the most practical sources, have come to the conclusion that the Marseilles system can easily be brought into use in this country. Along with this system they also recommend should be adopted the practice of selling linseed by weight, that is, by the ton of 20 cwt., or 2,240lb., whether sold on delivered, or cost, freight, and insurance terms.

"A strong opinion having been expressed in committee by the members from Hull that it was necessary to get the large interest connected with the seed trade at that place and in its vicinity to confirm the views entertained by the committee, a public meeting was called there, at which a deputation from London attended.

"At that meeting, after a full discussion of the whole subject, it was resolved—

"'That it is the opinion of this meeting that the Marseilles system should be adopted in this country for all qualities of linseed imported from any port whatever, with the amendment proposed by Dr. Longstaff, that when linseed contains a less admixture than is equal to 4 per cent. of non-oleaginous seed, the difference should be added to the selling price.'

"At the meeting of your committee, after the return of the deputation from Hull, the following resolutions were passed:—

"'1. That with a view to carry out the original recommendation of the trade, as expressed at the meeting of the 17th of December last, and further confirmed at the public meeting held at Hull on the 17th inst., this committee unanimously recommend that the Marseilles system, modified, as suggested at Hull, should be adopted in all sales of linseed.

"'2. That an association, to be styled 'the Linseed Association,' be formed to promote the importation of linseed as pure as possible, and for the purpose of properly sampling and analysing the seed on arrival.

"'3. That a committee of management be nominated at the next meeting of the trade, who shall be empowered to frame rules for carrying out the above recommendations.'

"The committee consider that, if seed is in future to be sold by weight, the standard of 2,240lb., or per ton of 20 cwt., will be found most convenient. Oil and cake are both sold in this way. Mediterranean and Black Sea grain and seed freights are now so calculated, and it is probable, before long, the duties on grain and seed will be levied on the ton of 20 cwt.

"Your committee have only further to suggest that this meeting do at once form an association, and proceed to elect the gentlemen to form the committee of management, who they trust will be able to bring the new system into operation, at latest, by the 1st of January next.

"J. C. D. Bevan, Chairman.
"J. M. Eastty, Hon. Sec."

The Oil Springs of America and Canada.—(*Review for April*)—The uncertainty of commercial prescience was never more completely affirmed than it has been in petroleum during the month that is now past. Practical traders made a solicitous point of clearing out all their stock, ready to buy in at a summer's reduction; and with sapient avidity, contracted at very low rates with their less experienced brethren! Speculators drawing their conclusions from, and adopting the same policy, made very free sales at prices almost incompatible with the cost of production; the penalty of which, as will be seen, even now, lies heavily upon them. Others, not prepared to see an advance such as has been attained, stocked themselves, however, with propriety, ready to invest in more at a discount, and be guided by progressing events. The knowledge that what was shipping on the other side is totally insufficient for our European wants—that the present worth of petroleum is utterly inadequate to its merits and universal application—that temporary troubles with the Federals would double, treble, or quadruple prices—that a strong export demand had set in here, and that the longest day was only six weeks off—all these circumstances combined, suggested a general investment, every one feeling that the loss could only be little to the minus, while the gain might be much to the plus. The result shows that this logic was cogent, and it seems probable that for the remainder of this year the petroleum trade will be profitable and gigantic. In the first three months, the American export was equal to the whole twelve months of 1862, and assuming, as we have a right to do, that it will continue at the same, or at an increased ratio, the importations into Europe will approach the value of 4,000,000*l.* sterling for this year; an augmentation and a position strongly endorsing the prediction that was given, viz., "that the petroleum trade in its day would be second only to cotton." The importations into Liverpool this year are over 70,000 casks and cases, against 3000 casks same time last, yielding in dock and town dues alone nearly 2,000*l.* for the four months.—*From Alex. S. Macrae's Circular, May*, 1863.

SCIENTIFIC SOCIETIES.

MEETINGS FOR THE ENSUING WEEK.

May 11. *Monday.*

Geographical—15, Whitehall Place. 8.30 p.m.

12. *Tuesday.*

Medical and Chirurgical—53, Berners Street. 8.30 p.m.
Civil Engineers—25, Great George Street, Westminster. 8 p.m.
Zoological—11, Hanover Square. 9 p.m.
Syro-Egyptian—22, Hart Street, Bloomsbury. 7.30 p.m.
Royal Institution—Albemarle Street. 3 p.m. Professor Tyndall, "*On Sound.*"

13. *Wednesday.*

Society of Arts—John Street, Adelphi. 8 p.m. Dr. J. L. W. Thudichum, "*On the Collection and Utilisation of Excrementitious Matter*;" and the Rev. H. Moule, "*On a System of Earth Sewage.*"
Graphic—Flaxman Hall, University College. 8 p.m.
Microscopical—King's College, Strand. 8 p.m.
Literary Fund—4, Adelphi Terrace. 3 p.m.
Archæological Association—32, Sackville Street. 4 p.m. *Anniversary.*

14. *Thursday.*

Antiquaries—Somerset House. 8.30 p.m.
Royal Institution—Albemarle Street. 3 p.m. Prof. Ansted, "*On Geology.*"

15. *Friday.*

Philological—Somerset House. 8 p.m. *Anniversary.*
Royal Institution—Albemarle Street. 8 p.m. Dr. Odling, "*On the Molecule of Water.*"

16. *Saturday.*

Royal Institution—Albemarle Street. 3 p.m. Prof. Max Müller, "*On the Science of Language.*"

ANSWERS TO CORRESPONDENTS.

*** All *Editorial Communications* are to be addressed to the Editor, and *Advertisements* and *Business Communications* to the Publisher, at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

Vol. VI. of the Chemical News, containing a copious Index, is now ready, price 10s 8d., by post, 11s. 2d., handsomely bound in cloth, gold-lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. if sent to our Office, or, if accompanied by a cloth case, for 6d. Vols. I. and II. are out of print. All the others are kept in stock. Vol. VII. commenced on January 3, 1863, and will be complete in 26 numbers.

W. A. Dixon.—For the present we are unable to entertain our correspondent's proposition. We are, however, obliged for the offer.

THE CHEMICAL NEWS.

Vol. VII. No. 180.—May 16, 1863.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Disassociation of Water, by M. S. C. Deville.*

When a current of hydrogen is passed through a porous earthen tube, even rapidly, the gas which issues from the tube is found to be not hydrogen, but atmospheric air. The hydrogen during its passage is diffused through the porous tube, and atmospheric air passes through into the interior by means of endosmose, notwithstanding the pressure caused by the dipping of the discharge tube for several centimetres into water or mercury.

When this porous tube is enveloped in an atmosphere of carbonic acid, by enclosing it in a shorter tube of glazed porcelain, the ends of which are closed by corks perforated to admit the porous tube through them, and also fitted with tubes for passing a current of gas through the annular space thus formed, and a regulated current of hydrogen is passed through the porous tube, while a rapid current of carbonic acid is passed through the outer tube, hydrogen makes its appearance at the discharge from the outer tube, while carbonic acid, almost pure, escapes from the interior tube.

Thus, by means of endosmose, the two gases have changed places, each of them traversing in opposite directions the porous wall of the interior tube. This fact is quite in accordance with the phenomena observed by Mr. Graham and M. Iamin.

When these tubes, thus arranged, are heated to a temperature of from 1100° to 1300° C., and a current of water vapour is passed through the interior one, while the current of carbonic acid is kept up in the outer tube, the gas collected consists of hydrogen and oxygen mixed with carbonic acid.

A part of the water vapour undergoes decomposition in the porous tube. The hydrogen, attracted by the carbonic acid in the outer tube, is separated, as if by a filter, from the oxygen; traversing the permeable wall of the tube, while carbonic acid is attracted in the opposite direction, as in the previous experiment, and mixes with the oxygen.

About one cubic centimetre of an explosive mixture of hydrogen and oxygen was obtained from each gramme of water used.

These phenomena, however, are not quite so simple as above described.

In the first place, whenever hydrogen is in contact with carbonic acid, water and carbonic oxide are formed. Secondly, notwithstanding all precautions in closing the apparatus hermetically, it is impossible to prevent the loss of some hydrogen; consequently, the oxygen is always in excess. Lastly, it is impossible to prevent the presence of a small quantity of atmospheric air being introduced in a state of mixture with the water and carbonic acid. The composition of the gas collected was, in 100 parts:—

	Oxygen.	Hydrogen.	Carbonic oxide.	Nitrogen.
1.	55·7	24·3	0	20
2.	48·6	13·1	25·3	13

The separation of the hydrogen and oxygen may perhaps be effected mechanically as well as by endosmose. This M. Deville is at present endeavouring to ascertain. He has ascertained that water vapour heated in a platinum tube to a temperature near the melting point of that metal is not decomposed to any extent, or at least is reproduced in escaping from the tube.

According to the experiments of MM. Deville and Debray, the temperature of combustion of hydrogen in oxygen does not reach to 2500° C. At this temperature the gases occupy a volume nearly tenfold that which they occupy at 0° C. This is the limit of temperature above which water is entirely decomposed. That decomposition is accompanied by a considerable absorption of heat requisite for maintaining the molecules of hydrogen and oxygen at a distance greater than the range of their affinity.

Thus the decomposition of a substance is in all respects analogous to the ebullition of liquids, where the principal characteristic is the invariability of the temperature in the influence of a source of heat of any intensity, provided the pressure remains the same. Vapour of water is unable to resist the influence of a temperature that expands it to tenfold its volume at 0° C. It is then decomposed, while its constituents absorb heat, which M. Deville calls the latent heat of decomposition.

M. Clausius has shown that the specific heat of gases and vapours does not vary with the temperature, and this law has been verified by M. Regnault for air between 30° and 225° C. The quantity of heat generated by the combination of one gramme of hydrogen with eight grammes of oxygen is 34,500 units, according to Dulong, Favre, and Silbermann; consequently, 3833 units are generated by the formation of one gramme of water. The quantity of heat absorbed by one gramme of water heated from 0° to 2500° C. is expressed by the formula

$$637 + (2500 - 100)0{\cdot}475 = 1680,$$

in which 637 represents the quantity of heat requisite to convert a gramme of water at 0° C. into vapour at 100° C., and the term (2500—100) 0·475 represents the heat required to raise the temperature of the vapour from 100° to 2500° C. The difference between 3833 and 1680 is 2153, which represents, in heat units, the quantity of heat absorbed by the elements of the vapour at the moment of separation—the latent heat of decomposition.

The relation between the effects of affinity and cohesion in the case of solids and liquids holds good also in the reverse phenomena of volatilisation and decomposition. Admitting this resemblance, it will be seen that the phenomenon of decomposition at a relatively low temperature, or disassociation, corresponds to the vaporisation of a liquid at a temperature below that of ebullition,

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* *Comptes-Rendus*, lvi., 195.

and that the quantity of a substance so decomposed at any given temperature is proportional to its tension of disassociation expressed in millimetres of mercury, just as the quantity of vapour formed above a liquid at a given temperature is proportional to the maximum tension of its vapour.

A liquid does not possess any tension in its own vapour, and the quantity of water vaporised in a closed space, whether a vacuum or not, compared with the volume of the water itself, is small, and in general may be disregarded. In the same manner, the quantity of water vapour decomposed when inclosed at 1200° C. in a porcelain flask is so small that the density of the vapour is not affected.

If water at the ordinary temperature is inclosed in a vessel of small bulk, the quantity vaporised is very small, the tension of the liquid becomes *nil* as soon as the space is filled with vapour; but, if a fragment of chloride of calcium is introduced, the water evaporates until the chloride is dissolved, and, while the chloride is becoming saturated, the tension of the vapour remains constant the whole time.

This is the part played by silver and oxide of lead in water vapour decomposed at 1000° C. They absorb the oxygen, and, if they can get rid of the hydrogen, the decomposition of water continues, until those substances have become quite saturated; the tension of disassociation of the oxygen—expressed by a column of mercury—remains constant during the operation.

Lastly, when water vapour is heated to a high temperature in the apparatus above described, the effect produced is analogous to that produced when a volatile liquid is exposed to a current of gas. The carbonic acid carries away oxygen and hydrogen in such quantities as they are separated by the tension of disassociation at that temperature.

When melted platinum is poured into water there is an abundant evolution of explosive gas, a mixture of hydrogen and oxygen with a small quantity of nitrogen that was dissolved in the water, and disengaged by the heat.† This phenomenon is a repetition on a large scale of Mr. Grove's experiment of decomposing water by contact with platinum heated to its melted point.

Starting from this point, M. Deville‡ has endeavoured to ascertain the cause of the apparently contradictory facts that platinum melts so readily under the influence of the temperature developed by the combination of hydrogen and oxygen, and that at the same time platinum when melted, or merely heated to whiteness, decomposes water.

It then remained to explain the decomposition in the particular case observed by Mr. Grove.

A current of carbonic acid saturated with water vapour by passing it rapidly through a vessel containing water at 90° to 95° C. was then passed through a porcelain tube five or six centimetres diameter, and filled with fragments of porcelain previously heated to redness. While the tube was maintained at a high temperature a small quantity of water vapour was decomposed. By passing the gas issuing from the tube, through caustic potash, to separate carbonic acid, an explosive gas was obtained, consisting of

	Oxygen.	Hydrogen.	Carbonic Oxide.	Nitrogen.
1.	46·1	35·4	12	6·5
2.	46·8	31·9	10·7	10·6

Hence it appears that the dissociation of water may take place even without the aid of the porous diaphragm. But in that case the quantity decomposed is less, amounting to only one-fourth in the same time—a result which is evidently due to the circumstance that the oxygen and hydrogen, not being separated from each other as the decomposition proceeds, recombine in the cooler parts of the tube. This recombination is not complete for two reasons. First, the incombustibility of an explosive mixture diffused through a large quantity of inert gas, such as carbonic acid or nitrogen. Secondly, the rapidity of the gas current passed through the porcelain tube, which determines the rapidity of cooling, or the return of the oxygen and hydrogen to the temperature at which they no longer combine when disseminated through a large bulk of carbonic acid.

In Mr. Grove's experiment the melted platinum vaporises a small quantity of the water round it, and this vapour, being intensely heated, is partially decomposed to an extent proportionate to the tension of disassociation corresponding to the temperature of the melted platinum. This portion becomes greatly increased in volume, and it is rapidly cooled as it rises towards the surface of the water, the rapidity of cooling being so great that a part of the gases escape recombination. In short, the perfect inflammation of a given quantity of an explosive mixture requires a given time, as is demonstrated by the action of platinum plates upon mixed oxygen and hydrogen. Besides this, the gas thus obtained contains a large quantity of nitrogen, which probably acts as a further obstacle to the recombination.

TECHNICAL CHEMISTRY.

Molecular Constitution of Steel.

Karsten observed that by the action of acids upon soft steel, a graphitic substance is obtained as a residue, which is not obtained by the action of acids upon hardened steel. This substance he represented to be a definite carburet of iron FeC_6. Berthier also stated that cast steel contains another carburet, CFe.

M. Caron* infers, from the results of numerous analyses, that these carburets are probably mere variable mixtures of carbon and iron, in which the iron is protected mechanically by the carbon from the solvent action of the acid.

He has experimented upon steel in three different conditions:—

1. Steel as it comes from the cementation chambers.
2. The same steel hammered for a long time.
3. The same steel hardened.

By the action of hydrochloric acid on these three kinds of steel the quantities of graphitic residue obtained are very different, being for 100 parts as follow:—

	Graphitic residue.	Carbon.	Iron.	Silicium.
Ordinary steel .	1·624 =	0·825	0·557	0·242
Hammered steel .	1·243 =	0·560	0·445	0·238
Tempered steel .	0·240 =	trace.	trace.	0·240

These results show that the effect produced by tempering is to some extent produced by hammering, and the quality of the steel appears to improve in proportion as the carbon is intimately combined with the iron.

In the same manner forging, which improves the quality of steel, also reduces the quantity of carbon that is separated by solution in acids. Rolled steel gives more graphite than hammered steel. The influence of

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† *Comptes-Rendus*, lvi., 322.

‡ *Ibid.*, xlv., 857, and Feb. 2.

* *Comptes-Rendus*, lvi., 43.

heat in this respect is the reverse of that produced by hammering and tempering, and re-heated steel does not recover its primitive quality, or its chemical properties, as regards acids, until after it has been hammered or tempered.

White pig iron presents the same variation in the quantity of carbon that it contains in a free state, and the duration of the annealing. However long this may last, there is still some carbon remains uncombined.†

In the tempering of steel, the rapid cooling has a compressing influence upon the metal, almost instantaneous, and presenting the closest analogy with the blow of a hammer.

A bar of steel rapidly heated to the temperature requisite for tempering, and immediately plunged into cold water, underwent the following alterations of bulk:

	Before tempering.	Red hot.	Tempered.
Dimensions in centimetres .	20·00	20·32	19·95
	1·00	1·03	1·01
	1·00	1·03	1·01
Volume, ditto .	20·00	21·557	20·351

The effect produced by the tempering consists in a rapid approximation of the particles acting in every direction. It is this concussion which M. Caron regards as producing the combination of the iron and carbon. The increase of temperature has the effect of dilating the metal and giving to the molecules a mobility necessary to enable them to unite; the sudden cooling bringing them rapidly together, causes combination.

A bar of iron heated to bright redness was briskly hammered on an anvil covered with finely-powdered carbon; when it had cooled to a dull red heat, it was suddenly plunged into cold water. Some parts of the bar were then found to have been converted into steel at the surface, and resisted the file. The same iron heated to redness, and cooled in the midst of charcoal without being hammered, did not present any trace of being converted into steel when tempered in the same manner.

Hammering does not effect so complete a combination as tempering, because it causes the concussion of the particles only in one direction, while the sudden cooling in water causes their concussion in every direction simultaneously. Moreover, the temperature of the metal after hammering tends to destroy the combination that has been effected by it. On the contrary, the metal being quite cold after the concussion produced by plunging it into water, no further reaction is possible, except by heating the metal again.

Reaumur‡ and Rinman§ state that the volume of tempered steel is $\frac{1}{48}$ greater than that of soft steel. Karsten,‖ on the contrary, says that it is not quite certain that all steel expands by tempering, and becomes of less density. In M. Caron's experiments on this subject, he heated the steel in earthern tubes filled with hydrogen. A bar of hammered steel 1 centimetre square and 20 centimetres long, was tempered, and found to be reduced in length to the extent of half a millimetre; the other dimensions were increased 0·06 mm.; the density was rather less than before, 7·796, in place of 7·817. These differences being small, the experiment was repeated several times with the following results:—

	Before tempering.	After tempering 10 times.	20 times.	30 times.
Dimensions in centimetres .	20·00	19·50	18·64	17·97
	94	96	97	1·00
	93	96	97	1·00

Hence it appears that the reduction in the length of bar after thirty temperings was nearly one-tenth. The diminution in length was not due to oxidation, and the sharpness of its edges was retained almost entirely. The bar was then cleaned, and the density found to be 7·743; consequently the volume had increased.

Similar experiments with a great number of bars of steel gave like results, and M. Caron concludes, that in tempering, steel bars are reduced in length, while the width and thickness augment in the same proportion as the density is reduced.

The opinion expressed by Karsten, and the differences observed in steel of different manufacture, induced M. Caron to continue his examination to drawn and sheet steel, with the following results:—

	Dimensions before tempering.	Dimensions after tempering.
Round drawn bar steel . .	20·05	19·98
	1·16	1·16
Rolled steel (German plates) .	20·00	20·45
	1·51	1·51
	3·70	3·70

These results, and those already given, show that in tempering—

1. Hammered steel bars are reduced in length.
2. Round bars partly hammered and partly drawn are scarcely altered in length.
3. Plate-steel increases both in length and width.
4. In all cases the density diminishes,—a result which agrees with M. Regnault's observation, that soft steel is denser than tempered steel.

It appears, therefore, that a bar of steel may, in tempering, change its dimensions in a different manner, according to the mode of its manufacture. This accounts for the fact that slender articles, such as files, become twisted; this would naturally be the result if, in forging, one side were more hammered than the other. At the moment of immersion in water, the side that has been most hammered diminishes in length more than the other, so giving rise to the defect.

The effect of the sudden cooling of steel in tempering, may be compared in other respects to the effect produced by the blow of a hammer. Thus, the more sudden it is, the more considerable is the corresponding force exercised, and the greater will be the hardness. This may be inferred from the following table, representing the time of cooling of a bar of steel and the corresponding degrees of temper produced in the metal:—

	Water.	Water.	Water with 10 per cent. dextrine.	Alcohol.
Original temp. of the liquid	10°	50°	10°	10°
Final temp. of the liquid .	22°	61°	23°	30°·5
Time of cooling . .	4″7	11″3	13″2	21″7
Degree of temper . .	good.	slight.	very slight.	none.
Shortening of the bar .	$\frac{1}{28}$	$\frac{1}{147}$	$\frac{1}{172}$	insensible.

The result of a great many experiments with various liquids—mercury, saline or acidulated solutions, water covered with oil, or containing in solution mucilaginous or syrupy substances, oil, &c.—appeared to indicate that the hardness and other characters produced by tempering are inversely proportionate to the square of the time of cooling.

M. Deville¶ regards these results as accordant with the results of his observations on the remarkable physical and chemical properties communicated to substances by rapid cooling, or, in other words, the

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† *Comptes-Rendus*, lvi., 211.

‡ "L'Art de Convertir le Fer Forgé en Acier," p. 338.

§ Rinman, I., 220 to 228.

‖ Karsten, III., 380

¶ *Comptes-Rendus*, lvi., 325.

excessive proportion of latent heat, or heat of constitution, they retain when suddenly cooled.

Those observations showed that substances might, in this respect, be divided into two very distinct classes.

One class comprises sulphur, selenium, silicium—or rather its compounds, silica and silicates—and the experiments of Jacquelain, Lavoisier, and Silliman justify the addition of carbon to these. The other class includes lead, tin, bismuth, and probably metals generally. The substances of the former class are "superfusible," and capable of being tempered, and acquiring the vitreous or amorphous condition. The substances belonging to the second class present the same molecular condition after cooling either rapidly or slowly, their density remaining constant.

M. Deville considers that the results obtained by M. Caron may be explained by regarding iron and carbon as belonging respectively to these two different classes. Those results and Karsten's show that iron and carbon combine at a high temperature; that if the compound is allowed to cool slowly, the constituents crystallise separately, the mass acquires a maximum density, and acids effect its separation into iron and a graphitic substance. On the contrary, when the cooling is sudden, the carbon remains in the state of superfusion, and communicates the property to the compound containing it in the same manner that silica communicates this property to alkalies and metallic oxides.

When the proportion of carbon is larger than in steel, rapid cooling gives white pig iron, and slow cooling grey pig iron.

From this point of view, steel may be compared to glass, which becomes divitrified when it is heated, or to sulphur, which becomes octahedral sulphur when heated to near 100° C. It is, indeed, worth inquiry whether, in the tempering of steel, the effect produced is not absolutely similar to that observed in tempering sulphur; that is to say, the production of two distinct layers, one superficial and very thin, the other internal, and corresponding respectively to insoluble and soft sulphur.

In any case the sudden approximation of the molecules determined by tempering, and compared by M. Caron to the effect of hammering, maintains permanently a greater distance between the molecules than their gradual approximation by slow cooling.

As regards heat of constitution, there is in such a case a retention of a certain quantity of heat, which is, on the contrary, disengaged during slow cooling. This is the condition which M. Deville terms superfusion.

In the case of the different kinds of allotropic sulphur, it has been possible to some extent to measure the quantity of heat evolved during the transformation of soft sulphur and insoluble sulphur into octahedral sulphur. In the case of steel, the density and calorific capacity afford only indirect evidence of the existence of similar conditions.

M. Deville considers that not only carbon is capable of converting iron into white pig iron or steel, but that, in accordance with the views above expressed, any of the electro-negative elements placed in the same condition as carbon would have that effect. Nitrogen he considers to be especially suited for this purpose.

PHYSICAL SCIENCE.

On the Relation of Vital and Chemical Force to the Potential Energies of Matter, by James Croll.

The following relation between vital and chemical force, which does not appear to have been specially noticed by physicists, seems to follow as an obvious deduction from the principle of the conservation of force:—

The greater part of all the force available for mechanical purposes is derived from chemical combination. Chemical combination, again, depends upon the affinity of the atoms—their tendency to approach and unite. The greater this affinity the greater is the force, or, according to the dynamical view, the velocity with which they approach. The heat produced is also proportional to the affinity of the atoms; for the amount produced is proportional to the *vis viva* of the moving atoms at the moment of union. The force manifested in chemical combination we say existed in the atoms previously in a potential state.

Potential energy is transformed into actual energy by the mutual approach of the atoms under the impulse of their affinities. Potential energy can therefore only be restored to the atoms by pulling them asunder again. Chemical change consequently in every case reduces the total amount of potential energy possessed by matter, and increases its actual energy. It is true that decomposition in most cases occurs along with combination, but the potential energy gained from the decomposition never can equal the amount lost by the accompanying combination. By what means, then, does matter regain its potential energy? The agency of heat alone is not sufficient, for although heat expands bodies by separating their molecules, yet it does not, except in a few cases, separate the atoms chemically united. Neither can it be done by the electric current, for the potential energy gained by electrolysis does not compensate for what is lost in generating the current.

The atoms of matter seem to be indebted to the agency of vital or organic forces for the restoration of their potential energies. Chemical agency separates the atoms possessed of weaker affinity, in order that they may unite with others to which they have a stronger affinity. Vital agency, on the contrary, separates atoms which have a stronger affinity in order to unite them with others to which they have a weaker affinity. In the latter case we have a restoration of potential energy. But potential energy can only be restored at the expense of actual energy. Where, then, does the vital agent derive its supplies of actual energy? Evidently from the sun's rays.

Here we have two agencies constantly at work—the chemical and the vital. The former is continually converting the potential energy of matter into actual energy by the motion of the atoms towards each other. The latter, on the contrary, is continually restoring the potential energy lost by drawing the atoms asunder. The chemical agent produces actual energy by means of the consumption of potential energy; the vital agent restores the potential energy by consuming actual energy, viz., the sun's rays.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, May 7, 1863.

Professor A. W. Williamson, *Ph.D., F.R.S., President, in the Chair.*

The minutes of the previous meeting having been read and confirmed, Thomas Woods, Ph.D. was balloted for, and duly elected a Fellow of the Society; after which Messrs. Graham, Salt, and Stewart signed the statute-book, and were formally admitted Fellows of the Society.

"*A Note on Pickeringite,*" by Dr. HOWE, was read by the SECRETARY. This mineral occurs in the form of silky crystalline needles or fibrous tufts embedded in the slate rock of Iquiqui, in Peru. It was found on analysis to consist essentially of the sulphates of alumina and magnesia in combination with water, as will be seen by the following percentage numbers, the fractional proportions being disregarded :—

Alumina	11
Magnesia	4
Sulphuric acid . . .	36
Water	46

with traces of lime, copper, cobalt, and insoluble matter. In constructing a formula from these analytical results the author hesitated to ascribe twenty-four equivalents of water to the compound like common alum; and although it very much resembled in physical character the double sulphate of manganese and alumina described by Sir Robert Kane, under the anomalous formula,—

$$MnO, HO, SO_3 + Al_2O_3, 3SO_3 + 24 \text{ Aq.};$$

the salt under discussion was considered to have but twenty-two atoms of water, and to be represented by the formula,

$$Al_2O_3, 3SO_3 + MgO, SO_3 + 22 \text{ Aq.}$$

A former analysis by Stromeyer is quoted in Gmelin's "Handbook of Chemistry" as indicating twenty-five atoms of water, but the analytical numbers differ so widely from those required by calculation, that they really substantiate the author's later results.

The PRESIDENT referred to the circumstances of the crystalline form not being octohedral like common alum, and to the triatomic character of magnesia as reasons sufficient to account for variations in the proportion of water.

Dr. A. W. HOFMANN offered some explanatory remarks relative to the chemical reaction which he described and experimentally illustrated at the last meeting of the Society. It was stated on that occasion* that "benzidine when when submitted to the action of heat, splits up into azobenzol and two equivalents of aniline." On preparing, however, a larger quantity of material for purposes of further investigation it was discovered that the process employed did not yield the true benzidine but an isomeric body, to which he would give the name of hydrazobenzol. It was this substance which had been used in the experiment shown to the Society, and on attempting to distil the true benzidine in the same manner it did not furnish the products before observed, but sublimed without alteration. It differed likewise in regard to solubility, but yielded the same compounds under the influence of nitric acid, bichromate of potash, permanganate of potash, and other oxidising agents. Specimens of this isomeric body, $C_{12}H_{12}N_2$, were exhibited, and the author wished it to be understood that the reactions described at the last meeting are true of hydrazobenzol, not of benzidine.

Dr. LYON PLAYFAIR, C.B., F.R.S., then delivered a lecture "*On the Constitution of Salts.*" The author commenced by referring to the various systems of notation at present in use; particularly to those of Gerhardt, Kekulé, and the older modes of expression; the peculiarities of each were shown by the aid of a series of diagrams or comparative tables, which fully demonstrated the nature and application of the system advocated by the lecturer. As an illustration of the point of view from which the constitution of salts might be studied on the author's plan, the several oxides of manganese were written in series, thus :—

$$\begin{array}{ll} Mn_2O_4 & \text{Neutral;} \\ \left.\begin{array}{l} Mn\,O \\ Mn_2O_3 \end{array}\right\} & \text{Basylous;} \\ \left.\begin{array}{l} Mn\,O_3 \\ Mn_2O_7 \end{array}\right\} & \text{Strongly chlorous.} \end{array}$$

A neutral salt could be defined as a compound, whose basic atomicity is exactly gratified by a chlorous element or radical. By the following equations it would appear that the second atom of oxygen in the binoxide of manganese functioned chlorine, thus :—

$$\text{I.}\quad MnO,O + HO,NO_5 = MnO,NO_5 + HO + O.$$
$$\text{II.}\quad MnO,O + 2HCl = MnCl + 2HO + Cl.$$

The place of such oxygen might be taken indiscriminately by an acid (*i.e.* anhydride) or water, in any case a compound on the original type being formed, thus :—

Peroxide	$MO,O.$
Salt	$MO,A.$
Hydrate	$MO,HO.$

Many examples were adduced to show that acids displace oxygen, and, on the contrary, that oxygen displaces both water and anhydrides.

TABLE I.

Relations of Oxygen, Anhydrides, and Water in Salts.

I. $BaO, O + CO_2 = BaO, CO_2 + O.$ Acid displaces oxygen.

II. $BaO, HO + CO_2 = BaO, CO_2 + HO.$ Acid displaces water.

III. $BaO, O + HO = BaO, HO + O.$ Water displaces oxygen.

IV. $BaO, HO + O = BaO, O + HO.$ Oxygen displaces water.

V. $MnO,CO_2 + O = MnO, O + CO_2.$ Oxygen displaces anhydride or acid.

VI. $KO_2, O_2 + 2HO = KO, 2HO + O_3.$ Water displaces oxygen.

Reactions III. and IV. were suggested by Boussingault as a means of obtaining oxygen from the air. V. was described by Forchhammer, and forms the subject of Tennant's patent for recovering black oxide of manganese from chlorine residues. VI. is the reaction lately pointed out by Harcourt in a communication to the Society.

After the foregoing remarks, the lecturer referred to a diagram exhibiting a comparison between formulæ as written on the water type of Kekulé, and according to the peroxide type now advocated.

	Kekulé.	Peroxide.
Type	$\left.\begin{matrix} H_2 \\ H_2 \end{matrix}\right\} O_4$	$H_2 \left\{\begin{matrix} O_2 \\ O_2 \end{matrix}\right.$
Acid	$\left.\begin{matrix} H_2 \\ S_2O_4 \end{matrix}\right\} O_4$	$H_2 \left\{\begin{matrix} O_2 \\ S_2O_6 \end{matrix}\right.$
Salt	$\left.\begin{matrix} Zn_2 \\ S_2O_4 \end{matrix}\right\} O_4$	$Zn_2 \left\{\begin{matrix} O_2 \\ S_2O_6. \end{matrix}\right.$

Gerhardt's definition to the effect that "an acid is a salt whose basyle is hydrogen" is consistent with either view. In extending the application of this system to a vast number of compounds (a few only of which can be included in this report), the lecturer wrote the following substances, as under :—

Oxide of zinc.

$$Zn_2 \left\{\begin{matrix} O \\ O \end{matrix}\right.$$

Zinc methyl.

$$Zn_2 \left\{\begin{matrix} C_2H_3 \\ C_2H_3 \end{matrix}\right.$$

Dinitromethylate of zinc (Frankland).

$$\begin{matrix} Zn \\ C_2H_3 \end{matrix} \left\{\begin{matrix} NO_2 \\ NO_2 \end{matrix}\right.$$

Sulpho-methylate of zinc.

$$\begin{matrix} Zn \\ C_2H_3 \end{matrix} \left\{\begin{matrix} SO_4 \\ SO_4 \end{matrix}\right.$$

Dr. Playfair made no objection to the first two types of Gerhardt, viz., HH and HCl, but, instead of the ammonia and marsh-gas groups, he preferred to substitute the peroxide type already described, the formulæ being written according to circumstances.

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* *Vide* CHEMICAL NEWS, vol. vii., p. 211.

For teroxides—

$$R\,O_3$$
$$R\,O_2,O$$
$$R_2O_3,O_3.$$

Other formulæ may be represented thus:—

$$R\,O,\,O$$
$$R_2O_2,O_2$$
$$R_2O_3\;O.$$

Among the examples quoted in illustration of the lecturer's system were the undermentioned, and it should be noticed that the sesquioxides fall naturally into the teroxide group, in regard to which it was remarked that we do not here find neutrality—a feature which is more characteristic of the peroxides.

TABLE II.

Arsenious acid	AsO_2,O
Kakodylic anhydride . . .	AsO_2,C_4H_5
Diarsenic methide	AsO_2,C_2H_3
Basic chloride of antimony . .	SbO_2,Cl
Bloxam's salt	$AsO_3,3ZnO$
Arsenite of magnesia . . .	$AsO_3,2MgO$
Arsenite of ammonia . . .	AsO_3,NH_4O
Persulphate of iron . . .	$Fe_2O_3,3SO_2,O$
Iron mordant, "aged" . . .	$Fe_2O_3,2A$
Basic nitrate of iron . . .	Fe_2O_3,NO_4,O

It was possible for binoxides or bisulphides to play the part of primary radicals; thus phosphoryl, PO_2, might be said to exist both in phosphorus and phosphoric anhydrides.

$$\text{I.} \qquad P \left\{ \begin{matrix} O_2 \\ O \end{matrix} \right.$$

$$\text{II.} \qquad PO_2 \left\{ \begin{matrix} O_2 \\ O \end{matrix} \right.$$

The lecturer concluded by referring to a diagram in which was shown the constitution of salts on the peroxide type.

TABLE III.

Binoxide.	Protoxide.	Hydrate.	Salt.
NaO,O Binoxide of sodium.	NaO,NaO Soda.	NaO,HO Hydrate of soda.	NaO,ClO Hypochlorite of soda.
ClO,O Chloric oxide	ClO,ClO Hypochlorous anhydride.	ClO,HO Hypochlorous acid.	CaO,ClO Hypochlorite of lime.
Sn_2O_2,O_2 Binoxide of tin.	Sn_2O_2 Protoxide of tin.	$Sn_2O_2,(HO)_2$ Hydrated protoxide of tin.	$Sn_2O_2(S_2O_2)O_2$ Sulphite of tin.
$(S_2O_2),O_2O_2$ Sulphuric anhydride.	$(S_2O_2),O_2$ Sulphurous anhydride.	$(S_2O_2)O_2,(HO)_2$ Sulphurous acid.	$(S_2O_2)O_2,(NaO)_2$ Sulphite of soda.

Dr. A. W. **Hofmann**, among such a profusion of subjects as those alluded to in the lecture, would limit himself to asking in what manner Dr. Playfair proposes to adapt his formulæ to the requirements of the higher equivalent for oxygen = 16, which, in his opinion, was on account of numerous theoretical considerations rapidly gaining ground?

Dr. **Playfair** replied that it would not be difficult to double the formulæ of those compounds, which, like barium and magnesium, were now sometimes taken at half their former atomic weights, but he did not consider that this should be done with respect to potassium, sodium, silver, and gold.

Dr. **Hofmann** suggested that the recommendation of the lecturer would amount simply to "multiplying chemistry by two."

Dr. W. A. **Miller** made an objection to the mode of writing the formula of acetic anhydride, since it appeared in the diagram as only half a molecule.

Dr. **Frankland** did not agree in the fundamental principle, viz., that of taking compounds of simple radicals with oxygen, sulphur, or chlorine as the basis of classification; he would much prefer to arrange the elements in symmetrical groupings around that one which possesses the greatest degree of basicity.

Dr. **Odling** was of opinion that the system under discussion relied for its foundation upon a few exceptional reactions, and was altogether inadequate to explain the more numerous class of ordinary chemical changes. There were instances quoted in which one volume of a body appeared to displace two volumes of another, and *vice versâ;* many of the illustrations brought forward were susceptible of simpler expression; and the radicals did not occupy an uniform position, being placed sometimes on one side, and sometimes on the other side of the bracket.

Dr. **Playfair** remarked that the discussion was tending towards a division of the subject which was somewhat abstruse and doubtful; he should like to see attacked the main argument as shown in Diagram I.

Mr. **Smee** was prepared to rest his faith upon the electrolytic solution of the difficulty. There was not, in his opinion, any foundation for the statement that the equivalent of oxygen was 16, whilst hydrogen remained the standard of unity; for when water was decomposed by the galvanic battery, eight parts by weight of oxygen were liberated to one of hydrogen. The speaker quoted from Dr. Faraday's published researches in confirmation of his own views upon the subject, and stated his belief that the theoretical opinions recently advanced were evidence not of progress, but of a retrograde movement, which would end in becoming quite unintelligible to students.

Dr. W. A. **Miller** and Dr. **Odling** rose together for the purpose of explaining that it was necessary to distinguish between the terms atom and equivalent.

The **President** proposed a vote of thanks to Dr. Playfair for a lecture which was particularly acceptable, as introducing new methods of viewing old facts, and the interest of the subject had been proved by the fertile discussion which had arisen therefrom. He now adjourned the meeting until the 21st instant, when a lecture "*On Certain Effects of Intense Heat on Fluids,*" by W. R. Grove, Esq., Q.C., F.R.S., will be delivered.

ROYAL INSTITUTION OF GREAT BRITAIN.

Friday, May 8.

The lecture was by Professor **Voelcker**, "*On the Soils of Great Britain.*"

The chemical analysis of fertile soils, the lecturer said, always revealed the presence of certain constituents, lime, magnesia, silica, and phosphoric acid, for example, which were also found in the ash of plants, and were essential to the existence of the plant. In one sense these constituents were all of equal importance, but in another they were not equally valuable. They were all necessary, but their value in one sense depended on their greater or less abundance. Phosphoric acid, for example, was very minutely distributed, while lime, magnesia, and silica were very abundant. The merit of recognising the importance of the mineral constituents of plants as nutriment for them in the soil belonged to Liebig, who gave the deathblow to the old, or humus theory of vegetable nutrition. The necessity of these mineral constituents being demonstrated, the natural inference followed, that fertility depended upon the abundance of these. It was true within certain limits that the abundance of potash and phosphoric acid did decide the relative fertility of a soil; but there were instances in which all the essential mineral constituents of a plant were present in the soil, and yet the soil was unproductive. The physical conditions of surface and sub-soil greatly affected fertility, and the mineral ingredients must be present in the surface soil in an available form. Some of the purely physical characters of a soil had not been sufficiently well brought forward. The capillary power of a soil, for example, was of the utmost importance. An apparently exhausted soil might sometimes be restored by giving it increased capillarity. With regard to exhausted soils, it was a question whether they had even been fertile, as it was impossible to exhaust more than a small portion. The permanent fertility of a soil, the lecturer said, could never be economically altered. The productive power might be changed to a certain extent by heavy doses of manure, but the permanent fertility could not be increased; as soon as the manuring was left off, the soil sank back into its natural barren condition. The fertility of good land which had been badly treated might be restored by proper treatment, for nothing could permanently destroy the power of a soil. There were soils, however, so poor that no skill or treatment could permanently increase their fertility; still, there was no soil so poor, not even sand, that could not by a proper system be made to yield astonishingly large crops. The lecturer instanced Flemish husbandry, which produced so much from very poor soil. After a caution against placing too implicit a reliance on the purely chemical properties, the lecturer proceeded to notice Professor Way's experiments on what he termed the "chemico-physical" properties of soils, namely, their power of decomposing salts, and absorbing or fixing some bases. Professor Way had discovered that soils possessed the power of decomposing potash salts, retaining the base, and allowing the acid to pass away in combination with lime or soda. Ammonia was freed from its combinations, and retained in the same way. Phosphoric acid was also separated from soda, and laid hold of by the soil, while the soda, which was of no importance, passed away. The general conclusion arrived at from these experiments was, that soils possessed the power (in different degrees) of selecting and retaining matters which were essential to the growth of plants. Professor Voelcker had extended these investigations, and detailed some of the results obtained by passing liquid manures through soils of known composition. He had found that a calcareous clay absorbed a good deal of ammonia and potash, and a little silica, losing a good deal of lime. A sandy soil absorbed but little ammonia and potash, but it took some lime, and allowed some silica to pass away. Professor Way ascribed his results to purely chemical causes, and supposed they were obtained in consequence of the decomposition of certain double silicates which existed in the soil; but there was no proof, the lecturer said, that these double silicates were really present in soils. The lecturer believed the cause was partly chemical. Lime, which was present in all soils, would fix phosphoric acid, and hydrated sesquioxide of iron would fix both potash and ammonia, and in general all compounds having the formula R_2O_3 would fix alkaline substances. The lecturer then referred to the harm resulting from having the mineral constituents of plants in too soluble a condition, and explained how this was controlled by the rain-fall. He then referred to Liebig's latest work, and said that the statements made therein were true only in special cases, and had no bearing on general practice. In attempting to give too comprehensive a view, Liebig had missed the truth. His statements, however, did not deserve ridicule. The office of the soil was not merely to support the plant, but to manufacture the food into a condition for assimilation; it was the workshop in which the food of plants was prepared. The chemistry of agriculture was a subject full of practical interest, but also full of difficulties, which, however, might be overcome by extended observation and study. Professor Voelcker then referred to the threatened exhaustion of the soils of England, and said that that was a matter about which we need be in no great anxiety at present. Although phosphoric acid was very minutely distributed in our soil, yet there were large deposits of it in various parts of the globe, which English enterprise was converting to use. English soil had wonderfully improved within the last fifty years, and was improving still, notwithstanding that our sanitary laws compelled us to waste much of that which ought to be returned to the soil. In 1775, Norfolk produced fifteen bushels of corn to the acre; it now produces from thirty-two to thirty-six bushels. In some other counties there was an equal improvement,—the result of proper drainage, proper manuring, and the selection of proper plants. In everything concerned with agriculture, from the plough-boy upwards, there had been a most marked progress in the last fifteen years. Superior mechanical skill and knowledge had been called into action. Still, however, the primeval curse remained, that man must earn his bread by the sweat of his brow. But if man be only true to himself, the promise also remained that "seed-time and harvest should never fail."

SOCIETY OF ARTS.

April 29.

Sir Thomas Phillips *in the Chair.*

A paper, by Professor Ansted, was read on Wednesday last, "*On the Varieties of Combustible Minerals Used Economically, considered in Relation to their Geological Position and Relative Value for Certain Purposes.*" After describing the general characters of carbonaceous combustible minerals, including those rare kinds used only for ornamental purposes, and arranging those that are met with in sufficiently large quantity to be employed for industrial purposes, as fuel and otherwise as—1. Coal; 2. Lignite; 3. Bituminous shale; 4. Asphalte, petroleum, &c., he went on to state the geological conditions in which coal is met with, and the differences of the several varieties, pointing out that the true practical distinction between lignite and coal does not, as many geologists have considered, bear any constant relation to the geological position of the mineral. Bituminous shales were described as minerals of a peculiar class, containing so large a percentage of earthy substance that they could not be advantageously used as fuel. The richest of the numerous varieties of this class of minerals are found in Scotland associated with coal. Under the names of Boghead coal, Torbanehill mineral, &c., they have come into use for gas-making and for the manufacture of oil by distillation at a low temperature, in the manner practised some thirty years ago by Selligue, in France, and since introduced into Scotland by Mr. James Young. These minerals were described as being intermediate between coal and shale. Some of them burn like coal, others are unlike it in every respect; but it is impossible to fix any line of distinction between them and coal. All agree, however, in refusing to coke, and in retaining their slaty appearance after burning. The native bitumens were described as being in some instances analogous to coal, in yielding, by distillation, oils; but they were regarded as having an origin different from coal, and petroleum was mentioned as being related to these minerals. Next to the common coals used for fuel and gas-making, the shales—from which light illuminating oils, heavier oils, lubricating oils, and paraffin, can all be obtained—are among the most interesting; they are also the least known. Professor Ansted considered it to be certain that the slaty and stratified minerals, capable of distillation for these very useful purposes, are not strictly bituminous. The term bituminous shale is familiar, but not correct, since no bitumen, properly so called, is contained in these shales. They agree, however, in yielding valuable hydrocarbons by a process of distillation now familiar enough; and in this they agree with some of the bitumens properly so called. It has been proposed to call them pyroschists, but this name is yet more objectionable. In reference to the subject of shales valuable for distillation, he pointed especially to the considerable number of substances of this kind that exist, most of which are still untried. The partings of coal seams, the tough brown and black shales of the old red sandstone and Silurian periods, and the so-called bituminous schists of other series, many of them not highly-coloured, may prove extremely valuable. Some of them would probably fail, but the means of ascertaining their value are neither troublesome, tedious, nor costly. Hitherto it is chiefly the black schists, about whose value there can be no doubt, rather than those that apparently occupy doubtful ground between coal and shales, that have been used for slow distillation on the Continent; while only the rich and remarkable Boghead and Torbanehill minerals have been extensively employed in the British islands. It is not everywhere that such rich or profitable minerals can be found, but valuable results might perhaps be obtained from minerals that have not yet been much thought of. In this case, as in the case of iron ores, there are many very unpromising materials that may turn out when examined to be the most valuable of all. There is no doubt that the coal measures have hitherto yielded the most numerous and the richest bituminous shales, as well as the largest and most valuable supplies of coal. It is equally certain that in England the deposits of this period are most likely to retain their reputation in all these respects. Anthracite, steam coal, bituminous coal, cannel coal, Boghead and Torbanehill minerals, are all of the same age. But, although this is the case in England, and, to a great extent, in Belgium, Prussia, France, Spain, and Bohemia, and in the chief American coal fields, still there are marked differences in the position of the different minerals, and there are wide and important deposits of coal, some older, and others much newer. The Indian coal fields are probably very new. The Virginian coal fields have long been known to belong to the secondary period. There are excellent lias coals worked in various places on and near the Danube, in Hungary—there are cretaceous coals in Hungary, and excellent tertiary coal as well as lignite in the same country. It is also remarkable that, although in England and Scotland the valuable and rich bituminous shales are close to the coal, in France and elsewhere they are often altogether removed. The great deposits of bitumen in North America are below the bottom of the whole series. Rich beds of shale, yielding 50 per cent. of all kinds of oil, near Autun, in France, are far above the highest bed of coal —the lias bituminous shales are quite unconnected with any workable coal seams, and so are those of tertiary age on the Rhine. There is, then, no geological limit of the age for these minerals; neither is there any limit of colour and appearance. Many of the blackest shales are worthless, and some that are of a very pale brown are very rich—experience and analysis are required to form an opinion in any particular case. The relative value of the combustible minerals, of all of which carbon forms a large proportionate part, is not, on the whole, difficult to determine. Pure carbon does not afford the best fuel. Pure, or nearly pure, carbon, such as is seen in some samples of anthracite, is, indeed, a very difficult and unmanageable combustible, and can hardly be regarded as an economical fuel at all. A very strong draught, or mixed gases supplied rapidly, are necessary to enable it to continue burning when once heated. A coal containing a moderate percentage of oxygen and hydrogen gases, and a reasonably small percentage of ash, is the best for all purposes as a combustible. Some kinds, especially of the Newcastle coals, cake, or run into a kind of slag while burning; other kinds, such as Yorkshire and Lancashire coals, burn through without caking, and fall to a white and red loose powdery ash. None of them, however, retain their exact form when burnt, either in an open fire or in a retort. Most of the cannel coals, and those varieties called Boghead coal, and Torbanehill minerals, are very valuable for distillation of all kinds, but they retain their form, and behave like slates in the retort. Some of them, however, are capable of being used alone as fuel. They are all especially rich in gases, paraffin oils, and lubricating oils, but their peculiar value in this respect is accompanied by a corresponding diminution of their value as true coals. The lias coals of Europe and America, the oolitic and cretaceous coals, and such tertiary true coals as are known, all resemble rather the Yorkshire and Lancashire varieties than the caking coal of Newcastle. Most of the French coals and many of the Belgian varieties are of the same kind. It is difficult to draw an exact line that shall separate the true coals from the richer, blacker, and more coal-like shales used for obtaining gas or paraffin oils, but it seems that the slaty character recognised in burning affords a good practical definition. It is a point of considerable importance that some of the lignites may also prove available for distillation, but of this there is at present hardly sufficient evidence. The assistance of the chemist is required to determine how far the varieties that are most abundant are available for this purpose, and whether some modification of the process or the addition of some ingredient may render profitable operations at present not worth carrying on. The demand for light oils, for illuminating purposes and for paraffin, seems only limited by the price, and the manufacture is extending in most countries of Europe. It is certain that there is no geological period and no rock that may not contain useful minerals of this kind. Mineral oils and true bitumens are less common than bituminous shales, but they also are more widely dispersed than is generally thought, and when found in large quantity they possess great value. Some are obtained from considerable depth, and are very tenacious; others, as naphthas, are thin, and float on water; some occupy definite geological positions in certain rocks, while a few, comparatively rare, offer peculiar mineral combinations, and are found described in mineralogical works by special names.

Mr. Sopwith expressed a high opinion of the importance of the subject of Professor Ansted's paper, and suggested that collections of such minerals as were referred to, should be made in various localities, and that experiments should be conducted with a view to ascertain their value. He considered the subject worthy the notice of Government.

Dr. Milligan described the lignite and dysodile seams of Tasmania as having been distilled experimentally.

(To be continued.)

NOTICES OF BOOKS.

The Collected Works of Lavoisier.

The second volume, containing sixty-one memoirs written between the years 1770 and 1792, has recently been published, and M. Dumas, in announcing its completion to the Académie des Sciences, remarked that these memoirs constitute an essential and characteristic portion of the labours of Lavoisier, and that, after careful examination of the subject, he had decided upon arranging them in chronological order, leaving each one unaltered in its title and form, and avoiding the addition of any notes.

Lavoisier had himself purposed publishing an abstract of his memoirs—indeed, a portion of it was printed, and he intended arranging them in systematic order. Without speaking of the difficulty which would now attend the execution of such a plan, or even the impossibility there might be of according, as he might have done, and as he alone had the right to do, a place to certain contemporaneous works with his own memoirs according to the opinion he held of their importance, M. Dumas was also induced to adopt the chronological arrangement by a motive that appeared to him decisive.

In Lavoisier's time, the new chemistry, scarcely born, was ill comprehended, and still imperfectly accepted; it would have met with numerous opponents. It would have been of great interest to present its various details in a connected and logical form. It would then have been convenient to arrange together the memoirs relating to particular subjects; and that the discrepancies of nomenclature current at the time of their publication should have given place to the use of a uniform nomenclature. No doubt Lavoisier, for the sake of his own repute, would have desired to publish his memoirs thus systematically arranged; but, with the more pressing desire of propagating his ideas at the same time, he would have understood how to combine his own labours and those of his friends in the order most suitable for initiating his readers in a knowledge of chemical truths, for enforcing conviction by demonstrative evidence, and rendering them familiar with the use of the new nomenclature. At the present time there is no occasion for such an exposition, since it is abundantly afforded by the elementary works that are in everyone's hands.

But, on the contrary, it seems that when the discoveries made by Lavoisier have become common property, and when the language of chemistry has taken possession of all the literature relating to the subject, it then only remains necessary, not only in justice to Lavoisier, but also in the interest of science, that everyone should assist continually and in some way to the elaboration and evolution of the ideas which have so radically reformed natural philosophy, and inaugurated with so much brilliance the chemistry of the present.

Nothing is more striking than to observe what may be accomplished in this way by a man of talent in the space of some twenty years for the good of humanity, and for his own fame, when unobstructed by any of the difficulties of life, in carrying out his ideas—an advantage which Lavoisier enjoyed, and which he cruelly expiated.

In 1770, the nature of water was unknown, as well as that of air, or the causes of the calcination of metals of combustion, nor was it known how acids acted upon the substances they dissolved.

Soon after the balance had been made the basis of investigations into chemical reactions, it was found that substances in burning are augmented in weight, and that the augmentation is due to fixation of air. Air was then analysed; water decomposed and reproduced; the true composition of the acids of carbon, sulphur, and phosphorus made known; the solution of metals explained; and the nature of salts defined.

The phenomena of combustion became the subject of a series of investigations which served, in the most striking manner, to elucidate their nature; respiration was found to be intimately connected with them; and, at the date of the last memoir in this volume, not only was mineral chemistry shown to be subject to fixed laws, but the nature of organic substances had been revealed, the cause of animal heat recognised, the phenomena of fermentation comprehended, a new future was opened up to physiology and medicine, and chemistry itself took its place among the best guides of agriculture.

In 1770, Lavoisier, while still a young man, examined a question which realises to us the gross barbarism of science at that time. Is water converted by long-continued boiling into earth or not? Twenty years later—years which appear as centuries when measured by the continuous and connected series of discoveries they produced—Lavoisier, after having marked out the path for the advancement of physical sciences, medicine, and the arts, was led to consider agriculture, as being a stupendous chemical operation that must be studied by the aid of the balance, and to write of it in terms that will now be fully appreciated.

"It is not only in the study that political economy is to be cultivated, it is by the reflective consideration of an extensive working of land, by calculations followed up through a long number of years in regard to the distribution of reproductive wealth, that there is a possibility of forming just ideas as to what contributes to national prosperity."

"The work on agriculture, on which I am engaged, has already cost me nine years of attention and labour, but it has made me acquainted with great truths, which even the best informed persons have perceived only in a vague manner. It has led me to entertain the hope of being able to contribute to the national prosperity, by inducing landed proprietors, capitalists, and people of moderate means to invest their superfluous income in the cultivation of land. Such an investment, it is true, does not offer the brilliant prospects of stock-jobbing, but it is not accompanied by the same risk or the same reverses; the success attained by its means is not a cause of grief to any one, it is, on the contrary, accompanied by the blessings of the poor. A wealthy proprietor cannot increase the value of his property or improve it without diffusing round him comfort and happiness; his trouble is repaid by a rich and abundant vegetation and a numerous population, the images of prosperity."

In 1770, it was scarcely known what a gas was. Twenty years later the intimate connexion between gases and vapours was recognised. It was shown that liquids may become permanent gases at a sufficiently high temperature. It was inferred that gases might be liquefied by cooling to a sufficient degree, and those were good grounds for saying, "These terms, air, vapour, aeriform fluids, express only a mode of material existence. If the earth were suddenly placed in a very cold region, the atmosphere, or a part of the substances that it consists of, would cease to exist in the state of an invisible fluid, and that change would produce new liquids, of which we have no idea." This is a remarkable prediction of the interesting discoveries of Faraday, Bussy, and Thilorier, relating to the liquefaction of gases.

In 1770, there was not the least idea of the intimate relation between the phenomena of heat and the manifestation of chemical action. Twenty years later heat was defined in terms which all treatises on physics might have reproduced; the expansion of solids was measured by means of instruments, the introduction of which opened a new era of precise physical research. It was shown that the heat evolved or absorbed by substances in changing their condition might be estimated; the numerical values of the specific heat of substances most frequently met with were ascertained; the heat evolved in certain chemical reactions was measured; the heat generated by living animals was compared with that which would be evolved by their respiration, considered as a phenomenon of combustion.

Since that time new co-efficients have replaced those which Laplace and Lavoisier contributed to science; their ice calorimeter has been succeeded by apparatus more manageable, but up to the time when M. Regnault resumed the study of heat, the plan they conceived, the systematic arrangement of their experiments, the means by which they submitted their numerical determinations to correction, even the formulæ they employed, and the devices they adopted for introducing corrections necessary for the precision of the final results, opened to physics a career from which it scarcely deviated for sixty years.

The equations, by means of which the chemical reactions of substances are now represented with such accuracy, were first employed in 1782, not only with the view of expressing material changes, but also with the object of introducing the relative values of the phenomena of heat accompanying the reciprocal action of substances.

The specific heat of substances being known, it was shown that there was an equality between it and the heat retained in the products of chemical action and that evolved at the moment it takes place.

Chemical action itself was constantly represented as having the most intimate relation to heat, chemical combination being considered as always accompanied by a disengagement of heat, and substances as ceasing to unite when their contact ceased to give rise to the generation of sensible heat.

M. Dumas adds:—It was after having reflected long on the generality of Lavoisier's views, that he determined, sixteen years ago, to abandon thenceforth in his public lectures the electro-chemical theories, which have never yet afforded a faithful representation of the phenomena observed in regard to the constitution of substances, and to return to the primary and simple views by means of which Lavoisier constantly associated chemical action and the disengagement of heat by which it is necessarily accompanied,—the equation of ponderable substances, and the equation of force or of heat.

It is scarcely necessary to add that the ideas enunciated by Lavoisier did not comprise the views recently developed by Mayer and Tyndall, who refer the heat generated by chemical action to the concussion of the molecules of the substances among each other, and the heat of the sun to the collision of asteroids falling upon it with great velocity.

Ninety years ago the phlogistic theory was the basis of chemical doctrines; the Aristotelian elements were scarcely banished from the schools; the most ordinary and the most important physical phenomena were unexplained, and shrouded in profound obscurity; the chemist was guided only by a collection of empirical recipes; the industrial arts, agriculture, physiology, received no elucidation from him, and were not subject to his influence. Now, though scarce two generations of chemists have succeeded Lavoisier, it is almost impossible to estimate the innumerable advantages reaped by the civilized community under the guiding influence of the light thrown by Lavoisier on the true elementary nature of substances, the constitution of compounds, and on the whole range of physical science, on physiology, agriculture, the arts, and hygienics.

It has been the good fortune of others to lay open a knowledge of the heavens, to trace back the revolutions of our planet to the most remote periods, to extend their investigations through both space and time, but it was reserved to Lavoisier to confer upon humanity an instrument of investigation, of analysis, and of application capable, like his new doctrine, of affording an indestructible basis for natural philosophy, of defining the principles of all things, and of furnishing science and industry with creative powers the limits of which are yet unknown.

A few weeks before his death, Lavoisier, referring to his part in the establishment of the new theory of chemistry, wrote as follows:—"It was in 1772 that I formed the opinions with regard to combustion which I have since published. In 1777, I had carried it out almost to the condition in which it is at present. That theory, however, was not recognised by Fourcroy until the winter of 1786—87; it was not adopted by Guyton de Morveau until a later date; in 1785, Berthollet still wrote in accordance with the phlogistic doctrine. That theory, therefore, is not, as is commonly stated, the theory of the French chemists; it is my theory, and I claim my title to it from my contemporaries and from posterity."

At the same time, he referred to the contents of a sealed packet deposited with the Secretary of the Académie on November 1, 1772, and opened at the meeting on May 5 following. Its contents were as follows:—

"About eight days ago I discovered that sulphur in burning, instead of losing weight, on the contrary acquires weight; that is to say, from a pound of sulphur it is possible to obtain more than a pound of sulphuric acid, allowance being made for the moisture of the air. The same is the case also with phosphorus. This augmentation of weight arises from a prodigious quantity of air that is fixed during the combustion, and which combines with the vapours.

"This discovery, which I have confirmed by experiments that I consider to be decisive, led me to think that the result which is observed in the combustion of sulphur and phosphorus might probably take place in regard to all substances which acquire weight by combustion and calcination, and I was persuaded that the augmentation of weight in metallic calces is due to the same cause.

"Experience has completely confirmed my conjectures. In reducing litharge in closed vessels by means of Hale's apparatus, I observed that at the moment when the calx became converted into metal, a considerable quantity of air was disengaged, and that the volume of this air was a thousand times greater than that of the litharge. This discovery appearing to me one of the most interesting of those that have been made since Stahl, I considered it necessary to establish my claim to having made it by placing this deposition in the hands of the Secretary of the Académie to remain private until the time when I publish my experiments. (Signed) "Lavoisier.

"Paris, November 1, 1772."

M. Dumas considers this important document not to be open to any objection; but, in examining the registers of the Académie des Sciences, has been surprised to find that on November 2, 1772, the day mentioned as the date of the deposition, there was no meeting of the Académie, and that at the public meeting on the 14th and on the 18th of the same month, no mention was made of the document sent in by Lavoisier. However, on May 5, 1773, there is, in accordance with the statement of Lavoisier, the following memorandum:—

"The Secretary opened in the presence of the Académie the deposit No. 152, made by Lavoisier on November 2, 1772, and endorsed the document in order to certify its date. (This deposit was not mentioned at the time, the meetings not having taken place at that period)."

M. Dumas has also succeeded in discovering the original document deposited by Lavoisier, and endorsed by the Secretary of the Académie at the time of opening the sealed packet containing it. It is entirely in his own handwriting, and bears the following note, which is also wholly in the handwriting of the Secretary, M. Defouchy:—

"This document was placed in my hands, sealed, by M. Lavoisier, on November 1, 1772, for the purpose of being deposited in the Archives, which was done, and it was opened in the presence of the Académie, at the request of the author, who demanded this notification in order to certify its date. (Signed) "Defouchy."

M. Dumas considers that these verifications, though unnecessary in the presence of Lavoisier's own statements, should not be neglected, on account of the silence which he observed in reference to this important subject for twenty years, and especially since, by an extraordinary circumstance, the deposition of the document was not mentioned at the time in the register of the Académie, and since no copy of it was taken in the register at the time of its being opened.

This document was not reproduced quite exactly in the printed collection, in which, according to the wish of Lavoisier, it was published a few years after his death by his widow and friends. They considered themselves authorised in suppressing a phrase, which, in all probability, Lavoisier himself would not have allowed to remain. The original document is as follows:—

"This discovery appears to me one of the most interesting that have been made since Stahl, *and as it is difficult to avoid communicating to one's friends in conversation something that might make them acquainted with it, I have thought it necessary to deposit this statement* in the hands of the Secretary *of the Académie, in anticipation of the publication of my experiments.*"

In reading the memoirs of Lavoisier, it seems almost as if they were written only yesterday, such is the freshness of the ideas. The reasoning by its soundness, the opinions by their natural conformity to, or their easy connection with the knowledge of the present time, inspires the mind of the reader with confidence and satisfaction. The thoughts, by their demonstration, arrange themselves without difficulty among the more modern ideas we now possess, and even add clearness to their arrangement and force to their application and significance.

After the lapse of a century the doctrines of Lavoisier have not lost any of their original lustre. A perusal of his memoirs will therefore have the effect of illustrating to young chemists that if the gifts of imagination and persevering labour can always be rendered of service in studies of research or precise observation, still, the habit of strict reasoning, and a thorough knowlege of physical laws, will alone afford a protection from error and delusion, in the attempt to construct a theory of chemistry, to develop its consequences, to carry out its legitimate applications, and, above all, to devise a rigorously correct form of enunciating it.

Practical Advice to Amateur Photographers on the Direct Negative Process versus Strengthening Positives. By H. Matheson. J. How, Foster-lane.

This little manual of the positive and negative collodion process contains valuable practical information conveyed in plain and concise language. The author is principal operator at the Crystal Palace, Sydenham, and the numerous examples of excellent photographs that have come from his hands are a sufficient guarantee that he is well acquainted with the subject of which he treats. He not only gives directions for the successful practice of photography in our own country, but adds some valuable information as to the precautions to be taken against the numberless obstacles to success met with by the amateur photographer in tropical climates.

We are glad to see that Mr. Matheson favours the direct negative process rather than that for strengthening positives into negatives. There is but little doubt that the former method of taking pictures will produce the best results, the proof of what we allege being that most professionals who have tried the iron process have gradually returned to the old method of developing with pyrogallic acid.

Mr. Matheson recommends dilute sulphuric acid for cleansing the glass plates; we fear that although in the hands of careful professionals this method would answer well, still sulphuric acid would be rather a difficult detergent for a careless amateur to prevent finding its way into the nitrate bath. As a specimen of practical information given in a practical manner, we heartily recommend Mr. Matheson's pamphlet to our photographic readers. The little work concludes with a few hints on the construction of glass-houses, that may be perused with great profit by both amateurs and professionals. We should think, however, that the glass-house figured on page 31 would be perfect in fine summer weather, but that in winter there would be too little light to produce good results.

NOTICES OF PATENTS.

42. *Manufacture of Starch.* W. T. Kite, Wallingford, Berkshire. Dated January 6, 1862. (Not proceeded with.)

For this purpose the inventor prepares a dough from flour by kneading it with a small proportion of water. This dough is then submitted to the action of a running stream of water in an apparatus provided with a series of sharp points or spikes, which, being fitted into a frame, are caused to move up and down, and in this way, by continually pricking the mass, to facilitate the access of water to every part. The flour being thus resolved into its components, starch and gluten, the latter remains chiefly upon the perforated shelf of the apparatus, the rest being separated by the sieves through which the starch and water make their escape, or finally skimmed from the surface of the starch after subsidence. The addition of a small quantity of acetic acid to the water employed for washing the starch is recommended, this being used by preference in the final stages of purification.

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50. *Soap.* L. Wunder, Liegnitz, Prussia. Dated January 8, 1862. (Not proceeded with.)

The fatty matters employed in the manufacture of soap, according to this invention, are tallow and cocoa-nut oil in equal parts. These are first refined by boiling with a solution of alum and common salt, any dirty impurities being scraped from the lower part of the block when cold. The fats are next saponified by treatment with a mixture of alkalies, 90 lbs. of soda and 100 lbs. of potash being used for every 200 lbs. of the compound fatty matters; and to the soap are added small quantities of catechu, tincture of arnica, and, by way of perfume, to every hundredweight two ounces each of oils of cumin, cinnamon, and lavender.

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55. *Rendering Certain Substances less Pervious to Air and Liquids.* John Stenhouse, Barnsbury Road, Middlesex. Dated January 8, 1862.

150. *Protection of Metallic Surfaces, &c.* J. Stenhouse, Barnsbury Road, London. Dated January 21, 1862.

For the applications severally described in the headings, Dr. Stenhouse employs paraffin, either in a melted state alone, or dissolved in a suitable solvent, which may be one of the following:—coal-tar naphtha, petroleum, or bisulphide of carbon. The mode of using this substance varies with the object in view. When applied to cloth or other textile fabrics, for the purpose of rendering them waterproof, these materials are stretched upon a frame, and laid face downwards upon a clean, flat, metallic surface, an iron plate for instance, which is heated by steam or direct fire to a temperature between 130 and 250 degrees Fahrenheit. As soon as the cloth has become sufficiently warmed through, a large block of solid paraffin is rubbed over the wrong side of the cloth, and this surface coated as evenly as possible. In order then to distribute the paraffin among the fibres, or force it into the substance of the cloth, a hot flat-iron is passed over it, or lengths of the material pressed between hot rollers, and when the impregnation is completely effected the cloth is removed and allowed to cool.

A similar mode of proceeding may be carried out in the treatment of leather, fur, felt, silk, calico; and trifling modifications in the mechanical arrangements will permit of its adaptation to rope and hempen goods, thread, and artificial flowers.

In protecting metallic surfaces, the patentee employs paraffin, melted or dissolved as before, for coating all kinds of gilded or tinselled articles, especially those covered with Dutch gold, silver leaf, bronze powders, and tin or copper foil.

By the process of Dr. Stenhouse it is easy to confer upon cloth any required degree or character of waterproofing; the use of a small proportion of paraffin appears to have great power in enabling the fabric to repel moisture, whilst a more thorough treatment effectually closes the interstices of the cloth against rain or water.

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147. *Preparation of Colours suitable for Dyeing and Printing.* E. C. Nicholson, Locksfields, Walworth. Dated January 20, 1862.

The patentee converts the red aniline dye into a deep violet or purple colouring matter by heating to a temperature between 390 and 420 degrees Fahrenheit. The substance is quickly transformed, with evolution of ammonia, into a dark-coloured tenacious mass, which is dissolved in acetic acid and diluted with alcohol for the preparation of a dye of convenient strength.

The French mode of proceeding appears to differ from the foregoing by requiring the addition of aniline or toluidine to the magenta compound.

CORRESPONDENCE.

A Simple Self-Adjusting Barometer.

To the Editor of the Chemical News.

Sir,—The less complex a barometer is, generally speaking, the greater may be the reliance on its indications; and, if self-adjusting, and not liable to error of any kind (save that from heat), nor subject to derangement (and if accidentally deranged easily repaired), and if perfectly portable so much the better. While thinking over these and other the essentials of a perfect instrument, the following adaptation of the siphon barometer occurred to me, which, if the idea be of any value, your readers are welcome to:—

The requisite tube is the ordinary siphon, with the shorter limb pressed as near to the longer one as the process of filling with mercury will allow. On the surface of the mercury in the shorter limb is to be placed a float made of some light substance, to which a silk string, which passes over a pulley placed a few inches higher than the top of the longer limb is to be attached. The height of this pulley, as well as the length of the shorter limb, will depend on the intended use of the instrument. A light graduated scale is to be attached to the string opposite the level of the mercury in the longer column, which, together with the float, should be nicely balanced by a weight on the opposite side of the pulley. By this simple contrivance the scale will rise and fall along with the mercury beneath the float, and thus will indicate double its own (which will be the true) rise and fall of the mercurial column.

By placing a cistern on the shorter limb, the play of the scale would of course be greatly diminished, but it would render the instrument more unwieldy, and doubly or more expensive.

Neither of the above instruments would be very portable without a stop-cock; but the addition of one to the former would be an improvement, inasmuch as the limbs could be brought nearer together, and thus the scale would be brought into close proximity to the mercurial column.

By using a stop-cock two straight tubes would be required, which would have to be joined by it at the bottom, and thus, by turning the tap and corking the open end of the shorter tube, all egress of mercury or ingress of air would be stopped.

The best form of junction-pipe or stop-cock will suggest itself to any one capable of constructing a barometer; but one thing should be borne in mind, viz., that it will be almost, if not quite, impossible to fill the tube if no provision is made in the junction-pipe for that purpose.

I am, &c.

H. Gornall.

MISCELLANEOUS.

Weights and Measures.—Mr. Ewart has obtained leave to bring in a bill for decimalising our existing system of weights and measures, and for establishing an accordance between them and those of foreign countries.

Rouge for the Complexion.—A correspondent in the East writes that, having lately wondered what was the composition of the rouge with which the Arabian girls paint their faces, he analysed a bottle of it bearing a French and Arabian label, and was not a little surprised to find a solution of Hofmann's "acetate of rosaniline" in rose water.

SCIENTIFIC SOCIETIES.

MEETINGS FOR THE ENSUING WEEK.

May 18. Monday.

British Architects—9, Conduit Street. 8 p.m.
Asiatic—5, New Burlington Street. 3 p.m. *Anniversary.*

19. *Tuesday.*

Civil Engineers—25, Great George Street, Westminster. 8 p.m.
Statistical—12, St. James's Square. 8 p.m.
Pathological—53, Berners Street, Oxford Street. 8 p.m.
Ethnological—4, St. Martin's Place. 4 p.m. *Anniversary.*
Pharmaceutical—17, Bloomsbury Square. 8 p.m. *Conversazione.*
Royal Institution—Albemarle Street. 3 p.m. Professor Tyndall, "*On Sound.*"

20. *Wednesday.*

Society of Arts—John Street, Adelphi. 8 p.m. Dr. Paul, "*On Destructive Distillation considered in Reference to Modern Industrial Arts.*"
Geological—Burlington House. 8 p.m.
Pharmaceutical—17, Bloomsbury Square. 11 a.m. *Anniversary Meeting.*
Royal Society of Literature—4, St. Martin's Place. 4.30 p.m.

21. *Thursday.*

Royal—Burlington House. 8.30 p.m.
Chemical — Burlington House. 8 p.m. G. W. R. Grove, Q.C., "*On certain Effects of intense Heat on Fluids.*"
Antiquaries—Somerset House. 8.30 p.m.
Numismatic—13, Gate Street, Lincoln's Inn Fields. 7 p.m.
Philosophical Club—6 p.m.
Zoological—11, Hanover Square. 4 p.m.
Royal Institution—Albemarle Street. 3 p.m. Prof. Ansted, "*On Geology.*"

22. *Friday.*

Royal Institution—Albemarle Street. 8 p.m. Prof. Roscoe, "*On the Sun's Chemical Action.*"

23. *Saturday.*

Royal Botanic—Inner Circle, Regent's Park. 3.45 p.m.
Royal Institution—Albemarle Street. 3 p.m. Prof. Max Müller, "*On the Science of Language.*"

ANSWERS TO CORRESPONDENTS.

⁂ All *Editorial Communications* are to be addressed to the Editor, and *Advertisements* and *Business Communications* to the Publisher, at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

⁂ In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

R. and Co.—The means to be adopted for deodorising hydrocarbon oils depend upon their nature, or rather upon the source from which they are obtained. What would be effectual in one case would not be so, perhaps, in another.

A Friend.—Our reporter most unaccountably neglected to attend the meeting of the Society. We will endeavour soon to obtain some information respecting it.

W. J. P.—1. It appears with tolerable regularity, and can be obtained by subscribing through any foreign bookseller. 2. We do not know M. Violette's address. A foreign bookseller would probably procure a copy of the memoirs for you.

THE CHEMICAL NEWS.

VOL. VII. No. 181.—May 23, 1863.

CHEMISTRY OF AGRICULTURE.

JUST sixteen years have elapsed since the sixth edition of Baron Liebig's work on the "Chemistry of Agriculture and Physiology" was published in Germany, and during that time much has been achieved in regard to this important subject both in observation, in thought, in the more just appreciation of long-known facts, in the interpretation of experimental results, and in a more critical examination of theories. During that time, great changes have taken place in various branches of the practice of agriculture. Not to speak of the application of steam in cultivation, the use of artificial manures has extended to a degree far exceeding the most sanguine anticipations that were formed when guano and soluble phosphates were first recommended to the attention of the farmer. The results of these changes are generally regarded as highly favourable, both in regard to the extent of land under cultivation, and as regards the produce obtained.

It has generally been supposed that, in this country at least, the recognition of scientific truth, as a safe guide in the practical operations of farming, has been the means of effecting very important improvements, and that the principle embodied in the motto of the Royal Agricultural Society—"Practice with Science"—had become, directly or indirectly, the general and leading principle of the prosecution of this art.

The prominent part that Baron Liebig has taken in contributing to this modern development of agriculture is too well known to need any minute account of the service he has rendered in suggesting and stimulating inquiry in this direction. This circumstance, together with the fact that he has continued to devote considerable attention to the chemistry of agriculture, will alone be sufficient to excite great interest in the new edition of his work that has just appeared. This edition contains, besides the original work, a new volume devoted to the elucidation of the "Principles of Agriculture," and an introductory essay that is not the least interesting portion of it.

In this introductory essay, Baron Liebig expresses an opinion widely at variance with that above mentioned as generally entertained, and he appears to regard the present condition of agriculture as being more retrogressive than progressive. It is true that he treats of agriculture generally, as practised in Germany as well as in other countries, but on the whole this view of the state of agriculture which he puts forward has also especial reference to this country.

In the preface to this new edition he says, that, since the publication of the last edition, he has had ample opportunities of becoming acquainted with the obstacles which prevent the diffusion of scientific doctrines in the sphere of practical agriculture. He refers these obstacles chiefly to the absence of any communion between practice and science. Agriculturists, he says, are very generally impressed with the prejudices that a degree of education inferior to that required in other arts is sufficient for the practice of their art; that the practical capability of the farmer is injured by a habit of reflection, and by the adoption or consideration of the results of scientific inquiry; that whatever involves the exercise of thought is designated by him as theoretical, and, as the direct antithesis of practice, is either little valued or disregarded.

There are many who will regard this as a remarkable statement, so far, at least, as it is intended to apply to this country, where it has been believed that the characteristic feature of our modern agriculture was an eager acceptance of the aids of science, and the expression of a demand for that aid even greater than science was able to gratify. Baron Liebig, however, admits it to be the fact, that the attempt to apply theory or scientific doctrines in agriculture has frequently been attended with disadvantage, that the means adopted by the farmer with the view of improvement, have often turned out failures; but he does not allow such unfavourable results to be any sufficient reason for a disinclination on the part of the farmer to depart from long-established custom. He throws the blame of these results upon him because he did not know in what way to apply scientific doctrines rightly. But this censure is unjust. If it be the business of the farmer to know the use of scientific doctrine, and to learn the art of applying it to his purposes, it is not the less the business of the chemist to teach him that knowledge and that art; to furnish him, in an available form, with those results of science which are capable of being applied usefully. It cannot be expected that the farmer will consider critically the value of the suggestions of the chemist before he has put them in practice. It is only by the result of their adoption that he is capable of judging, and if, following the guidance of the chemist, he finds that the promised advantage is not realised, it is quite natural that he should form an unfavourable opinion of the proposed innovations, and become confirmed in that tendency to adhere to customary routine which is a universal, and not an irrational, characteristic of agricultural practice.

The customary routine of agricultural operations, however defective they may be in particular cases, has become established by the exercise of precisely those mental functions which are employed in scientific research—observation and experience; and though, in agriculture, their exercise is limited by the practical object in view, there can be no doubt that, in reality, agricultural customs represent natural facts to a greater extent than even science is competent to explain. It is no reproach, therefore, to the farmer if he adheres to a system that he knows by experience to have brought him certain results, or that he refuses to adopt another system until he has sufficient evidence that its results are more advantageous than those with which he is familiar.

This natural conservative tendency has been strengthened by the failures resulting from premature application of scientific doctrines to agricultural practice. This is really the reason why farmers are in many cases averse to the adoption of improvements suggested by science. It is not because they are "destitute of all understanding," as Baron Liebig asserts, or because they pretend to deny the existence of any connexion between scientific doctrines and the phenomena presented by agriculture, but because it is still questionable whether those doctrines are correct, and consequently whether they really furnish the means of improvement. That is the reason why they select to be guided by tradition and received opinion rather than by the untried precepts of science. The science that is to be of value in agriculture, and whose value will be recognised by farmers, must grow out of the practice of the art, and not be a mere speculative grafting upon it. The accumulated data of ordinary experience must furnish the basis for its construction, and its doctrines must rest on evidence of reality, and not of mere probability. It may be a slow growth, but it must be a sure one. If there be an established and invariable connection between the use of superphosphates and the production of a large root crop, between the use of ammoniacal manures and the increase of corn crops, these manures will continue to be used, and their use will constitute the scientific practice of agriculture, independent of the questions whether they be specific manures for those crops, or what becomes of that nitrogen which is applied as manure and does not reappear in the crop. These are abstract scientific questions of high interest for the chemist, and of probable importance to the future of agriculture, but it is by no means a logical conclusion that the undoubted fact that the use of ammonia as a manure has a remarkable influence upon the production of corn, greater than in practice appears to result from the use of any other manure, should be disregarded because the precise functions of ammonia are as yet but partially known; nor is it rational for this reason to denounce the application of that fact, to the extent to which present knowledge reaches, as a folly and an error.

In reference to the controversies that have been maintained of late years as to agricultural subjects, Baron Liebig maintains that practical men have not understood the conclusions of science, that the disputes have not been as to scientific principles, but as to the false conception of those principles. He complains that his views have been altogether misunderstood, and claims the right to "purify his doctrine from the dirt by which it has for so many years been sought to render it unrecognisable." But the justice of this complaint is more than doubtful. Few scientific men have enjoyed greater popularity, in reference to any subject, than Baron Liebig has, in this country, in connection with the chemistry of agriculture, or have had their views so widely propagated and so generally received, as those put forward by him some years ago. Chemists of distinction have vied with each other in the translation of his writings, and in the exposition of his views. Agriculturists have accepted them unconditionally, and with the fullest confidence. It is, indeed, difficult to imagine any more thorough recognition possible than was accorded in this country to Baron Liebig's views on agriculture. The number of his adherents is now undoubtedly very much smaller than it was, but that circumstance is less referable to misconception of his views than to an alteration of opinion with regard to them. If Baron Liebig's views have suffered in this country, it is rather in consequence of the eagerness with which they were accepted in the first instance, and the too great significance attached to them as first propounded, and while still too insufficiently founded upon fact. Whatever defects may have been found in those views do not arise from want of regard for, or of acquaintance with them.

The exhaustion of the soil is a subject on which Baron Liebig especially dwells. He denounces the modern systems of agriculture as "systems of plunder," and predicts the reduction of land to an irremediable state of sterility as a necessary consequence of their continuance. He regards it as a signal defect that farmers do not keep account-books, in which each of their fields are entered, and a debtor and creditor account kept of all that is removed from, and supplied to them in the shape of mineral constituents. He considers that many farmers are, in consequence of this defect, ignorant of what condition their fields are in. It may be added also that, if this plan were adopted, there would be as much difficulty in determining by its means when a field was in good or bad condition. For with the almost infinite diversity of soils, as regards their resources of manure constituents, it would be impossible to establish any standard by which to make the determination. Moreover, the chemical composition of a soil is far from being the only condition of fertility. Many other conditions, that are as yet but little understood, contribute more or less to the capability and value of soils, and there is much to be learnt with regard to these conditions before there can be any sufficient grounds for recommending a refinement of such a nature as keeping accounts of the mineral constituents of his soils, the utility of which is still doubtful.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Absorption of Gases by Charcoal.

Dr. R. A. Smith[*] has given the following as the results of his experiments on this subject:—

Charcoal abstracts oxygen from atmospheric air at ordinary temperatures.

The absorption of oxygen continues for a long time, but the greater part takes place during the first part of the time the charcoal and air are in contact.

Hydrogen, nitrogen, or carbonic acid are not absorbed at the same time as oxygen, from gaseous mixtures.

The amount of gas absorbed by charcoal is in some degree proportionate to the condensibility of the gas; but this is not the only determining circumstance, in the case of oxygen at least.

The separation of oxygen that is absorbed by charcoal cannot be effected without production of carbonic acid, even at 212° F., and more slowly at lower temperatures.

The absorptive power of charcoal differs for different kinds very much, so does the capability of combining with oxygen; animal charcoal excels wood charcoal, in this respect.

Nitrogen and hydrogen absorbed by charcoal, diffuse into an atmosphere of other gases with a force equal to a three-quarter inch column of mercury.

Water expels mercury from the pores of charcoal instantaneously.

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[*] *Proceedings of the Royal Society*, xii., 424.

Dr. Smith concludes from his observations:—

That the selective character of porous bodies may be closely related to—

1. The condensibility of gases;
2. Attraction, and perhaps tendency to combine;
3. Capacity of combination.

That in any case the attraction which determines the condensation of a gas is exercised at a distance greater than the distance of the atoms or molecules in combination.

That the gases in porous bodies lie in concentric strata, around the particles of the solid, which strata are unequally attracted according to their distance.

That chemical attraction cannot be distinguished from physical attraction, though attraction may exist without effecting its ultimate result of combination, and that the influence of mass upon chemical action may be due to this connection.

That there is no evidence of the existence of a specific attractive force determining chemical change, but only evidence of combination.

Mr. Hunter, of Queen's College, Belfast, has also published some results relating to this subject.† He has adopted the method employed by De Saussure. The charcoal was heated to redness before each experiment, and, while incandescent, plunged into the dry gas confined by mercury. The absorption generally terminated at the end of twenty-four hours. The results obtained were as follows, for a temperature of 0° C., and pressure of 760 mm:—

TABLE II.

	Ammonia.		Carbonic acid.		Cyanogen.	
	Vol. absorbed reduced to 0° C. and 760 mm.		Vol. absorbed reduced to 0° C. and 760 mm.		Vol. absorbed reduced to 0° C. and 760 mm.	
	Experiment.	Mean.	Experiment.	Mean.	Experiment.	Mean.
Logwood . .	112·4 111·3 110·1	111·3	53·1 56 2	54·6	87·3	
Ebony . . .	106 6 104·4 108·7	106·7	46·6 50·3 44·0	47·0	89·6	
Camwood . .	93·0 89·5	91·2	44·7 51·3 44·7	45·4		
Green Ebony .	96·0 84 6	90·3	37·4 44·8 41·3	40·8		
Fustic (Cuba) .	89·7 89·1	89·6	61·7 54·9	58·0		
Lignum Vitæ .	88·2 89·2 83·0	89·0	47·2	47·2		
Boxwood . .	86 6 84·5	85·6	31·2 31·2 30·2	31·2	28·8	
Jamaica logwood	65·0 73·8	69·5	33·3	33·3		
Sapan wood .	69 9 69·8	69·9	32·2	32·2		
Beech . . .	54·6 61·3	58·0				
Rosewood . .	50·6	50·6				
Westaria Sinensis	44·03	44·03				
Vegetable ivory :	...	...	47·5 58·6 50·3	50·5	57·3	

The absorption was found to be very rapid in cases where the quantity of gas absorbed is large. The differences in the amount of absorption, sometimes considerable, are considered to be in part due to differences in the pieces of charcoal. In order to ascertain the influence of long-continued heating on the absorptive power of charcoal, a piece of boxwood charcoal was kept at a strong red heat for more than an hour. Its absorption was then found to be 79, while in the ordinary way of heating it was 85. It would appear from these results that different kinds of charcoal differ in their absorptive power for different gases. Logwood charcoal absorbs most ammonia; fustic charcoal most carbonic acid; and ebony charcoal most cyanogen.

Disassociation of Carbonic Acid.

M. **DEVILLE*** has continued his investigation of the phenomena of disassociation, or the decomposition of substances at temperatures below those at which they are permanently reduced to their elements. He regards the conversion of compound substances into a mixture of elementary substances, or more simple compounds, as being a change of state accompanied by all the circumstances which attend change of state effected solely by cohesion. Whenever heat is generated by combination, or when heat is absorbed by decomposition, the constancy of the temperatures at which the changes take place is a necessary feature of the phenomenon; it is the result of the sensible heat of combination or of the latent heat of decomposition. In combination, as in the condensation of vapour, heat is evolved; in decomposition, as in ebullition, heat is absorbed, and becomes insensible.†

Disassociation, or the decomposition of a substance, below the normal temperature of decomposition, is compared to the evaporation of a liquid below its boiling-point, as being necessarily but partial, and dependent upon the temperature of the medium in which it takes place. By the term tension of disassociation is denoted the quantity of any substance decomposed in its own vapour, compared with the whole mass submitted to the action of heat.

In the same manner that water may be decomposed at a moderately elevated temperature, either under the influence of a solvent which abstracts one of its elements, or by means of mechanical action, so, likewise, can carbonic acid be decomposed, and more easily, in consequence of the slight tendency of carbonic oxide and oxygen to unite when disseminated through a large quantity of carbonic acid. The apparatus employed for this purpose consisted of an iron tube filled with fragments of porcelain, and passing through another tube of porcelain. The whole was heated to a temperature of about 1300° C., and a current of pure carbonic acid passed through it into a bath containing caustic potash, in which were immersed tubes filled with potash solution. The gas, which was sent through the tube at the rate of 7·83 litres per hour, was not entirely absorbed, and in the same time about 20 or 30 centimetres of an explosive gas was collected. The composition of this gas was constant, as follows:—

Oxygen	30·0
Carbonic oxide	62·3
Nitrogen	7·7
	100·0

The same quantity of carbonic acid passed through the potash solution in the bath gave a residue amounting to 1·4 cubic centimetres, consisting of—

Oxygen	14
Nitrogen	86
	100

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† *Phil. Mag.*, xxv., 364.

* *Comptes-Rendus*, lvi., 729.

† CHEMICAL NEWS, vol. vii., 229.

This accounts for the presence of nitrogen in the products of disassociation.

This result, simple as it appears, does not admit of the tension of disassociation being calculated for the particular temperature at which it was obtained, even supposing that temperature to be known, for a portion of the disassociated gases may have recombined during the cooling.

M. Deville then goes on to consider the very important phenomena recently observed by M. Pébal ‡ and Messrs. Wanklyn and Robinson,§ viz., the decomposition of ammonium chloride into hydrochloric acid gas and ammonia by the action of heat, and the decomposition of phosphorus perchloride and sulphuric acid. M. Deville considers that the conclusions which have been drawn from these results are incorrect, and that they are analogous to the phenomena of diffusion observed by Mr. Graham. Thus, for example, when potassium bisulphate or alum are submitted to diffusion, it is found that these substances, whose existence at the ordinary temperature is unquestionable, cannot diffuse throughout an indefinite quantity of liquid without undergoing decomposition, by reason of the unequal diffusive capability of their constituents. It follows, therefore, that there is an actual exercise of force by which the constituents are separated, and which must not be overlooked in the explanation of the phenomena observed by Messrs. Pébal, Wanklyn, and Robinson. The same view is applicable to the diffusion of vapours whose constituents possess unequal capabilities of diffusion or transpiration. This agent of decomposition, observed by Mr. Graham, is so energetic that it is no longer possible to regard the decompositions effected by it as being spontaneous. That view does not afford any explanation of the fact that ammonium chloride, sulphuric acid, or phosphorus perchloride are susceptible of decomposition in their own vapour at the temperatures employed by the experimenters. The experiments of M. Marignac are much more decisive in their results as regards sulphuric acid; but they also show that the decomposition is very small in amount.

Referring to the application of the phenomena of disassociation by Cannizzaro and Kopp, with the view of showing that even the most complex vapours cannot represent eight volumes to an equivalent, M. Deville adds that the number of substances which may be so regarded is now so considerable that it would be inconsistent with the experimental results obtained by him and M. Troost to reject those facts on the pretext of considering those vapours which represent eight volumes as being decomposed at the moment of taking their density. Moreover, that argument would be dangerous when it serves only to support conjectural views as to the atomic constitution of chemical compounds. He has therefore thought it necessary to make an experiment with regard to this subject which would remove all doubts.

A vessel maintained at a constant temperature of 350° C. by means of mercury vapour was fitted with an air thermometer, which soon acquired a condition of equilibrium. By means of tubes connected with this vessel, two separate currents of hydrochloric acid gas and of ammonia were passed rapidly through it, and at an equal rate. At the moment the gases combined the air of the thermometer expanded suddenly, indicating a sudden elevation of temperature. By closing the leg of the thermometer at the moment when the disengagement of vapours was sufficiently copious, the temperature of the vessel was found to be raised to 394°·5 C., notwithstanding the constant cooling effected by the mercury vapour, which tended to reduce the sides of the vessel to the temperature of 350° C. It appears, therefore, that ammonium chloride does not undergo decomposition at 394°·5, but that its constituents combine at that temperature with a disengagement of heat much more considerable in amount than the above numbers appear to indicate. By taking the density of the ammonium chloride vapour at 350° C. in mercury vapour, it was found to be 1·00 instead of 0·93, as indicated theoretically for eight volumes.

This experiment shows that it is advisable not to reject as inadmissible, the results of experiments because they are not in accordance with atomic theories, which, however useful they may be as guides in new paths of research, should, from the variations they have undergone, be regarded as essentially ephemeral.

Action of Nascent Hydrogen on Aldehydes.

It is known that MM. Wurtz and Friedel have converted several aldehydes into the corresponding alcohols by means of nascent hydrogen. They were unable to effect this change with the gas obtained by the ordinary methods of converting nitrobenzol into aniline, but they succeeded by using an amalgam of sodium.

It has occurred to M. Lorin* to effect this change by means of hydrogen obtained by the decomposition of water, at a moderately elevated temperature, by zinc in the presence of ammonia—the means by which M. Berthelot converted acetylene into ethylene. He has found that this may be done, that aldehyde and acetone are respectively converted into the corresponding alcohols, and that, in the case of aldehyde, this change is also accompanied by the production of ammoniacal compounds.

Dry aldehyde-ammonia was placed in contact with a water solution of ammonia and small fragments of zinc. The disengagement of hydrogen took place under slight pressure, and at a temperature of from 30° to 40° C.

After filtering the liquid, and distilling off one half, the distillate was saturated with dilute sulphuric acid, then again distilled from a water bath, and one-fourth collected. Carbonate of potash caused the separation of an inflammable liquid, decomposable by sulphuric acid with production of olefiant gas, and yielding with acetate of soda and sulphuric acid, acetic ether; in short, possessing all the characters of ordinary alcohol.

Acetone, carefully purified and treated in the free state in the same manner, gave rise to the production of propyl alcohol.

The quantities of the alcohols thus obtained did not amount by weight to more than one-fifth of the quantities that should have been obtained if the conversion had been complete. Accessory products were formed by the action of the nascent hydrogen, either on the aldehydes or on the alcohols.

In the experiment with aldehyde, the residue, saturated with potash and carefully distilled into hydrochloric acid, gave a crystalline deliquescent salt, soluble for the most part in absolute alcohol, and evolving an inflammable alkaline vapour when mixed with lime.

Nitrobenzol, treated in the same manner as above, was converted into aniline.

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‡ *Annalen der Chemie und Pharmacie*, xlvii., 199.

§ *Comptes-Rendus*, lvi., 547.

* *Comptes-Rendus*, lvi., 845.

TECHNICAL CHEMISTRY.

Action of Magnesia on Alkaline Fluorides.

Hydrate of lime decomposes fluoride of sodium with great facility, giving rise to hydrate of soda and fluoride of calcium. It is by means of this reaction that the residues of the manufacture of aluminum have hitherto been turned to account.

$$\left.\begin{array}{l} Al_2Fl_3,3NaFl + 3Na = Al_2 + 6NaFl \\ \qquad\qquad\qquad\qquad 6CaO,HO \end{array}\right\} = \left\{\begin{array}{l} 6NaO,HO + \\ \quad 6CaFl. \end{array}\right.$$

The action of magnesia upon fluoride of sodium is somewhat different, by reason of the formation of a double fluoride of magnesium and sodium. However large the excess of magnesia employed, only two-thirds of the sodium in the fluoride is obtained as soda. M. Ch. Tissier* has studied this reaction by converting fluoride of sodium into sulphate, and by acting upon an equal quantity of fluoride with magnesia in excess, and then converting the soda produced into sulphate. He thus obtained from 100 parts fluoride of sodium—

	Sulphate of soda.	Equivalents.
By direct action of sulphuric acid .	170	3
By the action of magnesia . .	121·4	2
By the action of lime . . .	170	3

The reaction with magnesia would be represented by

$$3NaFl + 2MgO,HO = 2NaO,HO + (MgFl)2NaFl.$$

Cryolite—Al_2Fl_33NaFl—resists almost entirely the action of magnesia. On the other hand, fluoride of magnesium resists the action of lime.

Chemistry of Steel.

A continuation of M. Caron's† researches on this subject relates to the function performed by manganese in the production of steel. Regarding it as a fact that all good steel is obtained from spathic, or hematite ores, highly impregnated with manganese, and assuming that the presence of this metal is almost essential to the production of steel, he has endeavoured to ascertain the nature of its influence. He states that, by means of a suitable addition of metallic manganese, pig iron may be deprived of the sulphur and silicium it contains, but that no influence is exercised in the separation of phosphorus.

The pig iron with which he experimented is described as of very good quality, containing only traces of silicium, and it was melted by means of wood charcoal as free as possible from phosphorus, sulphur, and silicium.

One portion of this pig iron was melted with a known quantity of iron phosphide; another portion with iron sulphide, and a third with iron silicide. The three kinds of iron thus obtained were the subject of the experiments.

Two equal quantities of the phosphuretted iron were melted in crucibles; in the one instance without any addition, in the other instance with 6 per cent. of metallic manganese. The manganese used gave on analysis:—

Iron	1·0	100
Carbon	5·5	
Silicium	0·5	
Manganese . . .	93·0	

The metal was kept in fusion for one hour, and then poured out. During the operation the metal was slightly decarbonised by the oxidising action of atmospheric air. Analysis of these two portions of iron gave the following percentage results:—

	Phosphorus.	Manganese
Phosphuretted iron . . .	0·83	
No. 1. Phosphuretted iron melted alone	0·82	
No. 2. Phosphuretted iron melted with 6 per cent. manganese	0·80	4·58
No. 1 Melted again without addition	0·79	
No. 2 Melted again without addition	0·78	3·74
No. 1 Melted a third time without addition	0·78	
No. 2 Melted a third time without addition	0·76	1·62
Phosphuretted iron melted with 10 per cent. iron oxide	0·76	
Phosphuretted iron melted with 10 per cent iron oxide and 6 per cent. manganese	0·74	1·57

These results appear to indicate that, in refining phosphuretted pig iron, manganese does not effect the separation of phosphorus.

	Sulphur.	Manganese.
Sulphuretted iron . . .	1·15	
No. 1. Sulphuretted iron melted alone	1·14	
No. 2. Sulphuretted iron melted with 6 per cent. manganese .	1·15	3·92
No. 1 Melted again without addition	1·05	
No. 2 Melted again without addition	0·10	2·81
No. 1 Melted a third time ,, .	0·96	
No. 2 Melted a third time ,, .	0·08	1·73
Sulphuretted iron melted with 10 per cent. iron oxide .	1·08	
Sulphuretted iron melted with 10 per cent. iron oxide and 6 per cent. manganese .	0·07	1·22

These results show that, by simple fusion with access of air, manganese effects the separation of $\frac{7}{10}$ths of the sulphur in the pig iron. Repeated melting, without further addition of manganese, does not produce any considerable effect, and it appears that the proportion of manganese should be large, for by remelting the same metal with a fresh quantity of manganese the sulphur may be almost entirely separated.

	Silicium.	Manganese.
Siliciетted iron . . .	0·99	
No. 1. Siliciетted iron melted alone .	0·88	
No. 2. Siliciетted iron melted with 6 per cent. manganese . .	1·30	4·77
No. 1 Melted again without addition	0·80	
No. 2 Melted again without addition	1·66	2·98
A. Siliciетted iron melted with 10 per cent. iron oxide . . .	61	
B. Siliciетted iron melted with 10 per cent. iron oxide and 6 per cent. manganese . .	0·37	2·52
A, Melted again with 10 per cent iron oxide	0·52	
B, Melted again with 10 per cent. iron oxide	0·18	1·10

The manganese used in these experiments contained silicium, and in this way, as well as by reducing the silica of the crucible, added to the amount of silicium in the iron. But this is not the case when the iron is decarbonised by the addition of iron oxide.

It may be inferred from these results therefore that manganese determines the separation of a large portion of the silicium in pig iron.

These results agree with practical experience. The ores above mentioned, that yield the better kinds of steel often contain sulphur, but never contain phosphorus.

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[*] *Comptes-Rendus*, lvi., 848.

[†] *Ibid.*, lvi., 828.

and it is a noteworthy fact that, although those ores contain copper pyrites, the pig iron they yield does not contain sulphur.

Although manganese does not determine the conversion of iron into steel, as Karsten has well shown, it is nevertheless certain that—besides rendering steel capable of being welded—it has an influence in improving the quality of steel. M. Caron considers that this influence may be explained by the aid of facts which he has formerly made known. When a sufficient quantity of manganese is added to grey pig iron, the carbon of which is to a great extent in a free state, white pig iron is obtained, in which the carbon is almost entirely in a state of combination. The effect is the same with steel; a very small addition of manganese is sufficient to retain the carbon in a state of combination, and, in consequence, to confer on the metal the characters peculiar to good steel. However, the amount of manganese in steel must not exceed $\frac{5}{1000}$th; more than that renders steel hard and brittle, the surface of fracture becomes crystalline, and the metal is deficient in tenacity.

It is a common practice in refining iron to mix ordinary pig iron with manganiferous pig iron, and the above results show that the beneficial effect of this practice is greater according to the amount of manganese present. It appears therefore to be a point of practical interest to reduce manganiferous ores in such a manner as to obtain the largest possible amount of manganese in the pig iron. The spathic ore of the Siegen district contains from 15 to 20 parts manganese for 100 of iron; but the pig iron made there does not contain more than 6 or 7 per cent. of manganese. M. Caron considers that if this amount could be increased to 10 per cent. the commercial value of the pig iron would be increased.

Quantity of Air Indispensable for Respiration During Sleep.

M. Husson* remarks, in reference to the note on this subject by M. Delbuck,† that if the majority of animals seek retired places in order to sleep, their object is merely the same as that of a man who locks his bedroom door. If the soldier sleeping in the open air covers his head with a cloak, it is chiefly with the object of protecting himself from cold. The reaper or haymaker, when resting, merely seek the shade, and do not cover their faces unless it be for the purpose of keeping away insects. At the same time, it must be remembered that even in a waking state man sometimes finds it necessary to cover his face. The fact that children cover their heads with the bed-clothes before going to sleep is rather the exception than the rule, and those who do so instinctively throw off the covering during sleep.

There is no doubt that it is possible to exist for a brief time in a vitiated atmosphere, but it is not possible to continue in such an atmosphere without prejudice to health.

As for birds, who tuck their heads under their wings before sleeping, they at the same time place their beaks so that air has free access.

Employment of Cryolite in Manufactures.

A few years ago cryolite was found only as rare specimens in mineralogical collections. M. H. Rose was the first to propose this mineral as a raw material for the manufacture of chemical products. On the discovery of large deposits of this body, he indicated its utilisation in the manufacture of soap. He had remarked that by boiling cryolite, which is a double fluoride of aluminium and sodium, with milk of lime, a double decomposition took place, giving rise to fluoride of calcium, an insoluble compound, and soluble aluminate of soda, which is available instead of caustic soda in the saponification of fatty matters.

In 1860, M. Weber, of Copenhagen, obtained from the Danish Government the monopoly of the cryolite deposits in Greenland. In his manufactory he treats the mineral by sulphuric acid, and transforms the resulting sulphate of soda into carbonate of soda and sulphate of alumina.

In 1860, MM. Müller, of Berlin, and Hasberg, of Harbourg, entered into an agreement with M. Weber, who undertook to furnish them with 1000 tons of cryolite per annum. This cryolite M. Hasberg decomposes in the following manner:—

The mineral is broken up and ground in vertical cast iron mills. This is easily effected, and the mineral is then sifted by means of inclined bolters.

One hundred parts of this cryolite are mixed with 127 parts of carbonate of lime also reduced to powder. These quantities correspond to one atom of cryolite and six atoms of calcareous carbonate. The mixture is placed in a reverberatory furnace with two compartments, and heated with coke. The operation is two-fold,—that is to say, the mass is first dried in the cooler part of the furnace, and then made red hot in the other. When properly heated, the mass is withdrawn by means of fire-irons, and thrown on a grating, which retains the conglomerated pieces. These pieces are pounded, and used in the following operation:—

Cryolite is transformed into aluminate of soda and insoluble fluoride of calcium by means of carbonate of lime. The powder, after being calcined, is taken away in sheet iron boxes, and lixiviated in the manner usual in soda factories.

Besides fluoride of calcium and ferric oxide, the brown residuum contains a small excess of undecomposed cryolite, and traces of aluminate of soda. No use has, as yet, been found for this material.

The aluminate of soda solution thus obtained has a slight brown colour. It is introduced into cylindrical sheet iron cauldrons, placed horizontally, and furnished with stirrers, into which the gases of the reverberatory furnace are passed. Besides the products from the combustion of coke, these gases contain carbonic acid from the carbonate of lime, and are sufficiently hot to give the aluminate of soda the right temperature for obtaining oxide of aluminium as a compact precipitate.

A draught is obtained from a ventilator placed at the end of the cylindrical cauldrons.

Carbonic acid completely decomposes aluminate of soda in a few hours. The separation of oxide of aluminium from the carbonate of soda is effected by decantation in very tall sheet iron cauldrons.

The solutions of carbonate of soda are evaporated to obtain *soda* and *soda crystals;* but the greater part of these liquids is treated by hydrate of lime to produce hydrate of soda. Products prepared in this way are very pure. The carbonate of soda naturally contains no chloride, and is contaminated only by traces of sulphite and hyposulphite proceeding from the impurities of the coke.

The caustic soda furnished by the Harbourg factory contains 75 per cent. of oxide of sodium; we find

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* *Comptes-Rendus*, lvi., 127.

† Chemical News, vii., 221.

besides from 4 to 5 per cent. of carbonic acid. The hydrate of alumina is perfectly white, and quite free from iron.

The sulphuric acid required for the transformation of hydrate of alumina into sulphate is prepared in the same factory by the combustion of sulphur. The sulphur furnaces are constructed of bricks, joined by *a cement composed of lime and chloride of sodium*, which is gradually converted into a very resisting mixture of sulphates of lime and soda. For nitric acid, a mixture of Chili saltpetre and sulphuric acid is substituted, and is put into small cast iron cauldrons. One hundred parts of sulphuric acid require about seven and a-half parts of nitric acid.

M. Sauerwein proposes to treat cryolite by the wet way—hydrate of lime taking the place of carbonate. By boiling one part of cryolite with six of hydrate of lime are formed fluoride of calcium and aluminate of soda.

Decomposition takes place readily. M. Sauerwein's reaction is so far analogous to that produced in the Harbourg factory by the dry way, but he does not decompose aluminate of soda by carbonic acid; to obtain hydrate of soda he treats the aluminate by a fresh quantity of cryolite, so as to transform all the soda into fluoride, and the aluminium into hydrate.

Simply boiling the aluminate with the cryolite produces the double decomposition.

Thus are formed a solution of neutral fluoride of sodium and a hydrate of alumina precipitate mixed with the excess of cryolite employed. The deposit, properly washed, is treated by weak sulphuric acid, which dissolves only the alumina.

By boiling fluoride of sodium with hydrate of lime we obtain a precipitate of fluoride of calcium, hydrate of soda remaining in solution. This process has the advantage over that used at Harbourg, of requiring less fuel; but the sulphate of alumina is impregnated with small quantities of iron, proceeding from the hydrate of lime used for the decomposition.

By treating cryolite by hydrate of lime, M. Tissier obtained only a third of the aluminium in the state of aluminate of soda. M. Sauerwein did not arrive at the same results; he obtained all the sodium and aluminium contained in the cryolite, with the exception of the losses unavoidable in such operations.

By using for the decomposition of the cryolite only half the lime required for the transformation of the aluminium into aluminate of soda, we obtain simultaneously the mixture of aluminate and cryolite necessary to the formation of fluoride of sodium and hydrate of alumina. Thus the two operations can be amalgamated, by boiling the cryolite with a quantity of calcareous hydrate insufficient to produce the solution of fluoride of sodium. The author is, however, of opinion that the first process is preferable, because it is to be feared that in dissolving the alumina in sulphuric acid, the fluoride of calcium will be partially decomposed by the sulphuric acid, which would then be wasted.

The treatment by carbonate of lime by the wet or dry way seems preferable to treating cryolite by sulphuric acid, as is done at Copenhagen, where a mixture of sulphate of soda and sulphate of alumina is obtained and purified by repeated crystallisations. The sulphate of alumina is, of course, contaminated with all the impurities of the cryolite; moreover, sulphate of soda is of much less value than either the carbonate or hydrate.

It is also possible that cryolite, decomposed by an insufficient quantity of sulphuric acid, may be transformed into soluble sulphate of soda and fluoride of aluminium. The fluoride of aluminium might be treated separately; but in this case also the soda would be obtained only as sulphate, and M. Sauerwein has not ascertained whether this reaction is practicable.

Holmes's Magneto-Electric Light.

The applicability of the electric light as a means of illuminating the signal stations and lighthouses round our coast is a question which has lately been discussed in the House of Commons and in the pages of the public journals. On the 17th ultimo, Lord Lovaine made inquiries of the Government relative to the character of the reports which had been furnished to the Trinity Board by Professor Faraday and others in reference to this system of illumination; and, if successful, what steps had been taken with a view to supplying this apparatus instead of the oil lamps to the Portland lighthouse? The importance of the question led to further inquiries in the House on a subsequent occasion, and, inasmuch as there appear to be many circumstances which bespeak for this subject an attentive consideration, we propose to introduce some of the most prominent points for discussion in the columns of this journal.

In our critical notice of the "Report of the Commissioners appointed to Inquire into the Condition and Management of Lights, Buoys, and Beacons,"* we referred to the fact of Professor Holmes's light being then, or recently on trial at the South Foreland, but its employment had not at that time been sufficiently protracted to warrant an extension of the principle, or admit of implicit reliance being placed in its performance. Later results have shown, however, that for an unbroken period of eleven months the electric apparatus has not failed in its action, and the satisfactory accounts of the light at Dungeness prove that the system is now working in a manner calculated to win the confidence of the mariner. In tracing the history of the experiment, it is stated in these reports that the magneto-electric light dates from December, 1858, when it was first placed at the South Foreland. Its use at that station was discontinued for short periods, in order to introduce improvements, which resulted in its being so far perfected that on August 22, 1859, it was again started, and remained for six months in uninterrupted service. On June 6, 1862, it was permanently fixed at the Dungeness Lighthouse, where it has continued in operation to this day. Several official reports appear to have been transmitted to the Trinity House, from which we gather that on April 29, 1859, the Royal Commissioners observed this light (then shown at South Foreland) from a point in the Channel midway between Dover and Calais, who reported of it that it was "far brighter than any other lights, visible either on the French or the English coast;" that "at a distance of several miles it cast a shadow which could be seen clearly on the palm of the hand; and, further, it appeared that there was the greatest facility in managing it, and that merely ordinary care was required to insure its regular exhibition."

In a report bearing the same date (April 29, 1859), Professor Faraday stated his opinion "that Professor Holmes had practically established the fitness and sufficiency of the magneto-electric light for lighthouse purposes, so far as its nature and management were concerned;" that "the light produced was powerful beyond any other that he had yet seen so applied, and in principle might be accumulated to any degree;" that "its

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* *Vide* Chemical News, vol. iv., p. 133.

regularity in the lantern was great, its management easy, and its care might be confided to attentive keepers of the ordinary degree of intellect and knowledge." Again, on February 28, 1860, he reported to the Trinity House, after a subsequent experiment of six months' duration, that "the light has proved to be practicable and manageable, and has supplied the means of putting into a lighthouse lantern, for six months or longer, a source of illumination far surpassing in intensity and effect any other previously so employed;" that he "did not know at what cost this result had been obtained, but that, unless that was very great indeed, he was of opinion that a large increase upon the expense of the old method (estimated upon the quantity of light obtained) ought to be permitted in the first establishment of a mode of illumination which apparently promised many improvements during its further development."

The simple mention of these facts, coupled with the favourable opinion of Professor Faraday, is sufficient to insure for this scheme the amount of attention which it deserves; and with the view of aiding the discussion upon this subject, we propose giving shortly a detailed account of the mechanical arrangements and mode of operation of Professor Holmes's magneto-electric apparatus.

PHYSICAL SCIENCE.

Experiments on Ozone, by M. Soret, *in a Letter to Professor* Tyndall.*

You have perhaps remarked that I have made a note of one or two observations relative to ozone. This leads me to say a few words to you on some experiments which I have since then made on this subject in M. Bunsen's laboratory. I had formerly ascertained (*Archive*, 1854, vol. xxv., p. 263) that the quantity of ozone is greatly increased when the voltameter is cooled, as you have also found. I had made some analyses by arsenious acid, but the method was not perfect. I resumed experiments of the same kind, using M. Bunsen's method with sulphurous acid and a standard solution of iodide of potassium. My first idea was to try and ascertain whether less or more ozone was produced in light than in darkness. I found no appreciable difference, but I found that, with the voltameter which I used, I produced even at ordinary temperatures very considerable proportions of ozone. I worked with a voltameter of 500 to 700 cubic centimetres capacity, filled with acidulated water (one volume of HO, SO_3 to five volumes of water), with very fine wires of platinum-iridium for electrodes; the negative electrode was surrounded by a porous cell, and the gases were consequently not mixed. I obtained at ordinary temperatures, much higher than that of 0°, a quantity of ozone varying with the circumstances, but amounting almost to 1 per cent. for the whole of the oxygen disengaged. The gas appeared to be capable of being dried by passing through sulphuric acid without appreciable loss of ozone. By surrounding the voltameter with a mixture of ice and salt, and allowing the oxygen to pass directly into iodide of potassium, I obtained a quantity of iodide corresponding to 20 milligrammes of oxygen; the oxygen collected, after passing through the iodide, filled a flask of 720 cubic centimetres capacity. If, then, we assume that the 20 milligrammes of oxygen absorbed represent ozone, we obtain a proportion of $\frac{2}{105}$. By collecting the gas under water, and absorbing with iodide of potassium after all the gas has been disengaged, less oxygen is found, because water dissolves a considerable proportion (about one-fifth of the ozone liberated in an experiment in which water was analysed).

These proportions are, then, very appreciable, and the essential conditions for obtaining them appear to be—

1. The use of large voltameters, to avoid the heating by the passage of the current, and perhaps a perturbing action of the oxygenised water.
2. The separation of the gases.
3. The use of electrodes of platinum-iridium exercises possibly also an influence.
4. The cooling of the voltameter.
5. The use of a sufficiently concentrated solution of sulphuric acid.

I availed myself of the facility of the production of ozone to repeat Baumert's fundamental experiment, in which dried electrolytic oxygen, passed into a glass tube coated with a slight layer of anhydrous phosphoric acid, dissolves this deposit if the tube has been heated at one point—an experiment which, according to Baumert, proves that ozone contains hydrogen. In operating with a voltameter in which the two gases were disengaged (they were separated by a porous cell), I found, in fact, that water was formed by heating the disengaged oxygen. But by effecting the decomposition in a large vessel containing acidulated water, in which the positive electrode was placed, and a large porous cell filled with sulphate of copper, in which the negative electrode was placed, so that there was no disengagement of hydrogen, there was not the smallest trace of water in the oxygen after its passage through the heated tube. I prove, then, that in Baumert's experiment, as M. Marignac had supposed, it is an imperfect separation of the gases which has led him to a conclusion different from that usually admitted.

I continue to work at this subject, but I have thought that these first results might possess some interest for you, and hence I have taken the liberty of communicating them to you before my investigation is terminated and ready for publication.

PROCEEDINGS OF SOCIETIES.

SOCIETY OF ARTS.

April 29.

Sir Thomas Phillips *in the Chair.*

(*Continued from page* 236.)

Mr. Paul wished to draw attention to one particular referred to by Professor Ansted, namely, the essential characteristics and differences of those kinds of combustible minerals belonging to the carboniferous series. Taking, in the first place, those minerals commonly known as coal, there might be selected from among the numerous varieties certain of them presenting well-marked characters of difference. Thus, for instance, anthracite—the free-burning "steam-coals" of the Welsh coal field—the coals of the Lancashire district and Midland counties, the caking coals of the Tyne district, and the cannel coals of Scotland. Between well-defined specimens of these varieties of coal there were great apparent differences. But a closer examination showed that those differences were only in degree. Coals might be easily found which, while presenting all the apparent characteristics of anthracites, more closely resembled bituminous coal in their composition. Between the bituminous coals of the Tyne district and the "steam-coal" of Wales, there was, in many instances, no kind of difference in composition. Between the cannel coals of

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* Communicated by Professor Tyndall to the *Philosophical Magazine.*

Scotland and the bituminous coals of Newcastle, the difference in composition was small as a rule, and there were cannel coals which approximated more closely to the coal of Newcastle and Lancashire than they did to the better and more characteristic varieties of cannel coal. The term "bituminous," when applied to coal in the sense of its containing actual bitumen, such as that of Trinidad or Jew's pitch, was a false designation. The character properly denoted by the term "bituminous," as applied to coal, was the existence in the coal of a portion of its substance which was capable of being volatilised by heat, and which, though it was not true bitumen, was analogous to bitumen. This character bore no recognisable relation to the amount of hydrogen in the coal; but coals differed considerably in regard to the amount of bituminous substance they contained. Thus, for instance, some of the well-known steam coals of Wales, such as the Resolven, Graigola, Merthyr, Llangennech, and others, were in composition almost identical with the Hartley coals. The really important practical difference between these coals consisted in their yielding different amounts of volatilisable substance when heated. The Welsh coals yielded but little; the Newcastle coals yielded half their weight in some cases. These volatile products, being swept away from the furnace before there was time for their being burnt and rendered available as a source of heat, appeared at the chimney mouth as smoke. Hence arose the difference sometimes found in the fuel value of these coals; for the volatile substances given off were difficult to burn, unless special provision was made for the purpose. It was owing to this circumstance that in the examination of coal for the Admiralty, the value of the Newcastle coals was placed so far below that of the Welsh coal. A mistaken system of trial was adopted; both kinds of coal were burnt under the same conditions, whereas, if the mode of using the coals had been adapted to their peculiarities, a different result would have been obtained. The experiments subsequently made, at the instance of the coalowners of the Newcastle district, proved most conclusively that the Hartley coals had a higher fuel value than was assigned to them by the results of the Admiralty trials, and that they were in this respect equal, and in some respects superior, to Welsh coal. Throughout the whole range of varieties of coal, though it was possible to select typical specimens that were widely different, still no essential difference could be recognised. Between all of those typical varieties there were intermediate varieties so numerous, and presenting such minute gradations of difference, that it was impossible to draw any absolute line of distinction between them. It was very important that these facts should be borne in mind, more especially since it was convenient to use the term cannel coal, caking coal, free burning coal, &c., and to comprise the whole of the varieties of coal, excepting anthracite, under the term bituminous coal. The bituminous character of coal from the carboniferous series of rocks was a well-marked feature of distinction from lignite, which, though it contained volatilisable substance, approximated more closely to wood in its nature generally, and in the nature of the volatilisable portion. Considering only the applicability of a mineral as fuel, it was equally impossible to establish any absolute distinction between coal and those minerals associated with it which were capable of being burnt, or which at least contained combustible substance; but which differed from what was practically recognised as coal, in containing a larger amount of earthy substance, and which were generally called bituminous shale. A mineral of this kind, containing 20 or even 30 per cent. of ash, might be used as fuel at the pit's mouth, but it would not be sent up to London for use while other minerals could be got to send that contained only 2, 3, or 5 per cent. of ash. In the absence of minerals of the latter kind, which were commonly called coal, and in the absence of other fuel, some of the bituminous shales, or such minerals as that of Boghead, might be used as fuel. Altogether, the application of the term coal was merely conventional, and it was a convention determined altogether by local circumstances. But from another practical point of view there were differences that had not hitherto been much regarded, and which might be made a means of determining a difference between coal and shale. It was, for instance, a recognised characteristic of coal that it would leave when heated a fixed carbonaceous residue, called coke, amounting to a considerable portion of its weight. Coals varied very much in this respect; anthracite gave from 93 to 80 per cent. of coke. The Welsh steam coals gave from 10 to 70 per cent., while the Newcastle coals gave only from 70 to 60. The cannel coals gave from 60 to 40 per cent. The Boghead mineral gave 30 per cent. of fixed residue. But this mineral contained 22 per cent. of ash, while ordinary coal did not contain more than 5 per cent. of ash. Therefore, to find the difference between coals and other minerals in this respect, the ash must be deducted, and the ratio of the fixed carbonaceous residue to the volatilisable portion ascertained. Then there was a very marked difference recognisable. No true coal, as the term was generally understood, gave less than one-third its weight of fixed carbonaceous residue. Even the Albert coal of Nova Scotia, which was in many respects an exceptional coal, containing less than 1 per cent. of ash, and being applicable for the manufacture of oil and paraffin, yielded 33 per cent. of fixed carbonaceous residue, and 67 per cent. of volatile substances. Newcastle coal would yield 66 per cent. of fixed carbonaceous residue, and 37 per cent. volatile substances. The cannel coals, on the average, would yield half their weight of volatile substances, and an equal portion of carbonaceous residue. But the Boghead mineral would yield 88 per cent. of volatile substances, and only 12 per cent. of fixed carbon. Here was a wide difference from coal generally, and even from cannel coal. Certain bituminous shales again, that were not used as fuel, but for the manufacture of oil by distillation, would yield from 80 to 95 per cent. of volatile substance, and only 5 to 20 per cent. of fixed carbonaceous residue. Among the shales here referred to there was one from Vouvant, analysed by Dumas, and worked by Selligue for oils and paraffin in 1833, and another, an English shale, analysed by Laurent, in 1833. It was a remarkable fact that the increase in the proportion of volatilisable substance, as compared with the fixed carbonaceous residue, bore a direct relation to the amount of earthy substance in the minerals. This was very noticeable in some of the poorer kinds of cannel coal, containing a large amount of earthy substance, or ash. The greater the amount of ash, the greater was the ratio of volatilisable substance to the fixed carbon. In this respect the Boghead mineral appeared as far removed from ordinary coal as it was by the external characters it presented and the amount of ash it contained. It resembled bituminous shale far more than it did coal, and it could not be doubted that, by reason of these characters, it would not be used as fuel, nor would it be used at all if it were not for its capability of yielding gas and oil. Another difference between coal and the bituminous shale associated with it, consisted in the nature of the oily products obtainable from them by distillation, as well as their amount. For use as fuel, it was not a matter of any great importance what was the nature of the volatile products given off by a mineral, provided they were combustible and could be burnt. But as regarded the use of these minerals for the manufacture of oils, the nature of the volatile products, no less than their amount, was of paramount importance. Coal, properly so called, yielded oily products that were different in kind from those yielded by bituminous shales and by the cannel coals that were most closely allied to bituminous shale. This was a most important distinction between them as regarded the manufacture of oil, and in reference to the question that had been raised as to whether the Boghead mineral was a coal or a shale. Judging from the character which rendered this mineral so eminently well suited for that purpose, it should unquestionably be regarded as a shale, and not as a coal, by reason of its far greater resemblance in that particular to shale than to coal. Considerable attention had already been directed to the use of bituminous shales for manufacturing oil from them by distillation. The first attempts that were made, long ago, to establish this branch of manufacture, were made before there was any demand for the oils which were now so largely used as a source of light and for lubricating purposes, and it was only since the use of these materials became general on the Continent, and a large demand for them had been created, that any attempts to manufacture them in this country proved successful. Now, the demand seemed to be limited only by the extent of the supply. This manufacture had been carried on since 1833 in France and in Germany, and it was now being prosecuted in the neighbourhood of Linlithgow, with some shales occurring at Broxbourne; and at several other places between Glasgow and Edinburgh there were works of a similar nature. As regarded the use of anthracite as fuel, the objections that were formerly found to obtain against it were only due to the misapplication of this mineral as fuel. Means had now been found of using it with success, and, in many instances, with great advantage, inasmuch as its large percentage of carbon rendered it highly valuable when intense heat was required. Bituminous coal, a century or so since, was objected to in a similar manner. People had been accustomed to use only wood-charcoal, and it was found that bituminous coal, used like wood charcoal, was not suitable for smelting iron and other metals. But it was very soon found that this was only a consequence of improper use, and the right mode of application was soon learned. Now, as was well known, scarcely anything else was used as fuel for these purposes.

NOTICES OF BOOKS.

The Elements of Chemistry. By J. C. Buckmaster. Second Edition. Longmans. 1863.

This is one of the very numerous popular elementary works which it is our painful task to condemn somewhat strongly. It is a collection of chemical facts most loosely put together, in the driest possible language, at least seven years ago, and republished in 1863 with a new title-page and preface. Such every-day matters as spectrum analysis, dialysis, ozone, the aniline dyes, paraffin, the manufacture of sulphuric acid from pyrites, Spence's alum process, even the daguerreotype and photography are wholly ignored.

Mr. Buckmaster announces himself on his title-page as "Certificated teacher of science by the Department of Science and Art," and "Examiner in Chemistry and Physics in the Royal College of Preceptors." Pray does he teach his pupils as well as his readers that the only uses of nitrate of silver are "as a dye for the hair, and for the manufacture of marking-ink for linen?" (page 169), or that nitre "becomes liquid (deliquesces) in a moist atmosphere," and that "it is a poison when taken in large quantities?" (page 177). Here we have been using a deliquescent material for gunpowder for the last three or four hundred years. Why has not Mr. Abel been long since drummed out of Woolwich Arsenal for having overlooked this important fact? The facts that are contained in the book are strung together in the most careless manner, the repetitions being numberless, and the language being in many instances ambiguous. Printers have a great deal to answer for, but we fear that Messrs. Clowes and Sons can hardly be made responsible for such culpable blunders as binoxide of *barytes*, binoxide of *strontia*, sesquichloride of *alumina*, *conocine* (?) *emeline* (?) *metamorphis*, and a number of others. It may be argued that in so small a work all the modern discoveries in chemical science could not be included, but we think that a short account of some of them might have been given, instead of some thirty pages of useless repetition in the form of a *resumé*, and fifty pages containing the South Kensington examination papers for the last four years, the only use of which could be to show the student the antiquity of the preceding matter.

A glossary is added at the end containing some singular definitions, such as "Allotropic, a body having different physical forms. Carbides, substances in which carbon forms one of the constituents."

Even the index misleads us as to the page on which at least twenty of the words are to be found. Mr. Buckmaster should call in this second edition of his book at once, or, if he does not do so, Messrs. Longmans ought to consider whether it is worth while to allow their reputation as first-class publishers to be injured by the circulation of a book such as this is with their name appended to it.

NOTICES OF PATENTS.

158. *Improvements in the Treatment of Fousel Oil, and for Various Applications of the same to Useful Purposes.* A. J. Martin, High Street, Bow. Dated January 21, 1862.

The inventor prepares an oil or fluid suitable for illuminating purposes by dissolving coal-tar, pitch, or other hydrocarbons, in the refuse of spirit distilleries known as "feints," or fousel oil.

163. *Treatment of Mineral Oils.* L. Martin, Paris. Dated January 22, 1862.

This process refers to the treatment of natural oils, such as petroleum and rock oil, or to products obtained in the distillation of peat, wood, asphalte, and bituminous shale. It is carried out by submitting the crude oils first to the action of an aqueous solution of caustic soda of the strength indicated by 36° Beaumé; these are mixed in an agitator, and, after a sufficient interval, a small quantity of warm water is added; the alkaline solution being now separated, the partially purified oil is treated with diluted sulphuric acid, and finally with water. Having undergone the above operations, the oils are distilled from a retort heated by direct fire, or carried over by the aid of superheated steam, the products obtained below 400° Fahr. being mixed with colza or rape oil for illuminating purposes; whilst the distillate having a higher boiling point, from 437° to 600° Fahr., may be in like manner combined with cocoanut oil for similar applications.

204. *Manufacture of Colours for Dyeing and Printing.* W. Smith, Manchester, and C. Tiessel, Boulogne-sur-Mer, France. Dated January 27, 1862. (Not proceeded with.)

The object of this invention appears to consist in the preparation of aniline and certain coloured derivatives from coal-oils and hydrocarbons by processes of distillation.

220. *Improvements in the Means of Preserving Stone, Brick, Slate, Wood, Cement, &c., from the Injurious Action of Atmospheric and other Influences, also in the Application of Colours to the Surfaces of Stone, Brick, Wood, Slate, &c.* A. H. Church, Great Portland Street, London. Dated January 28, 1862.

For the purpose of protecting stone, &c., from decay, the patentee washes the surfaces alternately with a solution of silica in water, or with a liquid which may be considered chemically equivalent, viz., a mixture of silicate of potash or soda, and sulphuric, hydrochloric, or other mineral acid; which treatment is preceded or followed by an application of caustic baryta, or strontia, in aqueous solution.

With the first announcement of Professor Graham's discovery of the principle of dialysis, it was anticipated that his method would furnish the means of preparing a solution of silica in pure water, which might either directly serve for the hardening of stone, or be applicable, in conjunction with lime or baryta, to the purposes described by Mr. Church. These anticipations have not, however, been realised, for it has been found, when working on the large scale, impossible to produce in this way a solution of silica in water of a greater degree of concentration than about 5 per cent.

262. *Treating Fatty and Oily Matters for obtaining their Acidification.* P. Scheurweghs and A. J. A. H. De Boisserolle, Paris. Dated January 31, 1862.

According to this invention, the patentees effect the acidification of the fatty matters by the employment of a mixture of nitric and sulphuric acids; and for the purpose of raising the acid to the required temperature, and of causing the disengagement of nitrous acid gas, they add a certain quantity of alcohol, and the fat itself is melted prior to being brought in contact with the before-mentioned materials.

This operation is conducted in a closed vessel, so that the gases before escaping may be compelled to exert their full action; a slow current of air is introduced for the purpose of imparting motion to the two layers, and of ensuring proper contact. Towards the end of the process the speed of the air current may be accelerated, in order to cool the materials, and cause the fatty acids to solidify. A modification of the mechanical arrangements will permit of the operation being continuous.

The primary object of this patent appears to be very similar to that of M. De Moreau,* who employs starch in conjunction with nitric acid for effecting the same kind of conversion.

Grants of Provisional Protection for Six Months.

355. Henry George Williams and Rowland Gwynn Price, Bontddu, near Dolgelly, Merionethshire, "An improved machine for crushing and amalgamating auriferous quartz, and pulverising and washing ores." Petition recorded February 9, 1863.

731. William Lorberg, Wyld's Rents, Bermondsey, Surrey, "Improvements in the treatment of rags, and obtaining valuable chemical products from the animal fibre therein." Petitions recorded March 18, 1863.

761. William Clark, Chancery Lane, London, "Improvements in the separation or obtaining of ammonia from azoted matters in the preparation of manure." A communication from Lucien Henry Blanchard and Théodore Chateau, Boulevart St. Martin, Paris. Petitions recorded March 21, 1863.

878. Richard Archibald Brooman, Fleet Street, London, "Improvements in the manufacture of baryta and its derivatives, in obtaining by-products, and in revivifying or recovering certain agents employed in such manufacture." A communication from Julien Gabriel Lelong-Burnet, Paris. Petitions recorded April 7, 1863.

3410. William Perkins, Churchfield House, Margate, Kent, "Improvements in the manufacture of a substitute for turpentine, which is also applicable to the manufacture of varnishes, and to purposes to which turpentine is now ordinarily applied."—Petition recorded December 20, 1862.

742. William Reay, jun., Thropton Hill, near Rothbury, Northumberland, "An improved machine for amalgamating minerals and other metalliferous and metallurgical products or substances."—Petition recorded March 20, 1863.

815. John Dale, Manchester, and Gustav Bischof, jun., Swansea, Glamorganshire, "Improvements in the manufacture of aniline, naphthaline, and other analogous bodies, and in apparatus connected therewith, which apparatus is also applicable to obtaining finely divided metallic iron for other purposes."—Petition recorded March 28, 1863.

821. William Edward Newton, Chancery Lane, London, "An improved process for producing yellow colouring matters and other colours, which may be derived therefrom."—A communication from David Clovis Knab, Colmar, France.

849. John Cassell, La Belle Sauvage Yard, London, "Improvements in stills for the distillation of petroleum and other heavy oils."

859. William Henry Perkin, Seymour Villa, Sudbury, Middlesex, "Improvements in the manufacture of red and orange colouring matters."

861. Josiah Gimson, Leicester, "Improvements in the means of actuating shuttles in looms for weaving narrow fabrics."

863. Peter Spence, Newton Heath, near Manchester, "Improvements in the manufacture of sulphuric acid and sulphate of iron."—Petitions recorded April 4, 1863.

891. Arthur Kinder, Cannon Street, London, "Improvements in coating or covering lead or alloys of lead with tin or alloys of tin, and in the apparatus employed therein."—Petitions recorded April 8, 1863.

910. Robert Smith, Northampton Terrace, Crompton Road, Islington, London, "An improved medicated oil for the preservation of metal, wood, or stone."

928. James Lark, White Lion Wharf, Bankside, Southwark, Surrey, "Improvements in the manufacture of artificial fuel and cement."

998. Frederick Edward Bryant, Alfred Street, Bedford Square, London, "Improved apparatus for ascertaining the temperature of steam and its power of tension."—A communication from Hans Ambrouseous Königsberg, Prussia.

Inventions Protected for Six Months by the Deposit of Complete Specifications.

1020. Robert Lavender, Goswell Road, Islington, London, "A new compound to be used as a lubricator."—Deposited and recorded April 24, 1863.

1065. George Washington Fuller, Cambridge Port, Massachusetts, U.S., "A new and useful or improved submarine lantern to be used in explorations beneath the surface of the ocean or any other large body of water."—Deposited and recorded April 28, 1863.

Notices to Proceed.

3465. Frederick Tolhausen, Faubourg Montmartre, Paris, "The use of petroleum or coal oil as fuel, and also for machinery and apparatus to be employed for this purpose.—A communication from Edward John Biddle, Faubourg Montmartre, Paris.

3469. William Billinghurst and Josephus Requa, Rochester, New York, U.S., "An improved portable battery."—Petitions recorded December 29, 1862.

13. Frederick Collier Bakewell, Haverstock Terrace, Hampstead, "Improvements in apparatus for burning oils and other inflammable fluids as fuel."—A communication from Augustus Theodore Schmidt, Pittsburg, Pennsylvania, U.S.

126. William Johnson, Buchanan Street, Glasgow, Lanarkshire, N.B., "Improvements in the manufacture of chlorine and bleaching powder, carbonate of soda, and soda ash, and sulphate of iron."—A communication from Thomas Macfarlane, Acton Vale, Bagot, Canada.—Petitions recorded January 14, 1863.

185. William Clark, Chancery Lane, London, "Improvements in preparing and obtaining photogenic pictures or representations,—A communication from Adolphe

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* *Vide* Chemical News, vol. vi., p. 203.

Teissonniére, Boulevart St. Martin, Paris.—Petition recorded January 21, 1863.

272. Arthur Pritchard, Derby, "An improved method of preserving the contents of packages from air, water, or damp."—Petition recorded January 29, 1863.

667. William Wood, Monkhill, near Pontefract, Yorkshire, "Improvements in the manufacture and ornamentation of Pomfret or liquorice cakes, rolls, sticks, and pipes, and other similar articles of confectionery."—Petition recorded March 11, 1863.

3176. John Halford, Great Barr, Staffordshire, "Improvements in the preparation and treatment of small coal or slack, whereby a certain carbonaceous product is obtained and rendered available for use in the manufacture of iron and steel in the processes of casting and moulding metals, and in the manufacture of paint and such like articles."

3221. Peter William Reuter, Buckland Crescent, St. John's Wood, London, "The preparation of a new compound to be used for dyeing and printing purposes."—A communication from Edward Reuter, Paris.—Petitions recorded December 1, 1862.

307. William George Valentin, Oxford Street, London, and Frederick Levick, Blaina, Monmouthshire, "Improvements in generating combustible gases, and in the apparatus employed therefor."—Petition recorded February 3, 1863.

3317. Edward Toynbee, Lincoln, "Improvements in extracting oils and fatty matters from shoddy or refuse wool, skins, or skin pieces, glue pieces, cotton waste, and other animal or vegetable matter, and in producing an artificial manure." Petitions recorded December 10, 1862.

CORRESPONDENCE.

A New Hemostatic Agent.

To the Editor of the Chemical News.

Sir,—At a recent meeting of the Société des Sciences Médicales et Naturelles of Bruxelles, to which I have the honour to belong, the subject of a new hemostatic agent, lately discovered by Dr. Piazza, Professor of Organic Chemistry in the University of Bologna, was brought forward. This substance, destined, as its name indicates, to stop a flow of blood from wounds, appears to be one of the most successful therapeutic agents ever prepared, and I hasten therefore to call attention to its composition; being yet known only in Italy and Belgium, it is certainly destined to render great services in our Hospitals, &c. Before alluding to the manner in which it is prepared, I should perhaps state that the liquid in question has been experimented already with the greatest success in the hospitals of Parma, Modena, and Bologna; also in several hospitals in Bruxelles by members of the Society above named, and has been unanimously declared to be the most effective agent in stopping a flow of blood from a wound ever discovered.

In making numerous experiments upon the substances which are capable of coagulating blood, Professor Piazza found that the coagulum produced by chloride of iron was rendered so compact by an addition of chloride of sodium that the vessel containing the mixture may be reversed without a drop flowing out. No other coagulating liquid will produce such an effect as this. Now, when chloride of iron is used alone, it is necessary to employ it tolerably concentrated (25° to 30° Baumé) which occasions much pain and irritation. But if common salt be added, the liquid is efficacious with a much weaker solution (10° to 15° B.) of chloride of iron. The best means of preparing the new hemostatic liquid consists in taking a solution of chloride of iron marking 10° to 15° of Baumé's areometer, and adding to it an equal volume of a concentrated solution of pure chloride of sodium. One precaution must be attended to, namely, that the chloride of iron contain no free acid. This is avoided by evaporating the iron solution to dryness, and redissolving in water. The liquid is used by immersing in it the linen destined to bind the wound, &c.

I am, &c.

T. L. Phipson, Ph.D., F.C.S., &c.

London, May 12, 1863.

MISCELLANEOUS.

School of Practical Chemistry.—A Society has been formed in France for the purpose of establishing by subscriptions a chemical laboratory for the purpose of instruction. This project has originated with some manufacturers, who, considering the services rendered to the arts by chemistry, believe that still greater advantages would be gained if there were adequate means of acquiring a knowledge of the science.

SCIENTIFIC SOCIETIES.

MEETINGS FOR THE ENSUING WEEK.

May 25. *Monday.*

Geographical—15, Whitehall Place. 1 p.m. *Anniversary.*

Linnæan—Burlington House. 3 p.m. *Anniversary.*

26. *Tuesday.*

Medical and Chirurgical—53, Berners Street. 8.30 p.m.

Civil Engineers—25, Great George Street, Westminster. 8 p.m.

Zoological—11, Hanover Square. 9 p.m.

Royal Institution—Albemarle Street. 3 p.m. Professor Tyndall, "*On Sound.*"

27. *Wednesday.*

Society of Arts—John Street, Adelphi. 8 p.m.

Archæological Association—32, Sackville Street. 8.30 p.m.

28. *Thursday.*

Royal Institution—Albemarle Street. 3 p.m. Prof. Ansted, "*On Geology.*"

29. *Friday.*

Royal Institution—Albemarle Street. 8 p.m. Prof. Max Müller, "*On the Vedas.*"

30. *Saturday.*

Royal Institution—Albemarle Street. 3 p.m. Prof. W. Thomson, "*On Electric Telegraphy.*"

ANSWERS TO CORRESPONDENTS.

⁂ All *Editorial Communications* are to be addressed to the Editor, and *Advertisements* and *Business Communications* to the Publisher, at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

⁂ In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

Vol. VI. of the Chemical News, containing a copious Index, is now ready, price 10s. 8d., by post, 11s. 2d., handsomely bound in cloth, gold-lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. 6d. if sent to our Office, or, if accompanied by a cloth case, for 1s. Vols. I. and II. are out of print. All the others are kept in stock. Vol. VII. commenced on January 3, 1863, and will be complete in 26 numbers.

J. M.—The nitric acid is got rid of by heat: it may be detected by sulph in digotic acid.

A Practical Man.—The terminology of organic chemistry is no doubt a great obstacle to many students, but further study and perseverance will show you that it is not quite so absurd as you suppose. All attempts hitherto made to simplify it have proved failures. If the two substances quoted should even receive a technical application they will soon get simpler names.

H. M. H.—There is no light which will answer your purpose. Strongly naphthalized would give a very faint picture; but the time of exposure must be very long.

THE CHEMICAL NEWS.

Vol. VII. No. 182.—May 30, 1863.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Note on Iodine and Bromine, by John Horsley, *F.C.S.*

It is a curious fact, that if the smallest portion of a dry iodide or bromide be smartly triturated in a mortar with two or three times its weight of powdered red prussiate of potash, the odour of the metalloids is strongly evolved, which may be utilised in the preliminary examination of powders and the saline residue of the evaporation of water for those substances. Upon the application of a very slight heat in a crucible over a jet the vapours will be still more perceptible. Powdered bichromate of potash possesses this property in a far less degree than the red prussiate, as was subsequently proved by the addition of the latter salt to a mixture of bichromate and an iodine, when starch-paper was strongly tinged, and the mixture continued for hours to evolve iodine most potently.

Density of Vapours at Elevated Temperatures, by MM. Sainte-Claire Deville *and* Troost.[*]

The study of the analogies among simple and compound substances has advanced immensely since the time when the law of volumes established by Gay Lussac admitted of a new mode of comparison between them, which has since acquired a gradually increasing importance. M. Dumas, by devising productive experimental methods, has contributed to a generalisation of the law of volumes which did not exist when the discovery was newly made, and which the researches of Mitscherlich have still more extended. However, some substances appear to deviate from the law, not on account of their vapour density, as observed experimentally—for singularly enough it so happens that their vapour densities might have a simple relation to the theoretical numbers—but because that relation is not what would have been expected.

M. Cahours, by showing the variation, within certain limits, of the vapour densities according to their temperature, was the first to establish the rule followed at the present time, and to show the necessity of observing vapour densities at a temperature so high, in regard to the boiling point of liquids, that the density of the substances in the state of gas might be thenceforth determined. The advance made by this new method has been considerable, and gradually all vapour densities have been found to conform with the law of simple relations established by Gay-Lussac, Dumas, and Mitscherlich, for a great number of substances.

However, sulphur still constitutes an exception; to remove that exception it would be necessary to find a safe means admitting of the determination of the vapour density at a high temperature. This step MM. Deville and Troost have endeavoured to make, by extending the limits of the temperature that may be employed to the boiling point of zinc, which they have determined to be 1040° C.

The principles of that method[†] have already received numerous applications. Among other instances, they have compared the weights of equal volumes of vapour of iodine and of other substances at temperatures determined by the boiling of various liquids.

The liquids hitherto employed are mercury, sulphur, cadmium, and zinc, the respective boiling points of which are 350°, 440°,[‡] 860°, and 1040° C.

But these temperatures are still insufficient for a great number of substances, and the porcelain flasks[§] used for the observations are capable of supporting much higher temperatures without being altered in form.

The experiments were made in a muffle placed in a furnace of such form that the temperature could be always raised to nearly the same degree. The fuel employed was gas coke, which developes a great heat, and, by reason of its bulkiness, maintains the temperature almost constant.

Two porcelain flasks containing dry air, weighed with their stoppers[||] and surrounded with plates of platinum to prevent the glaze from adhering to the earthy walls of the muffle, were placed in similar positions in the muffle. One contained the substance under examination, the other iodine or dry air. These flasks were made in the same mould, and were of exactly the same capacity. When they had acquired the desired temperature they were both closed at the same time by means of an oxyhydrogen blow-pipe.

The data obtained in these experiments were compared with the theoretical values of the vapour densities, calculated by multiplying the equivalents of the substances by the density of hydrogen 0·0693. When the products thus obtained require to be doubled in order to equal, or be nearly equal to, the observed densities, the equivalent is considered as corresponding with one volume of vapour. When it requires to be multiplied by 1, $\frac{1}{2}$, $\frac{1}{4}$, the equivalent is considered as corresponding with two, four, or eight volumes of vapour. In these comparisons the numerical value of the chemical equivalents, adopted in almost all standard works, and employed in teaching, were used. Those

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[*] *Comptes-Rendus*, lvi., 891.

[†] *Annales de Chimie et de Physique* (3), lviii., 257.

[‡] M. Regnault has quite recently found the boiling point of sulphur to be 447·7°. This number does not affect the determinations of vapour density obtained by MM. Deville and Troost, which are quite independent of it. But in cases where some corrections require the use of this temperature, as it is a function of the co-efficient of expansion of the glass they used, and which they have not yet determined, they retain their number, not because it is more correct, but because it suits the conditions under which they experimented.

[§] The porcelain flasks used were made with great care by M. Gosse, of Bayeux.

[||] *Annales de Chimie et de Physique* (3), lviii., 273.

numbers might be doubled or quadrupled, as has been done lately, in order to represent, according to a particular system of explanation, the analogies of any substances with those related to it; but those alterations, though based on theoretical views, very often plausible and often satisfactory, are not by any means necessary.

The results obtained by MM. Deville and Troost are therefore expressed in accordance with the usage hitherto common among chemists, and in other respects they are consistent with the phenomena on which the fundamental principles of science are based.

Substances whose Equivalents correspond with One Volume of Vapour.

	Temperature. Degrees.	Density.	
		Observed.	Calculated.
Oxygen¶ . . .	0	1·1056	1·1082
Sulphur . . .	860	2·23	2·22
,, . . .	1040	2·23	2·22
Selenium . . .	1420	5·68	5·54
Tellurium . .	1390	9·00	8·93
,, . . .	1439	9·08	8·93
Phosphorus .	near 500**	4·35	4·29
,, . . .	1040	4·50	4·29
Arsenic . .	near 564††	10·6	10·38
,, . . .	860	10·20	10·38

It is interesting to remark that the oxygen group of elementary substances comprises only substances whose equivalents correspond with one volume of vapour. Two of them, sulphur and selenium, present the singular character of having vapour densities, or, what comes to the same thing, co-efficients of expansion in the state of vapour, which vary very considerably, within certain limits, with the temperature. The following data establish the fact for selenium:—

Temperature. Degrees.	Density.		
	Observed.	Calculated.	
860	7·67	5·54	$\frac{1}{3}$
1040	6·37	5·54	$\frac{7}{6}$
1420	5·68	5·54	$\frac{102}{100}$

This variation could not be determined in the case of tellurium, from want of the pure substance, by operating at temperatures lower than those stated. It would also be very interesting to ascertain whether the co-efficient of expansion for oxygen would augment at temperatures much lower than can be produced now, and, if so, in what ratio.

Substances whose Equivalents correspond with Two Volumes of Vapour.

	Temperature. Degrees.	Observed density.
Tantalum chloride . .	350	9·6
Niobium chloride . .	350	10·9

Substances whose Equivalents correspond with Four Volumes of Vapour.

	Temperature. Degrees.	Observed.	Calculated.
Ammonia hydrosulphate .	99·5	1·26	1·18
Monohydrated sulphuric acid .	440	1·74	1·70

The neutral ammonia hydrosulphate consists of four volumes of ammonia and two volumes of hydrosulphuric acid. According to Gay-Lussac's rule, they should condense to four volumes, as is found to be the case.

Substances whose Equivalents correspond with Eight Volumes of Vapour.

	Temperature. Degrees.	Density.	
		Observed.	Calculated.
Ammonia hydrochlorate .	350	1·01	0·93
,, ,, .	1040	1·00	0·93
,, hydrobromate .	440	1·67	1·70
,, ,, .	860	1·71	—
,, hydriodate . .	440	2·59	2·50
,, ,, . .	860	2·78	—
,, bisulphate . .	56·7	0·89	0·88
,, hydrocyanate .	100	0·79	0·76
Ethylamine hydrochlorate .	350	1·44	1·41
Aniline ,, ‡‡ .	350	2·19	1·83
Double ammonium and mercury chloride $HgCl + NH_4Cl$.	440	3·50	3·25

(To be continued.)

*Methods of Estimating Carbon in Iron and Steel,** *by Dr.* EGGERTZ.

THE method hitherto adopted for this purpose is both difficult and tedious. It requires great care, and apparatus which is not generally to be found in metallurgical laboratories. Dr. Eggertz has therefore sought to devise another method more convenient for the purpose.

He first tried dissolving the iron by copper chloride, burning the residual carbon by means of oxygen, and weighing the carbonic acid after absorption by potash.

The use of iodine instead of copper chloride has been recommended by some for dissolving iron, and the weight of the carbonaceous residue obtained was thought to afford an approximate indication of the amount of carbon. But for this purpose it is necessary to dry the carbonaceous residue under the air-pump, or at a temperature of 120° to 130° C.

Dr. Eggertz found that the quantity of carbon thus obtained was too great, though the weight of the residue was constant. It was also found that the residue contained iodine and water. To ascertain whether the quantity of carbon in the residue so obtained was uniform, he burnt the residue from several different kinds of pig iron and hard steel, and found that the average amount of carbon was 59 per cent. Other analyses of the residue obtained from white pig iron (free from graphite) gave as a mean result, after deducting the silica, the following as the composition of the carbonaceous residue:—

Carbon . . .	59·69	100	The variation in the amount of carbon in 4 analyses was 0·5 per cent.
Iodine . . .	16·07		
Water . . .	22·50		
Nitrogen . . .	0·13		
Sulphur . . .	0·23		
Loss	1·38		

Disregarding the sulphur and nitrogen, the ratios of carbon, iodine, and water are such as may be expressed by the formula $C80I + 20HO$, which would require—

Carbon.	Iodine.	Water.
60	15·86	22·50

The amount of carbon in the residue being taken as 60 per cent., since the quantity of this residue may vary to the extent of 1 per cent., owing to the different amount of sulphur in iron, the error in the estimation of carbon in steel containing 2 per cent. carbon would not amount to more than 0·03 per cent.

The carbonaceous residue does not change in weight when heated from 95° to 110° C.; but it loses 9 per cent.

¶ Dumas and Regnault.

** M. Dumas does not mention this temperature in his memoir; it is doubtless below 500° C.

†† Mitscherlich.

‡‡ And, doubtless, the salts analogous to it.

* Translated from the original by Mr. Sandberger, a Swedish metallurgist.

by heating to 150° C., and about 33 per cent. by heating to 240° C. Heated for a long time in a water bath with hydrochloric acid, its character seems to change, iodine and water being disengaged, and oxygen absorbed.

One gramme of iron—reduced to small pieces, or white pig iron crushed in a steel mortar, or grey pig iron in small chips—is mixed with 5 grammes of pure iodine and 5 cubic centimetres of water in a small flask, covered with a watch glass, and placed in ice-cold water before adding the iron. It is to be kept for twenty-four hours at 0° C., and frequently stirred meanwhile. By keeping the liquid cold, no carburetted hydrogen is produced. The greater the amount of silicium in the iron the greater is the tendency to the production of carburetted hydrogen.

The residue of carbon and silica left after the iron is dissolved, is collected on a filter of known weight, when it is dried at from 95° to 100° C., washed thoroughly with hot water. After twelve hours, it is to be washed with a mixture of hydrochloric acid and twice its volume of water, heated to 70° or 80° C., until the filtrate ceases to give a blue colour with ferrocyanide solution. The object of leaving the residue for twelve hours is to allow any small particles of iron, remaining undissolved by the iodine, being oxidised by atmospheric air, and prevent disengagement of hydrogen when the hydrochloric acid is added.

After the hydrochloric acid is washed out of the filter, it is dried with its contents at 95° to 100° C., until constant in weight.

This weighing gives the amount of the carbonaceous residue and silica (but not the whole of the silica, because some part of it would have been dissolved), and by burning the carbon away and weighing the silica the weight of the carbonaceous residue may be ascertained.

The carbonaceous residue may consist either of graphite or of the substance already mentioned, if the carbon was combined with the iron.

To ascertain which is the case, 1 gramme of iron is dissolved in 15 cubic centimetres of hydrochloric acid (1·12 density) in a flask covered with a watch glass, and, when the iron is dissolved, the solution boiled for half an hour. All the carbon combined with the iron is disengaged in the form of carburetted hydrogen gas, while the graphite and silica remain.

If the carbonaceous residue, left after dissolving the iron, comes in contact with atmospheric air before the liquid is boiled, it is so altered that it is not dissolved and disengaged as gas.

The graphite that remains after boiling the liquid is collected on a filter of known weight, washed, dried, and weighed. It is then burnt, and the residual silica weighed to ascertain the quantity of graphite.

A mixture of white and grey pig iron analysed in this way was found to contain 1·25 per cent. graphite. By dissolving a known weight of the iron with iodine, it gave 5·5 per cent. of carbonaceous residue. Deducting the amount of graphite, there remained 4·25 per cent. carbonaceous substance, and the amount of carbon in that being 60 per cent., the iron contained 2·55 per cent. combined carbon with 1·25 per cent. graphite.

Very satisfactory results have been obtained by this method. The differences do not amount to more than 0·1 per cent. When the quantity of carbon to be estimated is very small, more than 1 gramme of iron must be used in the analysis. The smallest amount of carbon that has been found in iron manufactured by Bessemer's method is 0·08 per cent. When the amount of carbon in bar iron is as much as 0·5 per cent., the iron is harder and more steely than ordinary bar iron. The hardest steel used for ordinary purposes seldom contains more than 1·5 per cent. carbon. Hammered knife steel has been found to contain 3·3 per cent. White pig iron contains from 2·7 to 4·5 per cent., the latter being "spiegel" iron containing much manganese, the former was iron obtained with too heavy a burden in the blast furnace. Damasked steel from Latoust was found to contain 1·25 per cent. combined carbon and 0·68 per cent. graphite.

When pig iron or steel is dissolved with nitric acid, a carbonaceous substance is produced, which colours the solution, and the larger the amount of carbon combined with the iron, the darker is the colour so produced. A solution of pure iron oxide in nitric acid has only a slight greenish colour if not much concentrated.

Nitric acid has no influence on the graphite contained in iron.

These facts serve for establishing a method of estimating carbon in iron.

One-tenth of a gramme of iron is dissolved in about 1·5 to 5 cubic centimetres of pure nitric acid (1·2 density) contained in a test-tube four inches long and half-an-inch wide. The larger the quantity of carbon in the iron the more acid must be used, for the carbonaceous substance dissolves more readily in the presence of a large quantity of acid.

When the iron is dissolved, the test-tube is to be heated in a water bath to 80° C., at which temperature the carbon is dissolved and carbonic acid produced. When the production of carbonic acid ceases completely, which in the case of steel takes place after two or three hours, the solution in the tube is allowed to cool, and transferred to a graduated burette without allowing any of the dark particles remaining undissolved to pass with it. A few drops of nitric acid is to be added to this residue, and heat carefully applied. If there be no evolution of gas from the particles, they consist altogether of graphite or slag.

The solution is then diluted with water until it is exactly of the same colour as a normal solution, so prepared that its colour is the same as a solution of iron containing in each cubic centimetre a quantity of carbon equal to 0·1 per cent. of carbon in the iron. Thus, if the solution of iron in the burette amounted to 7 cubic centimetres, then the amount of carbon in the iron under examination would be 0·7 per cent.

As it is difficult to dissolve 0·1 gramme of iron in less than 1·5 cubic centimetre of nitric acid, it is not possible to determine carbon in iron by means of the above-mentioned normal solution when it amounts to less than 0·15 per cent. of the iron. Such a case, however, seldom occurs. So long as the amount of carbon in the iron is less than 0·5 per cent., the solution is so concentrated that its colour inclines to green, which causes some difficulty in comparing it with the normal solution. In such a case it is better to make a more dilute normal solution by mixing 6 cubic centimetres of water with 3 cubic centimetres of the ordinary normal solution. This gives a solution corresponding to an iron solution containing in each cubic centimetre a quantity of carbon equivalent to 0·033 per cent. of carbon.

When the amount of carbon is very large, as in white pig iron, only 0·05 gramme is to be used for the assay and then 0·5 cubic centimetre of the solution brought to the same colour as the normal solution represents 0·1 per cent. carbon in the iron.

To ascertain the amount of combined carbon in iron also containing graphite, this latter substance must be separated by filtration from the solution of iron before it is introduced into the burette.

The normal solution is prepared by dissolving cast steel, containing a known amount of carbon, in so much nitric acid (1·2 density) that each cubic centimetre may represent 0·0001 gramme of carbon. If, for instance, the steel contains 0·76 per cent. of carbon, and 0·1 gramme is dissolved, it will make 7·6 cubic centimetres normal solution. To compare the normal solution with the solution of iron under examination, it should be contained in a tube of the same kind, and when the two tubes are held together by daylight before a thin sheet of paper, the colour should be exactly the same in both of them.

As the normal solution alters slightly in colour by keeping, and begins to become paler after twenty-four hours, it is not possible to keep such a solution for use in a tube hermetically sealed.

A solution of burnt sugar* in weak alcohol gives a solution of exactly the same shade of colour as the normal solution, and maintains its colour for a considerable time when protected from the light. But the best plan is to make the solution fresh, as it is required, by dissolving 0·1 gramme of steel, containing a known amount of carbon, in 5 cubic centimetres nitric acid, and diluting it to the requisite degree, which may be indicated by a mark upon the tube corresponding to the percentage of carbon in the steel.

If an iron solution exactly the same colour as the normal solution is diluted with one-tenth its bulk of water, the colour becomes distinctly paler, so that the delicacy of the method may be judged of from this.

At several iron works where the Bessemer method is in use, this mode of estimating the amount of carbon in iron and steel has been adopted, and has introduced great facility and certainty into the assortment of the steel, which was previously regulated by trials of forging and tempering, which were not very safe.

The most simple plan of adapting this method to practice seems to be that of indicating different degrees of hardness in iron and steel, by dividing the quantity of carbon into thousandths. It must be remembered, however, that there are differences in the iron obtained from different ores, and in the hardness and amount of carbon in iron resulting from the manufacture. By heating steel to a high temperature for welding the amount of carbon is often reduced.

New Mode of Producing the Anhydrides of Monobasic Acids.

M. H. Gal† has obtained acetic and benzoic anhydrides by the reaction of the corresponding chlorides with anhydrous baryta. It is necessary to avoid using an excess of baryta. Anhydrous lime and oxide of lead produce the same result; the latter at a temperature of 150°; the former with such violence as to become incandescent. The reaction with acetyl chloride commences at the ordinary temperature, and is completed after the mixture has been kept for a few hours at 100° C. In the case of benzoyl chloride, a temperature of 140° to 150 C. is required.

TECHNICAL CHEMISTRY.

The Chemistry of Agriculture.

The *resumé* of Baron Liebig's own views with regard to this subject, combined with his criticism of the views held by others, as given in the introduction to the new edition of his work on Agriculture, possesses, in many respects, so much interest, that some account of it will not be out of place, especially as this introduction is not translated in the English edition of the book.

It commences with a brief sketch of the condition of agriculture before 1840, and another of its condition after 1840.

Towards the end of the last century nothing was known by agriculturists of the causes upon which the fertility or barrenness of land depended. Sunshine, dew, and rain were almost the only conditions that were regarded by the farmer as necessary for the growth of plants. Many considered the soil to serve only as a support for the plant in growing. But for centuries it had been known that the tillage of land increased its fertility, and that this was also increased by manure.

The efficacy of manure was ascribed to a specific virtue, acquired by the materials of animal food in passing through the system, and not capable of being produced artificially. It was considered that with a proper stock of cattle, and a suitable rotation of crops, manure might be produced on a farm in any quantity, and without cessation; and, as the crops were often increased in proportion to the industry and skill of the farmer and the adoption of a proper system of rotation, it came to be generally considered that to obtain abundant crops and to render barren land fertile was only a matter of art. While one man was ruined in a farm, another grew rich in it; the produce depended on the farmer.

It was thought that seeds and land possessed powers by which crops were produced; that land became exhausted by the growth of plants as animals by labour, and that its powers were restored by rest or by manuring. Both crops and manure being products of the land, it was thought to resemble a machine in reproducing power when a fraction of its produce was returned to it.

At a later period, the source of this power of land was considered to be humus—an indefinite combustible substance of organic origin, a kind of manure—that the fertility of land was proportional to the amount of humus it contained, and that this might be increased by manure or by cultivation.

The truth of this view consisted merely in the fact that there is a greater growth on fertile land than on barren land, and that in rich land, for that reason, there is a greater accumulation of organic remains.

According to this view, the immediate cause of fertility was a power latent in the soil, and capable of being brought into activity by art—like the nutritive and medicinal powers of the older physiologists—and the efficacy of this power depended upon a cyclical metamorphosis of substances, which, in the form of humus, supported the life of plants, and in the form of plants supported animal life, while returning to its original condition. It was considered that this power existed generally in land, and that the nature of the land was of little consequence.

Humus being regarded as the source of fertility, its absence was of course the reason of barrenness. The efficacy of marl, gypsum, lime, &c., in augmenting the fertility of land was ascribed to their acting as stimu-

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[*] Slightly burnt sugar gives a yellow solution; highly burnt sugar gives a brown solution, but by mixing these solutions the right colour may be obtained.

[†] *Comptes-Rendus*, lvi., 360.

lants much in the same way as condiments act upon the digestion. The efficacy of bones was ascribed to the organic substance they contain.

Cultivation was based upon the production of manure, as the means of restoring the lost power of land, and thus obtaining a succession of the same crops.

Some plants, such as fodder plants, were regarded as manure-makers, and manure was held to make crops. Fodder was the chief thing; plenty of fodder made plenty of flesh and manure; plenty of manure made large corn crops. With enough fodder, corn followed necessarily.

It was an established principle that manure was the raw material, which, by the farmer's skill, was converted into corn and flesh: only corn exhausted land, fodder crops improved it. When the consecutive cultivation of graminous plants on the same land failed to give remunerative crops, the land was said to be worn out, but when other crops would no longer grow upon it, then the land was said to be sick. Thus there were two distinct ideas of the same phenomenon. The exhaustion of corn-fields was ascribed to a deficiency of soil constituents; the unproductiveness of fodder fields to a disturbance in the normal action of the land. The remedy for the former was manure; the latter was treated like a sluggish horse—with the whip.

The practical farmer pursued his craft like a shoemaker who forgot that his stock of leather would not last for ever. It never entered his mind that the plant, as a living thing, has special requirements, but he worked his land as if it were a piece of leather that grew at one end as fast as it was cut at the other. Manure was the means of making the leather pliable.

At agricultural schools was taught the art of cutting the greatest quantity of shoes from the inexhaustible supply of leather. The farmer who continued to obtain large crops was considered skilful, though he owed to his land what others, with equal industry and intelligence, could not obtain from theirs.

The innumerable instances of diminished fertility in all countries was confidently ascribed to the ignorance of the farmer, to the want of manure or tillage. The man who grew good crops of clover and turnips, could not comprehend that others were unable, by the largest application of labour and manure, to get a good crop of clover from their land, nor could he imagine that his land would ever fail to yield crops of clover or turnips.

At this period the investigations of De Saussure and of Davy received no attention from the practical farmer; he regarded them as having no connection with practical agriculture. The literature of that period was monopolised by descriptions of different systems of agriculture, Danish, English, French, and German. In Germany a small plot of land at Möglin served as the model farm. There it was believed to have been ascertained that a given quantity of manure produced an equivalent of corn, and that this must be true for every other place, on the principle that manure was the raw material for making corn or flesh. The manures which were beneficial at Möglin were thought to be so everywhere else. Bones did not affect the corn crop there, consequently they would not elsewhere. But among the various kinds of good advice that farmers gave each other, they paid no heed to geographical situation or altitude, annual rainfall, the distribution of rain in different seasons, extreme and mean temperatures, the physical geognostic and chemical characters of land.

"Theory" was the term applied by the practical farmer to the accidental conjectures and explanations as to the phenomena of cultivation, and it was held as self-evident that "theories" had no value; that the farmer should be guided, not by "theories," but by "circumstances" and "conditions," but what they were he did not know. "Ability" or "practice" was the main thing; whether or not it was known what "ability" depended upon was no matter.

Practical people regarded agriculture as an art; its results dependent upon skill. They held to this opinion for centuries, cultivating fertile land until famine came, and then—when fodder plants would no longer grow, and even land rich in humus produced no manure—the experienced man was helpless as a child; what he called experience had no basis.

An eminent practical man* of that time thus represents the capability of science to aid agriculture:—"If science were to place in our hands the means of growing clover, lucerne, &c., on the same land with equal advantages, and oftener than is the case according to present experience, the philosopher's stone would be found for agriculture, for then we would soon be able to provide for their conversion into the necessaries of man."

Towards the end of the last century the farmer had found in gypsum and marl means of increasing, without humus or manure, his crops of clover, and, consequently, the production of manure, and when these magical materials would no longer serve that purpose, when his skill and experience was no longer sufficient to effect this object, science was invited to contribute a fragment of philosopher's stone for the purpose of making clover, and turnips, and beans grow again. No one could say how long he could calculate upon obtaining crops. Most believed they would have no end, and that the fault was not in the land if it ceased to be fertile. The practical farmer knew that his ancestors had obtained as good or better corn crops from the same fields without any extraneous supply of manure, but it never entered his mind to reflect why the fodder crops no longer grew so well as formerly. He could not conceive that the true cause of the deficiency of manures, by which he was embarrassed, was in the land.

But the practical farmer is as he was thousands of years ago. The avowed enemy of all "theory," he has built up the theory that land is inexhaustible in fertility, and the modern farmer directs all his proceedings by the theory that the extraneous sources whence he derives the means of restoring the productiveness of his land are inexhaustible. It does not trouble him what will become of the land or of the population if these sources were exhausted. The careless and ignorant housekeeper holds to the belief that to-morrow will be as to-day is.

These were the prevailing ideas of practical agriculture up to the year 1840. At that time chemistry was so far developed as to participate in the development of other arts and sciences, and the investigation of the phenomena of animal and vegetal life brought it into contact with agriculture. The alteration of atmospheric air by vegetation—the influence of carbonic acid in plant growth—and the elimination of oxygen by the influence of sunlight and chlorophyl—were already known, but the sources of the hydrogen and nitrogen were still uncertain; it was believed that the presence of the saline and earthy substances which remain after incinerating plants was only accidental, that these substances varied according to situation.

But when the chemistry of vegetation came to be

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* S. Walz, Director of the Agricultural Academy at Hohenheim, *Die Ernahrung der Cultur-Pflanzen*, 1857.

studied, these ashes were found to be constant in all cases; that, instead of being accidental, they were essential to the growth of plants, being in plant nutrition what bread and meat or fodder are in animal nutrition; that fertile land contained these substances in abundance, while barren land contained but little of them; and, that barren land became fertile when these substances were added to it.

The necessary consequences of these facts was that land must gradually become unproductive as the stock of these substances is diminished by cultivation, and it also followed that to maintain the fertility of land these substances must be returned to it in the same amount that they are abstracted; if the restoration is incomplete, a continuance of the same crops could not be effected; and the augmentation of the crops could be effected only by increasing the quantity of those substances in the land.

Chemistry also showed that animal nutrition resembled in some sort the burning of fuel in a furnace. Animal excreta, containing the mineral substances of the food consumed, resembled the ash of fuel. Hence the fertilising efficacy of manure was easily intelligible, since it restored to the land what had been abstracted from it in crops. It also became evident that the fertility of land could not be maintained by the manure of cattle, because it did not restore the mineral substances contained in the corn and meat sent away from the land. Consequently, it was evident that the farmer required to replace, from other sources, those substances that were wanting in cattle manure, in order to maintain the fertility of his land. If this were not done, the land would eventually become unproductive.

The true object of the farmer is to obtain larger and larger crops permanently; and if he would take the pains to reflect, it would be evident to him that it is a fallacy to suppose any art or skill can be effectual in producing a remunerative return from land that has not a suitable composition. All that lies in his power is to ascertain the deficiencies of land, and to overcome the obstacles to a remunerative return for the care he bestows on it. The "circumstances" and "conditions" according to which he regulates his system of cultivation are natural laws which he must know, in order to take advantage of them, otherwise he would remain only subject to them.

Nothing that science teaches would divert him from his true object, but, on the contrary, it alone can give his exertions the right direction and result. His skill and experience would be quite indispensable in order to render his knowledge of these "circumstances" and "conditions" useful and advantageous. "Knowledge" is not the antithesis of "ability," but merely the means of rightly exercising "ability."

The history of science shows that its progress does not consist in the development of the doctrines prevailing at any period, but in their substitution by doctrines directly opposed to them. The development of a false doctrine may take place in the same way as that of a sound doctrine; but the one dies out for want of foundation, while the other grows and expands. The one leads to conclusions that are recognisably impossible, and then it must give place to another that is the opposite of it, inasmuch as truth is always the opposite of error.

Thus, the phlogistic theory, which regarded combustion as a decomposition, was succeeded by the antiphlogistic theory, which regarded combustion as a combination; but it must be remembered that the new doctrine was a consequence of the development of the older one. This was necessarily abandoned when it led to the absurd conclusion that phlogiston possessed negative gravity, making substances lighter by combining with them and heavier by separating from them.

So stood the new doctrine of plant life, in relation to the older doctrine, that regarded the food of plants—which contributed to their increase in mass, and determined their agricultural production—as being of organic nature, and produced in the organisms of plants or animals. The new doctrine, on the contrary, taught that the food of all plants was inorganic; that mineral substance became in the plant organism, the medium of organic activity; that the plant produced from inorganic substances all the constituents of its structure, and built up out of simple substances, the highly complex constituents of which the animal organism is constituted. This theory, by reason of its antithetic relation to the older doctrine, received the name of the "mineral theory."

Use of Sulphurous Acid in the Manufacture of Sugar.

M. A. Reynoso* points out that the use of calcium bisulphite is prejudicial rather than advantageous. The excess of acid uniting with oxygen produces sulphuric acid, which reacts upon the sugar, converting it into grape sugar, and eventually into ulmic and formic acids, and ulmin, thus tending to produce colour, instead of bleaching the saccharine liquid. Hence, he states that when the sulphite is used it should always be with an excess of lime. It is indispensable that the cane-juice should be so alkaline as to turn litmus-paper blue. In the absence of litmus-paper, the alkalinity of the cane-juice may be ascertained by its turbidity, and by the formation of a pellicle on the surface of a small portion when it is breathed upon.

JURY REPORT ON THE INTERNATIONAL EXHIBITION OF 1862.

Class 2.—Section A.

CHEMICAL MANUFACTURES.

By Dr. A. W. HOFMANN.

Numerous inquiries as to the chemical report on the Exhibition having been made, the following abstract has been taken from the *Moniteur Scientifique*, in which a portion of the report has just been published:—

The manufacture of soda from salt by Leblanc's method is so necessarily connected with the production of sulphuric acid and the hypochlorites, that they require to be considered together, and in conjunction with the production of hydrochloric acid, they may be considered as the principal basis of manufacturing chemistry.

The manufacture of sulphuric acid on a large scale dates from 1746, when Dr. Roebuck constructed his first vitriol chamber at Preston Pans. The manufacture of soda from salt is rather more recent. Its progress in this country was slow at first, until, in 1823, it was developed by Mr. James Muspratt. The manufacture of chloride of lime was established in 1799 by Mr. Charles Tennant, of Glasgow.

In all essential particulars the manufacture of sulphuric acid and of soda has not changed since the year 1851, but, independent of a vast development in these branches of industry since that time, the improvements in the products, and the reduction of price, that have taken place since then, indicate considerable progress in the details of the manufacture.

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* *Diario de la Marina* and *Comptes-Rendus*, lvi., 46.

The changes that have been introduced in the manufacture of sulphuric acid during the last ten years appear at first sight unimportant. Though the method has not been essentially altered, it is not from want of proposals of alteration. An examination of some of them will serve to show the directions in which this manufacture has need of improvement.

Attempts to replace Lead Chambers by less costly arrangements.—The object of the manufacturer being to produce, from a given quantity of sulphur, the largest quantity of sulphuric acid, with the least outlay of capital and labour, invention has been exercised in various directions to attain this end. With the view of doing away with the cost of lead chambers and the presence of lead in the acid, it has been proposed to substitute stone, vulcanised gutta-percha, slate, or a compound of sandstone and sulphur. None of these substitutes have been found to answer the purpose.

Mr. Ward has recently proposed to introduce into the lead chambers a series of glass plates, with the view of increasing their surface and accelerating the production of acid. This idea is not new, glass partitions having been employed in France during the last ten years. They are of no practical importance, since the production of acid is independent of the surface of the chamber.

Since the proposal made in 1832 by Mr. Phillips and M. Kuhlmann, to effect the conversion of sulphurous acid into sulphuric acid by means of atmospheric oxygen heated in presence of finely divided platinum, the idea of producing oil of vitriol without lead chambers, has never been lost sight of. The jury report of 1851 contained some details relating to a system of stoneware vessels, suggested by M. Lepelletier, which were said to be in use at that time in the well-known factory of Javel. The condensing power of this arrangement of Woulf's bottles was said to be one-third more than that of a chamber of the same capacity, while the first cost of construction was in proportion to that of a lead chamber as 12 : 100, and the cost of keeping them in working order, nothing. It was stated that one-third of the vitriol produced at Javel in 1857, amounting to 3,600,000 kilogrammes, was manufactured with this arrangement. However, the use of these vessels has been abandoned at Javel, and it would appear that they were used only as supplementary to the lead chambers for the better condensation of gases disengaged, as is still the case in many French factories.

M. Persoz has proposed to effect the production of sulphuric acid by passing sulphurous acid gas through nitric acid, diluted with six times its volume of water, and heated to 100° C., or through a mixture of nitric and hydrochloric acids. This reaction is effected in a comparatively small apparatus, and is facilitated by means of an agitator. The gases disengaged by the reduction of the nitric acid pass into condensing towers, where they meet an ascending current of air and a descending current of water, so as to recover the whole in the state of nitric acid. This method appears at first sight satisfactory, not only in dispensing with lead chambers, but also in avoiding loss of nitric acid, at least from a theoretical point of view, and in admitting of the use of sulphurous acid derived from any source, even when mixed with carbonic acid, nitrogen, or other gases. However, it has never been employed in practice, evidently from the want of a material capable of resisting the action of these strong acids.

M. Kuhlmann has proposed passing a mixture of sulphuretted hydrogen and air through nitric acid, which converts nearly all the sulphur into sulphuric acid. This plan also has not been adopted in practice.

The same is the case with the method proposed by Mr. Petrie of passing regulated currents of sulphurous acid, air and nitric acid through columns of stoneware filled with flints.

Mr. Gossage has endeavoured to recover the sulphur of soda waste in the form of sulphuretted hydrogen, by decomposing the waste by means of carbonic acid, obtained from combustion of coal. The gas so obtained contains so much nitrogen and other gases that it will not serve for producing sulphurous acid for use in chambers. He has endeavoured to overcome this difficulty by burning the sulphuretted hydrogen with atmospheric air, and forcing the products of combustion, previously cooled, to pass up through a column containing small fragments of coke, moistened by a continuous current of cold water, so as to obtain a saturated solution of sulphurous acid. This solution is allowed to filter down through another coke column, through which a current of hot air is passed, setting free the sulphurous acid, and converting it to a great extent into sulphuric acid. The residual gas is sent into a leaden chamber with nitrous gas, where it is converted into sulphuric acid in the usual manner. This method, however, does not succeed in practice.

Mr. Petrie has sought to reduce the consumption of nitric acid by passing a mixture of sulphurous acid and air, heated to 300° C. through a shower of water, descending through stoneware cylinders containing flints; and MM. Schmersahl and Bouck pass a mixture of sulphurous acid, air and steam through horizontal tubes of earthenware or cast iron, filled with asbestos or pumice-stone, &c., and heated in a furnace. The vapour of sulphuric acid produced is condensed in suitable condensers. In 1852, Professor Wohler drew attention to the remarkable facility with which oxide of copper, sesquioxides of iron or chromium heated to redness, convert a mixture of sulphurous acid and oxygen into sulphuric acid. This method has been tried on a large scale at the Oker Factory, but the results have scarcely been satisfactory.

Attempts to Produce Sulphuric Acid without using Nitric Acid.—The immense production of hydrochloric acid in the manufacture of soda has led to the idea of using it in the state of chlorine for the conversion of sulphurous acid into sulphuric acid. The reaction that takes place between sulphurous acid, chlorine, and water is the basis of the method proposed by Hœhner. When the operation is well conducted neither sulphurous acid nor chlorine escape alteration. The sulphuric acid produced contains an equivalent of hydrochloric acid, which can be separated by distillation, or the mixed acids may be used in the manufacture of sulphate of soda. The value of this method evidently depends on the ruling price of manganese, which is requisite for generating the chlorine, as compared with that of nitrate of soda requisite to furnish nitric acid.

Hitherto no substitute has been found for nitric acid as the oxidising agent in producing sulphuric acid; but the mode of using this acid varies much. In most factories the acid vapour, disengaged from the nitrate by means of sulphuric acid, is conducted at once into the chambers; in others, liquid nitric acid, prepared by distillation, is employed. For some time it was found convenient to deoxidise the acid by means of molasses, which thus yielded oxalic acid; but this practice seems to have been abandoned. Mr. C. Dunlop introduced a method of obtaining nitrous acid by decomposing a mixture of nitrate and chloride of sodium with sulphuric acid. The nitrous acid was separated from the chlorine by passing the mixture through concentrated sulphuric acid and the chlorine used for making bleaching powder. The solution of nitrous acid in sulphuric acid was made to flow into chambers, where it was mixed with water, which disengaged the nitrous acid. The products of the action of sulphuric acid upon a mixture of nitrate and chloride of sodium vary to a certain degree according to the concentration of the acid and the temperature. The principal product, besides sulphate of soda and chlorine, is probably nitrous acid.—

$$2\mathrm{NaNo_5} + 4\mathrm{NaCl} + 6\mathrm{H_2SO_4} = \begin{cases} 6\mathrm{NaOHSO4} + \\ 4\mathrm{Ci} + 3\mathrm{H_2O} + \mathrm{N_2O_3} \end{cases}$$

In Messrs. Tennant's works the method proposed by Gay-Lussac for absorbing the nitrous acid that escapes from the vitriol chambers is adopted. Mr. Dunlop's method is employed only to supply a quantity of nitrous acid equivalent to the loss always sustained, notwithstanding the adoption of Gay-Lussac's method. Hence it appears that in these works sulphuric acid is produced without an express consumption of nitrate for obtaining the nitrous acid which serves for the oxidation of the sulphurous acid.

Besides those cases in which sulphurous acid is obtained by the combustion of sulphur in atmospheric air, sulphurous acid is also obtained from the oxidation of iron pyrites and copper pyrites. It may also be obtained by the decomposition of some metallic sulphates, gypsum, or heavy spar. Fremy has shown that gypsum or heavy spar calcined with sand or clay, are converted into silicates, and the sulphuric acid is disengaged as sulphurous acid and oxygen, which may be passed at once into the vitriol chambers. This reaction at first sight seems promising; the materials are abundant and cheap, both sulphurous acid and the oxygen requisite for its conversion into sulphuric acid, are produced together, and hence the immense admixture of nitrogen, introduced when atmospheric air is employed for oxidising sulphurous acid, is avoided, and the vitriol chambers may be proportionally reduced in size. But unfortunately this reaction takes place only at very high temperatures, and with difficulty. To facilitate the decomposition of gypsum it has been proposed by Cary-Mantrand to employ hydrochloric acid, but this method involves difficulties hitherto insurmountable, for not only must the acid be used in the anhydrous state, but the chloride of calcium first produced, melts and protects the remaining sulphate from further decomposition. The proposition made by Kœhsel to reduce gypsum by means of coal to sulphide, to decompose this by carbonic acid, and to convert the sulphide of hydrogen into sulphurous acid by combustion, has been equally unfortunate in its results. This proposition was not new, for the decomposition of calcium sulphide by carbonic acid, and the conversion of hydrogen sulphide into sulphurous acid by combustion, was suggested by Mr. Gossage in 1838.

Attempts have also been made to substitute hydrogen for the metals in sulphates, and thus to obtain directly sulphuric acid from them. One of the first methods proposed to effect this, originated with H. Seckendorff and Mr. Shanks. It consists in decomposing finely-powdered gypsum in the presence of water, by means of lead chloride, then treating the lead sulphate produced, with hydrochloric acid, which liberates sulphuric acid, the lead chloride obtained at the same time being available for decomposing a further quantity of gypsum. This method, ingeniously based upon chemical reactions, would serve to utilize the hydrochloric acid which causes so much embarrassment in soda factories. It was worked for some time on a large scale, but the operations are complicated, and the difficulty of obtaining the acid free from lead is almost insurmountable. Hence it has been abandoned.

Among other reactions of a similar nature, but whose applicability is still problematical, are the decomposition of lead phosphate by hydrochloric acid, suggested by Margueritte, and the decomposition of calcium sulphate at a red heat by the phosphoric acid thus obtained. The calcium phosphate produced at the same time, yields a fresh quantity of lead phosphate by treatment with lead chloride, in the presence of water. Keller proposed to decompose lead sulphate by the action of hydro-sulphuric acid—obtaining this latter by heating to redness a mixture of gypsum or heavy spar and coal, in an atmosphere of steam, or by acting upon soda waste in the same manner. These methods are evidently incapable of industrial application.

The production of sulphurous acid from pyrites is the most striking modern improvement in the manufacture of sulphuric acid. Scarcely twenty years ago, almost all the sulphuric acid manufactured, was produced from Sicilian sulphur; at the present time some is still produced from the same material; but at least nine-tenths of the whole quantity now made is derived from pyrites. According to general opinion, the idea of employing pyrites for this purpose arose from the narrow policy of the Neapolitan Government in prohibiting the export of sulphur in the year 1838, but in reality the indirect application of pyrites in the manufacture of sulphuric acid is of much older date. From the latter part of last century up to the present time, pyrites has always been employed as a source of sulphur. In many parts of Germany—the Hartz, in Prussia, Bohemia, and Croatia—considerable quantities of sulphur have been produced from pyrites. Even in Ireland pyrites was at one time used as a source of sulphur, during the war with France, when the price of sulphur rose to 20*l.* and even 30*l.* per ton.

There are different methods of extracting the sulphur from pyrites. Sometimes it is roasted in conical tubes, placed on an incline in a furnace, and closed at the upper widest end, the sulphur vapour being condensed in iron vessels. Sometimes the mineral is heaped up in thick layers, and ignited at the lower part, a moderate current of air being admitted below, so that the heat developed by the combustion of the lower layer suffices to expel the sulphur from that above. However, the production of sulphur from pyrites is insignificant compared with the quantity furnished by Sicily; the entire quantity obtained in Prussia during the year 1858 was not more than 500 tons.

Pyrites, partially deprived of its sulphur by roasting, has long been employed in the manufacture of iron sulphate, which is used in making the fuming sulphuric acid of Nordhausen. At Fahlun, the sulphurous acid required for making sulphuric acid has long been obtained from the pyrites which is so abundant in that part of Sweden. In France the idea of using pyrites as a source of sulphur appears to have been originated by Clément Desormes. The numerous experiments that he made with this view failed in consequence of his mixing coal with the pyrites to increase its combustibility. The large admixture of carbonic acid with the sulphurous acid which resulted from this plan, deranged the action of the chambers. MM. Perret, of Chessy, were the first who employed pyrites successfully in France; they were led to do so by the necessity of condensing the sulphurous acid evolved in the roasting of a mineral from which they extracted copper. They studied the conditions necessary for the combustion of the mineral, and it is to them that the credit is due of having first overcome the difficulties attending the solution of this problem. In 1833, these manufacturers were already successfully burning pyrites, and their method was described in the specification of a patent obtained in 1835. In 1837, Wehrle and Braun employed iron sulphide in Bohemia, to produce sulphurous acid, but it was not until the year 1838 that the manufacture of sulphuric acid by means of pyrites was commenced on a large scale in England. In that year the late King of Naples conceded the monopoly of the sulphur trade of Sicily to MM. Taix and Co., of Marseilles, and the price rose from 5*l.* to 14*l.* per ton. The consequence naturally was that it became necessary to obtain other sources of sulphur, and in less than a year after this absurd monopoly had been established numerous methods of obtaining sulphurous acid from pyrites had been devised. During that time fifteen patents were taken out for this purpose. It has been believed that Mr. Farmer was the first who, in 1839, employed pyrites for the manufacture of sulphuric acid in England; but later information leads to the conclusion that Mr. James Muspratt burnt pyrites at his works long before that date.

The sulphur monopoly had but an ephemeral existence, and the prohibition was soon withdrawn by the Neapolitan Government; but the spirit of independence had been roused, and the use of pyrites has continued to increase since that time to the present, resulting in a total change in the source of sulphur employed in the sulphuric acid manufacture.

Pyrites is almost always mixed with small quantities of foreign substances capable of affecting the purity of the sulphuric acid produced from it. The discovery of selenium is due to the use of pyrites in the manufacture of sulphuric acid at Fahlun. Lately a similar discovery has been made by Mr. Crookes in the deposit from a vitriol chamber. But selenium and thallium are inoffensive substances, while, on the contrary, pyrites frequently contains arsenic. It is to this circumstance that may be attributed the fact that the use of pyrites, has not yet become universal. In the combustion of arsenical pyrites the arsenic passes into the vitriol chamber as arsenious acid, and contaminates the sulphuric acid. In using this acid for the soda manufacture the arsenic is again eliminated, and hence sulphuric acid derived from pyrites is almost always used in this manufacture.

But there are many applications of sulphuric acid in which the presence of arsenic could not be tolerated. Thus, for instance, in the manufacture of tin plates, the metal, before immersion in the tin bath, requires to be cleaned by plunging it in dilute sulphuric acid, and if this acid contains the least arsenic, it is deposited upon the iron here and there, preventing the adhesion of the tin at those parts. Moreover, the sulphuric acid employed in the manufacture of substances used in pharmacy or in domestic economy must be free from arsenic. Some years ago, traces of arsenic found in unfermented bread were ascertained to originate from arsenical pyrites, by the preparation of the hydrochloric acid used in making the bread, with sulphuric acid obtained from arsenical pyrites.

(To be continued.)

NOTICES OF BOOKS.

Lectures on Coal-Tar Colours, and on Recent Improvements and Progress in Dyeing and Calico Printing: embodying Copious Notes taken at the International Exhibition of 1862, and Illustrated with Numerous Specimens of Aniline and other Colours. By Dr. F. C. Calvert, F.R.S. Manchester: Palmer and Howe.

Though Dr. Calvert commences his Lectures with the remark that he does not presume to give a full account of all the improvements and inventions which have been made in the extensive arts of dyeing and calico printing since the Exhibition of 1851, a perusal of these Lectures is sufficient to convince any one conversant with this subject that he has contrived to condense into this small pamphlet of 64 pages a surprising amount of very valuable information with regard to the progress of these arts during the last ten years.

To those unacquainted with the arts of dyeing and calico printing, it will at once be apparent that during this period there has been a most surprising advance in regard to the means of obtaining dyeing materials—an advance consisting in discoveries of great scientific interest as well as of remarkable technical importance.

Those who are specially acquainted with the subject of these Lectures will find that they contain an excellent *resumé* of the improvements introduced lately, and a great number of useful facts as to the dates at which the various inventions were brought into use.

The history of the dye substances derived from the products of distillation of coal of course occupies a prominent place in these Lectures, and the history of the development of this new industry, given by Dr. Calvert, is both instructive and suggestive. In reference to these dyes, he remarks that, if the use of these colours were encouraged, this country, instead of being dependent on others for dye stuffs, might, in time, become the purveyor of dye substances to the whole world.

A Manual of Chemical Analysis, Qualitative and Quantitative. For the use of Students. By H. M. Noad, Ph.D., F.R.S. London: Lovell Reeve and Co.

The first part of this manual, comprising qualitative analysis, not only contains copious details relating to the practice and application of the arts of analysis, but also affords signs of such careful and laborious preparation of the materials distributed through journals and memoirs as will be exceedingly useful to students and beginners.

Works on analysis have lately grown to such a size as to be inconvenient to those who are learning, and not much advantage to practised analysts. Dr. Noad's work will therefore be acceptable both from its brevity and comprehensiveness as well as from its affording a good representation of the analytical methods now most practised and recognised as trustworthy.

NOTICES OF PATENTS.

Grants of Provisional Protection for Six Months.

960. Alexander Samuelson, Cornhill, London, "Improvements in the construction and arrangement of machinery and apparatus for the manufacture of oil."—Petition recorded April 16, 1863.

1012. Thomas Richardson, Newcastle-upon-Tyne, and James Cochran Stevenson, Jarrow Chemical Works, Durham, "Improvements in the manufacture of sulphate of soda."—Petition recorded April 23, 1863.

1112. Bevan George Sloper, Walthamstow, Essex, "Improvements in apparatus for separating metals from earthy and other matters mixed with them."

817. Thomas Barnes, Earl of Dundonald, Queen's Gate, Hyde Park, London, "Improvements in treating fats and fatty oils and volatile oils or essential oils."—Petition recorded March 30, 1863.

839. William Clark, Chancery Lane, London, "Improvements in preventing fermentation in alcoholic and other liquids while drawing them from their containing vessels, and in apparatus for the same."—A communication from Charles Pierre Laurens, Adolphe Peret, and François de Trégomain, Boulevart St. Martin, Paris."—Petition recorded April 1, 1863.

Notices to Proceed.

3167. Thomas Marwood Elton, St. Luke's Soap Works, Golden Lane, Barbican, London, "Improvements in the manufacture of soap, and in the machinery employed therein."—Petition recorded November 25, 1862.

3320. John Robert Breckon, Darlington, and Thomas Douglas, Peases West Colliery, Crook, Durham, "Improvements in the manufacture of fire-bricks and other articles usually made from fire-clay."

3334. Samuel Fox, Stockbridge Works, Deepcar, near Sheffield, Yorkshire, "Improvements in retorts and apparatus employed for the manufacture of gas, and also in purifying gas." Petitions recorded December 12, 1862.

3375. Fedor de Wylde, Great College Street, Camden Town, London, "An improved means for the protection and preservation of lead surfaces exposed to the action of water, and for the protection of such surfaces from decomposition by atmospheric action." A communication from Henry Schwartz, Breslau, Prussia.

3389. Jules Pernod, Avignon, France, "A production derived from madders, called 'purpurine.'"

97. William Clark, Chancery Lane, London, "Improvements in the preparation of green colouring matter." A communication from Charles Jules Usèbe, Boulevart St. Martin, Paris. Petition recorded January 12, 1863.

717. Georges de Laire, Imperial Mint, Paris, "Improvements in the manufacture of brown colouring matters." Partly a communication from Charles Girard, Lyons, France. Petition recorded March 17, 1863.

752. Fedor de Wylde, Trinity Square, Tower Hill, London, "Improvements in the manufacture of cement from gypsum." Petition recorded March 20, 1863.

878. Richard Archibald Brooman, Fleet Street, London, "Improvements in the manufacture of baryta and its derivatives in obtaining by-products, and in revivifying or recovering certain agents employed in such manufacture." A communication from Julien Gabriel Lelong Burnet, Paris. Petition recorded April 7, 1863.

26. Stephen White, Suffolk Grove, Southwark, Surrey, "Improvements in the method of, and apparatus for, purifying, bleaching, and refining oils and oily and fatty matters."

117. Jules Albert Schlumberger, Bâle, Switzerland, "An improved process for manufacturing colours for dyeing and printing."—A communication from Jean Jacques Muller, Bâle, Switzerland.—Petition recorded January 14, 1863.

1020. Robert Lavender, Goswell Road, Islington, London, "A new compound to be used as a lubricator."—Petition recorded April 24, 1863.

1065. George Washington Fuller, Cambridge Port, Massachusetts, U.S., "A new and useful or improved submarine lantern, to be used in explorations beneath the surface of the ocean, or any other large body of water."—Petition recorded April 28, 1863.

CORRESPONDENCE.

Preparation of Sulphate of Copper.

To the Editor of the Chemical News.

Sir,—A few weeks ago I was some little amused in reading in your valuable Journal a process which has been patented, and is alleged to be an invention of Mr. B. W. Gerland, for the manufacture of sulphate of copper from malachite, &c. That process is valueless, and certainly contains nothing new.

My attention has also been called to another invention relating to the manufacture of sulphate of copper, patented and claimed as a grand invention by the same able but ambitious chemist. On procuring his specification, dated June 11, 1862, I find it to be a facsimile of an invention for which I obtained protection in January, 1858. This process I worked for some time on a large scale, and ultimately abandoned it as unremunerative.

I will here describe the outlines of my invention, and give my reasons for abandoning the same. If Mr. Gerland has improved upon any points which I considered impracticable, well and good; but his claim should be based solely on such improvements, and he should not seek credit for an invention worked four years ago. My process consists in extracting copper from the native ores or other compounds after some preparation with muriatic acid, by which solutions of chlorides of copper and other metals are obtained. Such a solution is then reduced to dryness, and treated with sulphuric acid.

The muriatic acid evolved is collected, and the remaining mass is roasted in a reverberatory furnace to peroxide the iron. It is then dissolved in water, and allowed to settle; the liquor is now drawn off and evaporated, when crystals of sulphate of copper, comparatively pure, are obtained. These can be further purified by any known means; or another method is this:—I treat my solutions of copper, &c., in muriatic acid direct, by boiling with sulphuric acid, thereby liberating muriatic acid (which is condensed).

The sulphate so obtained is reduced to dryness, and roasted in a reverberatory furnace for the separation of the iron. The sulphate of copper is then dissolved out and crystallised. Or, the chloride (obtained by dissolving the compounds or ores in muriatic acid) may be reduced to dryness, and the dry mass carefully calcined in a furnace, to decompose the chloride of iron, &c., and, after washing out the comparatively pure chloride of copper, it can be treated either in the dry or liquid condition with sulphuric acid, to convert the same into sulphate of copper.

My objections to the above-named processes were as follows:—

1. Chloride of copper cannot be entirely decomposed and formed into sulphate by the addition of sulphuric acid.
2. In calcining the mixed chlorides of copper and iron in a reverberatory furnace for the purpose of separating the iron, a large percentage of the chloride of copper is volatilised and decomposed.
3. In calcining or roasting the sulphate of copper and the sulphate of iron for the separation of the iron, a large percentage of the sulphate of copper is decomposed and remains insoluble in water.

Lastly, I would remind Mr. Gerland that he cannot claim the use of oxide of copper for precipitation of the iron out of cupric solutions, as this has been openly done in our establishment for the last thirty years.

I am, &c.
John A. Bouck.

Chemical Works, Newton Heath, near Manchester, May 20.

New Brown Dye.

To the Editor of the Chemical News.

Sir,—I beg to enclose pattern of a new brown dye I lately discovered, prepared from aniline. It is quite fast as to soap, and, as far as I can judge, also to light.

I merely wish to secure the priority by requesting your notice of it, and to avoid the chance of prosecution by designing monopolists. I am, &c.

John S. Blockey.

Hyde Park, Leeds, May 13.

[The dye, on wool, appears very fast, and is of a bright colour. To printers in search of such a colour it cannot fail to be of value.—Ed. C. N.]

Patents for Applications of Paraffin.

To the Editor of the Chemical News.

Sir,—On looking over the pages of the last Number of the Chemical News I find, much to my chagrin, that Dr. Stenhouse has obtained Letters Patent for certain special uses of paraffin, uses to which I have applied this substance on a somewhat extensive scale long anterior to the date of these patents.

It is not, however, my intention on this occasion to trouble you with evidence in support of the position here assumed, other than to observe that in February of last year—some months prior to the publication of Dr. Stenhouse's specifications—several articles treated with the substance in question were deposited by me at the International Exhibition. I am, &c.

Thomas Cattell, M.D.

30, Euston Square, May 21.

Church's Patent for Preserving Stone, &c.

To the Editor of the Chemical News.

Sir,—Will you allow me to ask in what publication the anticipation of my patented process was made? It is said (May 23, page 251), that "with the first announcement of Professor Graham's discovery of the principle of dialysis, it was anticipated that his method would furnish the means of preparing a solution of silica in pure water which might . . . be applicable in conjunction with lime or baryta, to the purposes described by Mr. Church." It is further stated that these anticipations have not been realised on account of the impossibility of getting a strong solution of silica in large quantities. I claim as my own discovery the consecutive application of solutions of silica and baryta to stone and other materials for the purpose of hardening and preserving them; and I deny that my discovery has been anticipated. The employment of an aqueous solution of lime, one part of which substance requires 730 of water for solution, is of course futile. So far from its being necessary to employ a very strong solution of silica in water, I find that the best results are obtained when the strength of the solution is less than that named in the notice (5 per cent.).

My process, you will see, produces nothing but the insoluble protective silicate of baryta, by a simple union of the silica with the baryta, and not by double decomposition. Generally this latter process yields, together with the insoluble material, an immense quantity of some soluble and deleterious salt. This is the case where silicate of soda and chloride of calcium are used, 100 parts of the dry materials yielding some 40 per cent. of common salt, thus introduced into the stone. A fatal addition! This objectionable result occurs in most cases of double decomposition, both in recent applications of the principle and in that old process in use fifty years ago for the same purpose, where alum (sulphate of alumina) in solution is first applied to brick and stone, and then soft soap (stearate, &c., of potash).—I am, &c.

The Patentee.

70, Great Portland Street, W.

MISCELLANEOUS.

A New Claimant for the Discovery of Thallium.—Within the last few days a statement has appeared in the *Mining Journal* that thallium was discovered as far back as the year 1857. As no statement is given as to how the supposed new metal was obtained, it is, of course, impossible to comment on the reality of the discovery; but that the readers of the Chemical News may be in possession of the grounds upon which this statement is based, the following extracts are reprinted for their information from the *Mining Journal* of 1857 and the 23rd inst.:—

" *Sulphurium.*—Mr. Joseph Jones, of Bolton-le-Moors, states that he has discovered the perfect metal sulphurium, which is of the same class as arsenium, silver, aluminium, &c. Oxide of sulphurium is the refuse of the manufacture of sulphuric acid or brimstone, and has no commercial value, persons being paid for carting it away. In its refuse condition it has almost the specific gravity of iron, and the atoms are very fine, malleable, ductile, &c." —*Mining Journal*, July 14, 1857.

" *Sulphurium*—Mr. Joseph Jones has, as I see by your last journal, made a discovery in chemical science of as great importance as the discoveries of Sir Humphry Davy. It has hitherto been supposed that sulphur was a simple combustible, non-metallic substance, of a peculiar yellow colour—this is Dr. Ure's definition; and he further adds that it is an abundant product of nature, existing sometimes pure or merely mixed, and at other in intimate chemical combination with oxygen and various metals, forming sulphates and sulphurets. As I do not consider that chemistry has reached anything like the perfection that it will attain, but rather that at a future time our notions on the subject will be regarded with as much ridicule as we regard the attempts of the old alchemists to produce gold by combining and manipulating various baser metals, &c., I am willing to admit that Mr. Jones's discovery might really be what he considers it to be; but upon the rough assertion that he has put forth, I am inclined to think that he has obtained his results in consequence of want of care in conducting his experiments. He says the metal is of the same class as arsenium, silver, aluminium, &c., and that the oxide of sulphurium is the refuse of the manufacture of sulphuric acid. Now, sulphuric acid is manufactured, ordinarily at least, by the combustion of sulphur with nitre (nitrate of potash); then, as sulphuric acid is a combination of sulphur, oxygen, and water, the question is, what becomes of the potassium and impurities of the sulphur? They, no doubt, form part of the refuse, and, combined with the other refuse matters, form what Mr. Jones pleases to designate oxide of sulphurium. As he says that the metal is of the same class as arsenium, silver, aluminium, &c., and as potassium and aluminium might be easily mistaken for each other by an inexperienced hand, I do not hesitate to suggest that Mr. Jones may have produced an impure potassium, which he has mistaken for a new metal, and designated sulphurium. However, I am as likely to be mistaken as Mr. Jones, and as it would be interesting to many to know what he has really discovered, even if it be but that potash produces potassium, I trust that he will not allow the subject to be left in its present unsatisfactory state.—E. D. H."—*Mining Journal*, July 21, 1857.

" *The New Metal—Thallium.*

" Sir,—As we are all desirous of securing all the honour and fame to which we are entitled, I may take the opportunity of referring the readers of the *Mining Journal* to a few remarks which were printed over my initials, dated July 21, 1857, and bearing upon a paragraph published the previous week, announcing the discovery of what I have now no doubt was the metal thallium; at the same time I must express my sincere regret for having written in such disparaging terms of Mr. Joseph Jones's discovery, more especially as I believe he was a man without means or friends, and consequently unable to substantiate his claims, and secure the advantages and fame to which he was entitled. The paragraph, with reference to which I wrote, was the following:—

" ' *Sulphurium.*—Mr. Joseph Jones, of Bolton-le-Moors, states that he has discovered the perfect metal sulphurium, which is of the same class as arsenium, silver, aluminium, &c. Oxide of sulphurium is the refuse of the manufacture of sulphuric acid or brimstone, and has no commercial value, persons being paid for carting it away. In its refuse condition it has almost the specific gravity of iron, and the atoms are very fine, malleable, ductile, &c.'

" Now, I think few can read this notice without concluding that the merit of having first discovered the metal which is now, by general consent, called 'thallium,' is that to which Mr. Jones alludes as having even in 1857 already discovered. That Mr. Jones was inaccurate in describing the new metal as 'sulphurium,' or the metal of sulphur, I admit; but we must remember that even within the last five years the science of chemistry has much progressed, and that evidence which the most experienced chemist would in 1857 have received as conclusive would now be objected to as requiring very much further confirmation before being regarded as the proof of a fact. We must remember, too, that Mr. Jones could not have had the advantage of making the spectrum analysis which led Mr. Crookes to the discovery, and that, consequently, the obstacles which Mr. Jones had to surmount in order to give even the outline contained in the above notice of the nature of the metal, were infinitely more numerous than those which Mr. Crookes could have had to encounter. Whether Mr. Joseph Jones be still living I know not; if he be, I trust that he will derive any benefit which may accrue from the discovery.

" When I wrote in 1857, I remarked that I did not consider that chemistry had reached anything like the perfection that it would attain, but rather that at a future time our notions on the subject would be regarded with as much ridicule as we regard the attempts of the alchemists to produce gold by combining and manipulating various baser metals, yet I confess that I then little thought that I should so soon have to regard my own views as to a new metal from sulphur residue as so supremely absurd. It is true that Mr. Jones did not give such ample particulars as could have been wished, although it would now appear pretty certain that he possessed them, but there is no doubt that he, seeking rather profit than fame, withheld them in the hope of finding a capitalist to aid him in turning his discovery to commercial advantage. In 1857, I suggested, from Mr. Jones's description of his discovery, that he might have produced an impure potassium, which he had mistaken for a new metal, and designated sulphurium; but I am now inclined to believe that his greatest error was in regarding the metal as the actual metal of sulphur, instead of simply a new metal, occurring in combination with certain sulphide ores.—E. D. H., May 19."—*Mining Journal*, May 23, 1863.

"*The Discoverer of Thallium.*—It appears that there are some grounds for doubting whether either Mr. Crookes or the French chemist for whom the merit has been claimed are entitled to the honour of having first discovered the metal thallium, for so long since as July 18, 1857, a notice was published in the *Mining Journal* stating that Mr. Joseph Jones, of Bolton-le-Moors, claimed to have discovered a metal which it now appears was thallium, although by Mr. Jones it was designated sulphurium. That Mr. Joseph Jones really succeeded in isolating the metal is beyond doubt, for he stated that it was 'of the same class as arsenium, silver, aluminium,' &c. Mr. Jones also stated that this new metal was obtained from sulphur residue. The inventor being a poor man, had little opportunity of making his discovery known, but that he is really entitled to the honour of being the first discoverer of thallium there seems no doubt."—*Mining Journal*, May 23, 1863.

Another New Metal.—A letter from France says that another new metal has been announced during the week. M. Osravais, Professor of Geology at Strasbourg, has obtained a hard shining metal, of the colour of gold, but soft as lead, from the mineral waters of Alsace. The metal, not admitting of a high degree of polish, will be useful to employ in the dull or coloured goldsmiths' work so much in fashion for ornament just now. The specimens, submitted to connoisseurs in Paris, have excited the highest admiration.—*Mining Journal.*

Aniline Blue.—At the meeting of the Académie des Sciences on the 18th inst. (*Comptes-Rendus*, lvi., 445; and *Cosmos*, xxii., 629) just as the proceedings were at a close the President announced the receipt of a telegraphic dispatch from Dr. Hofmann, containing a statement of a very simple result he had obtained in reference to the constitution of this substance, viz.:—"That aniline blue is a triphenyl-rosaniline; one molecule of rosaniline and three molecules of aniline containing the elements of one molecule of aniline blue and three molecules of ammonia."

The British Association.—The approaching meeting of the British Association in Newcastle is attracting much attention, and active preparations are making for the gathering, which will, no doubt, be a very large one. A special meeting of the local general committee was held in the hall of the Literary and Philosophical Society on Saturday. One of the most important features of the meeting will be a series of papers which are in course of preparation, on the chief branches of local industry, and at present include the following:—"Coal Mining, Coke, &c.;" N. Wood, J. W. Pease, and J. Taylor.—"Iron, and its Manufacture (Steel, Lead, Copper, Zinc, Aluminium);" J. L. Bell, T. Spencer, and T. Richardson, Ph.D.—"Iron Shipbuilding;" C. M. Palmer.—"Railways and Locomotives;" W. Hawthorn, J. F. Tom, and G. R. Stephenson.—"Engines and Hydraulics;" Sir W. G. Armstrong, C.B., J. F. Spencer, and P. Westmacott.—"Chemical Manufactures;" J. C. Stephenson, R. C. Clapham, and Dr. Richardson.—"Fire-clay Wares;" J. Cowen.—"Earthenware;" C. T. Maling.—"Glass;" R. W. Swinburne; and other papers are expected. Another feature of the meeting will be excursions to the various mines, ironworks, &c. One of them is arranged to visit the Cleveland district. On Tuesday, September 1, an excursion has also been planned for Marsden Rocks, South Shields, and Sunderland. An excursion to Alston and Allenheads, to visit the lead mines, is also decided upon; and arrangements are in progress for visiting various other mines, works, &c. The committee are desirous of receiving contributions of works of art, pictures, models, and other matters of interest for exhibition from private gentlemen and others disposed to furnish them, and request those willing to aid them to communicate with the secretaries at Newcastle.—*Mining Journal.*

The Royal Polytechnic Institution.—The energetic managers of the Royal Polytechnic Institution have commenced a series of fashionable Saturday morning entertainments, which, to judge from the two already given, promise to become very successful. One of the special features of them is the lecture by Mr. Pepper, on the newly-discovered element, Thallium, in which that gentleman renders the mysteries of spectrum analysis, and the part it had in the discovery of the new metal, clear to the most unscientific of his hearers. The galleries were carpeted for the occasion, and tastefully decorated with rare exotics. Mr. Pepper's "Strange Lecture" was given with several additional effects, and the *Der Freischutz* seems to have lost none of its freshness or attractions, either in a musical, optical, or humorous point of view. Last week the Prince and Princess of Wales paid a private visit to the Institution, and were practically inducted into the "Ghost Mystery" by Mr. Pepper, who exhibited to them one of their suite transformed into a spectre for the occasion, much to his own surprise and that of the Royal party.

SCIENTIFIC SOCIETIES.

MEETINGS FOR THE ENSUING WEEK.

June 1. Monday.

Entomological—12, Bedford Row. 7 p.m.

British Architects—9, Conduit Street. 8 p.m.

Asiatic—5, New Burlington Street. 3 p.m.

Royal Institution—Albemarle Street. 2 p.m. *General Monthly Meeting.*

2. *Tuesday.*

Civil Engineers—25, Great George Street, Westminster. 8 p.m.

Photographic—King's College, Strand. 8 p.m.

Ethnological—4, St. Martin's Place. 8 p.m.

Royal Institution—Albemarle Street. 3 p.m. Professor Tyndall, "*On Sound.*"

3. *Wednesday.*

Society of Arts—John Street, Adelphi. 8 p.m. W. Hawes, Esq., "*On the Results of the International Exhibition of 1862.*"

Geological—Burlington House. 8 p.m.

4. *Thursday.*

Antiquaries—Somerset House. 8.30 p.m.

Chemical—Burlington House. 8 p.m. M. Marcellin Berthelot, "*On Synthetic Methods in Organic Chemistry.*"

Linnæan—Burlington House. 8 p.m.

Royal Society Club—St. James's Hall. 6 p.m.

Royal Institution—Albemarle Street. 3 p.m. Prof. Ansted, "*On Geology.*"

5. *Friday.*

Archæological Institute—26, Suffolk Street, Pall Mall. 4 p.m.

Philological—Somerset House. 8 p.m.

Royal Institution—Albemarle Street. 8 p.m. John Ruskin, Esq., "*On the Alps of Savoy.*"

6. *Saturday.*

Royal Institution—Albemarle Street. 3 p.m. Prof. W. Thomson, "*On Electric Telegraphy.*"

ANSWERS TO CORRESPONDENTS.

C. G., Bristol.—The new edition of Liebig's "Chemistry Applied" is published by F. Vieweg and Son, at Brunswick. There is no new English edition.

THE CHEMICAL NEWS.

Vol. VII. No. 183.—June 6, 1863.

HOW PEOPLE ARE POISONED.

The excitement that prevailed some years ago, with regard to the subject of poisoning, seems, like most modern popular emotions, to have soon passed from a condition of exaggerated magnitude, to one of unreasonable disregard. The only result of all the declamation, the controversy, the squabbling, and the gossip bestowed upon this subject was, that nothing was done towards remedying the evils that were on all sides admitted to exist in connection with the sale and use of poisons.

Meanwhile, the papers from time to time report the occurrence of cases of poisoning, both intentional and accidental, with a frequency that leaves little ground for the belief that the safeguards against poisoning are, to to say the least, any greater now than they were some years ago, when every one had the fear of poison before his eyes.

Without at all desiring to excite a poison panic, it may at least be asked whether the accumulated evidence of danger and disaster, arising from the sale of poisons, and the absence of any general system of protection against those results, is not a great anomaly at the present time and in a civilised country?

The subject is one undoubtedly of very great difficulty in many respects, but that is no reason whatever that it should be left alone; it is, on the contrary, rather a strong reason that it should be taken vigorously in hand, and dealt with somehow and to some extent. It will not do to wait in philosophic contemplation of how people are poisoned, with the object of devising an unobjectionable and perfect system to regulate the condition under which poisons are to be obtainable and to be used.

Some step in advance, towards a protection against the possibility of poisoning, even if inconvenient in practice, would at least afford a better opportunity for devising and adopting more suitable means of arriving at the results desired.

The common use of a variety of substances, which, if they cannot be called poisons in the strictest sense of the word, are, at least, of so deleterious a nature as to demand an exercise of caution in their use, is now much greater than ever it was, and in itself constitutes no trifling ground for legislative enactments in regard to the use of these substances. It would be unreasonable to prohibit or impede the use of oxalic acid any more than the use of Epsom salt; the one is as much a necessary commodity as the other, and should be as readily obtainable. Of course it is desirable that when one asks for Epsom salt he should not be given oxalic acid. That danger may be provided against by restricting the sale of such dangerous substances as oxalic acid to persons capable of distinguishing them from others that are harmless but similar in appearance, as Epsom salt is to oxalic acid. But this is a very partial protection at best; the intelligence of the vendor of dangerous materials is no safeguard against the stupidity of the purchaser. It is, doubtless, a very great desideratum to raise the intellectual and educational standard of druggists, but that alone is no remedy for the evils resulting from the misuse of poisonous substances. An intelligent and well-qualified druggist may cause a stranger much trouble to obtain twopennyworth of oxalic acid or of arsenic, while he supplies without hesitation the same substances to a customer whom he knows to be a strawbonnet cleaner, or the ratcatcher's wife. It is after this point is passed, that the great danger of poisoning begins. The druggist may with all propriety have put on the packet of oxalic acid or arsenic, the label "Poison" without in the least contributing towards preventing the misuse of these substances by persons who can neither read it nor distinguish between arsenic and soda, or between oxalic acid and Epsom salt. It is in the misuse of these drugs that the mischief lies, and in this respect there is a vast difference between Epsom salt and oxalic acid. While the consequences of misuse, in one case, might be only inconvenient, in the other, they might be fatal.

A striking illustration of this has just been afforded by the accidental poisoning of a lady near Tunbridge Wells. Being in the habit of taking a solution of magnesia, she sent her servant about daybreak to fetch her a wineglassful, which she drank, and feeling a burning sensation, asked the servant to fetch the bottle it was taken from. That told her she had swallowed chloride of zinc. Instead of "Dinneford's fluid magnesia," the servant had filled the glass with "Disinfecting fluid." Within twenty-four hours she was dead. The jury returned a verdict of accidental death from the effects of a dose (*sic.*) of Sir William Burnett's disinfecting fluid, given for a dose of Dinneford's fluid magnesia; and they expressed their strong disapprobation that a mixture, of such a poisonous nature as the former, should be sold without being legibly labelled with the word "Poison."

It might have seemed harsh, though not altogether unreasonable, to have added to this opinion a censure of the negligence which was manifested by the fact that the two bottles were kept together on the same shelf, although they are described as being "exactly alike in size and form, and in the colour, both of the glass and the liquid," but containing substances so vastly different—a simple medicine and a powerful corrosive poison. It is very difficult to say how such cases as this can best be met. People may insist upon having a right to keep poison and physic together, and claim a right to the consequent risk of being poisoned. It would be difficult to prove them in the wrong. But when such a practice becomes fraught with risk to others who have no such crotchet, and who may become unwilling victims to it, there can be no doubt that such a mad confusion of the aids of living with the agents of death, is in the highest degree reprehensible, and equally culpable, whether it be done deliberately or from carelessness.

Notwithstanding the alarm that has been raised as to the extent of secret poisoning, there are good reasons for the opinion that accidental poisoning, in consequence of such circumstances as the above, is far more to be dreaded, and far more urgently calls for some steps being taken to provide against its occurrence.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Action of Sulphur on Saline Solutions having an Alkaline Reaction.

M. J. De Girard[*] has observed that by boiling a solution of soda pyrophosphate with an excess of well-washed flour of sulphur, the liquid rapidly becomes brown, in consequence of the formation of a polysulphide. By continuing the ebullition for several hours, hydrosulphuric acid is abundantly evolved. The liquid gradually becomes colourless, meanwhile disengaging hydrosulphuric acid. The filtered liquid contained soda hyposulphite and a salt of phosphoric acid. Silver nitrate solution was added as long as the white precipitate, first formed, was rendered black by the formation of sulphide, and after separating this precipitate the filtrate gave a yellow precipitate with the silver solution.

A solution of sodium sulphide boiled with excess of sulphur evolves hydrosulphuric acid, and when the liquid has become quite colourless and neutral to test-paper, it contains only soda hyposulphite. Under the same circumstances, sodium sulphide alone, decomposes water at 100° C., evolving hydrosulphuric acid.

Water boiled with washed sulphur is also decomposed with evolution of hydrosulphuric acid.

Density of Vapours at Elevated Temperatures, by MM. Sainte-Claire Deville *and* Troost.[†]

(*Continued from page* 254.)

All the compound substances which give eight volumes of vapour consist of four volumes of one constituent combined with four volumes of the other, including among them phosphorus perchloride, if the hypothesis proposed by M. Cahours be adopted.

It has been assumed lately that the equivalents of these substances really correspond with four volumes of vapour, but that their constituents are separated at the moment when their vapour densities are taken, and then correspond, only in appearance, with eight volumes. MM. Deville and Troost consider this hypothesis to have little foundation in fact, and, in addition to the known facts[‡] that render it inadmissible, they made some further experiments to demonstrate their view.

1. Ammonia hydrochlorate is not decomposed at a temperature which is sufficient to decompose ammonia in great part. To prove this, two tubes, one closed at one end, and containing sal-ammoniac, the other having a current of ammonia passed through it, were both heated side by side in the same furnace. While the vapour of sal-ammoniac presented no sign of decomposition, the gas issuing from the other tube consisted of—

Ammonia	53·2
Nitrogen and hydrogen . . .	46·8
	100·0

When the temperature was increased, the gas issuing from the tube containing sal-ammoniac consisted of—

	Observed.	Calculated.
Hydrochloric acid . .	32·0	32·2
Hydrogen	49·4	50·1
Nitrogen	18·6	16·7
	100·0	100·0

At the same time, the ammonia was decomposed in the following proportions :—

Ammonia	24·2
Hydrogen and nitrogen . . .	75·8
	100·0

The temperature might have been nearly 1100° C., for in taking the density of the ammonia hydrochlorate the following results were obtained :—

	Temperature.	Capacity of flask. c.c.	Gas remaining in the flask. c.c.	Sal-ammoniac remaining in the vapour.
A.	1075° C.	303·0	30·0	—
B.	1080° C.	309·8	36·1	25 per cent.

The gases remaining in the flasks had the following composition :—

	Observed.		Calculated.
	A.	B.	
Hydrogen . .	49	75·2	75·0
Nitrogen . .	49	24·8	25·0
Oxygen . .	2	—	—
	100	100·0	100·0

It was ascertained that a mixture of hydrochloric acid, nitrogen, and hydrogen, passed through a tube heated to redness, does not give rise to the production of sal-ammoniac even when the tube contains spongy platinum.

It is therefore impossible to suppose that sal-ammoniac was decomposed and reproduced in the experiments made to estimate the density of its vapour.

Similar experiments with ammonia hydrobromate and hydriodate led to the same result.

2. Ammonia hydrocyanate, which is produced at very high temperatures by the contact of ammonia and carbon, gives eight volumes corresponding with one equivalent; if it were decomposed it would not yield a mixture of ammonia and hydrocyanic acid, both of which would be decomposed before the hydrocyanate.[§]

3. Ethylamine hydrochlorate was to some slight extent decomposed, yielding hydrochloric acid and ammonia, in the experiments made to estimate the density of its vapour. These gases did not recombine.

These experimental results are considered by MM. Deville and Troost to be evidence of the importance of Gay-Lussac's law, and of its general applicability; to show what a happy influence has been exercised on chemical doctrines by the introduction of the methods proposed by Dumas and Mitscherlich, and which they have followed as closely as possible. The consequences of the accurate observations of M. Cahours are manifest throughout these results; they are entirely confirmed by the numerical data obtained by MM. Deville and Troost. Moreover, the rule laid down by Gay-Lussac as to the contraction of compound substances, calculated

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[*] *Comptes-Rendus*, lvi., 798.

[†] *Comptes-Rendus*, lvi., 891.

[‡] *Vide* Chemical News, vii., 243.

[§] Hydrocyanic acid is decomposed at a dull red-heat into a mixture of cyanogen and hydrogen with a slight deposition of carbon. When the cyanogen is separated by means of potash, the gaseous residue is found to consist of—

Hydrogen	91·3	83·7
Nitrogen	8·7	16·3
	100·0	100·0

The quantity of nitrogen increases with the temperature at which the gas is collected.

according to the relation of volumes of the gases which combine, acquires a greater generality. Lastly, they consider that the true advancement of science consists in reducing to this rule the rare exceptions which still remain; in multiplying and in studying, with perseverance the facts which relate to the history of the volumes of chemical compounds.

Though referring only to the discoveries of those who have treated this question from a purely chemical point of view, MM. Deville and Troost add that their experimental results, and the conclusions they have drawn from them, present an intimate relation to the results and the views introduced into science by M. Regnault, to whom they are indebted for the determination of most of the constants employed in their calculations, and for more precise knowledge of the manner in which the volume of a gas varies with the temperature and the pressure. The manner in which he has interpreted the irregularities which he discovered in the laws of Gay-Lussac and Mariotte has served for the explanation of the irregularities presented by the densities of vapours under the circumstances of temperature and pressure which they have studied.

On Cases of Isomerism in the Phenyl-Series, by Edmund J. Mills, *F.C.S.*

In the course of some experiments now in progress, I have been led to infer that the molecule of nitryl (NO_2),[*] as it exists in nitro-substitution compounds, is of two kinds; the chief difference appearing to consist in their mode of behaviour with a reducing agent.

It is not difficult to account for this diversity, if we suppose the atoms of oxygen which nitryl contains to be different, and differently arranged in the two cases. Adopting a fresh symbol for one of these atoms, we might write the compounds thus:—

$$(\alpha)\ N\left\{\begin{matrix}O\\ \Omega\end{matrix}\right. \qquad (\beta)\ N\left\{\begin{matrix}\Omega\\ O\end{matrix}\right.$$

On treatment with a reducing agent, one atom of oxygen would be removed, and azotyl left behind—

$$(\alpha)\ N\Omega \qquad (\beta)\ NO$$

—itself thus admitting of a double formula. In the last stage of the reduction, the whole of the oxygen is removed, and two different molecules of amide are the result:—

$$(\alpha)\ NH_2 \qquad (\beta)\ NH_2$$

We are thus lead *à priori* to expect the existence of isomeric amido-bases. The phenyl-series furnishes us with such a case. Griess,[†] in fact, has some time since indicated the occurrence of an isomer of aniline, which he has obtained from chloride of phenyl by Dr. Hofmann's process. and which he considers the true phenylamine. The chloride of phenyl he employed was obtained from ordinary hydrate of phenyl; the recent experiments of Church[‡] have shown that there is no reason to doubt its perfect identity with the chloride of phenyl obtained from benzol by means of nascent chlorine,—the method of Church himself. We may hence, I think, safely infer that, in phenylamine and amido-benzol, the difference lies not in the molecules of phenyl, but in those of the amide concerned in the two compounds.

Before these views are applied to the explanation of the existence of two nitranilines, two other considerations must be taken into account:—(1.) The identity of aniline[§] obtained from indigo with aniline prepared from nitro-benzol; and (2.) of bromaniline obtained from bromisatin with that which is furnished by the action of potash on bromphenylacetamide.[‖] It is hence clear that, whether we consider the phenyl or the amide it contains, the aniline prepared from indigo, from nitro-benzol, and from phenyl-acetamide, is one and the same. If, therefore, the nitryl-atom were not endowed with a dual nature, the nitraniline obtained from nitro-phenylacetamide should exactly resemble that furnished by the semi-reduction of dinitrobenzol. But this is well known not to be the case; the differences between the two kinds of nitryl base become expressed in their respective compounds. The occurrence of two dinitro-melanilines (Hofmann) is explicable on the same grounds.

This reasoning is further supported by a transformation undergone by nitrosaniline (azotyl-amido-benzol)—a coloured product obtained by Church and Perkin on partial reduction of dinitro-benzol, and having the formula—

$$C_6H_6N_2O = C_6H_4\left\{\begin{matrix}NH_2\\ NO.\end{matrix}\right.$$

These chemists have been careful to observe[¶] that this substance, when heated with soda-lime, gives off all its nitrogen in the form of ammonia and aniline. The reaction is quite intelligible if we suppose the amide of one order and the azotyl of the opposite order to be here simultaneously present. The hydrogen acting on the two molecules has consequently two different destinations, converting the one into ammonia, and the other (conjointly with the phenyl) into aniline. It appears from this result that phenylene-diamine contains amide of the two orders.

It is needless to enter at present into the discussion of the remaining cases of isomerism which exist in the phenyl series. Although the discovery of two kinds of phenyl is rather to be looked for than otherwise, I am inclined to think that those instances with which we are already acquainted are preferably to be explained by the dual nature of the nitryl-molecule, and its necessary consequences. The more general anticipations to which these considerations lead us—as, for example, the occurrence of two kinds of ammonia—are too obvious to require further exposition.

Glasgow, June 1.

Note on Experiments with Nitrates, by John Horsley, *F.C.S.*

If a small portion of pyrogallic acid from the point of a penknife be projected into a vessel containing about two or three drachms of water, it will, in the course of twenty-four hours, or less, assume a dark-brown colour, from the absorption of oxygen from the air, but not so if the pyrogallic acid be dissolved in water acidulated with a few drops of sulphuric acid.

It therefore occurred to me as a useful means of detecting nitrates and nitrites, as the following experiments prove:—

If into a test-glass containing two or three drachms of water, acidulated with a few drops of sulphuric acid, a small portion of pyrogallic acid be projected, and then a little strong sulphuric acid be carefully trickled down the sides to about one-fourth from the bottom, little

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[*] O = 16, &c.

[†] *Ann. de Pharm*, cxxi., 3.

[‡] *J. Chem. Soc.* [2], i., 77.

[§] Hofmann, Chem. Soc. Mem., ii., 272.

[‖] Mills, *Ann. Ch. Pharm.*, cxxi., 3; and *Griess* in *ext. loc.*

[¶] Chem Soc. Qu. J., ix., 1.

change will ensue beyond a faint violet coloured line at the point of contact of the two fluids. A grain of chloride of sodium being added, a brisk effervescence runs throughout the sulphuric acid from the evolution of chlorine, which somewhat increases the violet colour, but on projecting the merest particle of a solid nitrate (say nitrate of potash) it will, on falling, carry with it so much of the pyrogallic acid as to be reacted on by the nitre and produce an intense purple colour, which, gradually mixing with the upper layer of the pyrogallic acid solution, gives off streaks or rings of orange-yellow, by becoming oxydised at the expense of the decomposed nitric acid of the nitrate. On the other hand, if the nitrate be first mixed with two or three times its bulk of chloride of sodium, and then projected through the pyrogallic acid liquor to the sulphuric acid below, no purple colour is produced, but the resulting effervescence causes the evolved nitrous acid to act more uniformly on the whole of the pyrogallic acid, imparting a deep orange-brown colour.

Such is the sensibility of this reaction, that even a drop or so of a solution of a nitrate added to the pyrogallic liquor previous to the projection of the chloride of sodium suffices to show the discolouration caused by the liberated nitrous acid, the intervention of the chloride of sodium, from the smallness of the nitrate, being necessary to its production, and renders it more evident than when the nitrate is used alone. Only part, however, of the drop of nitrate solution which comes into contact with the sulphuric acid will be decomposed, and even that is sufficient to give an intense colour.

The nitrous acid evolved from nitrates by heat or otherwise is also detected in its dry state by the discolouration of a piece of white paper moistened with a tolerably strong pyrogallic acid solution.

In fine, I am of opinion that this method of testing for nitrates and nitrites is far more delicate than any at present in use. Simple, easy of execution, and perfectly unobjectionable.

It is desirable that a piece of white paper should be placed at the back of the test-glasses, in order to show the reaction best, and also that no more chloride of sodium be used than is necessary, or equal to the sulphuric acid, so as not to lay undecomposed at the bottom of the glass.

Preference would appear to be given to the use of the dry salt with the chloride of sodium rather than a liquid nitrate.

TECHNICAL CHEMISTRY.

The Chemistry of Agriculture.

(Continued from page 258.)

Baron Liebig gives the following history of the "mineral theory," for the purpose of elucidating the principles on which his views were founded, and of enabling his readers to judge of the opinions that have been expressed with regard to it, and of the opposition it has encountered during the last twenty years.

The fundamental principles of his theory were:—

That the food of plants are inorganic or mineral substances, carbonic acid, ammonia, water, phosphoric acid, sulphuric acid, &c., &c.

That there is a connection between all the constituents of the food of plants, of such a nature that, when one is wanting, the others are insufficient to support plant life.

That farm manure does not act by virtue of its organic portion, but indirectly by the products of its decay, by the conversion of its carbon into carbonic acid, of its nitrogen into ammonia.

These principles were in direct opposition to all previous views.

In regard to the source of the carbon in plants, the view universally received was that of De Saussure, who considered the absorption of carbon from carbonic acid, by plants, to be unquestionable, but in the case of cultivated plants he held that the principal part of their carbon was derived from humus, and from the soluble organic substances in the land.

The truth of this view was not proved, and upon the examination of the evidence on which it rested, it appeared to Baron Liebig to be incapable of proof. The demonstration of his own view involved no experiment, but merely a consideration of the natural relations of plants to the atmosphere and to plants; by comparing the phenomena of plant life with the chief functions of animal life, with respiration, and the constant amount of oxygen in the atmosphere, it appeared from the cyclical changes of oxygen that carbonic acid must be the only source of the carbon in plants. This view is directly and undeniably demonstrated by the late experiments of Knop and Stohmann.

Baron Liebig claims to have been the first to put forward the opinion that ammonia is the source of the nitrogen in plants, as a result of his researches on the phenomena of animal life, and of his knowledge of the changes that all nitrogenous organic substances undergo in decay.

He denies the accuracy of the remark in Schleiden's "Botany," "that De Saussure first sagaciously propounded the idea that ammoniacal salts are the source of the nitrogen in plants, and that Liebig subsequently developed this view." He states that De Saussure says nothing, in his work, of ammonia as a source of nitrogen, and that he elsewhere distinctly expressed the opinion that of all possible sources of nitrogen, ammonia was not one, opposing Liebig's view that it was assimilated by plants, and referring its value in vegetation to its solvent action on humus.

No special importance was assigned to nitric acid by Baron Liebig, only because he knew the nitric acid in soils to be always a product of the oxidation of ammonia. Hence he considered it capable of replacing ammonia as plant food.

De Saussure was the first to point out the necessity of lime phosphate for the development of plants, but his opinion received little attention. He had also investigated the question of the necessity of lime, potash, and magnesia, but unfortunately his investigation was limited to plants in which the amounts of potash, magnesia, and lime vary according to the land they grow upon.

The opinion that the alkalies and alkaline earths in plants are not accidental, but part of their food, is often ascribed to Sprengel, who regarded all the ash constituents of plants as necessary or useful for their growth. His opinion received little attention either in science or practice, since De Saussure's observation that plants take up by their roots all kinds of saline solutions, was supposed to show that the presence of ash constituents was not a proof of their being necessary.

The circumstance that rendered Sprengel's opinions unproductive of any results, was his ignorance of the composition of plant ashes. He assumed that in most cases they were the same as in wood ashes.

The necessity of these substances could not, in Baron

Liebig's opinion, be inferred before the special functions or relations of each ash constituent, in plant life, were known, except from other unquestionable relations, such as the functions of the ash constituents of plants in animal nutrition. If these substances were indispensable for animal life, they must, it was inferred, be indispensable for plant life, since, if they were only accidental in plants, animal life would have been endangered by a change of them.

Among the opponents of Baron Liebig's doctrines as to the source of carbon and the phenomena of animal and plant life, were Moleschott and Mulder, and he reproaches himself with not having paid more attention to the views of these men than their real importance demanded, for, as teachers in universities, both exercise a wide influence, and hence Baron Liebig's views are regarded in Holland as untenable, and having been disproved by Professor Mulder.

Baron Liebig refers to the investigation of frog's flesh by Professor Moleschott as illustrative of his scientific status, and characteristic of many physiological and agricultural researches. The conclusion to which he came from his observations, was that the liquids of frog's flesh contain oxalic acid and urea—substances that had not been observed in the liquids of any other flesh. But he found no creatin, creatinin, inosinic, or lactic acid, which are present in other flesh liquids. Soon after it was shown that what Moleschott had taken to be oxalic acid was phosphoric acid, and the crystals he took to be urea nitrate were potash nitrate. Grohe also found that frog's flesh contained, like other flesh, creatin, &c.

Commenting upon these results, Baron Liebig remarks that, in like manner as it is impossible to understand a book written in a foreign language without a knowledge of that language, so it is impossible to understand or judge of chemical phenomena without a knowledge of what they signify. A person who is unable to distinguish between the best known and most easily recognisable substances, by means of their reactions is regarded by the chemist as having no more claim to attention in chemical questions, than one unable to spell the words of a language, would have to give an opinion as to the meaning of a sentence in that language. To mistake phosphoric acid for oxalic acid, or potash nitrate for urea nitrate, are inexcusable errors, that show a total unacquaintance with these things.

It is obvious, says Baron Liebig, that it is not competent for any one who may take it in his head, to make a chemical experiment; this requires a capability that must be acquired by long practice, and which is very rare. Hence it follows that an inexperienced person even in simply repeating experiments will never obtain the results that are described, and, in making experiments of his own devising, he will never obtain the results he should obtain. If, in addition to this, there is a certain degree of vanity, he will, since he obtains different results, believe that he has disproved facts which are incapable of being disproved; or he will believe he has discovered new facts that exist only in his imagination; the opposite results to which he comes are so much the more striking, and the discoveries he makes are so much the more astonishing the more uninformed and incapable he is.

Agriculturists are in the same position with regard to chemistry that Professor Moleschott is, in regard to questions of physiological chemistry. It is quite impossible for a farmer, who does not possess any knowledge of chemistry, to understand rightly the explanation of chemical operations, or the significance of the facts relating to it; when such a man attempts to make experiments for the purpose of testing the accuracy of chemical facts, it is at once apparent that he really does not know what it is he is about; the question he endeavours to answer, is not clear to him, and, under such circumstances, even the most strenuous efforts cannot lead to a rational conclusion.

The worst of it is, that the multiplicity of facts, whencesoever they may be furnished, are to him of equal value, and that he does not know how to distinguish truth from error, or the valuable from the worthless. The greater the heap of facts the more significance he attaches to them; like a child, who has heard that gold rings are sometimes found in muckheaps, he believes there is treasure hidden in every muckheap. A sensible person seeking a guide in a strange place, would certainly choose one who knew the place, and had often travelled that road; but to the foolish, any one who offers himself will suit, and it is no wonder if he gets into a bog.

In illustration of Professor Mulder's scientific ability, Baron Liebig refers to the protein researches, and to the results of Fleitman and Laskowski, showing that this substance did not exist, and that its discovery was a delusion. Baron Liebig says he fell into the error of believing that Professor Mulder would thank him for communicating these results, but his reply showed Baron Liebig that he had become his enemy for life, and would do all he could to show that Baron Leibig was a great sinner, though he gave him fourteen days to retract the opinion that the unfortunate protein did not exist.

This he could not comply with, and Professor Mulder afterwards fell into the pitiable predicament of strengthening the evidence of the non-existence of protein, by means of two memoirs intended to demonstrate its existence. Since then enmity has existed, and in Professor Mulder's late work he has dwelt upon the insufficiency and defects of Baron Liebig's experiments on soils, but Baron Liebig, admitting those imperfections, laments that the suggestions of Professor Mulder have produced no good effects on him.

In reply to Professor Mulder's opinions as to the illogical nature of the alteration in the views held by Baron Liebig some years since, he admits that such is the case, and pleads as an excuse the progress of chemistry, and the necessity of a continual condition of moulting (*deplumatio; la mue*) in the chemist who wishes to keep pace with that progress. When new feathers grow, the old ones fall out of the wings, which will no longer carry him, and, after that, he flies so much the better.

Baron Liebig believes that a man like Mulder can derive but little true gratification from science, since he consumes his time and talent in the unproductive labour of exposing, in the careful and painstaking labours of others, the weakness and deficiencies that belong to all men's work. Those who labour much are naturally more liable to be so criticised; the fame of having fallen into no errors, belonging to those who do not labour, is not particularly enviable.

In conclusion, Baron Liebig refers to a biography of Professor Mulder, which relates that he could not understand the chemical lectures during the first year of his studies—a circumstance which troubled him, but did not repel him from the study of chemistry, in pursuit of which he learnt by heart Orfila's "Elements of Chemistry," and afterwards the first volume of Thenard's "Manual," certainly a singular way of qualifying himself as a chemist; but one Baron Liebig thinks which explains much that he has done.

However, Baron Liebig regards himself as being most of all to blame, as having done most harm in regard to the recognition and diffusion of his doctrines, and having been their greatest enemy; and that, by endeavouring to devise a manure which should serve to restore the fertility of land exhausted by cultivation. This manure is considered by Baron Liebig to mark an epoch in the development of his doctrine, to which he can look now back without regret, as to a point that has been passed. He therefore enters upon the history of this manure, without which he thinks it is probable that agriculture would not yet have gained that sound foundation on which it now rests!

(To be continued.)

PHARMACY, TOXICOLOGY, &c.

Blood Crystals.

The formation of crystals in blood excited great interest some years since, especially in Germany, and quite recently M. Pasteur* has observed that when fresh blood is placed in contact with pure air at 30° C., under such circumstances that it does not putrefy, these crystals are formed with remarkable facility.

During the first few days the serum gradually acquires a brownish tint, and simultaneously the globules disappear, while the serum and the clot become filled with well-defined acicular crystals of a brownish or red colour. After some weeks not a single blood-globule remains either in the serum or the clot. Each drop of serum contains thousands of crystals, and the smallest particle of the clot squeezed between glass plates presents the colourless fibrin, very elastic and associated with a mass of innumerable crystals, without any trace of globules.

PHYSICAL SCIENCE.

On the Colouration of Hydrogen Flame by Phosphorus and its Compounds.—Phosphorus Spectrum, by MM. P. Christofle *and* F. Beilstein.

M. Wœlher first announced (*Ann. der Pharm. und Chem.*, vol. xxxix., p. 251), now a long time since, that phosphorous acid communicated a beautiful green colour to a hydrogen flame, and that a very small quantity only was required to produce this phenomenon. M. Dusart (*Comptes-Rendus*, vol. xliii., p. 1126) has since carried out these experiments, by extending them to phosphorus, and on these facts M. Blondlot (*Journal de Pharmacie et de Chimie*, vol. lv., p. 25) has founded various methods for the toxicological investigation of phosphorus. We have taken up this inquiry, and, by means of spectrum analysis, have obtained very precise results.

We took a flask holding about one litre, furnished with a disengaging tube, and to one end of it we fixed a platinum point. In this balloon we produced a disengagement of hydrogen, and, after ascertaining that this flame produced no ray in the spectrum apparatus, we introduced about as much phosphorus as is found on a match end. The interior of the flame assumed almost immediately the beautiful emerald-green colour mentioned above.

On subjecting this flame to MM. Kirchhoff and Bunsen's apparatus there appeared to the left of the sodium ray two beautiful green rays, besides a third, a little less vivid, between the two first and the sodium ray.

We have given a plate representing the spectrum observed by us; the two rays α and β are nearly equally intense; ray γ is the most feeble, and the ray α the brightest. We give also the five green barium rays, because two of them are very similar to the phosphorus rays. In the first place the rays P β and Ba δ exactly correspond, and the rays P α and Ba δ are separated by only two divisions. We have many times repeated this experiment, both with ordinary and with red phosphorus, and have at every repetition obtained identical results.

The same phenomenon takes place with phosphorous and with hypophosphorous acids.

From the extreme sensibility of this reaction, and the exact results obtained, it is evident that the process may be very usefully employed for the detection of phosphorus in cases of poisoning. In M. Dusart's paper, he states that the peculiar odour of hydrogen obtained with iron, as well as the green colour of the flame, is due solely to the presence of phosphorus. The researches now proceeding in the Gœttingue laboratory tend to prove that this odour is owing to the formation of a hydrocarbide.

As to the colouring of the flame, our own experiments corroborate M. Dusart's results.

We took an iron wire, supposed to contain no phosphorus, and introduced it into an apparatus similar to that described above; and, to avoid any colouration from the hydrochloric acid, attacked the iron by sulphuric acid. The hydrogen flame thus obtained immediately assumed the green colour before mentioned, and with the spectral apparatus gave rays exactly similar to the phosphorous rays. We also took chemically pure iron, reduced from the oxalate by hydrogen, and, after attacking it by sulphuric acid, obtained only a colourless flame, producing no ray in the spectrum.

It should be remarked that phosphide of iron, prepared either by melting the metal with a mixture of calcined bones, charcoal and sand, or by attacking the iron directly with phosphorus, disengages no hydrogen. The iron is here rendered completely passive. But in the other case, the phosphorus volatilises, being in very small quantity compared with the excess of iron.

Acting on this remark, we introduced phosphide of iron, prepared by one of these methods, into a balloon disengaging hydrogen produced by zinc, taking care to test the flame before introducing the phosphide of iron. In this way we produced the green colouration and the three rays characteristic of phosphorus.

This fact appears analogous to that observed with antimoniide of iron, which does not disengage hydrogen, but when it is introduced into a balloon disengaging hydrogen this gas is evolved rich in antimony. One of us will shortly publish a memoir on the subject.

In conclusion, we may remark that in analysis it would be advisable to avoid losing the notable amount of phosphorus carried off with the hydrogen when iron is dissolved by an acid.—*Comptes-Rendus*, March, 1863.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, May 21, 1863.

Professor A. W. Williamson, *Ph.D., F.R.S., President, in the Chair.*

The minutes of the previous meeting having been read and confirmed, and the several donations to the library thankfully acknowledged, Mr. John Ewen Davidson, and

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* *Comptes-Rendus*, lvi., 739.

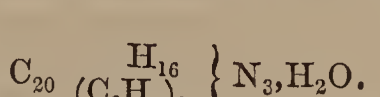
Mr. Samuel Parr, of Nottingham, were balloted for, and unanimously elected Fellows of the Society. Mr. Esson and Mr. F. Vernon Harcourt were, in the same capacity, formally admitted by the President.

A short paper, descriptive of the hydride of heptyl, was read by the Secretary. Among the several compounds of this group (C_7H_{16}) which had been prepared and analysed, were, besides the hydride, the acetate, chloride, iodide, and the alcohol. The hydriodate of heptyline had likewise been examined.

Dr. A. W. **Hofmann** gave a short account of the blue dye made from aniline, which, in the hands of Messrs. Simpson, Maule, and Nicholson, had recently become an important article of manufacture. About twelve months ago the speaker analysed the magenta dye and other compounds of rosaniline, and announced the formula of the base itself as being represented by

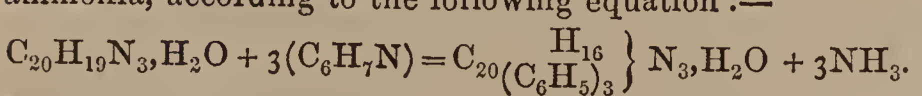
$$C_{20}H_{19}N_3,H_2O.$$

This substance, as well as some of its red saline combinations, might be converted into a splendid blue colouring matter by heating with an excess of aniline; this method constituted, in fact, the basis of a patent taken out in this country by MM. De Laire and Girard, of Paris. The details of their process had since been greatly improved and elaborated by Mr. Edward Chambers Nicholson, to whom the author was indebted for the pure sample of blue dye which had now been submitted to analysis. The experimental results indicated for the base itself the formula:—



$$C_{20}\left.\begin{matrix}H_{16}\\(C_6H_5)_3\end{matrix}\right\}N_3,H_2O.$$

So that this body would be termed "*triphenylated rosaniline*," and be produced with the liberation of ammonia, according to the following equation:—



$$C_{20}H_{19}N_3,H_2O+3(C_6H_7N)=C_{20}\left.\begin{matrix}H_{16}\\(C_6H_5)_3\end{matrix}\right\}N_3,H_2O+3NH_3.$$

Mr. W. R. **Grove**, Q.C., F.R.S., then delivered a most interesting lecture "*On the Action of Heat upon Certain Fluids.*" The author commenced by announcing that the facts which he proposed to bring under the notice of the Society were the experimental results of an unfinished series of researches made some sixteen years ago, but which had, nevertheless, the character of novelty, from the circumstance that many speculations had been advanced in the interim, and yet remained unanswered, for want of more precise information in regard to many points involved in the subject upon which he had formerly been working; the professional claims upon his time were now such as to preclude the idea of continuing these researches, and presenting them in a more complete form. Some very remarkable observations upon the ebullition of water were published many years ago by Professor Donny, of Ghent, and it was these statements principally which induced the author to study the action of heat upon various liquids under different degrees of atmospheric pressure and other alterable circumstances. Every one was acquainted with the fact of water boiling quietly in an open saucepan, whilst the same liquid behaved very differently when heated in a long glass tube drawn out to a fine point, which, permitting the escape of steam, would not allow the water again to absorb the gases of the atmosphere; under these circumstances, the "bumping" or concussions, which were also to be noticed in the distillation of sulphuric acid, were observed; and if, further, the air-pump were employed to facilitate the extraction of the air from the water, and the upper portion of the glass tube cut off with a file, it might happen that the renewed application of heat would induce a sudden and violent escape of vapour, amounting almost to explosion. Another illustration of the same fact could be seen by placing a flask with a little hot water under the receiver of an air-pump, and then arranging in connection with the same a platinum wire, which could be heated to a tolerably constant temperature beneath the surface of the water by means of an external galvanic battery. On exhausting the air, it would be found that the ebullition occurred only at intervals,—often a minute would elapse, and then a burst of vapour would almost eject the contents of the flask; but, this over, the water would again become perfectly tranquil, and remain so for another minute, when another tumultuous ebullition would occur, to be succeeded by a period of rest; and the same phenomenon would be repeated at such regular intervals, that the apparatus might almost serve as an indicator of time. In the opinion of the lecturer there was a parallel between the kind of ebullition here noticed and that observed in the Geysers, or boiling springs of Iceland. In both cases a column of water, partially deprived of air, was exposed to an external source of heat, and an interrupted kind of ebullition induced in consequence. Again, if, in the last experiment, a thermometer were placed in the flask of water, it would be found that the temperature alternately rises and falls some few degrees, and it could not be asserted that the boiling point of water was constant, for it depended upon the amount of air contained in solution; indeed, the lecturer believed that no one had yet succeeded in observing the boiling point of absolutely pure water. As a proof of the difficulty experienced in entirely expelling the air (or dissolved gas) from water, the following experiment was described:—A long glass tube closed at one extremity was bent in the middle nearly to a right angle; the closed limb was then half filled with water, from which, by long boiling, the air was supposed to have been expelled; the remaining space in the tube was then completely filled with olive oil, and the open extremity dipped into a small basin of the same. Heat was now applied to the tube until the water boiled, and this temperature maintained for a considerable time. Each bubble of steam which left the surface of the water passed through the column of oil, becoming smaller and smaller during its ascent, but never did it condense without leaving a microscopic bubble of gas, which at length accumulated so that it could be examined. It was found to consist of pure nitrogen. The author had never succeeded in expelling the whole of this gas from aqueous solution without evaporating the entire bulk of the water itself; the evaporation of even nineteen parts out of twenty did not secure the immunity from nitrogen of the remainder. On boiling ordinary water, air containing a slightly increased proportion of oxygen was first driven off, the oxygen gradually diminished until pure nitrogen was finally expelled. The avidity with which such water again absorbs air is remarkable; "it takes it up again almost like a sponge absorbs water." By a slight modification in the apparatus, the experiment was repeated with mercury, instead of oil, in contact with the boiling water. It furnished a similar result. The author next took bromine, in order to test the possibility of converting into vapour an absolutely pure liquid. This elementary substance was boiled in a long glass tube with only a narrow orifice for the escape of air and vapour; after long-continued ebullition the extremity of the tube was sealed at the blowpipe, and the contents afterwards examined. It was discovered in this instance that a certain quantity of oxygen occupied the space above the liquid bromine. He would hesitate to assert that this oxygen was originally held dissolved in the bromine, for it was still possible that a trace of water in the sample of bromine was decomposed, the hydrogen of which formed hydrobromic acid, and the oxygen in consequence liberated. The liquid chloride of iodine behaved in a precisely similar manner. Even sulphur and phosphorus when strongly heated in a closed tube over mercury, gave off in each instance a gas, which proved to be sulphuretted hydrogen, or the non-inflammable modification of phosphoretted hydrogen. It was whilst pursuing these experiments that the author first noticed the lines of stratification in the electric discharge. On one occasion phosphorus was heated in an exhausted receiver by means of the spark from a powerful induction coil, when suddenly an interrupted spectrum presented itself, the bands or striæ being accompaniad by the simultaneous conversion of the phosphorus into the red allotropic modification, which had not at that time been recognised. This optical phenomenon had no effect upon the barometric gauge; and, on repeating the experiment several times, it was always found to coincide with the moment of physical change; and when, from some undetermined cause, no red phosphorus was found, the striæ never made their appearance. Upon another occasion a powerful burning glass—a lens of eight inches diameter—was used for the purpose of directing the sun's rays upon the phosphorus inclosed within an atmosphere of nitrogen; the conversion of the white to the red modification was equally apparent, and suggested to the author the idea of allotropy, which subsequently was announced by Professor Schrötter. The red product could not in this case have been an oxide of phosphorus, as commonly supposed. With respect to the decomposition of water by heat, the lecturer described several experiments which indicated clearly the power of a heated platinum surface to effect the disunion of the gaseous components of aqueous vapour. Water was kept boiling in a long upright tube, the closed upper extremity of which had a loop of thin platinum wire sealed within the glass; by connecting this wire with a battery, and passing an electric current sufficient to heat it to redness, it was found, by condensing the steam, that a small bubble of oxy-hydrogen gas, mixed with a little nitrogen, remained. The volume of explosive gas did not, however, increase, for a state of equilibrium was soon arrived at, when the degree of dilution by steam was sufficient to counteract the tendency to recombination; but the author had, by a different arrangement, succeeded in augmenting this effect; for this purpose he used a glass bulb for the insertion of the platinum wires, and the steam, partly decomposed, escaped through a constricted passage into another tube or limb of the apparatus, where it was altogether out of the influence of the heat which would have caused its explosion. For the more rapid decomposition of water, an apparatus was devised, in which steam, being disengaged from a silver generator, was passed through a short tube of platinum intensely heated by means of a blowpipe flame; the mixed gases resulting were swept out of the apparatus as soon as produced, and might be collected in notable quantity. The lecturer made a remark to the effect that M. St. Claire Deville had recently devised an apparatus of a similar nature and purpose, and although that distinguished chemist had made a complimentary reference to his published investigations, he did not think that he could have read the original paper, or he would not again have described the apparatus figured among the illustrations appended to the author's Bakerian lecture, which appeared in the *Philosophical Transactions* of the Royal Society for 1847. A number of striking facts regarding the solubility of gases in water could be enumerated. It was well known that the two gases did not appear in exact proportion when collected in the voltameter. The gas battery (Grove's) furnished a very conclusive proof of the rapidity with which water absorbed oxygen from the atmosphere. In the ordinary construction of this instrument two tubes and platinum plates were supported in two of the three necks of a Woulff's bottle, the remaining aperture, provided with a stopper, being reserved for the introduction of the fluid to be decomposed. The wires leading from the platinum plates were connected with a galvanometer, and one tube only charged with hydrogen; the apparatus being left at rest, soon allowed the magnetic needle to stand at zero, but it was only necessary to remove the stopper for an instant, when immediately the needle of the galvanometer would be deflected several degrees. It had been supposed that oxygen disappears in the voltameter by forming ozone; there was another way in which the diminution in volume could be exhibited. Provide a test-tube having a single platinum wire hermetically sealed within the closed extremity; half fill with pure dry oxygen gas over a mercury trough, and then bring the poles of a Rühmkorff coil to discharge through the oxygen gas; a short time will suffice to diminish the volume, and, if a longer interval be allowed, the whole of the oxygen will disappear, being probably converted into ozone, and then absorbed by the mercury. The author was unable to say whether a minute pulverulent deposit observed upon the platinum wires was an oxide of that metal. The general conclusion drawn from the foregoing experiments was to the effect that water had a very powerful affinity for the gases of the atmosphere; that by several processes the oxygen could be eliminated, but the nitrogen resisted all attempts to expel it from solution; so much so, that it might be doubted whether chemically pure water (*i.e.*, a compound of two elements only) had ever been prepared; and further, that ebullition (as applied to water) under all circumstances, consisted merely in the production and disengagement of bubbles of aqueous vapour, formed upon a nucleus of permanent gas. The question was raised as to whether nitrogen is so absolutely inert a body as had formerly been supposed? Many facts seemed to show that there was some hitherto undetected relation subsisting between nitrogen and water, for they were always associated in nature, and, once combined, their disunion appeared almost impossible. In submitting these facts to the notice of the Society, the author regretted they had not been more completely worked out; but fearing that his own opportunities would not permit of his again attacking the subject and carrying it to a more worthy termination, he now invited further experiment in the several directions indicated, and would be well satisfied in the thought that his own inquiries had served as the fulcrum in future investigations.

The President, having lately been making some pneumatic experiments in conjunction with Dr. Russell, bore testimony to the extreme difficulty met with in attempting to exclude air from the vacuous space of the glass receiver. The bell jar had been screwed down upon the plate with india-rubber washers between, mercury joints, and a variety of adhesive substances had been tried, but they could not succeed in excluding the air for a lengthened period.

Professor Graham considered there must be a statical point at which the amount of nitrogen retained in solution by the water could only be got rid of by boiling, as the necessary consequence of its evaporation; it would be a matter of interest to determine exactly the relation between water and nitrogen at this fixed point. With respect to the decomposition and formation of water at the same temperature, it appeared that the affinities were often governed by mass; the disunion of the elements of water ceased when the explosive gas amounted to one four-thousandth part of the volume of aqueous vapour. In the case of steam being passed through a white-hot platinum tube, or one of clay, or porcelain, it appeared that the light accompanying this intense heat might be the agent directly effecting the decomposition; all these substances were distinguished for their powerful incandescence in the flame of the blowpipe.

Dr. A. W. Hofmann mentioned a fact in striking confirmation of Mr. Grove's results. Some years ago he had occasion to analyse an organic body, in which the nitrogen could not be determined by conversion into ammonia, and he had therefore recourse to M. Dumas' method of direct estimation, but, as a preliminary step, the apparatus required to be filled with pure carbonic acid. He prepared this gas in the usual manner—from marble and dilute hydrochloric acid—and attempted to displace the air from the apparatus, but, upon collecting portions of the gas from time to time over solution of potash, there remained unabsorbed "that eternal bubble of gas," which no amount of perseverance enabled him to overcome or diminish.

Dr. W. A. **Miller** believed that the want of accordance in the proportions of oxygen and hydrogen, when collected over water in the voltameter, might be accounted for by the greater degree of solubility of the gas first-named; he was not satisfied with the explanation based upon its supposed conversion into ozone, since the latter was less soluble in water than oxygen itself.

Mr. **Grove** replied to some of the statements and inquiries made in the course of discussion. The actions of liquid and gaseous bromine upon heated platinum wires; the difficulties experienced in hermetically sealing platinum wires within glass tubes; the power of air to creep along glass surfaces even under the level of mercury; the ascent of air into barometer tubes, and its detection by holding a red-hot iron above the mercury column; were among other points severally referred to by the lecturer.

A vote of thanks to Mr. Grove, proposed by the President, met with a hearty response on the part of an unusually large assembly. Before adjourning the meeting until June 4, it was announced that the following papers were already in hand, viz.:—"*On the Hexyl Group*," by MM. Wanklyn and Erlenmeyer; "*On a Remarkable Instance of Electrolytic Action*," by F. A. Abel, Esq., F.R.S.; M. Marcellin Berthelot, of Paris, would also deliver a lecture "*On Synthetic Methods in Organic Chemistry*."

ROYAL INSTITUTION OF GREAT BRITAIN.

Friday, May 22.

The lecture was by Professor **Roscoe**, "*On the Direct Measurement of the Sun's Chemical Action.*"

The lecturer began by stating that animal life might be considered as a process of oxidation—the tissues of the body undergoing combustion in the oxygen of the atmosphere, and forming carbonic acid. Without some counteracting agency, an animal, so long as it breathed, might be said to be slowly working its own destruction, by forming a gas incapable of supporting life. The counteracting agency Nature supplied in the life of plants, which might be described as the opposite process of reduction, carbonic acid being decomposed and oxygen evolved. Animal force then was derived from the vegetable organism, and animals did not create, but only manifested force, which was regulated by the same law as that of the steam-engine, the force representing exactly the amount of fuel consumed. For vegetable life a continual stimulus was necessary, and this was supplied by the sun, the chemical radiations of which brought about the decomposition of carbonic acid by plants, in the same way as they decomposed chloride of silver on photographic paper, and caused the combination of hydrogen and chlorine. [That all the rays of light were not of equal chemical activity the lecturer showed by exposing a thin bulb filled with a mixture of hydrogen and chlorine, first to red light, which did not, and then to violet, which did cause an explosion. The experiments (which were most effective) were made by burning phosphorus in oxygen in globes of red and violet coloured glass.] The measurement of the chemical activity of solar light, the lecturer continued, was a subject of great importance and interest, inasmuch as it afforded a means of estimating the vegetable, and consequently the animal producing power of a country or situation. He had therefore made experiments with a view to obtain an easy and exact method of measuring the chemical action exerted by the total sunlight at any point of the earth's surface under the different conditions of situation, climate, and state of atmosphere. It was known that the real climate of two countries might differ very widely, and yet the mean temperature of the two be nearly the same. This was shown by the vegetation of the two. To illustrate this, the lecturer exhibited photographic views taken near Carlisle and in one of the Shetland islands, the mean temperature of these two localities being nearly the same. The vegetation about Carlisle, however, was luxuriant, while not a tree was to be seen in the Shetland views. These differences were owing to the different intensity of the chemical action of light, and not to temperature. The foggy, cloudy atmosphere of the Shetlands absorbed the chemical rays which would stimulate vegetation. It was of consequence therefore to have a means of determining the chemical activity of light, the measurement of which would create a new and important branch of meteorological science. In conjunction with Professor Bunsen, the lecturer had some years ago constructed a chemical photometer for estimating the chemical activity of light by its effect on a mixture of hydrogen and chlorine. This instrument, however, was too complicated to be available for meteorological purposes. Another and simpler way of measuring the activity of diffused or direct light on any given spot was necessary, and the mode proposed was the registration of the tint produced on photographic paper. But before such a mode could be made available it was necessary to know how to obtain photographic paper of constant sensitiveness, and also what relation there existed between the degree of tint produced, and the time, and intensity of light necessary to produce it. After a series of experiments, the lecturer had succeeded in preparing a paper of constant sensitiveness, and in the method proposed it was not the tints, but the amounts of light which were compared. The method was founded on observation. It was possible to measure the time required to produce a given tint on standard paper, and the intensity which produces this tint in the unit of time is called the unit of chemical intensity. If two units of time are necessary, the intensity will be one-half, and so on. The time of insolation is measured by means of a pendulum photometer in which the strip of prepared paper is exposed for a varying, but exactly known time. By the action of the pendulum a piece of blackened mica is passed and repassed over the paper. Different parts of the paper are necessarily exposed for different lengths of time, but the time of exposure at any part can be calculated exactly from a scale. The strips of paper thus insolated exhibit a regularly diminishing shade from black to white, and the point at which the paper has a shade equal to a normal tint, fixed by means of hyposulphite of soda, can be accurately read, by comparing the two strips by the aid of monochromatic soda light. [The lecturer showed the effects of the soda flame on the words "monochromatic light" printed in letters of various colours, and also the way in which the exposed papers were compared; but it would be impossible to make the latter intelligible unless the apparatus was before the reader.] The experiments by Professor Roscoe and another at Manchester have shown that two observers may closely agree in their estimations of tints by this means. The lecturer then briefly referred to some measurements he had made of the chemical brightness of various points of the sun's surface. From these it appeared that the chemical brightness of various parts of the sun's disc is very different, the central part being from three to five times brighter than portions of the edge. He had found, too, that the brightness of the picture varied irregularly,—a coarse mottled appearance being produced, very different from the ordinary mottling seen on the surface of the sun. He had satisfied himself that this appearance was not due either to the paper or the lens, and he thought it probably arose from clouds in the sun's atmosphere, and might be connected with the red prominences seen in eclipses.

In conclusion, Professor Roscoe said he was convinced that interesting and valuable results might be obtained by the investigation he had described, and he looked forward to the time when all meteorological observatories would be furnished with instruments for pursuing it.

NOTICES OF BOOKS.

The Universal Text-book of Photography. Leeds: Harvey, Reynolds, and Fowler. Price 1s.

We have great pleasure in recommending this manual to our photographic readers, as conveying every possible information that is necessary for a practical photographer to know. It is published by Messrs. Harvey, Reynolds, and Fowler, of Leeds, one of the most celebrated photographic firms in the north of England, and is, we believe, from the pen of Mr. Fowler. It contains twenty chapters on all kinds of subjects, from choosing a lens to foretelling the weather. Besides the usual information conveyed in such books, it contains a chapter on "The Æsthetics of Photography," from the French of M. Disderi, one of the few photographers who are also artists. We should be only too glad if every photographer in England would read this chapter, and lay the truths and advice contained in it home to his own productions, we should then have fewer of those unartistic abominations which disfigure our albums and exhibitions. English photographers are notoriously unartistic, and we are sorry to say that but little improvement seems to be taking place in this respect. The carte de visite mania has reduced photography almost to the level of hair cutting; and such is the haste with which portraits are now taken, that a photographer in good practice boasted to us the other day that he had taken forty portraits in six hours.

The remarks made by Mr. Fowler on using different collodions and developers, according to circumstances, are excellent. A photographer should have it in his power (more especially in our changeable climate) to adapt his materials at a moment's notice to the various changes in light and temperature which are continually occurring. The chapter on "Enlargement" is evidently the result of much practical experience in this direction. The directions given for "vignetting" will do much to popularise this very artistic method of printing.

At the end we have a chapter on photographic copyright giving plain directions for the registration of photographs, to which is appended the "Copyright Act" in full. A chapter on "weather wisdom" will be found particularly acceptable to the field photographer. Mr. Fowler also gives an account of the new enamel paper for positive prints. This paper is prepared with a highly enamelled glossy surface, composed we believe of oxide of zinc. Some of the specimens that we have seen surpass every other style of printing that we know of, but we must warn our readers that there is a quality, imported we believe from Germany, which is perfectly useless. The author also gives some excellent advice about defects, and the quickest way of remedying them, which will be of especial use to the amateur.

We conclude by once more recommending this shilling's worth to our readers, as giving more chemical, manipulatory, and artistic information about photography than many other manuals a dozen times its size.

NOTICES OF PATENTS.

266. *Composition for Coating Wood, Metal, &c.* J. Gibbins, London Wall, London. Dated January 31, 1862. (Not proceeded with.)

The inventor employs as a protective coating linseed-oil which has been boiled with the oxides of lead and arsenic, and to which also gum copal, borax, and caoutchouc have been added. For some purposes it is recommended to introduce also small quantities of sulphur and oil of turpentine, the first of these ingredients being omitted from all compositions intended to be used afterwards upon such metallic surfaces as are liable to corrode under its influence.

281. *Processes for the Recovery of the Oleic Acid contained in the Residual Scouring Waters of Woollen and other Textile Materials.* M. A. F. Mennons, Paris. A communication. Dated February 1, 1862.

The patentee describes a series of processes for the separation and utilisation of the fatty acids from oil and other oleaginous materials which have served in the scouring of woollen fabrics. These refuse matters are converted into insoluble fatty compounds by saponification and precipitation with lime or other suitable base. By the addition of a mineral acid the insoluble soap is decomposed, and the fatty acid set free is purified by washing. The product is then pure enough for employment in many of the applications for which oleic acid is suitable.

The general purport of this invention is, we believe, very similar to the process by which Messrs. Owen and Merton manufacture their stearic candles from the refuse soap curd and liquors of bleaching and dyeing works.

294. *Improvements in the Manufacture of Hard and Soft Soaps, and in Washing Linen and other Textile Fabrics.* R. A. Brooman, Fleet Street, London. A communication. Dated February 4, 1862. (Not proceeded with.)

These improvements refer to the employment of alkaline silicates as ingredients in the manufacture of both soft and hard soaps. They are likewise directly applicable in the washing of linen and cotton fabrics.

The practice of mixing silicate of soda with hard soap is by no means uncommon, and dates long anterior to this specification; for some purposes this addition is said to prove economical.

305. *A Substitute for Gunpowder.* E. Harrison, Oldham. Dated February 5, 1862. (Not proceeded with.)

The explosive compound here described is a mixture of chlorate of potash, saltpetre, ferrocyanide of potassium, starch, and charcoal.

This invention, like the preceding, lacks novelty. For evidence upon this subject, see the remarks appended to the description of Mr. Harrison's former patent for a similar material. *

319. *Preparation of Pulp for Paper.* J. H. Johnson, Lincoln's-Inn-Fields, London. (A communication.) Dated February 6, 1862. (Not proceeded with.)

The inventor specifies more fully some of the details connected with the mode of manufacturing paper pulp from damaged grass, the refuse of beet-root, zostera, marina, and certain sea weeds, which were mentioned in a former patent bearing date August 22, 1861.†

338. *Treatment of Coprolites, &c.* M. A. F. Mennons, Paris. (A communication.) Dated February 10, 1862.

This invention relates to an improved method of treating coprolites, and other calcareous phosphates, by means of which these minerals are converted into a more soluble and consequently more highly assimilable manure, and at the same time are made capable of disinfecting certain animal products, and of extracting therefrom the greater part of their fertilising principles. The nodules of mineral phosphates to be operated upon are first pulverised as finely as possible by grinding, and with the powder so obtained is intimately incorporated from six to ten per cent. of organic matter highly charged with carbon and hydrogen, such, for instance, as the gas-tar or pitch produced in the distillation of coal. The mixture is then calcined at a temperature between 400° and 500° Centigrade in closed retorts containing charges of about four hundredweight

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* Chemical News, vol. vi., p. 238.

† *Ibid.*, vol. vi., p. 214.

each. By the decomposition of the tarry matter, abundance of gas, containing sulphuretted hydrogen, is evolved. which should be let into the furnace and there consumed. The carbon and hydrogen reduce partially the oxide of iron, opening up the structure of the mineral,—a disintegrating action which is assisted likewise by the escape of carbonic acid and water from the carbonate of calcium. When the disengagement of gas terminates (usually in about an hour), the operation is concluded by withdrawing the charge and placing it in sheet iron receptacles to cool. The product so obtained is a mixture of phosphates with small proportions of quicklime and carbon, and may either be directly employed as a manure, or enriched by an admixture of nitrogenous organic matters, such as blood, fæcal and liquid excreta, or other animal refuse, upon which substances it operates as a disinfectant.

The general scheme of this invention appears well calculated to fulfil the object in view. Sombrero Island phosphate stands especially in need of some such treatment.

Grants of Provisional Protection for Six Months.

1036. Alcide Poirrier, Paris, and Charles Chappat, Fils Paris, "Improvements in the manufacture of blue and violet colouring matters suitable for dyeing and printing."

1084. George Holcroft, Manchester, "Improvements in the construction of pyrometers."

1086. Michael Henry, Fleet Street, London, "Improvements in apparatus for manufacturing béton and artificial stone, pugging clay, and other similar purposes, and in the production of artificial stone, and artistic, ornamental, and decorative articles, works, and surfaces."—A communication from François Coignet, Boulevart St. Martin, Paris. —Petitions recorded April 29, 1863.

192. Heinrich Caro and John Dale, Manchester, "Improvements in obtaining colouring matters, part of which improvements is also applicable to dyeing and printing." Petition recorded January 21, 1863.

940. Richard Archibald Brooman, Fleet Street, London, "Improvements in hardening and colouring gypseous limestone and sand and calcareous stones." A communication from Pascal Nicolas Balthazar Fioravanti, Paris. Petition recorded April 14, 1863.

963. Richard Knight, Dunkirk, France, "Improvements in treating and preparing iron, copper, and other wires for telegraphic and other uses, for the purpose of preserving them from corrosion or decay."

967. Robert Calvert Clapham, Walker, Northumberland, "Treating the waste liquor from bleaching-powder stills, in order to obtain hydrochloric acid and other products therefrom."

973. William Stevenson Macdonald, Manchester, "Improvements in apparatus for drying animal, vegetable, and mineral substances."

985. Alfred Ford, Stewart's Buildings, Battersea Fields, Surrey, and Richard Rigg, Great Winchester Street, London, "An improved method of re-forming and re-using old or waste vulcanised india-rubber."

1015. John Benjamin Daines, Little Argyle Street West, London, "Improvements in the preparation of stone, plaster, compo, iron, wood, and such like substances, so as to preserve them from decay."

1025. William Anthony Shaw, New York, U.S., "A mode of lining lead pipe with tin or its alloys." Petitions recorded April 24, 1863.

1053. Frederick Bennet, Holywell, Flintshire, "An improved method of condensing lead and other metallic fumes and vapours from furnaces."

1089. William Clark, Chancery Lane, London, "Improvements in the manufacture of hydrocyanite of ammonia and of alkaline and earthy cyanides." A communication from Louis Joseph Frédéric Margueritte, Boulevart St. Martin, Paris.

1109. Edmund Richard Southby, Wareham, Dorsetshire, "Improvements in the extraction of scents from plants, flowers, and other odoriferous substances."

1116. William Walsh, Manchester, "Improvements in obtaining and purifying oxalate of soda, which improvements are also applicable to the manufacture of oxalic acid."

1148. Thomas Holliday, Huddersfield, Yorkshire, "An improved blue colouring matter." Petition recorded May 7, 1863.

Notices to Proceed.

42. Charles Tiot Judkins, Ludgate Street, London, "New alloys."—A communication from Moses Gerrish Farmer, Salem, Massachusetts, U.S.

44. John Leigh, Manchester, "Improvements in the treatment of gas produced by the distillation of coal, cannel, bituminous hale, boghead, mineral oils, petroleum, or other combustible substances, and for the obtaining of certain products therefrom."

48. Edward Vincent Gardner, Berners Street, London, "Improvements in the treatment of petroleum and mineral oils, and in apparatus employed therein."

70. Robert Thomson Monteith, St. Malo, France, and Robert Monteith, Crystal Terrace, Cecil Street, Greenheys, Manchester, "Improvements in the manufacture of dyes from aniline and its analogues."—A communication from Georges Auguste Jules Delvaux, Rue Corneille, Paris.—Petition recorded January 8, 1863.

522. Edward Brown Wilson, Parliament Street, Westminster, London, "An improvement or improvements in the manufacture of an alloy or alloys of titanium and iron."—Petition recorded February 25, 1863.

956. Isham Baggs, Cambridge Terrace, London, and William Simpson, Tovill Upper Mills, Kent, "Improvements in purifying and treating coal gas, sulphuretted hydrogen, and other gases containing sulphuretted hydrogen, and in obtaining sulphur, sulphuric, and other acids in such treatment."—Petition recorded April 16, 1863.

105. John Thomas Stroud, Birmingham, "Certain improvements in fixed and portable lights for domestic and other uses, applicable for burning gas and the mineral oils or spirits now so commonly used."

CORRESPONDENCE.

Colouring Matters from Aniline.

To the Editor of the Chemical News.

Sir,—Some time ago I discovered the green and blue colouring matters of Professor Calvert—"Emeraldine, &c." On informing him of these, he at once candidly told me he and others had previously patented the same thing —a parallel case to many inventions.

I have now to announce another source of green colouring matter, identical, so far as I have been able to ascertain, with emeraldine. I noticed in the laboratory where experiments are performed with aniline that the vessel containing bleaching powder was soon covered with a green film.

On submitting sheets of paper kept constantly moistened with aniline to the vapours emitted by "chloride of lime" so-called, I obtained a considerable quantity of this colour; also, I obtained it by replacing the powder by a mixture of nitric and hydrochloric acid. Air is necessary in both cases.

A yellow colour may be obtained from aniline, varying in shade to brown, by dissolving the base in acetic or hydrochloric acid, and passing nitrous fumes cautiously through the solution; the colour precipitates out in an oily layer. The colour is not bright, and I have not succeeded as yet in making either of these colours available as a dye.

I am, &c. John S. Blockey.

Hyde Park, Leeds, May 31.

MISCELLANEOUS.

National Academy of Sciences of the United States.—In the last Session of Congress a Bill was passed incorporating this institution, and on the 22nd of last month a meeting of the corporators named in the Act, was called by the Hon. H. Wilson, who introduced the Bill into the Senate, for the purpose of effecting a temporary organisation of the Academy. Mr. Joseph Henry, of Washington, was elected Chairman, and Mr. Alexis Caswell, of Brown University, Secretary *pro tem.*, and a committee was appointed to prepare and report rules for governing the Academy. The Academy is divided into two classes,—I. The Class of Mathematics and Physics, comprising the following sections :—1. Mathematics ; 2. Physics ; 3. Astronomy, Geography, and Geodesy ; 4. Mechanics ; 5. Chemistry. II. The Class of Natural History, comprising the sections,—1. Mineralogy and Geology ; 2. Zoology ; 3. Botany ; 4. Anatomy and Physiology ; 5. Ethnology. Members choose to which of the two classes, and to which section of that class, they will belong ; but they may also be elected honorary members of any section, by the members thereof, and the Academy has the power of transferring members from one section to others. There may be fifty foreign associates who will take no part in the business of the Academy, but will have the privilege of attending its Sessions, reading and communicating papers, and of receiving a copy of the publications of the Academy. The officers are to be elected for a term of six years. Each class will have a chairman and secretary elected annually. These officers, together with four members, also elected annually, will constitute a council for the transaction of business. The Academy will hold two special meetings in January and August of each year. The scientific meetings are to be open to the public, the business meetings closed. Communications by persons not members of the Academy are to be presented and read by a member, who will be responsible for the general propriety of the paper, but not for the opinions of the author. Propositions for researches, experiments, investigations, or reports, are to originate with the classes to which their subjects are appropriate, and are then to be submitted to the Academy for discussion and approval or rejection, excepting propositions from the United States Government, or any of its branches, which are to be acted upon by the President, who, in such case, will report at once to the Government, if necessary, and to the Academy at the next special meeting. The judgment of the Academy is to be at all times at the disposal of the Government, upon any subject of science or art within its compass, and the President will have the power, in special cases, of calling in the aid, upon committees, of experts, or of men of remarkable attainments, not members of the Academy. An annual report is to be presented to both Houses of Congress, and is to be prepared by the President of the Academy and submitted, first to the Council, and then to the Academy at the January meeting, before its presentation. The officers elected are Alexr. D. Bache, President, James D. Dana, Vice-President ; Louis Agassiz, Foreign Secretary ; Wolcott Gibbs, Home Secretary ; Fairman Rogers, Treasurer ; B. Pierce and B. A. Gould, Chairman and Secretary of Class A ; B. Silliman and J. S. Newberry, Chairman and Secretary of Class B. After the completion of the organisation, each member present, agreeably to the requirements of the organic law, took the oath of allegiance prescribed by the Senate of the United States for its own members, and in addition thereto took an oath faithfully to discharge the duties of a member of the National Academy of Sciences to the best of his ability. Born in the midst of a great political revolution, the National Academy of Sciences, created by the supreme law of the land, stands pledged to the power which has called it into being, and to the world to discharge its duties with fidelity. The members of the Academy named in the Act had before them simply to accept or to decline the trust reposed in them, by no choice of theirs. So far as they have accepted their position, we feel justified in saying it is with a conviction that there were many not named on the list who might most properly have been there, and with the assurance that, so far as any honour may attach to membership, it will be shared much more largely by those who shall hereafter be called by the suffrages of the Academy to fill such vacancies as must occur, than by the corporators who are named in the law. The National Academy of Sciences does not take the place of, or necessarily interfere with, the American Association for the Advancement of Science, as many persons seem to have supposed.—*From the American Journal of Science and Arts*, May, 1863.

SCIENTIFIC SOCIETIES.

MEETINGS FOR THE ENSUING WEEK.

June 8. *Monday.*

Geographical—Burlington House. 8.30 p.m.

9. *Tuesday.*

Medical and Chirurgical—53, Berners Street. 8.30 p.m.
Zoological—11, Hanover Square. 9 p.m.
Syro-Egyptian—22, Hart Street, Bloomsbury. 7.30 p.m.
Royal Institution—Albemarle Street. 3 p.m. Professor Tyndall, "*On Sound.*"

10. *Wednesday.*

Microscopical—King's College, Strand. 8 p.m.
Literary Fund—4, Adelphi Terrace. 3 p.m.
Royal Society of Literature—4, St. Martin's Place. 8.30 p.m.
Archæological Association—32, Sackville Street. 8.30 p.m.

11. *Thursday.*

Royal—Burlington House. 8.30 p.m.
Antiquaries—Somerset House. 8.30 p.m.
Philosophical Club—6 p.m.
Royal Institution—Albemarle Street. 3 p.m. Prof. Ansted, "*On Geology.*"

12. *Friday.*

Astronomical—Somerset House. 8 p.m.
Royal Institution—Albemarle Street. 8 p.m. Prof. Tyndall, "*An Account of some Researches on Radiant Heat.*"

13. *Saturday.*

Royal Botanic—Inner Circle, Regent's Park. 3.45 p.m.
Royal Institution—Albemarle Street. 3 p.m. Prof. W. Thomson, "*On Electric Telegraphy.*"

ANSWERS TO CORRESPONDENTS.

*** All *Editorial Communications* are to be addressed to the Editor, and *Advertisements* and *Business Communications* to the Publisher, at the Office, 1, Wine Office Court, Fleet Street, London, E.C.

*** In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

Vol. VI. of the Chemical News, containing a copious Index, is now ready, price 10s. 8d., by post, 11s. 2d., handsomely bound in cloth, gold-lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. 6d. if sent to our Office, or, if accompanied by a cloth case, for 1s. Vols. I. and II. are out of print. All the others are kept in stock. Vol. VII. commenced on January 3, 1863, and will be complete in 26 numbers.

Chemicus.—A scientific bookseller will be best able to give the information required.

J. A., Reading.—Sulphide of silver might be recovered by filtering the liquid in which it is suspended.

J. W., Neilston.—It is the character of bismuth salts to be precipitated by addition of water to their solutions in acids.

J. M., Huddersfield.—The method of preparing sulphindigotic acid is described in Gmelin's, Graham's, Fownes's, or almost any other work on Chemistry.

THE CHEMICAL NEWS.

Vol. VII. No. 184.—June 13, 1863.

PHOTOGENIC GAS.

A company has recently been formed for the purpose of introducing M. Mongruel's invention[*] into general use. According to the specification of his patent, this consists in a particular form of apparatus applicable to the saturation of ordinary gas or atmospheric air, with the vapour of a volatile hydrocarbon, so as in the one case to increase the illuminating power, and, in the other, to produce an illuminating gas.

The carburation of gas by such means is no novelty, inasmuch as it was one of the earliest improvements of gas-making suggested by Mr. George Lowe so far back as 1832.

The production of an illuminating gas by saturating atmospheric air with a volatile hydrocarbon vapour is also no novelty, since it is one of the means by which Mr. Mansfield proposed to use the more volatile liquids, obtainable from coal tar, for producing light.

When M. Mongruel's project was first made known in this country, however, the accounts given of it were calculated to lead to the belief that it was an entirely new invention. A certain amount of mystery was thrown around the liquid employed, which was not without its effect in bringing many people to believe that the invention was in that. But to any one at all acquainted with the subject it was evident that there could not be any great secret in this liquid, and that invention was out of the question with regard to it. There was no doubt that the liquid used was a hydrocarbon of sufficient volatility to be diffused through the air in such amount as to render it capable of burning with a luminous flame. Moreover, an abundant source of such a liquid had just before been discovered in the American petroleum, and precisely this very volatile portion was then still without any useful application. There is no longer any doubt about the nature of M. Mongruel's invention. It is simply a particular apparatus devised for the purpose of bringing air or gas into contact with the liquid hydrocarbon to be volatilised; in fact, a special means of carrying into effect the suggestions of Mr. Lowe and Mr. Mansfield.

The advantage claimed for this apparatus of not impoverishing the liquid diffused into the gas or air, will obviously depend solely upon the nature of the liquid itself. If the liquid employed is not uniformly volatile, but if, like the coal tar naphtha used by Mr. Lowe, it consists of a mixture of unequally volatile liquids, the result in M. Mongruel's apparatus will be the same as in Mr. Lowe's, viz., that only a portion of the liquid—the more volatile portion—will be volatilised, and the remainder will be, as M. Mongruel expresses it, "impoverished," or in plain English it will be incapable of being volatilised into the air or gas, and of being retained by them during their passage through the pipes to the place where the light is to be produced. This was just the result obtained in naphthalising gas by Mr. Lowe's method; and, in the case of Mr. Mansfield's plan of using benzol, the difficulty was in preventing the hydrocarbon from becoming solid, in consequence of the cooling effect of its evaporation.

Hence it is absolutely necessary to have for this purpose an extremely volatile liquid, and one which is at the same time of uniform volatility, not a mixture of liquids differing in volatility. No statement is made as to the cost at which a liquid so suited for the purpose can be obtained, though there are reasons for the opinion that the volatile oil of the American petroleum will maintain a high price.

It is stated that 1000 cubic feet of gas require nearly a gallon and a-half of the liquid, so that it may be expected that the cost of the gas will be doubled. At the same time it is stated that the illuminating power is increased fourfold.

One of the advantages claimed for this apparatus is the purification of the gas carburated, but it is not stated how the purification is effected, nor does there appear to be any possibility of the gas being in any way purified by this treatment.

Altogether the prospectus, though loud in praise of the illuminating power of the gas, is quite silent with regard to many important particulars on which the success of this project depends in a far greater and more essential degree than it does upon the use of M. Mongruel's generator, however ingenious that may be in its construction, and however efficient for saturating gas or air with a liquid of sufficient volatility. In one of the testimonials attached to the prospectus, incidental reference is made to a carburating liquid so volatile that it vapourised at a temperature one degree above the freezing point, and there is much reason to think that if such a liquid be requisite this plan of obtaining light would be very costly. Among other particulars of this kind, it might have been expected that some data would have been given as to the degree of volatility necessary for the liquid used, the cost of it, the cost of the apparatus, and the degree of security or provision against risk of explosion by the production of an explosive mixture of air and hydrocarbon vapour. These are all most important points, of which no mention is made.

It is not, therefore, to be wondered at that, in the absence of any satisfactory statements as to these points, on which the success of the company's enterprise is so greatly dependent, no little surprise is felt at the very large sum of 50,000*l.* proposed to be paid to the inventor of the mere apparatus for effecting a result which, if desirable, might be easily obtained by many other means far less costly, and probably not at all less efficient.

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[*] Chemical News, February 21, 1863.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

An Alleged New Metal.

Mr. Sonstadt,* who has recently obtained a patent for the manufacture of magnesium, considers that the black residue mentioned by MM. Deville and Caron, as being left when impure magnesium is distilled, contains a new metal. He describes the following process for obtaining it:—

An intimate mixture of calcined magnesia with one-eighth its weight of amorphous phosphorus, is ignited in a covered porcelain crucible until phosphorus vapour is no longer given off. The grey residue is extracted by hydrochloric acid, and the undissolved portion repeatedly boiled with hydrochloric acid. What then remains is nearly black; it is ignited with dry hydrate of soda, the mass digested in water, and the yellowish insoluble portion, well washed by decantation. This yellow substance, dissolved in acid, gives the following reactions, said to be characteristic of the new metal. The solution gives a blood-red colour with alkaline sulphocyanide, a blue precipitate with ferrocyanide solution, and, after treatment with reducing agents, a blue precipitate with ferrocyanide solution. But, unlike true Prussian blue, the colour of these precipitates is not changed by ammonia, unless they are contaminated with iron.

The oxide, precipitated by an alkali, is said to require much more intense heat for its reduction than iron oxide, and the dark spongy mass obtained, is not magnetic. Mr. Sonstadt has not found any reagent capable of separating the new metal from iron, nor yet any reagent capable of entirely separating it from a magnesian solution, although every trace of iron may be precipitated.

Anomalous Vapour Densities.

M. Cahours has long since shown that the vapours of some liquids do not present the characters of permanent gases, except at temperatures much above the respective boiling points. In determining the density of such a vapour at temperatures rising from near the boiling point of the liquid to a temperature at which the density presents no further change, it is easy to perceive that, until that limit is reached, the vapour is not in a definite condition, but the densities at different temperatures, represented graphically, correspond to a curve gradually approximating to the axis of the abscissæ, finally becoming parallel with it, and remaining so until the degree of temperature is reached at which there is a disassociation of the constituents of the molecules.

This anomaly, presented by many different substances, seems to be independent of the nature of the vessel containing the vapour. It may be that, at temperatures near the boiling point of a liquid, a portion of the liquid remains dissolved in the vapour, giving rise to the excessive density, and that, as the proportion of liquid so dissolved decreases with elevation of temperature, it ultimately disappears, and then the vapour conforms to the law obtaining for permanent gases.

It is of interest to inquire what is the effect produced by introducing into the molecule of a substance which gives vapour of anomalous density, other constituents in the place of its normal constituents. For example, acetic acid affords a remarkable instance of anomalous vapour density. Representing it according to Gerhardt, by the formula—

$$\left.\begin{matrix}C_4H_3O_2\\H\end{matrix}\right\} O_2,$$

substitution may be effected in the molecule either of the hydrogen in the acetyl, or of the hydrogen or oxygen combined with the radical. M. Cahours' experiments* have led him to the conclusion that when the hydrogen combined with the radical, is replaced by substances capable of giving rise to volatile derivatives, there are no longer such anomalies in the vapour densities as exist in the vapour density of acetic acid. Thus methyl, ethyl, and amyl acetates, and even acetic anhydride, which differs from the normal acid only in containing a second equivalent of acetyl, in place of the hydrogen united with the radical of the normal acid, all give vapours which conform to the law, and present the characters of permanent gases at temperatures very near their respective boiling points.

This is shown clearly by the following table:—

	Boiling point. Degrees.	Temperature of estimation. Degrees.	Density.	
			Observed.	Calculated.
Methyl acetate .	58	77	2·595	2·585
Ethyl . . .	74	98	3·087	3·079
Amyl . . .	125	148	4·602	4·558
Acetic anhydride	137—138	152	3·673	3·562
		172	3·580	—
		185	3·563	—
		228	3·534	—
		242	3·487	—
		255	3·489	—

On the contrary, when hydrogen is replaced in the radical, the anomalous vapour density reappears, and in a more marked degree, as shown below:—

	Boiling point. Degrees.	Temperature of estimation. Degrees.	Density.	
			Observed	Calculated.
Monochloracetic acid.	188	203	3·810	3·278
		208	3·762	—
		222	3·559	—
		240	3·445	—
		261	3.366	—
		270	3·283	—

When the oxygen combined with the radical is replaced by an analogous substance, such as sulphur, the anomalous vapour density is much less decided, as in the case of thiacetic acid, though the alteration of this substance at a temperature a little above its boiling point did not admit of the change in density being traced to a constant point:—

	Boiling point. Degrees.		Temperature of estimation. Degrees.	Density.	
				Observed.	Calculated.
Thiacetic acid.	93—95	colourless	110	2·936	2·634
		”	115	2·889	—
		reddish yellow	131	2·778	—
		liquid reddish	138	2·734	—
		reddish brown	151	2·864	—

These data demonstrate very clearly the fact that when the hydrogen existing in the condition of a metal in acetic acid, is replaced by another substance having analogous functions, the anomaly in the vapour density disappears, while it remains when the hydrogen of the radical is replaced. But in the instances given the substituted substance is of an opposite chemical nature to that which it replaces; it is therefore desirable to ascertain whether the same result would be produced when an equivalent of hydrogen in the radical is replaced by a

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* *Register of Facts Relating to Literature, Science, and the Arts*, June, 1863, p. 458.

* *Comptes-Rendus*, lvi., 900.

substance having analogous chemical functions, such as methyl, ethyl, &c.

Gay-Lussac's researches on gaseous combination have shown that when two gases combine in equal volumes, the compound gives a gas equal in volume to the joint volume of the constituent gases, while there is always a contraction, greater or less, when the gases combine in unequal volumes. There are but two exceptions to this rule,—chloro-carbonic and chloro-sulphuric acids.

Equal volumes of hydrochloric acid gas and of ammonia combine, and produce a neutral substance whose vapour volume is, according to Mitscherlich and Deville, exactly equal to that of the sum of the volumes of its constituents, that is to say, eight volumes.

The opinion held by Cannizzaro, Kopp, Pebal, Wanklyn, and Robinson, that this difference is due to the disassociation of the constituents of the salt, does not appear to be maintainable since the results obtained by M. Deville, showing that sal ammoniac has more stability than one of its constituents. This is not an isolated fact; the molecule of phosphorus perchloride also corresponds with eight volumes of vapour, and hence M. Cahours has regarded it as consisting of equal volumes of protochloride and of chlorine.

Hydrochloric acid also combines with oil of turpentine, amylene, caproylene, or caprylene in equal volumes producing neutral substances, and it might have been expected that they would correspond with eight volumes of vapour. But this is not the case, and experience has shown that, like most volatile organic substances, their molecules correspond with four volumes of vapour, as will be seen by the following table:—

	Boiling point. Degrees.	Temp. of observation. Degrees.	Density.	
			Observed.	Calculated.
Terebene hydrochlorate .	—	237 244	6·121 6·045 } 6·083	6·050
Amylene ,, .	85—90	141	3·750	3·721
Caproylene ,, .	108—112	162	4·256	4·214
Caprylene ,, .	155—160	196	5·311	5·200

M. Cahours considers that this difference arises from the circumstance that in the combination of ammonia and hydrochloric acid both maintain their normal volumes, the saturation of their constituents being complete, while, on the contrary, the carbon in the hydrocarbons is not saturated fully, and hence there is a tendency to the production of substances represented by the formula

$$C_{2m}H_{2m}X_2,$$

X_2 representing an elementary substance such as $H_2Cl_2Br_2Cy_2$, or their equivalents HCl, HBr.

In these compounds the chlorine or bromine no longer exists in the state of hydrochloric or hydrobromic acids combined with a substance which neutralises them, nor in the state of chlorine or bromine combined with a radical analogous to metals; but they exist, in some sort, in a latent condition, as is shown by the inactivity of these substances in regard to an alcoholic solution of silver nitrate as compared with the reaction between sal ammoniac and that salt.

The terms hydrochlorate, &c., applied to those compounds appear therefore to be improper, and it would be more consistent to regard them as isomeric with chlorhydramylic ether, &c., and differing from these substances only in a greater tendency to split into the hydracids and hydrocarbons from which they were produced.

Acetic acid combines with ammonia in equal volumes, producing a neutral substance, but M. Cahours has not obtained results indicating its vapour density to correspond with eight volumes, as in the case of sal ammoniac. This is not in consequence of the decomposition of the salt into binacetate, for by heating acetate of ammonia to 200° in a flask, the vapours given off were sometimes alkaline and sometimes acid, but the crystalline residue in the flask was neither acid nor alkaline. It disengaged ammonia abundantly when heated with potash, boiled regularly without alteration at 218° to 220°, and presented all the characters of acetamide. The vapour density was 2·10; calculated, 2·06.

Ammoniacal salts of oxyacids give the same results, so that there is no possibility of ascertaining the density of their vapours experimentally, or if their molecules are similar to those of the hydracid compounds of ammonia.

New Methods for Testing the Purity of Alcohols and Ethers, by M. Berthelot.

Though alcohols and ethers have been carefully purified by distillation and desiccation, there has hitherto been in most cases no means of controlling their purity. The following are the results of my researches:—

1. I take as a starting point the fact that a compound ether, if pure, is decomposable by an alkali, by saturating an equivalent weight of this alkali. By this means, as I showed about ten years ago, the analysis of ethers and analogous compounds, is founded on an alcalimetric test, based on the use of a standard solution of baryta.

2. By means of the same liquid the smallest quantities of compound ethers may be recognised and estimated in alcohol or in simple ethers, provided these bodies are not alterable by alkalies. 10 cubic centimetres of a standard solution of baryta, and a known weight of the body to be tested, are inclosed in a flask. It is then heated for about an hundred hours at 100°; if the alcohol is pure, as is oftenest the case with ordinary alcohol, the standard of the baryta does not change. Amylic alcohol, on the contrary, almost always contains a small quantity of compound ethers, as also does ordinary ether, even after digestion on milk of lime.

Glycol prepared by the ordinary methods, and rectified to a certain point, is particularly impure. I have found in it as much as 22 per cent. of combined acetic acid, corresponding to 40 per cent. of monoacetic glycol. This fact may occasion more than one error, and the knowledge of it will be useful to chemists occupied with this curious substance.

To recognise the presence, without estimating, of a neutral ether in an alcohol, I heat the alcohol with twice its volume of water, for twenty hours at 150°. Most of the neutral ether changes into acid.

3. The presence of a free acid in an alcohol or an ether is so easily recognised that I need not stop to describe the process. Formic ethers, for instance, are always acid; but they decompose so promptly as to prevent the exact estimation of the free acid. The free acid of other ethers, on the contrary, can be precisely estimated.

4. The presence of a small quantity of water in a neutral ether may be detected by heating this ether to 150° during twenty or thirty hours; the water decomposes an almost equivalent quantity of ether into acid and alcohol. The acid is then estimated by a standard solution of baryta. On submitting acetic ether, carefully purified by the ordinary methods to this test, it will obstinately retain a centième of water, which is with great difficulty eliminated.

5. The presence of a small quantity of water in alcohol may also be detected by mixing the alcohol with

a perfectly anhydrous compound ether, tested as above. It is then heated for twenty or thirty hours at about 150°. If the alcohol is anhydrous the mixture should not become acid.

6. The presence of a small quantity of alcohol in a neutral and anhydrous ether, acetic ether for instance, may be detected by heating the ether with a known weight of quite pure acetic acid. The standard of the acid will diminish according to the amount of alcohol.—*Comptes-Rendus*, lvi., No. 18.

TECHNICAL CHEMISTRY.

Assimilation and Elimination of Nitrogen by Plants and Animals.

MM. Dumas and Boussingault have represented the general features of the balance existing between the phenomena of animal and vegetal life, but many questions of detail still remain to be determined, and one of the most important is that relating to the restoration of nitrogen to the atmosphere by animals, its passage from the atmosphere to plants, and thence again to animals.

The experiments of MM. Edwards, Dulong, Despretz, Regnault, and Reiset have shown that animals eliminate nitrogen. The amount of nitrogen thus liberated is not more than about 1 per cent. of the carbonic acid expired, and it varies according to the feeding and condition of the animals.

In agriculture it has been considered that all the nitrogen of the food of animals, over and above that assimilated by them, is restored to the land in the form of manure. But this is not the case.

When the study of this subject was undertaken, in 1847, by M. Barral, only two experiments had been made with regard to it by M. Boussingault. He found that during twenty-four hours a horse eliminated twenty-four grammes of nitrogen, and a milch cow twenty-seven grammes; in the one case 17 per cent., and in the other 13 per cent. of the nitrogen in the food.

M. Barral* has since found that, in the twenty-four hours, an adult man eliminates from nine to fourteen grammes of nitrogen, an adult woman about twelve grammes, and a child five years old three grammes, corresponding to more than a third of the nitrogen in the food. A sheep in the same time eliminates about six grammes, one-third or one-fourth the nitrogen in the food. Very nearly the same results have recently been obtained by M. Reiset with fattening cattle.

The general result of the observations was, that in the twenty-four hours, forty-eight grammes of nitrogen were required in the food for each 100 kilogrammes of live weight, and that one-fourth of this was eliminated and returned to the atmosphere as nitrogen. In a year, the same live weight would eliminate 4380 grammes of nitrogen. In other words, a consumption of food equivalent to 6000 kilogrammes of hay involves a loss of 1500 kilogr.

This result confirms M. Boussingault's opinion that farm animals are not, as is supposed, producers of manure, but rather consumers of manure; they convert fodder into materials rapidly assimilable by plants, only at the cost of a considerable loss. It appears from this that the practice of green manuring is advantageous when it is not necessary to obtain manures more rapidly assimilable, or when the feeding of cattle is not remunerative. It also follows that the fertility of a farm supporting cattle, and not exporting any other provisions, cannot be maintained in its normal condition without an importation of manures from extraneous sources, unless indeed the culture of forage crops abstracted from the atmosphere the nitrogen that the cattle continually transferred to it.

With the known constant composition of the atmosphere, it appears, therefore, that there must be some mode in which the large quantity of nitrogen eliminated by animals is compensated, and which has the effect of abstracting from the atmosphere four or five kilogr. per hectare annually, taking the live weight of animals fed by a hectare at 100 kilogr.

There appears to be no evidence of the direct assimilation of nitrogen by plants. All the experiments that have been made on this subject have given negative results. Still, M. Boussingault's observations show that plants obtain a part of their nitrogen from some source other than the soil or the manure supplied to them. They obtain that nitrogen indirectly from the atmosphere. With the view of elucidating this subject, M. Barral, in 1851, commenced observations of the rain-water falling in the neighbourhood of Paris. He found nitrate of ammonia to be constant in the atmosphere, and that rain-water always contains sensible quantities of nitric acid and ammonia. But the quantity was insufficient to account altogether for the elimination of nitrogen by animals, though it was sufficient partly to account for the production of crops from unmanured land under the bare fallow system.

M. Barral considers that the oxidation of nitrogen in the soil may be the source whence plants derive great part of their supplementary nitrogen, and of withdrawing from the atmosphere the nitrogen supplied to it by animals. He has found that water discharged from drains contains nitrates in amount proportionate to the fertility of the land it comes from.

He does not agree with the opinion that nitrification is determined in soils by vegetation itself. By growing plants, in confined atmospheres of oxygen and carbonic acid, without any nitrogen, he always found some nitrogen eliminated, but that might originate from the decomposition of organic substances.

The Function of Atmospheric Oxygen in the Destruction of Animal and Vegetal Substances after Death, by M. Pasteur.*

The most ordinary observation has at all times demonstrated that animal and vegetal substances, exposed after death to contact with atmospheric air, or buried in the earth, disappear, in consequence of various transformations.

Fermentation, putrefaction, and slow combustion, are the three phenomena which concur in the accomplishment of this great fact of the destruction of organic substances—a condition necessary for the maintenance of life on the earth.

Dead substances that ferment or putrefy do not yield solely to forces of a purely physical or chemical nature. It will be necessary to banish from science the whole of that collection of preconceived opinions which consist in assuming that a certain class of organic substances—the nitrogenous plastic substances—may acquire, by the hypothetical influence of direct oxidation, an occult power, characterised by an internal agitation, communi-

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* *Comptes-Rendus*, lvi., 765.

* *Comptes-Rendus*, lvi., 734.

cable to organic substances supposed to have little stability.

In M. Pasteur's memoirs, and more especially in a recent communication, he has pointed out precisely what are, in his opinion, the true causes of fermentation, and has stated the principal result of his researches on putrefaction, properly so-called.

In every case, life, manifesting itself in the lowest forms of organization, appears to be one of the essential conditions of these phenomena, but life of a nature unknown hitherto; that is to say, without consumption of air or of free oxygen.

He now endeavours to demonstrate experimentally that the slow combustion which takes place in dead organic substances, when they are exposed to the air has, in most cases, an equally intimate connection with the presence of the lowest forms of life. This leads to the general conclusion that life takes part in the work of death in all its phases, and that the three terms of that perpetual return to the atmosphere, and to the mineral kingdom, of the elements which vegetals and animals have abstracted from them, are correlative acts of development and of the multiplication of organised beings.

In May, 1860, an exhausted flask, of 250 cubic centimetres capacity, containing 80 cubic centimetres of sugar solution with yeast, which had been heated to ebullition, was filled with air. Immediately after admitting the air, the point of the flask was again sealed by the blowpipe.

Under these circumstances, it often happens that the liquid in the flask does not give rise to the production of infusoria or micoderms, and that it remains limpid after the admission of air to the flask. This was the case in the above-mentioned experiment. The liquid was still unaltered in appearance on February 5, 1863, and at that time the air in the flask was found to consist of—

Oxygen	18·1
Carbonic acid	1·4
Nitrogen by difference	80·5
	100·0

This shows that, during the lapse of three years, the albuminous substances of the yeast water, associated with sugar, and exposed to ordinary atmospheric air under conditions in which animalcules or micoderms were not developed, had absorbed 2·7 per cent. of oxygen, which they had partially converted into carbonic acid. This direct oxidation or slow combustion was very slight, although the flask had been kept for eighteen months at a temperature of 25° to 30° C.

On March 22, 1860, a flask of 250 cubic centimetres capacity, and containing 60 to 80 cubic centimetres of boiled urine, was filled with air deprived of organised germs by heating it. The liquid was still quite limpid in January, 1863. Its colour had become slightly darker. A crystalline sandy deposit of uric acid had separated in small quantity on the sides of the flask. There were also a few bunches of acicular crystals of lime phosphate. The urine was still acid, but not quite so much so as at first. It smelt exactly the same as fresh boiled urine. The air in the flask consisted of—

Oxygen	11·4
Carbonic acid	11·5
Nitrogen by difference	77·1
	100·0

All the oxygen that had been absorbed had been converted into carbonic acid.

On June 17, 1860, a flask of 250 cubic centimetres capacity, containing 60 cubic centimetres of milk, boiled two or three minutes, was filled with air that had been exposed to a red heat. On February 8, 1863, the milk was almost neutral to test paper, with a tendency to alkalinity. It had the ordinary taste of milk, with a slight flavour of suet. By standing, the fatty substance separated in clots, and by shaking the appearance of fresh milk was again presented; it was not at all curdled. The air in the flask consisted of—

Oxygen	3·1
Carbonic acid	2·8
Nitrogen by difference	94·1
	100·0

The fatty substance of the milk had absorbed a large proportion of the oxygen, as in De Saussure's experiments with oil. But still, after three years, there remained some free oxygen, although the direct oxidation of fatty substances is considered to take place very readily.

By repeating these experiments under the same conditions, but under the influence of the development of organised germs of the lowest forms of animal and vegetal life, all the oxygen of the air in the flasks was absorbed within the space of a few days, with simultaneous disengagement of carbonic acid in varying proportions.

On February 26 last, a flask containing 10 grammes of oak sawdust, and some water that had been boiled with it, was filled with air that had been exposed to a red heat. A month afterwards the air in the flask consisted of—

Oxygen	16·2
Carbonic acid	2·3
Nitrogen by difference	81·5
	100·0

In a similar experiment, made without any precaution for excluding organised germs, the air of a flask containing four litres was found, after fourteen days, to contain 7·2 per cent. of carbonic acid, and 300 cubic centimetres of oxygen had been consumed, while in the former experiment only a few cubic centimetres were consumed. This ready combustion of sawdust when exposed to atmospheric air was long since observed by De Saussure in his researches on the formation of soils.

Whence arises this great difference between the results of the two experiments? At first sight there appears to be no clue to it. But if the surface of the sawdust is examined by the microscope, it is found, in the case where no precaution was taken to exclude organised germs, to be covered with a scarcely perceptible film of sporules and mycelium of various micoderms.

In examining the slow combustion of dead organic substances under the influence of atmospheric oxygen alone, it will be found that it varies in degree and in mode of action, according to the nature of the substances, almost as the oxidation of metals differs.

But the important fact is, that though this slow combustion takes place independently, it is scarcely sensible in air that is deprived of the germs of inferior organisms. This oxidation is disproportionately greater and more rapid when organic substances are in contact with mycoderms, &c. These minute beings are agents of combustion, whose energy, differing according to their specific nature, is sometimes extraordinary, as, for instance, in the oxidation of alcohol, acetic acid, sugar.

The constituents of living organisms might be regarded as being comparatively indestructible, if it were not for these minute beings, which are apparently without any use. Life would become impossible without them, for the restoration of that which has ceased to live, to the atmosphere and to the mineral kingdom, would be suddenly suspended.

The above experiments by themselves might be open to a serious objection. The organic substances were not only dead, but were always heated beforehand to the temperature of boiling water, and it cannot be doubted that they undergo change at that temperature. It was necessary therefore to examine the slow combustion of organic substances that have not been so heated.

M. Pasteur has succeeded in exposing to air, free from organised germs, fresh liquids that are in a high degree susceptible of putrefaction, such as blood and urine. Some flasks containing pure air and blood taken from a dog were deposited with the Académie last March, and after having been exposed, since then, to a temperature of 30° C., it had not undergone any kind of putrefactive change, and its smell was quite fresh.

By analysis of the air in flasks containing blood, and kept for a month or six weeks at a temperature of 30°, there is but a slight absorption of oxygen recognisable, amounting to 2 or 3 per cent.

Urine enclosed in flasks in the same manner, remained perfectly fresh. After forty days the air in the flask consisted of—

Oxygen	19·2
Carbonic acid	0·8
Nitrogen	80·0
	100·0

The conclusion to which M. Pasteur has been led by these observations, is therefore the same as in the case of organic substances that have been heated, and they serve to complete the evidence he has, during the last few years, brought forward to show that the doctrines of spontaneous generation and the modern theory of fermentation are no longer tenable.

On Destructive Distillation, considered in Reference to Modern Industrial Arts,* *by* B. H. Paul, *Ph.D.*

The effects produced by the application of heat to various substances must have been among the earliest observed chemical phenomena. The differences existing between the effects produced by heat upon different substances were recognised at a very remote period in the history of chemistry, and among them the phenomena of distillation received especial attention. In some cases the application of heat to a substance has the effect of dissipating it entirely; such substances, of which water is a familiar example, are said to be volatile, and if substances of this kind are heated in closed vessels of suitable construction they may be recovered again, in their original condition, by the condensation of their vapour. This, in the strictest sense of the term, constitutes distillation. The volatile substance, absorbing the heat applied to it, becomes converted into vapour;—by abstracting from that vapour the heat which has been absorbed, it is converted into the original substance. In this way distillation is employed as a means of separating volatile substances from others which are not volatile, and which are, in contradistinction, termed fixed substances. This distinction between fixed and volatile substances is, however, in most cases merely relative, and it applies only to such a range of temperature as is commonly attainable. There are good reasons for the opinion that the substances commonly regarded as fixed, might be converted into vapour if their temperature could be increased to a sufficient degree. But among the substances which, in this limited sense, are termed fixed, there are some which certainly cannot be converted into vapour, in any case, without entirely losing their identity; without, in other words, being converted into totally different substances. Thus, for instance, wood is not a volatile substance, and at the same time it is not a fixed substance, except within a certain limited range of temperature. When heated much above the boiling point of water, wood is partially converted into vapour, to an extent proportionate to the temperature employed, but the vapour so produced cannot be reconverted into wood by cooling it, as the vapour of water can be reconverted into water. The change produced by the heating is a true chemical change. Most substances analogous to wood undergo a change of this nature when heated in close vessels; they are, in chemical language, decomposed, and the substances into which they are converted are called the products of the decomposition. These products are partly volatile. It is only in this way that substances which are not in themselves volatile can be said to distil, and it is this conversion of substances, by the application of heat, into new substances, that constitutes what is termed destructive distillation.

The products of this alteration present, in all cases, a general similarity. There is, in the first place, the carbonaceous residue, which cannot be volatilised—the "coal," as it was formerly called. Amongst the volatile products, water and oil are conspicuous; there are generally some substances dissolved in the water, communicating to it peculiar characters, according to the nature of the material distilled, and in all instances some gas is produced.

In the earlier days of chemistry the destructive distillation of organic substances was considered to effect a separation of their component parts; it was looked upon as a means of analysing both vegetable and animal substances. But it was found that the products of the destructive distillation of a substance varied in amount according to the heat applied to it, and, consequently, when quantitative relations became an important consideration in chemistry, this opinion was abandoned, and it has long since been generally admitted that the alteration such substances undergo in destructive distillation is greater than a mere separation of pre-existing components,—that it consists in an entire destruction of the original substance, with simultaneous production of new substances.

This decomposition of an organic substance by heat consists in a disturbance of the chemical equilibrium upon which its existence depends; the products to which it gives rise are substances capable of existing at the higher temperature. All organic substances are characterised by their liability to decomposition by heat, but they differ among each other very much in their capability of supporting heat, or, in other words, in their liability to decomposition under its influence. For every organic substance there is a particular range of temperature within which its existence is possible, and beyond the higher limit of which it undergoes decomposition. Hence there is an intimate and essential connexion between the nature of the products and the temperature of the decomposition, and it follows that the special nature of the products obtainable in destructive distillation differs, according to the temperature at which it is

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* Read before the Society of Arts, May 27, 1863.

conducted, no less than according to the material from which they are obtained. These features of the decomposition of organic substances by heat were very clearly recognised by Lavoisier; they received at his hands considerable attention, and though they did not occupy a prominent place in the chemical phenomena that, in his time, were the object of general interest, his works contain important discussions as to the causes to which they were referable.

Prior to the time when Lavoisier wrote on this subject, the product of destructive distillation to which—with some few exceptions which I shall afterwards notice—most attention was directed, was the oily product. The characters of the oil obtained by this means from different substances are often described in old chemical works. Sometimes it was called tar, that term being applied to those kinds of pyro-oils which were resinous and dried up by exposure to air, as in the case of that obtained from pine-wood, and which at the present time is still commonly known as tar. Some of these pyro-oils figure as medicinal agents in the pharmacopœia of 1678, and amongst others the oil of coal—which is described as a fossil bitumen, bearing the names of carbo petræ, lithanthrax, sea-coal, or Newcastle coal—and the direction given is that "you may distil it as amber, so shall you have a spirit and oil." But this oil of coals soon became a matter of more extended observation, in consequence of the attempts made to use pit coal as fuel in smelting. For a long time these attempts were unsuccessful. At length, however, a method was found of removing the disadvantages of coal for smelting purposes. That method, as every one knows, was coking. The discovery of this method has been ascribed to Becher, who was in England about the year 1665, but he says himself that it was a German, of the name of Blavesten, who first suggested the idea of employing what he called "stone charcoal" for smelting iron. In any case the oily product obtained from the coal, by heating it in close vessels, attracted the attention of Becher, and he put forward a project for making tar from coal, apparently in conjunction with the production of coke, which is very often referred to in old works, but always in very vague terms, and nothing much seems to have come of it.

The German chemist Neumann examined the oily products of the distillation of coal, and described them in his works as consisting of a "thin fluid oil" and another "thick pitchy oil." He obtained these by distilling the coal of Halle "with a fire gradually increased," and he states that "the coal, during the distillation, looked like melted pitch." Still these products were not turned to any useful purpose.

However, the coking of coal, or the desulphurising, as it was sometimes called, became an important operation, and great interest was excited by it on the Continent. In 1765, the French Government thought it desirable to send a commission to this country, for the purpose of learning the art of coking. An account of their observations is given by M. Jars, the brother of one of the commissioners. He says:—"The English were the first to attempt rendering coal available for smelting purposes; the first trials are of a very remote date. And, among others, Swedenborg speaks of it as an art which in his time was not fully developed. But the industry of the English overcame all difficulties, and they succeeded, by means of very simple operations, in attaining the desired end, that is to say, in depriving pit-coal of the defects which render it unfit for smelting." The attempt to turn to account the volatile oily products obtained in coking coal was still continued, both in this country and on the Continent. At Liege, for instance, coal was distilled for oil, and similar attempts were made likewise in various parts of England, by the Marquis of Rockingham, near Sheffield, by a Dutchman named Van Haak, at Coalbrookdale and Newcastle, and by others.

One of the best known instances of the application of these volatile properties of coal, was one carried out in Nassau, shortly before the year 1768, at some iron works belonging to the Prince of Nassau-Saarbruck, at Sultzbach. This plan was described by M. Genssane to the French Académie des Sciences, and reported upon by Macquer. He says—"The whole art of the preparation of pit-coal, so as to render it fit for smelting, consists in depriving it of the bituminous and sulphury substances which render it too fat and energetic when it is used in its natural state. . . . This principle once established, it is easy to conceive that it is only by distillation and evaporation that these two substances can be separated from the coal."

The distillation of coal at these works was conducted in a kind of close oven, or muffle, heated externally by furnaces. "The fire was got up gradually, until the oven became slightly red-hot, and it was then kept at that degree. . . . The heat being gradually communicated to the coal within the oven, first of all expelled its bituminous portion, which distilled off through a pipe, and fell into a receiver; when the coal had given off its bitumen it commenced to become slightly red-hot.

"The oil and bitumen obtained in this operation almost paid the cost of it. . . The pure bitumen was very thick and greasy, and equal to the best carriage grease. . . The oil did not differ from that obtained by distilling petroleum, except in being much less readily inflammable than the latter, and it could be advantageously employed in lamps by the country people. Nothing else was used for burning in the mines at Sultzbach."

MM. Macquer and Montigny, in reporting to the Académie on this manufacture, speak highly of its utility, and when we consider the extent to which the manufacture of which this was the first germ, has now grown, it appears that their opinion was well founded.

The next person who made a step in this branch of manufacture was Lord Dundonald. The preparation of coke appears to have been still the predominating idea, but it was also thought that the volatile substances given off in this operation might be turned to account, as well as the coke. All the previous methods of obtaining these products consisted in distilling coal in close vessels heated externally, but Lord Dundonald's method consisted in partially burning the coal in a large chamber capable of being entirely closed, and admitting a regulated supply of air, just sufficient for maintaining the combustion of coal at the desired degree. The volatile products from the coal passed away through a pipe to a condenser, where they were collected. An account of the works erected on this plan at upper Cranston, is given in Sir John Sinclair's "Statistical Account of Scotland." The product obtained, besides coke, was a mixture of tar and water. This first product was submitted to distillation, yielding an oil lighter than water, and a solution of ammonia. This tar was sold for greasing cart wheels, at the rate of sixpence per Scotch pint. When the distillation was continued for four and a-half days, the residue, remaining in the still, was the tar suitable for coating ships, which was regarded as one of the most important of the products. When the distillation was continued for five and a-half days, the residue in the still was more pitchy; and after six and a-half days it was quite brittle.

(To be continued.)

Holmes's Magneto-Electric Light.

[SECOND NOTICE.]

On a former occasion* we traced the history of the mode of illumination which may almost be said to have been perfected, for lighthouse use, in the magneto-electric apparatus of Professor Holmes; it remains now to notice shortly the construction of the lamp and machinery employed for generating the electric current. The conditions under which the spark may be produced by the action of permanent magnets upon coils and soft iron armatures kept in rotation were originally demonstrated by Professor Faraday, and the same principle had received an economical application on the part of Messrs. Elkington and Company, who employed this source of power in the production of electric currents suitable for their electro-plating processes, prior to the adaptation to lighthouse purposes which was suggested and carried out by Mr. Holmes. In the improved construction of the magneto-electric machine here employed, the magnets are of the horse-shoe form, and are disposed in three concentric circles upon substantial mountings of brass. Between the poles of those forming the first and second rings, and also between the poles of the magnets forming the second and third rings, the rims of two large brass wheels revolve. The rims of these wheels are hollow, and contain the bobbing, or helices; but the soft iron cores around which the wire of the bobbing is wound are screwed into the sides of the hollow rims, and are turned off flush on the outside. The wires of all the helices are connected together, in series, in the rim of each wheel, and the terminal wires carried down to two sets of binding screws in connection with the hollow axle, and likewise with the metal of the bearings, through the medium of which they are joined to their respective commutators. The commutators are cylindrical in form, and it is by their agency that the alternating currents of electricity induced in the helices are directed; so that not only does the entire current pass from the machine in one direction, but the *dead points* in the currents are counteracted, and the current, instead of being intermittent, is constant.

The machine is put in motion by a direct-acting steam-engine, and by means of a crank fixed on the end of the axle opposite to that upon which the commutators are mounted. The manner in which the electricity is generated and evolved may be described thus:—At every revolution of the wheels the entire series of cores and helices on each wheel passes between the poles of its two rings of magnets, and consequently at every revolution about 85 lbs. of soft iron are magnetised N—S, and again changed to S—N, forty-four times; and, as the machine makes 110 revolutions per minute, these changes in the magnetic state of the soft iron cores, or armatures, take place 4840 times in a minute. In order to obtain the maximum of power from the machine, two points had to be specially attended to in its construction: first, that the magnets should retain their force for a lengthened period without sensible diminution; and secondly, that during their revolution the ends of the cores should pass as closely as possible, without actually touching, the poles of the magnet. By neglect of this latter precaution, a large machine, in which wood was employed in the mounting of the magnets, met with total destruction whilst under trial at Brussels, in consequence of the humidity of the atmosphere disarranging the adjustments; this casualty is now rendered impossible, by the use of a framework of brass, and of strong set-screws for fixing the magnets in position. The speed of the engine was not allowed by the Trinity House authorities to exceed 100 revolutions per minute, and other conditions were imposed, particularly with reference to the machine being driven by a direct-acting steam-engine without the intervention of a strap or band. It was necessary also to moderate the intensity of the electric current, so that the "shock" should be trifling, and not such as to affect the health of the attendants, who might occasionally, when on duty, place themselves in circuit. In ordinary cases the machine, with its boiler and steam-engine, would be placed in a building adjacent to the lighthouse, the wires being led thence to the lantern. At South Foreland, where two lighthouses on the mainland are situate within a thousand yards' distance, one machine-room, built in an intermediate position, and as nearly as possible half-way between the two, will suffice. It is always advisable to keep the engine away from the lighthouse itself, as a guarantee against the condensation of steam upon the glass reflectors and window panes.

With respect to the lamp and optical portion of the apparatus, it will be sufficient to describe the arrangements for a fixed light at Dungeness. Two regulators or lamps are employed for each lens, and these lamps are arranged on two travelling platforms in such a manner that a mere touch is sufficient to withdraw one and to slide another into its place. The manner of working in the lantern is this:—At starting, all the lamps are charged with carbons, two lamps being in their respective lenses, the others standing on their corresponding platforms behind, ready to replace those now in focus. At sunset the machine is started, and the attendant has merely to draw two bolts in either of the lamps in focus, and the light is instantly showing with its full intensity. This lamp does not require any attention for about three hours and a-half, if the carbons are well paired for hardness; but if these differ much, then the luminous point will gradually rise or fall above or below the focal plane, and will require occasional adjustment. After a few months' practice, ordinary light-keepers are able to pick out pairs of carbons that will retain their position in the focal plane during the whole time of their consumption. At about three hours and a-half from the time of lighting, the carbons will require changing. This is effected without extinguishing the light, as follows:—The attendant pushes in the two bolts which he withdrew at lighting, which stops the action of that lamp, but does not immediately extinguish it; he then passes to the lamp in focus at the other lens. On drawing the bolts of this lamp, the current is diverted, and this lamp now lighted. The first is now withdrawn, and the lamp standing on the platform behind it is pushed forward in its place; and when the used lamp is cool, the attendant mounts in it a fresh pair of carbons, and places it ready to be used again in its turn. Thus, while the speed of the machine is maintained and the lamps changed as directed, the light may be continuously exhibited any number of hours or days.

The internal construction of the lamp itself is not very different from those already known and used in this country. The carbons are mounted upon sliding brackets, the interval between them being regulated and maintained by the action of an electro-magnet. There are three or four screws or studs provided at the back of the lamp for altering the adjustments, and starting or stopping its action. The communication with the main wires is effected by means of a pair of metallic rails upon which the instrument rests when in position. The magneto-electric apparatus is equally applicable to revolving

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* *Vide* Chemical News, vol. vii., p. 247.

lights with only such modification in the lenses as would be necessitated by the old Argand system in present use.

The steam boiler provides a means of giving an effective fog signal by whistles, trumpets, or otherwise by sound. The electric lamp may be employed even in daylight for the purpose of communicating Greenwich time to vessels, and with coloured glasses for all kinds of signalling. The whole of the machinery above described is assumed to be practically incapable of becoming disarranged, no visible cause of getting out of repair being allowed to exist. In confirmation of this statement, it is only necessary to refer to the experimental results alluded to by Lord Lovaine in the House of Commons on Friday, the 5th instant, when this subject was for the third time brought forward for discussion, and to the reports to the Trinity House, from which it appears that during a trial of nine months at Dungeness the magneto-electric light had only failed once for two minutes, at another time for thirty seconds, and on a few other occasions for even shorter intervals; whilst under the existing system it is necessary to extinguish the oil lamps every night, at least once, for a quarter of an hour, to allow of their being trimmed.

Magnesium.

MM. Deville and Caron* have recently studied the characters of this metal, and have found the results obtained by M. Bussy and Herr Bunsen to be strictly correct. They give the following method for preparing this metal:—

Magnesium chloride is the substance they employ for the purpose. By reaction with sodium at a red heat it yields magnesium.

The magnesium chloride was obtained by evaporating to dryness a solution of the salt, mixed with ammonium chloride, heating the residue till it melts, and pouring out the melted salt. The sal-ammoniac prevents the decomposition of the magnesium salt by the water of crystallisation. The dry residue should be transferred to a red-hot crucible, so as to complete the operation quickly.

Magnesium chloride is described as being volatile, like zinc chloride, and capable of being distilled from porcelain vessels in an atmosphere of hydrogen. It is then obtained as a colourless butyraceous mass, which solidifies with lamellar crystalline structure.

There is great difficulty in separating the last portions of ammonium chloride from the magnesium salt, and to this circumstance is attributed the frequent presence of nitrogen in magnesium.

The flux employed was calcium fluoride. Pure crystals of this salt were powdered, moistened with concentrated sulphuric acid, and after some days washed with water. By this means the silica and phosphoric acid are removed.

To obtain magnesium, a mixture of

	Parts by weight.
Magnesium chloride . . .	600
Calcium fluoride	480
Sodium	230

is introduced into an earthen crucible heated to redness, and the cover is kept down by a weight while the reaction takes place. When the action has ceased, there remains a clear liquid without any perceptible metal, this being disseminated as microscopic globules through the liquid. After stirring the melted mass, the crucible is taken from the fire, and after being left to cool for a few minutes, small successive portions of dry powdered calcium fluoride are added, so as to hasten the cooling, stirring the mass meanwhile with an iron rod till it becomes pasty. By proper management, almost all the magnesium may be gathered together in a mass upon the rod before the denser saline mass becomes solid. If the metal should take fire meanwhile, the combustion may be stopped by adding a little calcium fluoride.

The magnesium may be separated from the slag by a few blows of a hammer, and it should amount to three-fourths the quantity equivalent to the sodium used. But by melting the slag several times it is possible to obtain a larger proportion of magnesium.

Good results were also obtained, according to a method previously published,† by using a mixture of magnesium chloride with one-sixth its weight of sodium chloride, or of the mixture of sodium and potassium chlorides recommended by Wöhler, and one-sixth of calcium fluoride. This mixture is melted with sodium, as above described.

Ingots of magnesium may be cast by melting it with a mixture of magnesium and sodium chlorides with calcium fluoride. By gradually increasing the proportion of the latter salt in the melted slag it is rendered less fusible than magnesium, and the metal may be poured off at the moment the slag solidifies.

Or, the globules of magnesium, cleaned by means of nitric acid, may be melted with mixed alkaline chlorides, and at the moment of solidification the magnesium becomes denser than the slag, and, sinking to the bottom, unites to a button.

Thus obtained, magnesium contains carbon, silicium, and magnesium nitride. The metal is volatile at nearly the same temperature as zinc, and like it boils. MM. Deville and Caron have purified magnesium by distilling it in carbon vessels‡ with an atmosphere of hydrogen. When it is impure, it leaves a very light, black residue, and the distilled metal is covered with small, colourless, transparent, acicular needles, which are rapidly converted, by contact with air, into ammonia and magnesia.

The melting point of magnesium is nearly the same as the melting point of zinc. At a temperature slightly higher the metal takes fire, and presents the same phenomena as burning zinc. The density of magnesium is 1·75. In the crude state the metal is brittle, but when pure it is very ductile. It tarnishes in the air, but not more quickly than zinc, and the oxidation is never very considerable.

NOTICES OF PATENTS.

349. *Improvements in Refining Cast Iron, Wrought, and other Malleable Iron, and in the Cementation of Iron.* W. Clark, Chancery Lane, London. A communication. Dated February 10, 1862. (Not proceeded with.)

In order to obviate the injurious action of sulphur contained in the coal, coke, or other fuel employed in the process of refining iron, and in effecting its conversion into steel, the inventor mixes with the fuel a small proportion of lime or carbonate of lime (either in the form of chalk or limestone), which, at the high temperature of the furnaces, becomes converted into quicklime, and is supposed then to be very efficacious in the removal of sulphur from the iron, and in absorbing the sulphuretted products evolved during the combustion of the coal.

It is generally acknowledged that, unless a considerable

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* *Annales de Chimie et de Physique*, lxvii., 340.

† *Comptes-Rendus*, xliv., 394.

‡ *Annales de Chimie et de Physique*, (3) xlvi., 182.

amount of lime be employed (in which case the brick lining of the furnace would be attacked), the quality of the iron, as regards sulphur, remains practically unaffected.

384. *Preventing the Corrosion of Steam Boilers.* T. Davison, Belfast. Dated February 13, 1862.

For the purpose stated in the title the inventor employs lime, carbonate of potash, or carbonate of soda, which he dissolves and introduces into the water of the boiler. Any other salt may be used which will have the effect of neutralising the injurious constituents present in the water.

These chemical agents have long since been employed for preventing incrustation in boilers; they will act also in preserving the metal from corrosion.

447. *Protecting Iron Boilers, Tanks, and Vats from Wear arising from Galvanic Action.* G. T. Bousfield, Loughborough Park, Brixton, Surrey. A communication. Dated February 19, 1862.

With the object of preventing the galvanic action which is sometimes set up in iron boilers as a consequence of their tubes, gauges, and fittings being constructed of metals, such as brass, copper, or bronze, which are electro-negative to iron, it is proposed to insulate these parts by washers and packing of india-rubber, or other non-conducting substance. In mining districts, and under special circumstances, minute proportions of copper and other metallic salts in the water are instrumental in causing the corrosion of iron boilers; where this is the case, it is recommended to pass the water through an intermediate vessel containing scraps of waste zinc, which may reduce and precipitate the foreign metals from the water before it enters the boiler.

395. *Coking Coal.* W. G. Valentine, Oxford Street, London. Dated February 13, 1862.

The coal is heated in close retorts of a construction which permits of the evolved gases being employed as a partial source of heat, and thus aiding by their combustion the process of converting the coal into coke.

414. *Treating Fabrics or Articles Composed of Animal and Vegetable Substances for the Purpose of Separating One Class from Another.* R. Bell, Dublin. Dated February 15, 1862. (Not proceeded with.)

The process of separation here described is applicable to the treatment of mixed fabrics, the cotton of which is said to be dissolved, and the wool or silk left unaffected. The solvent prescribed is the crude chloride of manganese, left as a residual product in the retorts used in the manufacture of chlorine; this is dissolved in muriatic acid, and the mixed fabrics submitted to its action; they are then washed and treated with urine or salt water, and finally acted upon with dilute alkali, when the vegetable matters are washed away, and the animal substances retained for further use.

This action must be partly mechanical, for it is well known that ligneous tissue will resist the solvent power of many chemical agents, more completely and for a longer period than other products of the animal class.

Grants of Provisional Protection for Six Months.

1156. William Clark, Chancery Lane, London, "Improvements in coating wrought or other iron to protect it from corrosion or oxidation." A communication from George Washington Holley, Niagara, New York, U.S. Petitions recorded May 8, 1863.

1170. Richard Archibald Brooman, Fleet Street, London, "Improvements in the manufacture of lampblack." A communication from Johan Ephraim Lundgren, Stockholm, Sweden.

1174. James Burrell, Back Church Lane, Whitechapel, London, "Improvements in salinometers." Petitions recorded May 9, 1863.

1021. Philip Passavant, Bradford, Yorkshire, "Improvements in the manufacture of blue colouring matter, and also of violet colouring matter."—A communication from Friedrich Bayer, Barmen, Prussia.—Petition recorded April 24, 1863.

1173. Charles Hanson Greville Williams, Burnford Street, Glasgow, "Improvements in the manufacture of colouring matters."—Petitions recorded May 9, 1863.

1205. Carl Ludwig Kensner, East Greenwich, Kent, "Improvements in the manufacture of hydrate of barytes, and in the manufacture of sugar."

1209. Richard Archibald Brooman, Fleet Street, London, "Improvements in the extraction of hydro-carburets from minerals, in the distillation thereof, and in apparatus employed therein."—A communication from Felix Alexandre Testud de Beauregard, Paris.—Petitions recorded May 13, 1863.

1264. Paul Addington, Phipps Bridge, Merton, Surrey, "Improvements in the manufacture of varnishes."

1278. Edward Sonstadt, Loughborough, Leicestershire, "Improvements in the manufacture and purification of the metal magnesium."

CORRESPONDENCE.

The Universal Text-book of Photography.

To the Editor of the Chemical News.

Sir,—Will you allow me to correct an error in your obliging review of our "Text-book of Photography" in your last number. You have attributed the little book to my pen, whereas I am indebted to the labour of my assistant, Mr. G. D. Scarah, for collecting most of the information. Your insertion of this in your next will only be fair.

I am, &c. R. J. Fowler.

10, Briggate, Leeds, June 6.

On Alum in Bread and Flour.

To the Editor of the Chemical News.

Sir,—In No. 154 of the Chemical News, I furnished a paper on the extraction of alum from bread and flour. Since then I have had frequent opportunities of testing the value of that method, and have received similar favourable opinions from other gentlemen, who consider it superior to Kuhlman's,—the one ordinarily used,—inasmuch as the latter leads to inaccuracy, from a phosphate being estimated as pure alumina, which is not the case with the tannin process.

Quantitative experiments have, however, shown that sometimes, owing to imperfect washing of the pulpy tannate, a little chloride of potassium is apt to be retained in the incinerated residue; it would therefore be better in all cases to throw this on to a little filter, and, having washed out the soluble matter with water, then to dissolve off the pure alumina with a little dilute hydrochloric or nitric acid, collect it in a small tared Berlin crucible, and evaporate to absolute dryness over a sand-bath.

In this way I obtained 0·33 of a grain of alumina, the entire quantity, from three grains of alum incinerated with 150 grains of flour.

Some writers considering that in aluminised flour the alum exists in a free state, imagine it can be easily extracted by water; numerous experiments, however, have proved the fallacy of such a notion, the affinity of alum for organic matter being so strong that no sooner is it wetted than it sinks in to form so insoluble a compound that neither digestion of the flour in plain water, or water acidulated with nitro-muriatic acid appears to make any impression on it, the merest traces only of alumina being perceptible. Nothing short of incineration and subsequent treatment with an acid and tannin will enable us to extract the full quantity of pure alumina.

As a general rule, I find that the appearance of the residue after incineration forms a tolerably sure guide as to whether further treatment with chemicals be necessary or not.

A quarter or half-pound of pure bread or flour, on being incinerated for an hour in a plumbago crucible placed in the centre of a clear, well-packed fire in an ordinary grate till all signs of combustion cease, yields a charcoal, which is perfectly black and uniform in colour, exhibiting scarcely any whiteness, whereas that adulterated with alum readily shows itself by being coated with thick, white spongy patches, which break up and separate in flakes on agitation. So that, where the charcoal is uniformly black, the bread or flour has not had any mineral matter added. On the contrary, any whiteness would lead us to suspect alum or gypsum.—I am, &c. John Horsley.

On Bathvillite, Torbanite, &c.

To the Editor of the Chemical News.

Sir,—The criticism on Mr. Williams' paper "On Bathvillite," which I sent you on a former occasion (Chemical News, vii., p. 191), seems to have been generally considered as a matter of personal spite. Permit me to say that I have no occasion for any such feeling towards this chemist, and that any such supposition is a mistake. I sent you the communication believing everything in it truthful, except only the pleasant exaggeration that he had written his paper as a satire—I still think it has a great resemblance to something of this sort. I was also impressed with the idea that any one who points out that two substances supposed to be different are the same, renders even a greater service to science than the discoverer of a new body, supposing them all of equal interest. Moreover, I imagined the term Torbanite to be used as "meaning mischief." I was not aware that any chemist or mineralogist of taste and discretion, having only a scientific interest in the matter, would employ the word; but this may be a mistake. I have never heard of Newcastle coal being termed Newcastlite, or Wigan cannel Wiganite. I believe that any one using such terms would be laughed to scorn. There is no sharp manager of a gaswork that cannot distinguish the various cannels he uses. If there are any, the fault does not lie with the coal, for they all differ quite sufficiently to be readily distinguished. Even Kimmeridge shale, a far more remarkable mineral than Boghead coal, is not termed Kimmeridgite. There is no one that I am aware of who has any particular interest in using such a name, or no doubt it would have been employed. To be satisfied that this is a very peculiar mineral compared with Boghead coal we have just to read Mr. William's article, "Naphtha," in "Ure's Dictionary" (fifth edition, vol. iii., p. 220).

Besides pointing out this new word Kimmeridgite, for the lovers of such I have the pleasure of proposing other two. We have here in Wales a mineral seam at Leeswood. This is vulgarly supposed to be coal, but in reality consists of two new minerals. One portion breaks with a minute conchoidal fracture, giving the body a remarkable appearance—they were wont to call it the curly coal at the pits. I beg to propose the term Leeswoodite for this portion of the mineral. The other they call the smooth coal; it is dark and glossy, breaking in many directions with a very large conchoidal fracture; its aspect bears the closest resemblance to a piece of pitch. For this remarkable mineral the appellation "Woodleesite" may be considered appropriate. Considering, however, that these minerals are merely composed mainly of coaly matter—insoluble in all the usual liquids employed for dissolving bitumens, and that they give products of distillation not distinguishable in the present state of science from those obtained from Torbanite and other cannels—I beg to propose that the use of these names be deferred until science has made a sufficient advance to show that those bodies have a different nature from cannels in general. But any one wishing to give their books a learned appearance, and to find work for index-makers, are quite welcome to use them at present.

I have lately perused your report of Professor Ansted's lecture at the Society of Arts. (Chemical News, vol. vii., p. 235.) This philosopher appears to discriminate between Boghead shales and cannel coal. I had intended asking him to condescend kindly to define the characters by which a specimen of Boghead shale might be known from one of brown methel coal. The Boghead I know to be the richest in oil, but some cannel must be the richest, seeing that they all differ. It appears, however, that "it is impossible to fix any line of distinction between Boghead and Torbanehill shales and coal." If the Professor lectures to his students in this lucid manner, I am afraid that the faces of some of them must often light up with a questionable smile.

I was not a little surprised to read in Professor Ansted's discourse, that, "under the names of Boghead coal, Torbanehill mineral, &c., they (bituminous shales) have come into use for gas-making, and for the manufacture of oil by distillation at a low temperature, in the manner practised some thirty years ago by Selligue, in France, and since introduced into Scotland by Mr. James Young." The word introduced here is so artfully put I wonder how many of the members of the Society fully appreciated the meaning of the pleasant grin that must have crossed the Professor's countenance on reaching this part of the subject. The general impression of the public on this interesting topic agrees much better with what Mr. Williams writes in "Ure's Dict." (art. "Naphtha," above referred to), viz., "It was, we believe, at first procured solely by the distillation, at as low a temperature as possible, of the Torbanehill mineral or Boghead coal." I suppose this is one of those cases in which Doctors are said to differ; it looks very like it, at any rate. The public, I have no doubt, would feel an interest in learning the party's name that knew the most profitable use to which the Boghead coal should be put until Mr. Young informed them. And if Selligue published these matters to the world thirty years ago, how is it that so many rich oil-yielding coals travelled far and wide, and yet were only used for gas-making? There is some mystery here not generally understood. A little more light on the subject, from so distinguished a geologist as Professor Ansted, would not be lost at all.

With regard to the term photogen, one of your correspondents remarks that he thinks it "a more elegant and terse appellation than paraffin oil." Perhaps so, but it is usual to respect those names of substances which their discoverer has bestowed on them, so long as this is not inconvenient and interfering with some systematic nomenclature generally approved of. There is nothing in the derivation of the word photogen implying any quality peculiarly characteristic of this oil any more than of hundreds of inflammable bodies. Paraffin oil I think even the better name of the two, being the oil which yields paraffin in the greatest abundance. I think I could beat photogen for a name myself. Torbanite oil, or Torbanine, must be more modern. I suppose it is quite unnecessary to use one word for a mineral and another for its naphtha.

If I were Mr. Williams I would adhere to my old terms Boghead coal and Torbanehill mineral in all scientific papers. The former term implies, to my mind, that its nature is similar to other cannels, the latter, that this is not certain. The word Torbanite ought to signify that it is different from other cannels, because none of them have any such special appellation.

It seems to me just possible that "Boghead Coal without Choler" might be the better of a little. It would sometimes enliven his correspondence, perhaps.

I was sorry to observe, Mr. Editor, that you are not fond of poetry in any scientific communications. I hope any lady contributor to the "News" will note this peculiarity.

I have no objection at all to the praise you have bestowed on some of Mr. Williams' papers, — those on chinchonine bases, Boghead naphtha, and india-rubber products I have studied with pleasure. His "Handbook" I have found useful on many occasions.—I am, &c.

Ein Untersucher.

Phwilhelli.

P.S.—I had just concluded my letter when your Number containing the report of Mr. Paul's speech arrived. The Society of Arts, I have no doubt, have now a very exact idea of the difference between the "Celebrated Torbanehill Mineral," as Mr. Gillespie finds pleasure in calling it, and cannel coal. At least this is to be hoped, after the very distressing efforts of these distinguished philosophers to enlighten them on the subject. First, Mr. Paul finds some difference in the quantity of coke they yield: cannel gave 60 to 40 per cent.; boghead, 30; Albert, 33. If I have not committed the same blunder at least half-a-dozen times, Mr. Paul has stated the Boghead 10 per cent. too low; and he might safely have gone over 70 with his maximum number from cannels. With such distinctive characters as this, science must by-and-by become greatly simplified. Mr. Paul's statement, that the greater the amount of earthy matter the richer the coaly matter of cannels and shales, must also be founded on the most limited experience. Cannels or shales may be found with very large quantities of ash, any amount in shales, and yet the coaly matter be either as rich, perhaps richer, than the coaly matter of Boghead coal or as poor in volatile matter as could be desired. The results of the efforts of these gentlemen to distinguish Boghead from other cannels could not prove more melancholy than some of them. Perhaps Mr. Paul means that when the coaly matter is rich the earthy matter is abundant, but his knowledge of the Albert coal precludes this supposition. Whatever he means, it is certainly wrong.

Again, as to any distinction to be found in the oils yielded respectively by Boghead and cannels, I think it will be generally allowed that this has nowhere been shown, and I am afraid that any very remarkable difference will not be apparent for a long time.—I am, &c.

Ein Untersucher.

MISCELLANEOUS.

Preservation of Stone.—Some observations on this subject were made in the House of Commons on the 1st instant. Sir J. Shelley observed—"Various processes have been tried to preserve the stone work (of the Houses of Parliament), and notably that of a Hungarian, a *protégé* of Sir C. Barry's, but the portion operated upon by him was found to be in as bad a condition as any other part. There was one preparation—that of Mr. Daines—which did appear to answer, and which was spoken of by Mr. Sydney Smirke and Mr. Gilbert Scott in very favourable terms. Mr. Cowper ought to be able to give some good reasons for not employing one who had been so successful." Mr. Cowper replied that "Mr. Daines' process had not been recommended by the Commission. It consisted in the use of a perishable material, and was not of a permanent character." Mr. Tite said "he had had the honour of being on the committee referred to, and he was sorry to say they could not come to any satisfactory conclusion on the subject. Mr. Daines' process had been brought under their consideration, and the eminent chemists who acted on the committee found that it amounted to covering the stone with boiled oil with a small quantity of sulphur in it." A preparation, as Mr. Coningham remarked, used abroad as a specific for the mange in dogs!

The Oil Springs of America and Canada.—It is fearlessly asserted that had petroleum been in any other hands than those of Liverpool merchants, a very different and doleful state of affairs would have been chronicled, in place of that to be read below. Americans, and American advices coming to this country, have over and over again assured us that we were to be deluged with this product; in return for which they have ever had the gauntlet of "supply" thrown down to them, and the result—performances as empty as their promises. Exporters, consumers, speculators, all in their turn have combined to intimidate importers and holders into hasty sales, and unpremeditated forward contracts; but the studied conviction of past experience and careful inquiry has demonstrated to the latter, "that if the very foul and inferior paraffin oil, in a ten years' career, was never known to be under 2s. 3d. per gallon, surely the circumstances must indeed be exceptional and temporary which could permit a much more superior burning oil, like petroleum, to remain at anything under 2s. per gallon." But paraffin oil has averaged 2s. 9d. and 3s. during the last six months of the last ten years; and if facts are to be the basis of prediction, and value accompany merit, petroleum ought to see much higher rates than these between this and Christmas-day next. By the last advices to hand from New York, the *Quickstep* is the only vessel loading petroleum for this market—the very month that a whole fleet should be preparing for European ports for the commencing June 20 (longest day) demand. It may not be for the interest of holders here to invite a supply thus unequivocally, but all feel that the remunerating future of petroleum depends entirely on its own merits, rather than upon what can be said or prescribed. The month of May, throughout, has shown great stability in refined, while the large demand and improved business in crude does not show a single cask on speculation, the whole being absorbed by the trade.—*From Alex. S. Macrae's Circular, June, 1863.*

SCIENTIFIC SOCIETIES.

MEETINGS FOR THE ENSUING WEEK.

June 15. *Monday.*

British Architects—9, Conduit Street. 8 p.m.
Asiatic—5, New Burlington Street. 3 p.m.

16. *Tuesday.*

Statistical—12, St. James's Square. 8 p.m.
Ethnological—4, St. Martin's Place. 8 p.m.

17. *Wednesday.*

Meteorological—25, Great George Street. 7 p.m. *Anniversary.*
Geological—Burlington House. 8 p.m.

18. *Thursday.*

Royal—Burlington House. 8.30 p.m.
Chemical—Burlington House. 8 p.m. Mr. Abel, "*On a Case of Electrolytic Action.*" Dr. Guthrie, "*On the Iodide of Iodammonium.*"
Linnæan—Burlington House. 8 p.m.
Antiquaries—Somerset House. 8.30 p.m.
Numismatic—13, Gate Street, Lincoln's Inn Fields. 8 p.m. *Anniversary.*
Royal Society Club—St. James's Hall. 6 p.m.
Zoological—11, Hanover Square. 4 p.m.

19. *Friday.*

Philological—Somerset House. 8 p.m.

ANSWERS TO CORRESPONDENTS.

⁂ In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

J. N. Simpson.—To burn six pounds of carbon you require sixteen pounds of pure oxygen. Air contains 22 per cent. of oxygen, therefore 22 : 16 : : 100 : x. x represents the number of pounds of air required to burn six pounds of carbon.

THE CHEMICAL NEWS.

Vol. VII. No. 185.—June 20, 1863.

WEIGHTS AND MEASURES.

The intrinsic excellence of the system of weights and measures established in France is almost universally recognised, and its superiority to the system, or rather the confusion of systems prevailing in this country, has long been felt. In scientific pursuits the French decimal system has been very largely adopted, with convenience in some respects, but with disadvantages resulting from the absence of any relation between it and the legitimate weights and measures used in commercial transactions.

The Parliamentary Committee that have lately been inquiring into this subject have come to the conclusion that there would be a great advantage in the general adoption of a system of weights and measures which should be uniform, in itself, and with the system now established in France. The bill now before Parliament proposes therefore to substitute the French decimal weights and measures for those hitherto used in this country. This proposal certainly combines very great difficulties with, perhaps, equally great advantages, and it will doubtless be the subject of considerable difference of opinion. The inconveniences resulting from its adoption cannot but be very great, though they may be only temporary, and the great point to be determined is, whether the advantages ultimately resulting from the adoption of a system of weights and measures uniform with that of other countries, would be so considerable in themselves, and so much more important than the inconveniences of the change, as to justify its adoption.

In France the introduction of the metrical system was effected at a period peculiarly favourable for a change of the kind, but still its practical recognition was a work of considerable time and difficulty. It may even be said to be scarcely complete at the present. The introduction of the metrical system, as the legitimate one, in this country, would probably be far from being generally followed by its actual adoption, just in the same way that the present legitimate weights and measures are not universally used. Local custom is so much more influential than sound principle, that even now transactions are carried on in many parts with weights and measures that are not the legitimate ones, and it is equally probable that if the metrical system were legitimately established, the actual use of the weights and measures now familiarly known, would be discontinued but very gradually. It would almost seem as if such a progressive change were contemplated by the framers of the bill as inevitable, since it provides that "For the more convenient subdivision of weights and measures, it shall be lawful to use the double and the half of all the said units, and their principal decimal divisions and multiples, *as well as any other* subordinate divisions which the Committee of the Privy Council for Trade may deem expedient." Thus it is proposed that the new pound should not be identical with the kilogramme, which is equal to 2·2046 pounds avoirdupois, but that it shall be equal to the half of a kilogramme, or 1·1023 pounds. This proposal seems somewhat inconsistent with the adoption of the decimal system. If it be admissible to use half the standard unit of weight = 1·1023 pound avoirdupois, should it not be equally admissible to use weights that would be respectively equal to 0·45359 parts of a kilogramme, and to 50·8024 kilogrammes, and which would be equal to the pound and hundred weight now in use? If the kilogramme be substituted for the present legitimate standard unit of weight, with the view of introducing the decimal system, it would seem that there could be no reasonable objection to the use of *any* fraction or multiple of that standard unit in commercial transactions, any more than there is to the use of fractions or multiples of the present unit of weight, such as half, or a quarter, or a sixteenth of a pound, and 112 or 2240 pounds. In this way it might be possible to introduce eventually the metrical system without the present inconvenience of altering the actual value of the weights and measures now in use, by merely altering the mode of expressing those values as regards the legitimate standard. It is conceivable that such a plan would open the way to the practical recognition of the new system more easily and more effectually than would otherwise be possible, and that it would remove much of the opposition which the contemplated change is likely to meet with. It would render that change practically but a change of standard units, leaving the customary weights and measures to become gradually obsolete perhaps, in the same way that the guinea has come to be in reality obsolete, and to exist only in name. To facilitate the eventual change, and to afford opportunity for the new system becoming familiar, customs and excise duties might be charged according to the legalised standard units, instead of the fractions, or multiples of them, used in ordinary commercial transactions. In any case, if the proposed alteration be made, some expedient will be necessary to lessen the inconveniences consequent upon it.

It appears to be a remarkable omission in the bill now before Parliament, that no provisions are made for the conditions under which the proposed new standard units of measure and weight are to be determined. This is more especially the case since, in clause 9 of the bill it is stated, that "all and every the provisions and provision which are by law in force with respect to the inspection, verification, &c., of the present imperial standard weights and measures, shall apply to and be in force with regard to the metric weights and measures in every respect as if the standard metric weights and measures were comprised in and designated by the imperial weights and measures in the Acts relating to such inspection, verification, &c." The unit of weight is to be the weight of a new quart of distilled water, or the tenth of the French cubic metre; but if this weight is to be estimated at 62° F. it will not be a kilogramme, since the kilogramme is the weight of a cubic decimeter of water at a temperature of 39·1° F., and under a barometric pressure of 29·922 inches.

The unit of linear measure is made referable to the present inch, and not to the natural basis upon which the French measure is founded, though it is implied that the inch is to be abolished, and the several standards of weight and measure are to be verified by comparison with the standards in Paris, though it does not appear why their accuracy should be made dependent on that of the French standards.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Contributions to the History of Thallium, by William Crookes, *F.R.S.*

In a paper submitted to the Royal Society in February last, I described the precipitation of metallic thallium in the crystalline form, by passing a voltaic current through sulphate of thallium, platinum terminals being used; and stated that the solution might be either acid or rendered alkaline with ammonia. During the electrolytic decomposition the formation of a brown crust was frequently noticed on the positive pole, and, in consequence of this, experiments have since been made to ascertain the conditions under which this deposit was formed, and also its composition.

A quantity of sulphate of thallium was re-crystallised twice, and ascertained to be free from all metallic impurity. A nearly saturated aqueous solution of this was prepared, and the following experiments were tried with it The battery was two cells of Grove's. Platinum terminals were used in every case:—

a. One portion, perfectly neutral to test-paper, was submitted to electrolysis. Thallium slowly made its appearance at the negative pole in the form of a crystalline, spongy mass. No oxygen was evolved at the positive pole; but it became coated with a film, varying in colour according to the time occupied in the formation, and also with the distance it was from the negative pole. By employing a large surface of polished platinum all the beautiful colours of thin plates could be seen on it, and rings, bands, or uniform surfaces of the most vivid colours could be obtained at will by varying the position of the negative wire. At one time a surface of platinum, nearly an inch square, was apparently converted into gold, so perfect was the yellow, metallic reflection. By allowing the action to proceed, the colours rapidly change, and the coating ultimately becomes deep brown. The platinum, with the coloured films on it, will bear washing and exposure for several days to the air without sensible diminution of brilliancy.

b. A second portion of the solution was acidulated with $\frac{1}{10}$th of its bulk of pure sulphuric acid. On passing the current a much more abundant metallic reduction took place; oxygen was freely evolved from the positive pole, no deposit whatever being formed.

c. A third portion was mixed with $\frac{1}{20}$th of its volume of pure ammonia, and the clear liquid decomposed as before. The metal came down in very much finer and more brilliant crystals than in either of the former experiments, long needles rapidly shooting out across the bottom of the vessel. The brown deposit was formed abundantly on the positive pole, no gas whatever being evolved. When the coating became of a certain thickness it sealed off and fell to the bottom of the dish, a fresh formation taking place as the operation continued.

d. The solution used in experiment *a* was submitted to long-continued electrolysis, the metal being removed from time to time as it accumulated round the negative wire. The brown deposit was at first formed with no evolution of oxygen, but, as the liquid became more and more acid, the accumulation ceased, bubbles began to form, and the coating already on the wire gradually dissolved. The solution now acted as the one in experiment *b*.

The platinum positive terminal used in experiment *c* was removed when thickly coated with brown deposit, and digested in cold dilute sulphuric acid. In a few minutes the coating entirely disappeared, without any evolution of gas, forming a colourless liquid, acting in all respects like a solution of peroxide of thallium, ammonia added to it producing an abundant brown precipitate of this oxide.

Therefore, upon passing a voltaic current through neutral or alkaline sulphate of thallium, the whole of the oxygen is not evolved at the positive pole, but some unites with the metal to form peroxide of thallium. When the neutral solution becomes acid by abstraction of the metal, this peroxide dissolves, and oxygen is then evolved. It became of interest to ascertain whether peroxide of thallium was equally formed in an acid, as in an alkaline solution. The liquid used in experiment *d* was therefore supersaturated with ammonia. The immediate precipitation of peroxide of thallium showed that the brown deposit had merely dissolved as such in the acid, without having been decomposed. The solution used in experiment *b* was then rendered alkaline with ammonia, when the same brown precipitate at once took place.

Peroxide of thallium is therefore formed in all cases at the positive pole, when the sulphate (as well as other salts which I have tried) is decomposed by the battery. In an acid solution the peroxide dissolves as soon as formed, whilst in an alkaline or neutral solution it remains insoluble.[*]

Is the peroxide of thallium dissolved in the acid solution ultimately decomposed? To ascertain this, a neutral solution of the sulphate was exposed to the action of the battery, as at *a*, for some hours. Peroxide was at first deposited, and ultimately dissolved. The metal came down freely, until at last only hydrogen was evolved. The current was then allowed to pass for some time longer, and the liquid was tested. Ammonia gave no precipitate, and further tests showed that all the thallium had been removed from the liquid.

Finally, it was necessary to ascertain whether the brown deposit formed on the positive pole under these circumstances was in reality identical with the brown peroxide of thallium, TlO_3, described by M. Lamy.

A large quantity of alkaline sulphate of thallium was decomposed by the battery, and the deposit collected. This was finely powdered in an agate mortar, well washed with hot water until no sulphuric acid was detected in the filtrate, and then dried.

About 40 grains were then taken for analysis. They were heated in an air-bath to 300° C., until they ceased to lose weight. Pure sulphuric acid was added, and the whole heated gradually, until the excess of acid was volatilised. The residue was then gradually heated to dull redness. A fused mass of white sulphate of protoxide of thallium was left. This was weighed. It was

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[*] Since trying the above experiments, I see from the *Annalen der Chemie und Pharmacie* for May, p. 181, that Professor Böttger has observed the production of the same brown deposit at the positive pole during the electrolysis of sulphate of thallium, and says that in all probability it is identical with the brown oxide produced by the action of an alkali on the sesquichloride.

then dissolved in boiling water, and a hot solution of bichloride of platinum was added. The resulting platinochloride of thallium was collected, washed with alcohol and ether, and weighed. The figures obtained agree closely with the formula TlO_3, taking the atomic weight of thallium at 203. This production of a brown peroxide by electrolytic means, still further increases the resemblance of thallium to lead and silver, and removes it from the potassium and sodium group.

I have to make a correction in a statement I formerly advanced respecting thallic acid. Upon recently attempting to repeat the experiment which led me to the supposition that such a body existed, I have been unable to obtain such decisive results as would confirm my former supposition. I know that the metal I am now working with is pure, and it is also more than probable that the few grains of metal with which some of my first results were obtained, having been extracted as long ago as September, 1861, contained some impurity, probably arsenic, which misled me. I now take the earliest opportunity of correcting an error.

The Relation between the Diffusion of Vapours and their Chemical Condition, by B. H. Paul.

In the evidence that has recently been brought forward with the view of showing that the vapour of a compound substance may, at a high temperature, lose its individuality and be resolved into a mixture of the vapours of its constituents; and, that the anomalous vapour density of some substances is due to this disassociation, the chemical influence of the means employed to show that such is the case appears to have been too much disregarded. The experiments of Messrs. Wanklyn and Robinson clearly show that, at a high temperature, and under conditions which admit of diffusion taking place, the vapour of sulphuric hydrate or of phosphorus pentachloride, is decomposed, and that, in the one case, sulphuric anhydride and water are produced, in the other phosphorus perchloride and chlorine. It is, however, by no means necessary to conclude, from these facts, that the vapours, in this case, consisted of mere mixtures of the vapours separated by diffusion, and that they were not really the vapours of sulphuric hydrate and phosphorus pentachloride.

The relative diffusibility of the constituents of a compound being, in itself, a determining cause of decomposition, the separation of those constituents by diffusion cannot be regarded as evidence of their existence in a state of mixture. To take the most simple case, in which the influence of temperature is not concerned, viz., the diffusion of alumina binacetate, or the diffusion of alum, observed by Mr. Graham to be attended with decomposition, it is still necessary to regard the unequal diffusibility of the constituents of a compound as an efficient agent of decomposition, and to infer that, under conditions which admit of diffusion taking place, there is a force exercised capable of overcoming the chemical attraction of substances that differ in diffusibility.

From M. Deville's late experiments of diffusion through a platinum diaphragm, it seems that the influence exercised by the diaphragm merely affected the rate of diffusion, conjointly with the temperature, and that the chemical attraction of the oxygen for hydrogen was independent of it, and determined only by the temperature. The decomposition of carbonic acid by diffusion, in one of those experiments, and the decomposition of water vapour by diffusion, show that the chemical force exercised in diffusion phenomena is very considerable. On the other hand, the stability of ammonium chloride vapour at a temperature above that at which ammonia is decomposed by heat alone, shows that the anomalous volume of the ammonium chloride vapour is not due to the disassociation of its constituents, and that the separation of those constituents by diffusion—even in the absence of a diaphragm—is no evidence of the vapour being a mixture of ammonia and hydrochloric acid.

Acetic Acid and its Homologs in Wine.

M. Bechamp* has observed that even new wine which has been fermented out of contact with the atmosphere always yielded an acid liquid on distillation. He obtained from wine about 0·4 per cent. of soda acetate, and by fermenting sugar in an atmosphere of carbonic acid he obtained a mixture of acetate and salts of fatty acids in small amount, the solution of which gelatinised when evaporated to the point of crystallisation, and after a time yielded crystals of soda acetate, amounting to about ·35 per cent. of the sugar fermented.

M. Bechamp considers that the acetic acid is not merely the result of the oxidation of alcohol, but that it, and the homologous acids accompanying it, are produced by fermentation. In support of this view, he refers to Lavoisier's observation,† that the product of the fermentation of sugar contained acetic acid.

M. Pasteur, ‡ commenting on this opinion, points out the probability that the acids which have long been known to exist in fermented liquors are the result of alteration of the ferment, and do not originate from sugar. For this reason he has not included them among the products of alcoholic fermentation. The presence of acetic acid observed in fermented liquids, he considers to be merely accidental, and due either to the special conditions which are determined by the growth of a mycoderm, or to the action of a specific ferment, different from the alcoholic ferment.

Though, under ordinary circumstances, acetic acid and its homologs are always recognisable in alcoholic liquids, this is not the case with liquids fermented with alcoholic ferment, which do not give any indication of the presence of those acids, however carefully they may be evaporated, either by the smell or by reaction upon test-paper.

As to the source of these acids, M. Pasteur is still undecided, and he regards the determination of this point as one of the desiderata of his researches on the subject of fermentation.

On the Estimation of Hydrogen and Carbon in Organic Analysis, by M. Ch. Mene.

There is no need to expatiate on the faults of the arrangement hitherto employed for organic analysis. Besides the unavoidable difficulty of filling the tube with the oxide of copper and the material, as well as the danger of the heating, the hygrometric property of oxide of copper, its defect in not parting with the oxygen required to burn the matter, except in contact with the substance, at a high temperature (which frequently spoils or breaks the tubes), added to the frequently insufficient oxygenation, have made this method a difficult, and, unfortunately, a neglected and rare experiment. Moreover, in many instances it is an uncertain operation,

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* *Comptes-Rendus*, lvi., 969.

† *Traité de Chemie*, i., 147.

‡ *Comptes-Rendus*, lvi., 989.

both on account of the necessarily small quantity of matter used, as of the delicate and peculiar manipulation required. I believe I have succeeded in avoiding almost all the previous drawbacks by substituting melted chlorate of potash for oxide of copper, and have consequently rendered organic analysis much easier, by the following modification of the process hitherto in use:—

Take a tube of white glass, about fifty centimetres in length, one in diameter, and one millimetre in thickness,* and closed at one end; introduce at the closed end a quantity of chlorate of potash (melted and pounded) equal to about two centimetres, into which pour the mixture of the matter to be analysed, intimately mixed with enough chlorate of potash to nearly fill the tube. Prepare the mixture of material and chlorate of potash by taking one gramme of the former, pounding it well in a crystal or agate mortar, and thoroughly stirring it with chlorate of potash, previously melted and pounded. The capacity of the tube indicates the amount of chlorate of potash to be introduced in the tube, with the material for analysis; my estimate is about fifty grammes. Little calculation is needed to show that fifty grammes of chlorate will furnish about eighteen litres of oxygen,—a gaseous medium capable of burning any kind of organic matter. As in the old process, introduce the whole into the tube by means of a funnel, but at the ordinary temperature, with no fear that the mixture will absorb any humidity. When full, close the tube with an amianthus plug, then with a cork traversed by a small tube for the disengagement of the gases. I generally lute the cork with sealing-wax. Suspend the tube thus prepared by two iron wires, so that it may be free and within reach of the operator; then, by means of an india-rubber tube, put it in communication with Liebig's bulbs and a U-shaped apparatus respectively filled with the sulphuric acid, potash, &c., requisite for the estimation of hydrogen and carbon.

In short, the principle of the modification I propose consists in embedding the substance in chlorate of potash, and burning it with this salt, under such conditions that the decomposition is slow and gradual, so as to allow of the regular disengagement of gases, as in an oxide of copper apparatus. I place an ordinary spirit-lamp under the tube, beginning to heat near the amianthus plug. The chlorate of potash melts almost immediately, burning the matter, and forming water and carbonic acid, which disengage quietly, according as they are heated. Though, in many instances (when the substance contains much carbon), ignition, and sometimes even deflagration accompanies combustion, yet there is no danger in the experiment. When the matter is burnt in the place first heated, the lamp is shifted, and so as successively to heat the whole length of the tube. The chlorate of potash at the closed end of the tube is heated last, so as to disengage oxygen, and thus take with it all the analysable gases which may remain in the tube. When the operation is presumed to be at an end, the tube is detached from the estimating apparatus, and all the chloride of potassium is eliminated by washing, to make ready the tube for fresh analyses. An analysis of this kind occupies twenty minutes; the operation may be carried on at the will of the operator; he can carry it out, interrupt it, or resume it as he pleases, without fear of unexpected and annoying absorptions or reactions.—*Comptes-Rendus*, lvi., No. 10.

TECHNICAL CHEMISTRY.

Chemical History of the Coal-tar Dyes.

Dr. Hofmann has recently pointed out that there are two substances—derivatives of ammonia—belonging to the phenyl series, which have the same composition, and which resemble each other in their general characters, but present differences that justify the opinion that they are chemically distinct. Hence he has applied to these substances the terms—α phenyl-diamine, and β phenyl-diamine. Seeking to obtain corresponding monamines, he has been led to compare various samples of aniline prepared by different methods.

Aniline obtained from indigo by distillation with potash was found to boil at 182° C., and to possess the characters generally ascribed to aniline. But when submitted to the action of mercuric chloride, stannic chloride, or arsenic acid, it did not yield any aniline red.

Aniline prepared from benzol, obtained by distilling benzoic acid with lime, also boiled at 182° C., but when acted upon by the ordinary reagents did not yield aniline red.

Aniline prepared from benzol, obtained from coal-tar, was also found to boil at 182° C., and was equally incapable of being converted into aniline red in the usual manner.

These remarkable facts were further confirmed by Mr. Nicholson, who had previously observed that perfectly pure aniline prepared from coal-tar benzol, and boiling at 182° C., would not yield aniline red.

A number of other samples of aniline obtained from different sources were found to yield aniline red in the usual way, but all these samples boiled at a temperature between 180° and 220° C. Hence Dr. Hofmann infers that commercial aniline contains, besides aniline, another base, which is essential for the production of the red dye.

Mr. Church has described, under the name of parabenzol, a substance obtained from coal-tar, isomeric with benzol, and boiling at 97·5°, and Dr. Hofmann suggests that this substance may be the source of a base isomeric with aniline, and capable of yielding the red dye.

In any case the facts above mentioned have especial interest both in a scientific and technical sense.—*Comptes-Rendus*, lvi., 1033.

The Chemistry of Agriculture.

(Continued from page 270.)

Baron Liebig commences the history of mineral manures from the time when he became convinced of the necessity of replacing all the constituents which are removed from land in crops, or of supplying, from extraneous sources, whatever deficiency of those substances might exist in the farm manure, and that by this means only could land continue to be fertile, and to satisfy the requirements of increasing population.

It was in 1844-45 that he was engaged in studying this subject; by analysis of the corn and other crops exported from land, he ascertained what was removed from it, and what must be returned to it, in order to sustain its productiveness. The question was, how this should be done?

In the case of phosphoric acid there was no difficulty; in the case of alkalies there was much. Phosphoric acid might be uniformly distributed through soil in the state of superphosphate, and it might be assumed that it would always meet with enough lime in the soil to be

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* I recommend these dimensions, because thicker tubes crack under heat, and thinner ones melt; in any case the flame of the spirit-lamp must not be brought quickly under the tube, because of the bad heat-conducting quality of glass.

converted into ordinary phosphate, and while thus rendered but very slightly soluble in rain-water, would be dissolved by the water containing carbonic acid in the soil. But potash salts being very soluble, it might be anticipated that they would be soon washed away by rain.

At that time it was generally considered that the food of plants could be rendered available to them only in the state of solution. No other opinion was possible then.

Baron Liebig endeavoured therefore to prepare a compound of potash having less solubility than its ordinary salt, and which would serve as a manure without being washed away by rain. He succeeded in effecting this, and believed the most important problem relating to the preparation of artificial manure was solved.

His manure contained phosphoric acid, potash, sulphuric acid, and ammonia. The addition of ammonia was, however, considered to be in all probability unnecessary for some crops, such, for instance, as foliaceous plants, clover, peas, and beans; a point of importance to farmers, inasmuch as the ammonia made a sensible difference in the cost of the manure.

But though this manure contained all the constituents of the most effective manures, its application was not attended with anything like the anticipated advantages.

In England a series of experiments were made with this manure by Mr. Lawes, a manure manufacturer, on his land at Rothamsted. They showed that its efficacy was slight. Baron Liebig considers those experiments were not originally instituted for the purpose of testing the soundness of his doctrine, but simply to try the value of the manure.

Baron Liebig's own observations on a piece of land near Giessen convinced him that the manure was not without some effect, but its action was so slow that it was inapplicable in agriculture. Why it was so he could not understand.

Further experiments were made by Mr. Lawes with other mixtures, and the results were satisfactory to Baron Liebig in regard to his doctrine, but bewildered him still more as to the reason why his own manure was ineffective.

Baron Liebig thinks that the experiments of Mr. Lawes would perhaps have been little heeded, and the facts ascertained by him would soon have found their true explanation as confirmations of his doctrine, had he not been so unfortunate as to find in Mr. Philip Pusey, the former president of the Royal Agricultural Society, an unmerciful enemy of his doctrine, and Baron Liebig refers to his expressions in the Journal of the Society as proving that in his opinion it had lost its right to existence, that it was literally slaughtered by Mr. Pusey in that journal, where he says:—

"The mineral theory, hastily adopted by Liebig, has broken down. The doctrine that 'crops diminish or increase in exact proportion to the diminution or increase of the mineral substances conveyed to them in manure,' has received its death blow from Mr. Lawes' experiments. Our best authority, Mr. Lawes, has established certainly so much, that of the two active principles in manure, ammonia is specially suited to corn, phosphorus to turnips." . . . .

"Except Liebig's suggestion for dissolving bones with acid, and Sir Robert Kane's for using flax-water as manure, I know no agricultural process arising out of chemical discovery." . . . .

"It is a great mistake to suppose that men can be made farmers by teaching them doubtful chemistry."

Baron Liebig considers these remarks, in reference to his doctrine and to the facts ascertained by Mr. Lawes, to be in the highest degree remarkable. Mr. Lawes had tried, one after the other, all the substances Baron Liebig had pointed out as inorganic plant food. Potash, lime, and magnesia had no influence on his crops. On the contrary, a very favourable influence was exercised by ammonia salts and lime superphosphate, the latter of which Baron Liebig had characterised as the most necessary manure for English land.

In reference to these results, Baron Liebig remarks that both these substances are inorganic, that their action is consistent with his mineral theory of plant nutrition, and that the fact of turnips not requiring ammoniacal manures is consistent with his opinion that some crops might be grown without a special supply of ammonia.

He also adds that before the mineral theory was made known, farmers knew nothing of superphosphate or of ammonia salts, and that if one of these substances be especially adapted for increasing corn crops, and the other especially suited for turnips, the knowledge of these facts would be most invaluable aids conferred by chemistry on agriculture.

However, access being denied him to the Journal of the Royal Agricultural Society, he published, in the new edition of his chemical letters, the arguments by which he endeavoured to prove that Mr. Lawes' results had been misinterpreted, that they were not opposed to, but a confirmation of, the mineral doctrine, and that the inefficiency of his own manure was no proof of the insufficiency of his theory.

Baron Liebig complains that his theory fared still worse after this explanation, since Mr. Lawes had not only proved his manure to be bad, but had also prepared for the overthrow of the theory, by putting forward one of his own, that, "manures are, in general, divided into two classes, organic and inorganic; organic manures are those which are capable, by decomposition or otherwise, of yielding to plants carbon, hydrogen, and nitrogen. Inorganic manures are those which contain the mineral constituents of which the ashes of plants consist."*

In regard to the value of ammonia as a source of nitrogen for plants, Baron Liebig remarks that, having in the first edition of his "Agricultural Chemistry," attached too great importance to it, he afterwards endeavoured to remedy the ill effects of this error. He had come to the conclusion that there could be no progress in agriculture if the increase of crops depended upon the supply of ammoniacal manures from extraneous sources. His observations had led him to consider that, independent of the ammonia in soils, the atmosphere alone supplied sufficient ammonia for producing the most abundant crops, and that for the growth of corn crops it was only necessary, by adopting a suitable rotation, to accumulate the ammonia supplied by the atmosphere. Since corn crops required more nitrogen for their growth than they actually contain, it was only necessary, by the growth of fodder crops, which furnish manure, to collect ammonia from the atmosphere in the same manner that a miller stores up water to drive his mill, when, during his busy time, he wants more water than his stream will supply.

Baron Liebig maintains that in his theory he included

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* This quotation from Mr. Lawes is not the same as the original passage. *Jour. Roy. Ag. Soc.*, vol. viii., p. 240. (The words omitted by Liebig in his quotation are italicised):—"*I now come to the action of* manures, *which* are generally divided into two classes—organic and inorganic. *Although this distinction is by no means satisfactory, I shall adopt it, as being generally understood.* Organic manures are those which are capable of yielding to the plant, by decomposition or otherwise, *organic matter*—carbon, hydrogen, oxygen, and nitrogen—*constituents which uncultivated plants derive originally from the atmosphere.* Inorganic manures are those substances which contain the mineral ingredients of which the ash of plants is found to consist."

ammonia among the "mineral constituents" of plant-food, and that when he pointed out the necessity of returning to land all the substances removed from it by a crop, he did not refer only to the constituents of the ashes. Upon this ground he maintains that the views put forward by Mr. Lawes and Dr. Gilbert, in opposition to his theory of the chemistry of agriculture, involve a misconception of that theory, and hence has arisen the controversy that has been maintained on this subject.

The history of the theory put forward by Baron Liebig in regard to the chemistry of agriculture, admits of a statement very different from that given by himself, and the changes it has undergone, the results that have been obtained in reference to it, and to the subject generally, present much that is of interest in illustrating the danger of a too eager application of theoretical views, and in showing the true mode in which scientific knowledge and research must be applied in order to be useful.

(To be continued.)

Porosity of Platinum Tubes.

MM. St. Claire Deville and Troost* have recently made some experiments, the results of which show that platinum vessels cannot be employed for experiments with gases or vapours at high temperatures.

By passing a uniform current of dry air through a platinum tube fitted within a porcelain tube, through which a uniform current of pure dry hydrogen was passed, and applying heat to the tubes, it was found that the air issuing from the platinum tube contained less and less oxygen as the temperature was increased, and that water condensed in the tube through which the air was discharged from the platinum tube. At about 1100° C. the gas consisted only of nitrogen and water vapour, and at the same time the hydrogen discharged from the outer porcelain tube was sensibly reduced in quantity.

The composition of the gas discharged from the platinum tube at gradually increasing temperatures above a red heat was—

Oxygen .	19	16·7	15·5	12·3	10·5	8·8	5·9	3·0	0
Nitrogen.	81	83·3	84·5	87·7	89·5	91·2	94·1	97·0	100

When the temperature exceeded 1100° C., the gas from the platinum tube also contained hydrogen, from 1·3 to 21·9 per cent., according to the temperature.

The water collected contained some nitric acid.

By reducing the temperature, the gas discharged from the platinum tube was eventually pure air, as at the commencement of the experiment.

During the whole time, the hydrogen discharged from the outer tube was entirely absorbable by oxide of copper.

By rapidly closing the tube supplying the hydrogen to the outer tube, and immersing the discharge tube in mercury while the temperature of the tubes was high, the mercury rose to the height of 602 millimetres (the barometer being at 753), the small difference of 15 centimetres being due to a slight leak in the apparatus.

By passing a current of pure dry carbonic acid through the platinum tube, and a current of hydrogen through the outer tube, a large amount of carbonic oxide was produced, and discharged with carbonic acid and hydrogen in the following proportions, according to the temperature:

Hydrogen . . .	12·7	7·3
Carbonic oxide . .	—	17·7
Carbonic acid . .	87·3	75·0
	100·0	100·0

When the current of hydrogen in the first experiment was replaced by a current of carbonic oxide, this gas did not penetrate to any extent into the platinum tube.

These experiments were made with two tubes one millimetre thick (about 0·04 inch) and six inches long. They were heated to about one-fourth of their length. These tubes were manufactured in the ordinary manner for sulphuric acid retorts; they maintained a vacuum, and had been tested by high pressure.

All the above experiments were repeated with a tube made of melted platinum by Messrs. Johnston and Matthey about 0·08 inch thick and 24 inches long, but, notwithstanding this greater thickness, the results obtained were exactly the same.

Manufacture of Magnesium.

Mr. Sonstadt's* improvements in the manufacture of magnesium consist in—

1st. The preparation of pure magnesia or carbonate of magnesia by decomposing magnesia sulphate, with soda or carbonate of soda, washing the precipitate, drying it, and again washing, repeating alternately the drying and washing till it is free from sulphuric acid.

2nd. The preparation of anhydrous magnesium and sodium chloride, by dissolving the pure magnesia or carbonate of magnesia in hydrochloric acid, and adding sodium chloride, free from sulphuric acid, in such quantity, that for every part by weight of acid (1·152 density) there may be about 0·55 parts sodium chloride. The residue obtained by evaporating the solution to dryness is to be fused in a platinum crucible and poured out on a clean iron plate.

3rd. The production of magnesium, by melting, at a full red heat, the above "material" with one-fifth its weight of sodium in an iron crucible with a close cover, dissolving the saline mass, when cold, in water, drying the globules of magnesium, melting the metal again with magnesium chloride, and compressing the melted metal with an iron rod while the chloride begins to solidify.

4th. The preparation of anhydrous magnesium chloride by slowly heating the hydrated chloride to redness in a current of dry hydrochloric acid.

Magnesium is described as a silver-white metal, very brilliant, malleable at a temperature below redness, somewhat brittle at the ordinary temperature. It melts at a red heat, and burns with a brilliant white light. It is little oxidisable at the ordinary temperature, even in moist air, and is not sensibly affected by sulphuretted gases. It is acted upon by ammoniacal gases. Its density is 1·74.

[This invention is a striking instance of the patent nuisances, that have now become intolerable. With the exception of the use of iron crucibles for reducing magnesium, there is positively nothing at all new in any of the details described in the specification, and if the use of an iron crucible for a smelting operation be a patentable invention, there can be little doubt as to the necessity for an amendment of the patent law.

These remarks must of course be understood to refer merely to those details of the specification that have any practical import; for, as usual with specifications of worthless inventions, or of repetitions of inventions, there are a host of details, with neither sense nor value, introduced to hide the nakedness that would otherwise be too obvious.]

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* *Comptes-Rendus*, lvi., 977.

* Specification No. 3021. May 8, 1863.

On Destructive Distillation, considered in Reference to Modern Industrial Arts,* *by* B. H. Paul, *Ph.D.*

(*Continued from page* 283.)

Just at the time when Lord Dundonald was carrying out his enterprise of coal distilling, the subject of destructive distillation was treated of by Bishop Watson, in one of his essays. He gives the results obtained by the distillation of pit coal, "with a fire gradually augmented," and describes one portion of the oil he thus obtained from Newcastle coal as being lighter than water, "more or less liquid and transparent, according as the heat used in conducting the distillation has been greater or less."

Another portion of the oil was black, thick, and tenacious, much resembling tar. He also states that, "the quality of the liquid separable from wood by distillation is wholly the same as that of the liquid separable from pit coal by the same means."

He also adds that "it is probable that the quantity of oil separable from the same kind of coal by distillation may be influenced in some degree by the manner of performing the operation; and there is, moreover, some reason to believe that in different kinds of coal the quantities may be very various."

This conjecture was soon supported by results of observation. M. Sage, in a paper on coal, published in 1789, describes English cannel coal as yielding "by distillation more than one-third of its weight of oil that solidified in cooling," while the French coal gave only one-sixteenth of its weight of oil. Newcastle coal, though containing "as much bitumen as cannel coal," was described by the same observer as being very different from it, and Scotch coal, which contained much less bitumen than either of the preceding coals, gave by distillation an oil that was liquid and floated on water.

Kirwan, in 1796, stated that almost all species of mineral coal yield on distillation more or less of both species of bitumen—solid and liquid—but that "the proportion is variable in every species, according to the degree of heat applied." Referring to Lord Dundonald's method of obtaining tar from coal, he says:—"By his lordship's mode of distillation, however, much seems to be lost during the internal combustion. I should think the Prince of Nassau-Saarbruck's method in this respect more advantageous. M. Sage tells us that by distillation he obtained from cannel coal one-third part of its weight of tar."

It is very interesting to find that Kirwan describes the coal used by Lord Dundonald for distillation as being a kind of cannel coal, similar to, but of a better sort than, the "stony or slaty cannel coal" from Ayrshire. This coal is described as burning like compact cannel coal, without caking, and leaving a stony residuum. It contained 20·83 per cent. of ash.

Proust also, in 1806, described the oils obtainable from coal by distillation: a light oil similar to that of amber, and a heavy oil like tar. He says that "the oily products of coal vary much in consistence, and it is necessary always to make an experiment to ascertain if it will yield thick oil or tar."

Lord Dundonald's method of distilling was carried out also in France by a M. Faujas de Sante Fonds, who claimed to have conceived the same idea of obtaining the volatile products given off in coking, before knowing of Lord Dundonald's plan, although he was confirmed in that idea by visiting the tar works in Scotland. Shortly after his return to France an experiment was made by him, by the order of M. de Calonne, to illustrate the operation, and an account of it is given in a scientific journal of that time.

Just at this time, however, a circumstance took place which exercised a great influence on the development of this young branch of manufacture. The oil manufactured by Lord Dundonald was just beginning to be employed for street lamps, and it is said that he was in treaty with the authorities with a view to the lighting of London by means of it, when a new project was started, viz., the use of gas as a source of light, which eventually proved the overthrow of Lord Dundonald's undertaking.

The chemistry of gases, or pneumatic chemistry, as it was then termed, had, during the latter half of the eighteenth century, gradually absorbed the attention of chemists. As far back as 1721, Hales had, in the course of his experiments on air and its relation to vegetation, observed that Newcastle coal yielded in distillation one-third of its weight of air. In 1739 Mr. Clayton was induced, by the observation of a natural discharge of combustible gas from the neighbourhood of a coal pit, to try the effect of heat upon coal, and he found that by distillation he obtained a similar inflammable gas, together with a black oil and some watery liquid. This spirit of coals, as he called it, served him for the amusement of his friends, and for the entertainment of the Royal Society, but for no further purpose. In 1759, Neumann stated in his "Chemistry" that vegetable substances in general, urged hastily by a strong fire, emit an aëreal elastic vapour. Bishop Watson also refers to the inflammable gas produced in distilling coal, &c. He says, "the products obtainable by distillation from bituminous and vegetable substances in general are water, gas, oils of different colours, weights, consistencies, and a black, coaly residuum." It was not, however, until shortly after the year 1790, that a Cornish engineer, Mr. Murdoch, not only observed that the gas given out by heating coal, wood, peat, &c., burnt with a bright luminous flame, but also conceived the idea of using the gas thus produced as a source of light. A somewhat similar idea was about the same time being carried out in France by Le Bon, the material from which gas was obtained being wood.

The announcement of this invention produced a most remarkable effect. Notwithstanding energetic opposition to its introduction, it rapidly gained ground; in 1810, the first Gaslight and Coke Company was formed, and ten years later gas was almost universally used in London.

Attention was thus suddenly diverted to the gaseous products of destructive distillation as a source of light; but the oily products were not wholly forgotten, nor did the nature of the relations existing between them and the gaseous products admit of their being overlooked. Long before any great attention had been directed to the gaseous products of destructive distillation, it had been observed that both the quantity and the quality of the oily products obtained from any given material depended upon the degree of heat employed in the distillation. But when the gas produced in the distillation also became an object of investigation, and when the means of producing it for practical purposes were being developed, it soon became evident that, to a great extent, gas and tar or oil were convertible substances: that whenever a great deal of oil was produced, only a small quantity of gas was obtained. This fact threw a new light upon the nature of destructive distillation, and since it was a most important desideratum, in regard to this new manufacture, to obtain the largest possible quantity of gas, the conditions requisite for effecting this

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* Read before the Society of Arts, May 27, 1863.

object was specially investigated at a very early period in the history of gas lighting.

The general result arrived at was, that the production of the largest amount of illuminating gas from coal required the distillation of the coal to be conducted within a certain range of temperature. When the temperature was much below an ordinary red heat, a smaller quantity of gas was obtained than when the distillation was conducted at a full cherry-red heat, and at the same time a larger quantity of the oily product was obtained. On the other hand, when the temperature was much above a full cherry-red heat, the quantity of gas was much increased, but its illuminating quality was very much reduced. Hence a cherry-red heat was fixed upon as the temperature to be employed in practice, because that was the temperature at which the largest quantity of the volatile products were converted into the state of gas of the greatest illuminating power, and at which the smallest quantity of those products remained in the liquid state.

Accordingly, in practical and scientific works, treating of the manufacture of gas and the phenomena of destructive distillation generally, this fact is prominently mentioned. Thus, for instance, Accum says:—"The production of carburetted hydrogen, both with regard to quantity and quality, from the same kind of coal, depends much upon the degree of temperature employed in the distillatory process. If the tar and oil produced during the evolution of the gas in its nascent state be made to come in contact with the sides of the red-hot retort, or, if it be made to pass through an iron cylinder or other vessel heated red-hot, a large portion becomes decomposed into carburetted hydrogen gas and olefiant gas, and thus a much larger quantity of gas is produced than would be obtained without such precaution, from the same quantity of coal." This was rendered still more evident by pointing out that every pound of coal-tar so decomposed produced "fifteen cubic feet of carburetted hydrogen, abounding in olefiant gas."

Ure, in 1824, describing the theory and practice of the production and use of coal-gas, says, "If coal be put into a cold retort and slowly exposed to heat, its bitumen is merely volatilised in the state of condensible tar; little gas, and that of inferior illuminating power, is produced. This distillatory temperature may be estimated at about 600° or 700° Fahr.

"If the retort be previously brought to a bright cherry-red heat, then the coals, the instant after their introduction, yield a copious supply of good gas, and a moderate quantity of tarry and ammoniacal vapour.

Dumas, in 1828, describing the general features of destructive distillation, says that the nature of the products depends on the temperature employed. "By heating gradually, oil is produced, and at a higher temperature no oil is obtained, because it cannot exist at the higher temperature." Describing the manufacture of gas, he adds that the products obtainable from coal by distillation are "coke, tar or oil, and gas." The relative quantities of each of these products are very different, not only according to the different kinds of coal, but still more according to the temperature at which the decomposition is effected. Experience has shown that the quantity of oil or tar, as well as that of coke, is greater when the temperature is low; while these products are formed in less proportion when the temperature is high. The quantity of gas, on the contrary, is greater at a high temperature than at a low temperature, that is to say, the more gas is obtained the less tar is produced. It is evident, therefore, that during the operation of gas-making a suitable temperature must be maintained. If it is too low, a large quantity of tar is produced and little gas.

Hence it is evident, as I have before remarked, that though attention was directed chiefly to the production of gas, the other product of destructive distillation—viz., the oil—was not forgotten, although it was not the desired object of the manufacture. And though gas had become the chief object of attention as a source of light, the production of oil, by destructive distillation, was not abandoned. So late as 1819 Waterloo Bridge was lighted with coal oil, and in the same year M. de Saussure published an account of a method of purifying hydrocarbon oil, obtained by the destructive distillation of a bituminous mineral found at Travers, in Neufchatel. That method is identical with those employed at the present time. But it was in 1833 that the first important impulse was given to this manufacture, by the investigations of Reichenbach into the chemical nature of the products obtained by the destructive distillation of organic substances. Up to that time the knowledge of the oily products of destructive distillation, in regard to their chemical nature and the means by which they might be purified, was very slender. It was known that different materials yielded different kinds of oily products, and that the quantity obtainable varied according to the nature of the material and the temperature of the distillation. Neumann described the oil first drawn in the distillation of organic substances by a fire slowly raised, as being commonly fluid and lighter than water, while that which followed at a higher heat was thick and heavy, and that forced out at last by the "greatest vehemence of fire assumed a pitchy substance." Accum described the tar from Newcastle coal as being specifically heavier than that produced by cannel coal; hence it sank in water, whereas the latter swam on the surface of water.

The tar obtained in gas-works required to be boiled down to give it a sufficient consistence, and render it fit for use in coating wood. When this process was conducted in close vessels, a portion of oil was obtained that was commonly known as oil of tar, and by carrying on the distillation still further, more oil was obtained and a residue of pitch. The crude tar gave about 25 per cent. of the first-named oil, and by further distillation about 47 per cent. of pitch. No great use was made of these oils, however, and, as is still the practice, gas tar was often boiled down in an open vessel without attempting to collect the oil.

While things stood thus, Reichenbach's first paper appeared. He referred to the oily products of destructive distillation as substances that had hitherto received but little attention from chemists, partly in consequence of their having been rare, and partly because those that were known possessed characters that prevented their being applied to practical purposes; thus, for instance, their use as illuminating materials was prevented by their copious production of smoke when burnt; by their too great inflammability or liability to alteration when exposed to the air, their offensive smell, and several similar obstacles. He therefore considered that a more complete investigation of these products was desirable, and the thorough practical spirit in which he undertook this investigation is apparent throughout his memoirs.

The first substance which he succeeded in obtaining from tar was paraffin. That substance is now too well-known to need a repetition of his description of it. I need only mention that he described it as existing in tar in considerable quantities. He pointed out that the application of tar as a material for greasing the axles of cart-wheels depended upon the presence of this substance in tar, and that it was likely to furnish an appropriate material for making candles. He showed, also, that its peculiar chemical stability, under the influence of powerful reagents, was such as to suggest various easy means of extracting it from tar on a manufacturing scale. He then adds:—"I have here spoken only of the tar of beechwood; however, it is not only in this, but also in the tar of pine-wood, that I have ascertained the existence of paraffin, and there is no doubt that it is produced by the distillation of all kinds of wood." Shortly afterwards he obtained paraffin by distilling animal substances. This led him to the conclusion that paraffin is a product of the carbonisation of all organic substances, and he then extended his investigation to mineral substances, principally coal. By distilling coal in an iron retort, "commencing the distillation at first with a gentle heat, then gradually raising it, till at last the bottom of the retort was made dull red hot, and then cherry red," he obtained an oil containing paraffin, and then pronounced paraffin to be a common product of all substances of organic origin.

(To be continued.)

JURY REPORT ON THE INTERNATIONAL EXHIBITION OF 1862.

Class 2.—Section A.

CHEMICAL MANUFACTURES.

By Dr. A. W. HOFMANN.*

(Continued from page 261.)

Purification of Sulphuric Acid.—Arsenic may be separated from sulphuric acid by boiling it with common salt. At Chessy, barium sulphide is used for this purpose. In the Hartz works, and those of Wagenmann and Seybel, near Vienna, sulphuretted hydrogen is used to precipitate the arsenic, and this plan has the further advantage of purifying the acid from oxides of nitrogen. This method was patented in England by Mr. Hunt.

M. Kuhlmann separates arsenic by passing the sulphurous acid from pyrites, through a small leaden chamber before leading it into the vitriol chamber, by which means the arsenious acid is deposited, together with the sulphuric acid produced, some iron peroxide mechanically suspended in the gas, selenium, and thallium. The introduction of long leaden flues between the pyrites burners and the vitriol chambers, adopted in England, appears to have been less successful.

Another advantage of the plan adopted by M. Kuhlmann is, that the wear of the vitriol chambers is not greater than when Sicilian sulphur is used, while manufacturers generally complain that in using pyrites the chambers do not last more than one-third as long.

However, all the methods for purifying sulphuric acid from arsenic involve difficulties, and Sicilian sulphur is still used for making acid free from arsenic.

Notwithstanding the enormous quantity of sulphur obtained from pyrites, the consumption of Sicilian sulphur has not decreased, but has increased, since 1853, nearly one-half, having been 97,268 tons in 1853, and 137,745 tons in 1860. This increase is partly due to the recent large consumption of sulphur for gunpowder and for the treatment of diseased vines, which has contributed, by raising the price of sulphur, to cause the use of pyrites for making sulphuric acid. There can be no doubt that many manufacturers would have recourse to Sicilian sulphur again if the price of it should fall.

Method of Burning Sulphur.—Whatever may be the source from which the sulphurous acid is obtained, it is necessary to limit the quantity of air introduced into the vitriol chambers, since an excess of air would cause loss of both nitrous and sulphurous acids.

Many plans have been proposed for regulating the supply of air for producing sulphurous acid and limiting it to just enough for producing sulphuric acid. The plan devised by Mr. Harrison Blair, of Bolton, is spoken favourably of. Its advantages consist in admitting of the production of a larger amount of acid than is the case when the chambers are supplied with sulphurous acid from generators of more intermittent action, and, since one furnace serves instead of ten or twenty, both capital and labour are economised.

In order to prevent volatilisation of sulphur by too great heat, M. Kuhlmann placed a cast-iron steam boiler over the sulphur furnaces, and in this way he also effected considerable economy of fuel. Experience showed that the metal was not acted upon by the sulphurous acid to any sensible extent, but, as the generation of steam was irregular, he abandoned this plan. Eventually he burnt sulphur in large semicylindrical cast-iron retorts, set in brickwork, and communicating through long pipes with the lead chambers. This arrangement, combined with the intermediate chamber, has been found very satisfactory.

In some factories the supply of air to the sulphur-burners is regulated by the escape of the gases from the vitriol chambers by means of a slide valve in a pipe communicating with a chimney.

M. Scheurer-Kestner effects the same result by means of an anemometer, through which the air passes to the burners.

In some Belgian factories, the plan proposed by M. Stas is adopted, but little more air is admitted to the burners than is sufficient for the combustion of the sulphur; and the oxygen necessary for converting the sulphurous acid into sulphuric acid is supplied by a separate pipe to the chambers.

Whatever plan may be adopted, the gas issuing from the chambers should not contain more than 2 or 3 per cent. of oxygen. By carefully regulating the working of the sulphur-burners, some manufacturers have succeeded in obtaining nearly the theoretical amount of 306 parts sulphuric acid (1·843) from 100 parts of sulphur. As a general rule, not more than 280 to 290 parts are obtained.

Methods of Burning Pyrites.—The regulation of the supply of air is still more important in burning pyrites, since a large quantity or oxygen is absorbed by the iron, and the amount of nitrogen mixed with the sulphurous acid is proportionately increased. Consequently the chambers require to be larger than when sulphur is used.

A further difficulty involved in the use of pyrites is its lesser combustibility as compared with sulphur. Though many arrangements of furnaces have been proposed, the ordinary furnace, on the principle of the lime-kiln, is still used, and it gives good results when the pyrites is in small pieces. Minerals containing 40 or 50 per cent. of sulphur rarely retain more than 2 or 3 per cent. when burnt in these kilns. But when the pyrites is in powder the loss of sulphur amounts to 8 or 10 per cent. In that case it is mixed with moist clay into balls, which are dried by the waste heat of the burners, and then burnt in the usual manner. Still, the last portions of the sulphur are given off so slowly that it is found most economical to stop the operation when the pyrites still retains about 4 per cent. Messrs. Imeary and Richardson have lately obtained a patent for a reverberatory furnace with a low vault, for the more complete combustion of pyrites in powder. The plan said to be the most successful, is that of Mr. Spence, of Manchester, which consists in gradually heating the sifted pyrites upon the floor of a furnace, and passing over it a current of air. It is said that in this way the whole of the sulphur may be obtained from powdered pyrites. One important advantage of this plan is the production of a considerable amount of sulphuric acid during the roasting, and the consequent saving of nitric acid in the chamber.

The pyrites used in the English factories was first

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* Abstracted from the Moniteur Scientifique.

obtained wholly from Ireland and Cornwall, and is now largely imported from Portugal, Spain, and Belgium. The Irish pyrites contains only 33 per cent. sulphur, while the foreign minerals contain from 42 to 50 per cent. Since the amount of sulphur varies considerably, the different samples of pyrites are always analysed, and it is also necessary to ascertain by analysis the amount of sulphur in the residues of the manufacture. For this purpose the method proposed by M. Pelouze is very appropriate,—melting the substance with sodium chloride, potash chlorate, and soda carbonate, and estimating the quantity of carbonate that remains undecomposed.

Extraction of Copper from Roasted Pyrites.—In the year 1850 Mr. Gossage showed that the copper, amounting to about one per cent. in Irish pyrites, could be extracted, and this is still more practicable in the case of Spanish pyrites, which contain about 3 per cent., and, after roasting, from 5 to 6 per cent. The extraction of copper is, however, rarely carried out by the sulphuric acid manufacturer. In England the copper is obtained in the dry way by successive meltings. In France the roasted mineral is exposed to the action of the air, the copper sulphate thus produced is extracted by water, and the metal precipitated by iron. More recently the copper has been extracted as chloride, by melting the roasted mineral with sodium chloride. The method patented by Mr. Henderson, is worked at Mostyn with the pyrites residues from Messrs. Muspratt's works, and works are being erected near Glasgow for treating the residues from Messrs. Tennant's works.

Application of the Sulphur obtained in Purifying Coal Gas.[†]—The method introduced by Mr. F. C. Hills for purifying coal gas from sulphur by passing it over a mixture of sawdust and hydrated ferric oxide. By exposing the iron sulphide, thus produced, to the air, it is oxidised, sulphur being separated, and hydrated ferric oxide reproduced. After this operation has been repeated several times, the sulphur will amount to about 40 per cent., and the material is then unfit for the purification of gas, but is used for producing sulphurous acid by roasting it in reverbatory furnaces, so as to present a large surface for oxidation. In 1859 the consumption of this material at Mr. Lawes' factory, at Barking Creek, was 737 tons, and in 1861 it was 2180 tons. This material is said to yield one and a fourth its weight of oil of vitriol.

Absorption of Nitrous Vapours by Gay-Lussac's Method.—By passing the gas discharged from the vitriol chambers through a column of coke, through which a shower of sulphuric acid falls against the current of gas, the nitrous vapours are absorbed, and can be again separated by diluting the acid and heating it. This plan was first adopted at the St. Gobain factory, and has since been very generally introduced. It is said to effect a saving of 50 per cent. of nitrate under ordinary circumstances, but it has been found difficult to absorb the vapours from the very dilute gas discharged from vitriol chambers worked with pyrites. Partly for this reason, and probably not less from the increased price of nitrate, it has been abandoned in many instances, although it is still continued in the works that are considered to be best conducted.

M. Kuhlmann recovers the nitrous acid from the discharged gas by passing it through a series of carboys, sometimes containing water, sometimes native baryta carbonate. He also passes the gas through coke columns against a shower of ammonia solution, and uses the ammonia salts thus obtained for manure.

Concentration of Sulphuric Acid.—For many purposes sulphuric acid is required of greater density than 1·750, as it is obtained by concentration in lead pans. In South Lancashire not less than 700 tons of concentrated acid is made weekly. The use of glass vessels for this purpose has been largely superseded by the use of platinum retorts. The enormous cost of these vessels is partly due to the unjust and unenlightened monopoly exercised by the Russian Government, and to the circumstance that the manufacture of platinum is in the hands of few persons.

Experience has shown that under the influence of boiling sulphuric acid, platinum is gradually acted upon,[‡] particularly in the presence of nitric or nitrous acids. To obviate these disadvantages, cast iron retorts, partly filled with sand or gypsum, have been tried, but not very generally. The proposal recently made by M. Keller, to evaporate by exhaustion, has not been more favourably received, though it would admit of vessels being used for the purpose that could not otherwise be used. This plan was proposed by M. Kuhlmann several years ago.

In America, Mr. Clough has obtained a patent for a method of concentration based on the fact that lead is not acted upon by sulphuric acid, even when concentrated, at the ordinary temperature. It consists in heating the surface of the acid in a leaden pan contained in a cast-iron case with a current of cold water between them. Mr. Shanks and others have for some time adopted similar plans, but they have not become general.

In 1850, Mr. Gossage, jun., obtained a patent for the concentration of sulphuric acid by means of a current of heated air passed through a column of flints in a lead cylinder, against a shower of the acid. He found that the concentration could be effected in this way at a much lower temperature than any other way, but unfortunately the acid was also evaporated with the water to a great extent.

In many factories, especially in Lancashire, platinum vessels are no longer used, and glass retorts of a larger size than were formerly used are being substituted in their place. These glass retorts are heated either by the bare fire or in a sand bath, and the temperature of the retort-house is kept very high, in order to protect the upper parts of the retorts from sudden cooling. The retorts are filled with hot acid, and the concentrated acid is drawn off by a syphon. In France platinum retorts are still used generally.

(To be continued.)

NOTICES OF BOOKS.

A Popular Treatise on Photography. Translated from the French of D. Van Monckhoven. By W. H. Thornthwaite. London: Virtue, Brothers.

This little manual will be found a safe and efficient guide for photographic amateurs. In a small compass, and with commendable simplicity, it describes all the various details of photographic manipulation, the methods of preparing the material required, and the conditions necessary for obtaining satisfactory results. The illustrations it contains are excellent, and are generally of such a nature as to be very useful to the learner.

The scientific principles involved in the construction and arrangement of lenses, cameras, the stereoscope, and the chemical operations belonging to photography, are briefly and well explained, partly in the body of the work, and partly with greater fulness in notes, so that almost any one of ordinary capacity, may not only acquire the art of producing photographs, but also a fair knowledge of the science of which it is an application. In this respect the book is an appropriate addition to the excellent series of manuals, treating of scientific applications, originally published by Mr. Weale, and which have become so popular on account of the sound useful information they afford at a very moderate cost.

[†] Taking the amount of sulphur in the coal used for gas-making at 1 per cent., the coal used annually for this purpose in London would contain more than 10,000 tons of sulphur.

[‡] It has been found that the quantity of platinum dissolved amounts to about $\frac{2}{1000000}$th, but when nitrogen oxides are present it may amount to more than four times as much. The effect upon new vessels is much weaker, sometimes barely half as much as in ordinary cases. M. Kestner has found that the alloy of platinum and iridium is very much less acted upon by boiling sulphuric acid than pure platinum.

Mr. Thornthwaite's translation should be in the hands of every one who wishes to be successful in the delightful and useful art of photography.

NOTICES OF PATENTS.

451. *Manufacture of Manganese, &c.* E. M. Stöhr, Manchester. (A communication.) Dated February 20, 1862. (Not proceeded with.)

This invention refers to the reduction of the metal manganese from its ores, and to the employment of the same in combination with tin, copper, zinc, nickel, iron, or other metals, in order to produce a variety of useful alloys. In effecting the reduction of the manganese the ore is finely pulverized, and mixed with charcoal and other carbonaceous substances, and the mixture then submitted to a very high temperature in clay crucibles. The properties of the metal are thus described:—"Very hard and tough, and does not oxidise by exposure to the air." The last of these qualities does not appertain to pure manganese, and the statement may be understood as indicating the formation of an alloy, or combination of that metal with iron, carbon, silicon, or some other element, contained in the ore, crucible, or fluxes.

471. *Manufacture of Sugar.* W. H. Ross, Liverpool. (A communication.) Dated February 22, 1862.

In the process of refining sugar the patentee employs sulphurous acid in combination with phosphate of ammonia, the latter salt aiding in the separation of lime and magnesia from the crude syrup.

495. *Gas for Lighting and Heating.* L. Davis, Gloucester-gardens, Hyde-park, and F. M. Parkes, Marylebone-road, London. Dated February 24, 1862.

The patentees claim the production of lighting gas from rock oil or petroleum, the manufacture being conducted as follows:—Vertical iron retorts are used, wherein the oil is subjected to heat, the gas and vapours first evolved being conducted a second time through the furnace, and thence through a condenser and purifiers. The gas so obtained may be stored and distributed in the same manner as ordinary coal gas.

512. *A New Composition for the Manufacture of Bread.* C. Kingsford, Fenchurch Street, London. Dated February 25, 1862.

According to the invention, the use of yeast, or other fermenting material, in the manufacture of dough is rendered unnecessary. The composition consists of 196 lbs. of flour intimately mixed by grinding with 27 ounces of bicarbonate of soda, 25½ ozs. of tartaric acid, and 14 ozs. of common salt; and for the purpose of making dough it is only necessary to mix this with the usual proportion of water or milk, when the carbonic acid gas liberated will distend and lighten the mass.

This composition appears to be similar in character to "Jones's Patent Flour," which was introduced many years ago; and the ingredients the same precisely as are contained in the ordinary baking powders, but with a larger proportion of flour.

521. *Colouring or Dyeing of Horsehair, Tresses, Hats, or Ornaments.* J. Dothée, Paris. Dated February 26, 1862. (Not proceeded with.)

The inventor employs in the colouring or dyeing of horsehair, &c., alcoholic solutions of vegetable colouring matters, or mineral pigments suspended in collodion, or any combination of these, with the object of avoiding the distortion and harshness consequent upon the use of aqueous dyes and slow-drying solutions.

Notices to Proceed.

106. Charles Henry Townsend and James Young, Bristol, "Improved compositions for preventing incrustation and corrosion in steam boilers and condensers." Petitions recorded January 13, 1863.

171. Henri Adrien Bonneville, Rue du Mont Thabor, Paris, "Improvements in colouring, bronzing, and preserving iron and steel." A communication from Claude Joseph Thirault, Rue du Mont Thabor, Paris. Petition recorded January 20, 1863.

192. Heinrich Caro and John Dale, Manchester, "Improvements in obtaining colouring matters, part of which improvements is also applicable to dyeing and printing."

271. Charles Hanson Greville Williams, Rumford Street, Glasgow, N.B., "Improvements in the manufacture of red colouring matters."

CORRESPONDENCE.

Carbonaceous Minerals, &c.

To the Editor of the Chemical News.

Sir,—Your lively correspondent, "Ein Untersucher," has favoured me with a postscript to his letter in the Chemical News of last week, in which he refers to some remarks made by me upon the characteristics of coal as compared with analogous minerals, on the occasion when Professor Ansted read a paper at the Society of Arts on "Combustible Minerals." I am unfortunately under the necessity of confessing that I am quite incapable of understanding more than one sentence of that postscript, viz., "Whatever he means, it is certainly wrong," and it seems to me that "Untersucher" labours under a similar disability in regard to the remarks which he criticises so freely. The sentence I have quoted admits, and the general tenor of the postscript indicates, that such is the case. As I may infer from the signature assumed by your correspondent, that he is a foreigner, it would be unreasonable to complain that his language is not English; but, since the office of criticism, whether compulsory or self-imposed, involves the necessity of understanding the subject of the criticism, I would suggest to your correspondent that this preliminary step would be of much more service to him, than his *ex cathedrâ* judgment, quoted above, can be to any one else.

I should have been glad to assist "Untersucher" in arriving at a perception of the purport of my remarks, as one should always be willing to assist a foreigner, even though this operation might have been as arduous as getting a Scotchman to comprehend a joke; but it would evidently be quite a work of supererogation to make the attempt, since it is prophetically decided that whatever I mean is certainly wrong.

Having no share in the paternity of the terms Torbanite and Bathvillite, of the statement as to the original source of naphtha, or of the distinction between Boghead mineral and cannel coal, to which the substance of your correspondent's letter refers, I must leave it to Mr. Williams and Professor Ansted to maintain or repudiate their legitimacy.

I am, &c. B. H. Paul.

8, Gray's Inn Square.

MISCELLANEOUS.

The Medical Council and Chemists and Druggists.—We give our pharmaceutical readers notice at once that the Medical Council have designs on their business and purses. We shall return to the subject in a very short time, and explain particularly what is intended; but in the meantime it behoves all who are interested to be on the alert. Now is the time when the strength of the United Society and all associations of chemists and druggists will be severely tested, and we hope no rivalries will stand in the way of an active and hearty co-operation.

Silkworms Fed upon Oak-leaves.—An interesting communication from M. Guerin-Menneville, on "Silk Culture," was read at the last meeting of the French Academy. This gentleman has succeeded in habituating silkworms, hatched from Japanese eggs (*B. yama-mai*), to feed on oak-leaves, and his paper was accompanied by some cocoons produced by worms so fed. He expressed hopes that the discovery might lead to the extension of silk culture in France, and we may add that it affords some hope that it might be profitably carried on in England.

Leeds Chemists' Association.—A special meeting of the members was held at their Library, 5, Cookridge Street, on Monday evening, the 8th instant, the president, Mr. Harvey, in the chair. The object of the meeting was to consider two bills now before the House of Commons, and to express the opinions of the members upon them. The following petition was offered for adoption:—"That a bill now before your honourable House, entitled 'The Poisoned Grain Prohibition Act,' will interfere with the trade of your petitioners, and be likely to expose them to vexatious prosecutions. That, in the belief of your petitioners, no sufficient grounds exist for the provisions of this bill, which entirely prohibits, under heavy pecuniary penalties, the use of arsenic for the preparation of seed-wheat, and of other poisons which experience has shown to be necessary for the protection of agriculture. That the clause in the bill permitting the use of poisons to destroy vermin is so framed as to be still to a large extent prohibitive. That a bill interfering with trade, and making illegal means heretofore deemed necessary for agricultural and other useful objects, ought not to pass, except on grounds of evident and urgent necessity, and as your petitioners believe no facts have been or can be stated to show this necessity, they beseech your honourable House not to pass the said bill into a law." The petition was moved by Mr. Smeeton, seconded by Mr. Haigh, and adopted after a full discussion, in which Messrs. Stead, Horsfield, Yewdall, and Rimmington (Bradford) took part, and in which the loosely-drawn nature of the bill was severely commented upon, since it neither defined the poisons which it prohibited, nor the "vermin" against which they were allowed to be used. The Association next considered the following draft of a petition:—"That your petitioners pray that the bill now before your honourable House for decimalising weights and measures may be passed into a law. That further, should such bill not be passed in its entirety, your petitioners pray that its provisions may become law so far as relates to the compounding of medicines and all the operations of pharmacy." Moved by Mr. Thompson, seconded by Mr. Brown, and carried. It was also resolved—"That this Association would respectfully urge upon the General Medical Council that the present opportunity of introducing metric weights and measures into the forthcoming National Pharmacopœia should be embraced." Moved by Mr. Reynolds, seconded by Mr. R. M. Atkinson. The course adopted by the Association with regard to the Decimal Weights and Measures Bill appears to indicate the direction of a judicious policy in connection with this subject. Much unthinking opposition will doubtless be raised to the general adoption of the measure, and in order that a preliminary trial of the system may be made, it would be an immense advantage that a special purpose like the compounding of medicines, requiring definiteness and accuracy, should be selected. The decimal system has long been adopted for the purposes of science, for which its accuracy and facility peculiarly fit it.

Effects of Tobacco on the Mental Faculties.—In reference to the question of the influence of smoking on the mental faculties, Dr. Richardson, in the *Social Science Review*, says tobacco, like all agents of its class, has the property of checking the oxidation of the body, and thus of diminishing waste. If mental labour is commenced when the system is well sustained, and the supply in excess of the waste, indulgence in smoking does produce in most persons a heavy, dull condition, which is difficult to throw off, because it stops the processes of assimilation and destruction. But if mental labour be continued until the wasting of the corporeal power is greater than the supply, then the resort to tobacco gives a feeling of relief; it checks the rapid waste that is going on, and enables the mind to bear up longer in the performance of its task. Many men who commence a day of physical or mental work on a good breakfast and tobacco, find that they go through their labours with much less alacrity than other men who are not smokers; while the majority of smokers feel that after a day's labour the resort to a pipe, if the practice is moderately carried out, produces temporary relief from exhaustion. He also adduces the well-known fact that many persons of great energy and industry cannot sleep owing to the actual severity of mental or bodily effort to which they have subjected themselves. In this condition there can be no doubt that tobacco produces a soothing effect, causing mental rest. Dr. Richardson does not advocate the necessity of tobacco as a requirement of the natural life. He believes that in this day we are not living naturally; we have run into the extreme of industry; have carried our exertions to the borders of insanity; and so it is to be admitted that to the natural man such adventitious aids as tobacco are unnecessary. He condemns the use of tobacco until the body is fully developed; and states that the indulgence in it by our children and youths is degrading the national intellect, and establishing a race which will transmit its own degradation to future generations.

Adulteration of German Yeast with Pipe-clay.—At the meeting of the Commissioners of Sewers on Tuesday last, Dr. Letheby reported that he had met with some samples of German yeast adulterated with from one-third to half its weight of pipe-clay. The Doctor pointed out how the presence of this latter substance in bread might mislead a baker, and occasion a false charge against him. The fact deserves the notice of chemists.

SCIENTIFIC SOCIETIES.

MEETINGS FOR THE ENSUING WEEK.

June 23. Tuesday.

Medical and Chirurgical—53, Berners Street. 8.30 p.m.
Zoological—11, Hanover Square. 9 p.m.

24. *Wednesday.*

Society of Arts—John Street, Adelphi. 4 p.m. *Anniversary.*

Royal Society of Literature—4, St. Martin's Place. 4.30 p.m.

25. *Thursday.*

Royal Society Club—St. James's Hall. 6 p.m. *Anniversary.*

27. *Saturday.*

Royal Botanic—Inner Circle, Regent's Park. 3.45 p.m.

ANSWERS TO CORRESPONDENTS.

Vol. VI. of the Chemical News, containing a copious Index, is now ready, price 10s. 8d., by post, 11s. 2d., handsomely bound in cloth, gold-lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. 6d. if sent to our Office, or, if accompanied by a cloth case, for 1s. Vols. I. and II. are out of print. All the others are kept in stock. Vol. VII. commenced on January 3, 1863, and will be complete in 26 numbers.

R. R.—Sulphide of carbon is the same thing as bisulphuret of carbon.

J. G. C.—Specimens of thallium, and many of its salts, can be obtained at a reasonable price from Messrs. Hopkin and Williams, New Cavendish Street, W.

THE CHEMICAL NEWS

VOL. VII. No. 186.—June 27, 1863.

THE PROPOSED NEW MEDICAL ACT.

WE do not suppose that any of our readers have paid much attention to medical politics. The subject is not an inviting one, nor is the study of it in any sense profitable, and we should owe an apology for introducing it now were it not that the interests of a large section of our readers are likely to be placed in some danger. We mentioned last week that the Medical Council appointed under the Act of 1858 had designs on their business and purses; and we now give such extracts from a Bill the Council hope some day to get introduced to Parliament as relate to *Chemists and Druggists*, reserving longer comment for a future time.

The preamble of the Act of 1858 was as follows:—

"Whereas it is expedient that persons requiring Medical aid should be enabled to distinguish qualified from unqualified Practitioners. Be it therefore enacted," &c.

The preamble of new Act runs thus:—

"Whereas it is expedient that persons requiring Medical aid should be enabled to distinguish qualified from unqualified Practitioners; and whereas it is moreover necessary for the safety and protection of the public towards securing adequately educated Practitioners in the several departments of Medicine, Surgery, *and Pharmacy;* be it therefore enacted," &c.

For Section XX. of the present Act the Council propose to substitute a Section to the following effect:—

"It shall be lawful for the General Council to lay down such regulations respecting the education and examination of Practitioners in Medicine, Surgery, *and Pharmacy* as may appear to them fitted to insure adequate knowledge and skill in the several departments of the Profession; and the said General Council shall then submit said regulations to Her Majesty's most Honourable Privy Council; and the said regulations, if sanctioned by the said Privy Council, shall then be obligatory upon all Universities, Colleges, and other bodies enumerated in Schedule (A) to this Act."

Section XXXI. of the proposed Act stands as follows, and we request our readers to notice the conjunctions italicised:—

"Every person registered under this Act shall be entitled, according to his qualification or qualifications, to practise Medicine, or Surgery, *or* Pharmacy; or Medicine, and Surgery, *and* Pharmacy, as the case may be," &c.

The present Act ends with the following Section:—

"*Nothing in this Act contained shall extend or be construed to extend to prejudice, or in any way to affect, the lawful occupation, trade, or business of Chemists and Druggists.*"

In the new Act it is intended to

"Omit the words 'Chemists and Druggists.'"

An entirely new clause is then added in the new Act, which runs as follows:—

"It shall not be lawful for any person to keep open shop for the compounding of Physicians' and Surgeons' prescriptions, unless he be a Licentiate of the Apothecaries' Hall of England or Ireland, or shall have received a certificate of competency to compound medicine from either of the above bodies, or from the Pharmaceutical Society, or from some other body duly authorised in England, Ireland, or Scotland, by the General Medical Council, to institute the necessary examination, and to grant such certificate, and at such rate of fee as the General Medical Council, with the approval of the Privy Council, may sanction; and any person keeping open shop for the compounding of medicine, unless qualified as aforesaid, shall, upon a summary conviction for any such offence, before any Justice of the Peace, pay a sum not exceeding 20*l.*; and, for the better protection of the public, and to ensure the carrying out of the provisions as aforesaid, it is hereby enacted that the Medical Council may appoint from time to time one inspector for England, one for Ireland, and one for Scotland, whose duties it shall be to inspect, as often as may be required, all shops where medicines are compounded, and to carry into effect the provisions of this Act in regard to such shops; and that such inspectors be paid such salaries out of the Consolidated Fund as the General Council, with the approval of the Lords Commissioners of Her Majesty's Treasury, may from time to time determine."

On this clause we shall have a good deal to say on a future occasion. We shall now merely point out that with characteristic coolness, the authors of this Bill make no provision for the registration of such Chemists and Druggists actually in business as are not members of the Pharmaceutical Society. It therefore proposes to take away the dispensing business from the bulk of the trade. The Act concludes with a clause to the following effect:—

"No patent quack or other medicine shall be sold unless a sworn certificate of its composition be lodged with the Registrar of the General Council, and a copy thereof be open for inspection in the shop or place in which such medicine is sold; and any person or proprietor of a shop selling any secret remedy shall, on summary conviction, for each such offence be liable to a penalty not exceeding 20*l.*"

The above will show what are the designs to which we alluded. We have little fear that the Bill, as it stands, will ever become law; but it may be necessary to take active steps to oppose it, and we recommend such of our readers as are interested to effect at once a hearty combination for the purpose.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On Some Curious Properties of Camphor, and its Use in Detecting the Presence of Grease, by JOHN LIGHTFOOT, *Chemist at the Broad Oak Print Works, Accrington.*

THE detection of minute quantities of grease has hitherto been an unsolved problem, and was much wanted in analytical investigations; this want was more especially felt in an inquiry which came before me concerning the purity or contamination of certain waters used for dyeing purposes, and involving great damages in a recent case* before a court of law.

Independently of the interests at stake, and the serious nature of this litigation, it was a matter of special scientific interest to discover a qualitative test of sufficient delicacy and unerring precision, to enable us to detect atoms of greasy matters amounting only to fractions of millions.

It was my good fortune to discover such a test at the very time that I needed it most for continuing my investigations, and which enabled me to ascertain the presence of quantities of grease so minute that they escaped other means of analysis.

These researches having yielded, in my hands, results of a useful practical character, I feel it a duty I owe to the scientific public to give an account of them, in my own way, more especially since a very partial and garbled version of a few of the facts which I have discovered have been communicated to a French periodical, viz., the *Répertoire de Chimie Appliquée*, in their third livraison for the month of March, 1863, by Mr. O'Neil, of Manchester, who has been wanting in candour in not connecting my name with his communication to the correspondent of that monthly record, although he alludes in general terms to the trial which occasioned these researches, and where I was employed as a chemical witness.

The following statements may perhaps prove of use to those interested in the production or purchase of dyed and printed textile fabrics, which form so large a portion of our commercial transactions.

It will hardly be required here to define the meaning and application of the term "grease," for several reasons which will appear in the sequel.

The term may be vague, but it is really the most convenient general epithet to denote a numerous class of substances, including fixed and volatile matters. Thus it will be perceived that spermaceti, although a fatty substance possessing many of the properties of this class, is yet singularly deficient in one of their characteristics.

When small bits of pure camphor, cut and separated from a larger lump with a clean instrument, not permitting the contact of the fingers, are dropped into a glass containing very clean and clear water, they will begin immediately to rotate and move about more or less rapidly, and with strange and erratic energy. This experiment is not new, but it constitutes in itself one of the most interesting and mysterious exhibitions with which we can entertain a social circle.

Being convinced that minute and close observation of natural phenomena always rewards our curiosity and enlarges our perceptions, I carefully studied and repeated this hitherto neglected experiment, and I noticed some additional new and striking features. Thus I found that, if instead of using the torn or cut fragments from lumps of camphor, I detached with a clean needle point one or two of those fine crystals which attach themselves to the cork of the wide mouth phial where camphor is kept, and let them fall on clean water, they at once began to move about with wonderfully increased rapidity, darting away in various directions, as if shot from some minature engine of propulsion, or as if endowed with life and a will of their own; they feared the searching eyes and the magnifying lens of the observer, in their endeavours to find a hiding-place; each crystal quivering and rocking on the water with an apparent high degree of indignation at their forced contact with the humid medium. This fury gradually diminishes, and a regular dance begins with the various additional particles that may be introduced to the company; they select partners, to some of which they will seem to cling with pertinacity, whilst others will either remain indifferent, or, if attracted, will only stay a very short time in their embrace, detaching and wandering again in search of more congenial floating associates. This sight is pleasing from its variety, interesting from its elegance and the manifestation of an invisible power.

Careful observation demonstrates this power to be a force of reaction, as it is called, such as we see exemplified in the recoil of a gun, or in the flight of a rocket; and, indeed, in all those cases where the removal of a certain resistance at one point of a body under the influence of some internal force or pressure causes that portion of the pressure opposite to the freed point to manifest itself. In the experiment described above, the primary cause of motion is the emanation of a vapour from the volatile camphor; this vapour has a very low tension, the water upon which it floats being capable of dissolving and diffusing this vapour more readily in certain directions of the crystalline axes, thereby removes sufficient vapour-pressure at those points for the opposite side to drive about (by recoil) the nicely suspended particle.

Thus, if we place on the water several well-defined hexagonal plates and prisms of camphor, we will soon notice the mutual attraction of the particles to be stronger in the direction of the radii or angles of the hexagon. This is also accompanied by a decided preference for one of the axes, generally the longest in prismatic crystals; which plainly indicates a polarity in their mode of self-grouping. In certain positions, two crystals of camphor will attract each other, whilst in other situations there is a mutual repulsion.

It sometimes happens that two crystals of camphor may be thrown on the water, and not have any tendency to locomotion; when this is the case, a continual trembling or vibration may be noticed to possess the crystal. Now, it may be observed, that, whenever two such stationary vibrating crystals come in contact by attraction, immediately an eccentric irregular change of place takes place, as if the force agitating each, previous to the grouping, acted, at first, in a downward direction (which we have every reason to conclude is actually the case), and, the two combined down-currents associated with the deviation from the horizontal vibration, produced a new resultant force, inclined from the perpendicular. Thus accounting satisfactorily, on mechanical principles, for the assumption of fresh paths, from two comparatively quiescent particles.

It also happens that the rotation of two independent crystals may be reversed, by grouping with one another, and again modified by a third adjunct, and so on for many other combinations of particles.

(To be continued.)

TECHNICAL CHEMISTRY.

The Chemistry of Agriculture.

(*Continued from page 294.*)

The experiments carried on during the last twenty years by Mr. Lawes and Dr. Gilbert, have led to results that are, in many important particulars, directly opposed to the theoretical views of Baron Liebig, and the general tendency of those results is to give the chemistry of

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* Steiner *v.* Pickup. Lancaster and Liverpool Assizes, 1862.

agriculture an aspect widely different from that indicated by Baron Liebig's theory, either in its present or previous phases.

The main point of difference, in regard to practical agriculture, consists in the views entertained as to which of the constituents of plant food are, under ordinary circumstances, most liable to become deficient, and which of them, consequently, require to be supplied from extraneous sources as artificial manure, or to be accumulated for the growth of particular crops, by systematic rotation, by fallow, and by feeding cattle to produce manure.

According to Baron Liebig's theory of plant nutrition, the main substance of plants—consisting of carbon, hydrogen, oxygen, and nitrogen—is regarded as originating from carbonic acid, water, and ammonia, furnished by the atmosphere, but the growth and full development of plants by the assimilation of these food materials is held to be dependent upon the presence of "certain mineral substances which are indestructible by fire, and remain as ashes after the incineration of plants." According to this theory, in its application to agricultural practice, the fertility of land depends upon the presence in it, of the mineral constituents of the plants to be grown; the degree of fertility being held to be proportionate to the amount of those mineral constituents in the land, while the maintenance or increase of its fertility is held to depend on the restoration of the mineral constituents removed by crops or the increase of their amount in the land.

The influence of manure was ascribed to the mineral substances it contained, and which originated from the plants consumed as animal food; but since the export of produce from land was attended with a permanent abstraction of mineral constituents, it was considered that, in this way, land gradually became impoverished, and that it would eventually become incapable of yielding crops.

One of the first fruits of the practical application of this theory was the invention* of artificial, manures to be applied for the purpose of preventing land from being thus impoverished, and also of augmenting its fertility, by giving it a greater power of determining the assimilation of atmospheric food by plants. The preparation of these manures was to be regulated according to the composition of the ashes of the plants to be grown, and the quantity applied to land was to be regulated by the quantity of ash constituents removed from it by the previous crop. The virtues of these patent manures, and the principles on which their manufacture was based, were fully expounded in a pamphlet † published in England, but the results obtained by their application were not satisfactory. This first attempt to apply the theory to agriculture was a decided failure. The ash theory of manures, derived chiefly from the consideration of the circumstances of plant nutrition in a natural state, was found to be insufficient to explain the ordinary routine of agriculture, in which the conditions of plant nutrition are essentially artificial. Analogy, that able but deceptive guide, failed to furnish a true theory of agriculture, by the application to it of abstract scientific principles.

The experimental investigation of this subject by Mr. Lawes and Dr. Gilbert, was commenced with a recognition of the artificial nature of agriculture. Its object was to obtain data that would serve as evidence explanatory of the established routine of agriculture. The action of manures was one of the first questions studied. Having observed that, by growing wheat continuously on the same land without manure, the crop was proportionate to the climatic conditions of the year, that it was largest in the year when the atmospheric influence was greatest, but was in no case a full crop, it was endeavoured to ascertain whether the deficiency was due to a want of available minerals in the soil, or to the want of a sufficient supply of ammonia. Wheat was grown on several plots of land, manured with different mixtures of mineral manures; one plot was left unmanured, another plot was manured with the ashes of farmyard dung, and another with an equivalent quantity of farmyard dung. In all the plots where mineral manures alone were employed, the crop was little more than that on the unmanured land, while on the plot manured with farmyard dung it was nearly half as much more than on that of the plot manured with only the ashes of farmyard dung, which was just the same as that on the unmanured land. In other plots of the same land, manured with minerals and ammoniacal salts, the crop was as large as on the plot manured with farmyard dung. Liebig's wheat manure alone, gave a crop but little larger than the unmanured land, while a plot manured with Liebig's wheat manure and ammonia, gave a crop nearly half as much more than that of the plot manured with Liebig's wheat manure alone.

It was evident from these results that, whatever the amount of ash constituents in the soil, or supplied to it by manure, they are not effective in augmenting the crop, unless ammonia be supplied at the same time. They show that the supply of ammonia from the atmosphere is not sufficient to produce a full crop, and that the mineral constituents were rendered available and effective in producing a larger crop only in those plots where ammonia was supplied as manure, either in farmyard dung or otherwise.

It is not surprising, therefore, that Mr. Pusey should have regarded these important results as completely establishing the opinion he had already expressed as to the entire failure of the mineral theory as a guide to the use of manures in practical farming. Nor is it any more surprising that Mr. Lawes and Dr. Gilbert should have deduced from these results the conclusion that, in practical agriculture, the supply of ammonia is especially important, since the produce is more in proportion to the supply of ammonia than to the supply of ash constituents, and since the ash constituents, however abundant in land, are not effective unless there is also abundance of available nitrogen in the soil—a conclusion directly opposed to Baron Liebig's mineral theory, according to which the crop is held to be directly proportionate to the ash constituents in the soil.

But Baron Liebig ridicules these results, and declares the experiments to be absurd. He maintains that the increased crop obtained by applying ammonia to a plot of land—that had the previous year been manured with superphosphate of lime and potash, and yielded then only the natural produce of unmanured land—proves nothing, and he ignores the fact that while the application of mineral manures on this plot, did not produce any effect beyond that on the unmanured land—the application of ammonia in the following year at once had the result of rendering those minerals effective, and of increasing the crop. He endeavours to make it appear that Mr. Lawes and Dr. Gilbert regard ammonia as being alone sufficient to produce wheat crops, and that

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* Specification, 10,616. October 4, 1845.

† "An Address to the Agriculturists of Great Britain, explaining the principles and use of his Artificial Manures." By Professor Justus Liebig. Liverpool, 1845.

they intend the application of ammonia—as carried out in their experiments, made for the purpose of investigation—to be adopted in agricultural practice exactly as in those experiments, and he then enters into a calculation to show that the quantity of ammonia obtainable in all Europe would be insufficient to supply a pound an acre to the land under cultivation. In this way he endeavours to prove that his theory is in no way affected by the results obtained by Mr. Lawes and Dr. Gilbert, that it is even more firmly established than ever.

But it has been most expressly pointed out by Mr. Lawes and Dr. Gilbert that they do not maintain that the mineral constituents can be dispensed with, but simply that, in the ordinary course of agriculture, they are abundant in relation to other constituents, and that, to obtain a full crop of corn, a liberal supply of available nitrogen must be provided, and that, notwithstanding the abundance of mineral constituents, they will not be effective without it. Baron Liebig's assumption that it is intended that this supply of available nitrogen should be effected by the direct application of ammonia is entirely gratuitous, and is quite inconsistent with the description given by Mr. Lawes and Dr. Gilbert of the means by which this supply is to be ensured. They say that besides being expensive, ammonia cannot be procured in the market in any large quantities, but by cultivating turnips and the leguminous plants, and by feeding cattle with these crops, and with imported food, a large amount of ammonia is accumulated upon the farm, for the growth of corn crops, which more especially require it; and that in this sense, rotation of crops may be considered as an economical means of obtaining ammonia. In spite of this, Baron Liebig describes the difference between his opinion and that of Mr. Lawes and Dr. Gilbert, as to the importance of ammonia, to consist in their recommending the purchase of ammonia as the only means of increasing the produce of corn, while his theory teaches how to attain that end without purchasing ammonia. He comments at great length upon the fact pointed out by Mr. Lawes and Dr. Gilbert, that the nitrogen obtained in the corn crop, is in no case as much as the nitrogen supplied to the soil as manure, and without attempting now, to question this fact, he argues that it is a proof that their opinion of the importance of ammonia for the growth of corn can only be the result of prejudice, or of an interest in the sale of ammonia salts.

With wonderful boldness, and in total disregard of the enormous consumption of guano, Baron Liebig declares that ammonia salts are not used by any farmers in any country, still repeating his assertion that the results of Mr. Lawes' and Dr. Gilbert's experiments fully confirm his theory, and declaring that it is impossible to say he is in error. He says, with admirable *naiveté*, that they should have followed his course, in order to have thought as he thinks, and to have come to the conclusions he has come to; but, though this is possibly true, it is not the less a fortunate circumstance that they have, as Baron Liebig says, followed their own course without heeding his theory, and although he considers they have consequently involved the clearest facts in an inextricable embroglio of misunderstanding, rendering them obscure and unintelligible, the real interests of agriculture are not likely to suffer from the knowledge of such facts as the Rothamsted experiments have established, however fatal they may be to theories.

The Rothamsted experiments prove that the mineral theory of manures, like so many other brilliant theories of Baron Liebig's, will not bear the test of practical application. Like the theories of animal nutrition, of fermentation and decay, its deficiencies have been hidden only by its attractive form, and the effective plausibility of its author's mode of exposition. The mutations that theory has undergone, since its first promulgation in 1840, are in themselves sufficient proof that, as a theory of agriculture, it was a premature growth, while a science of agriculture was wanting. It is but lately that materials have been accumulated for such a science, and they have been chiefly furnished by the experimental results of Boussingault, Lawes, and Gilbert. The attempt made by Baron Liebig to save his theory by maintaining that he included ammonia salts among the "mineral" constituents of land, and that the sources of nitrogen in plants, were comprised among the mineral constituents of plants—to be ascertained by analysis of their ashes, and supplied as manure—is a position so startling and inconceivable, that it is difficult to find words to describe its total inconsistency with truth. It must be remembered also, that there is a wide difference between what Baron Liebig now calls his "mineral theory,"[‡] namely, the theory that all plant food consists of inorganic substances, and the application which he made of that theory to agricultural practice. It is not in consequence of misunderstanding the application of his theory, that Baron Liebig is represented as having held that the fertility of land is in direct proportion to the amount of the mineral constituents of the ash of plants to be grown upon it, and that the true practice of manuring was to supply those constituents only, leaving the atmosphere to supply ammonia. This is no misunderstanding or misinterpretation of Mr. Pusey's or of Mr. Lawes' and Dr. Gilbert's, but the distinct expression of the application of his theory as given by himself in the "Chemistry Applied to Agriculture;" in the "Address to Farmers," and in the specification of his patent. In every illustration he gives of that application, it is potash, phosphoric acid, lime, silica, of which he speaks as being permanently abstracted from the soil, and requiring to be replaced by manure.

The influence of manure was uniformly ascribed to its ash constituents; the practice of rotation was explained on the assumption that the alternate crops required, in one case, potash, in another, lime, in another, silica; the benefit of fallow was ascribed to the disintegration of alkaline minerals in the soil; in no case was ammonia or nitrogen-yielding manure mentioned, except in direct antithesis to the mineral or ash constituents of manure. In no case is the importance of available nitrogen in the soil, as the essential condition of the efficacy of the mineral constituents of plant ashes, mentioned. In no case is it shown that rotation or fallow are means of providing a supply of available nitrogen for the growth of corn. This is the case not only in the earlier editions, but also in the later publications of Baron Liebig on this subject. Nitrogen is everywhere spoken of as one among the atmospheric materials of plant food which, like carbonic acid, was supplied from that source, in amount sufficient for all purposes of agriculture, and without the necessity for any special provisions for its being supplied in greater proportion to particular crops. It was even pointed out, as "of great importance for agriculture, to know with certainty that the supply of ammonia is unnecessary for most of our cultivated plants, and that it may be even superfluous, if the soil contains a sufficient supply of the mineral food of plants, when the ammonia required for their development will be furnished by the

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[‡] See Chemical News, ante, p. 258.

atmosphere."§ Agricultural practice was represented to be "based upon the fact that air and rain supplied plants and land *annually with more nitrogen than they required for their fullest development.*"

Whatever exhaustion of the soil he held to result from export of produce, related to these ash constituents, and, so far from his considering that there was any exhaustion as to nitrogen-yielding substances, he taught that there was naturally an accumulation of those substances in the ordinary course of farming.‖ The exception taken to Mr. Lawes' definition of manures¶ is quite unfair, since that definition is really adopted from Baron Liebig himself; it is used only provisionally, with a decided protest of dissent, and is not, as Baron Liebig says, put forward as a new theory in opposition to his.

Throughout the whole of Baron Liebig's remarks he endeavours to make it appear, not only that his theory is misunderstood, and that consequently the objections to its application are of no value, but also strives to make the experimental data on which those objections rest appear to be destitute of scientific import. He does not hesitate to speak of Mr. Lawes as a man who never looked into a chemical manual; who knows nothing of agriculture; and to represent the whole of the Rothamsted experiments as being merely undertaken in the interest of manure manufacture. On this latter point there may be some slight excuse for Baron Liebig's intolerance, since his own experience as a manure manufacturer cannot be very agreeable. But it would be difficult to find any excuse for the wholesale denunciations of personal enmity and interested motive, which he flings at every one who ventures to dissent from his theory. His mention of Mr. Pusey is strikingly inconsistent with the way in which Baron Liebig's influence on agriculture is spoken of by him in the report to the Royal Agricultural Society. Short of adopting his errors, nothing can be more courteous or considerate.

To add force to the insinuation that the Rothamsted experiments have not been conducted in a scientific manner, Baron Liebig altogether omits to recognise the fact that they have been conducted under the care of Dr. Gilbert, with the most ample regard to the requirements of modern scientific investigation. With equal unfairness he strives to make it appear that the results of those experiments support his theory instead of disproving it; and, consistent only in adhering to his favourite weapon of ridicule, he actually endeavours to explain the inefficacy of his manures upon the assumption that they were not sufficiently soluble, while admitting the falsity of the opinion that plants obtain nutriment only from solutions formed in the soil; and to maintain his original theory, that the atmosphere is an amply sufficient source of ammonia for all purposes of agriculture, by having recourse to the recent questionable observations of Schönbein.

NOTICES OF BOOKS.

A Dictionary of Chemistry and the Allied Branches of other Sciences. By Henry Watts, B.A., F.C.S. London: Longman. Second Notice.

The three numbers of the Dictionary that have appeared since the first number of the work was noticed in this Journal, in no respect fall short of the first in those characteristics which bid fair to render this work the standard work of reference on chemistry. Gmelin's "Handbook of Chemistry," published by the Cavendish Society, has long fulfilled this part but imperfectly, even as regards the portion devoted to mineral or inorganic chemistry, and the arrangement adopted in the portion treating of organic chemistry is not such as to be useful to the majority of those who require to refer to it for information. Mr. Watts' dictionary may therefore replace it as a book of reference, if the plan adopted in the numbers already published is carried out fully But it seems scarcely possible that this could be done within the limits that have been fixed for the extent of the work. The four numbers already published go no further than Carbon, and they contain nothing that could with justice have been omitted. Hence it seems scarcely possible that the remaining material can be condensed sufficiently to come within the compass of other twelve numbers, without the necessity of adopting an abbreviation that would be detrimental.

Among the articles contained in the late numbers, those on analysis constitute almost a treatise on this subject, in all its branches. The articles on atomic weights and atomic volumes furnish the clearest and best exposition that has yet been given of those aspects of chemical science that are now in a condition of active transformation. Throughout the whole of the articles the descriptions given of chemical substances and operations is clear, explicit, and intelligible, and the copious references to original memoirs will be of great assistance to those seeking for more detailed accounts of special investigations than can be given in a general work.

The great convenience of a dictionary arrangement of chemical subjects has already been referred to, and a very cursory examination of the parts of Mr. Watts' dictionary will be sufficient to show how much preferable it is to a systematic arrangement of the kind adopted in Gmelin's "Organic Chemistry."

CORRESPONDENCE.

Manufacture of Magnesium.

To the Editor of the Chemical News.

Sir,—A remark made in the Chemical News for June 20, 1863, at the close of the paper entitled "Manufacture of Magnesium" being such as to impugn my moral character, I am restrained from replying to the accompanying remarks, which question the worth or novelty of my invention. This question, although suffered for the reason stated to remain unanswered in the pages of your Journal, will be met in two courts,—one, that constituted of scientific men; the other, of practical men. Requesting that you will publish this note, in order that your readers may know the grounds on which I decline controversy with you, I am, &c. E. Sonstadt.

Loughborough, June 20.

[No reflection upon Mr. Sonstadt's moral character was either intended or implied in the remarks we felt it our duty to make respecting his patent, and we can only regret that Mr. Sonstadt should have so interpreted them. Our comment had reference simply to the merits of the invention as described in the specification, and we therefore hope that Mr. Sonstadt will not abstain from any defence of the novelty of his method of manufacturing magnesium, or from pointing out in what essential particulars it differs from the published methods adopted by Deville and others.—Ed. *C. N.*]

Pickeringite from Nova Scotia and Peru.

To the Editor of the Chemical News.

Sir,—Would you oblige me by correcting an error into which your reporter has fallen in giving an account of my analysis of pickeringite, in your Number for May 16, p. 233.

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§ "Chemistry Applied to Agriculture," Third Edition, p. 213.

‖ *Ibid*, p. 43.

¶ See Chemical News, ante, p. 293.

The mineral I analysed was not from Iquiqui, as there stated, but from Nova Scotia, where it occurs in slate, and it was found to agree in chemical composition and in crystalline form with the Peruvian mineral from Iquiqui, analysed by Dr. Hayer, of Boston, U.S. You will perceive that this is an important point in the history of a mineral shown to differ from true alums, among which, with some other native salts, it has hitherto been erroneously classed.—I am, &c. Henry How.

Windsor, Nova Scotia, June 11.

MISCELLANEOUS.

A New Claimant for the Discovery of Thallium.—In our Number for May 30, a few extracts were republished from the *Mining Journal*, in which the honour of the discovery of thallium was claimed for a Mr. Joseph Jones. Subsequent correspondence has at last elicited the following detailed account from Mr. Jones himself. An apology is almost due to the readers of the Chemical News for placing such a letter before them, but having published the commencement of the correspondence, simple justice to all parties concerned demands that its close should be given:—

" *The New Metal—Thallium.*

" Sir,—In treating of the new metal which I discovered in certain sulphide ores, in 1857, I will endeavour to point out its properties, its uses, the cost of producing it, and its value when produced, in order that not only your correspondents, but the readers generally of the *Mining Journal*, may have convincing proof that the new metal—thallium—was really first discovered by me.

" Beginning with the last, allow me to mention that the cost of getting it, compared with its quantity and value, is little. The requirements are:—1. A furnace with three heights, or three separate furnaces, crucible or reverberatory, as the case may be. 2. Their complements, and a few chemicals. 3. It will be more remunerative if sulphuric acid be made from sulphur, as the sulphuric acid now sold is manufactured from sulphur-stone, which will not answer the purpose so well. The investment will the more surely pay, as some gold, silver, and copper is also found in sulphur refuse. The refuse itself can be put to a variety of uses as an alloy (when refined): also sulphur may be used for coating baser metals; and the patent right, which I propose to secure, can extend over these points:—For the discovery of the metal and its uses, also for the discovery of the uses of the base of the metal generally.

" When I last experimented on my new metal, I computed its specific gravity to be nearly one and a-half times that of iron. Some of its best properties are tenacity, hardness, and non-liability to rust or oxidation. Sulphuric acid and nitric acid have no effect upon it; it is also proof against hydrochloric acid and sulphuretted hydrogen. The quantity of metals produced from some metals is equal to one-tenth; this includes thallium, silver, gold, and copper, and out of these metals about two-thirds is thallium. It must not be assumed strange that gold is in this refuse, for gold has recently been found in Italy in the vicinity of sulphur.

" Being able to resist the effects of sulphide of ammonia, my thallium would be useful in the manufacture of carbonate of ammonia (for linings), and make it healthier to mix in confectionery. The superior cheapness of thallium as compared with platinum, aluminium, &c., will find it a variety of uses in addition to what it is directly adapted for. Again, what appears to be a difficulty (its withstanding a great heat) is a superiority, and renders it particularly useful for the steps or bearings of machinery. Concomitant with this good quality is the fact that it expands and contracts very little; it is therefore applicable to preventing explosions of locomotive boilers and cannons, and this is done to a great extent by merely lining the interior with my thallium. Heat cannot (then) conduct. In case of a scarcity of silver, it will form with baser metals a good alloy for coin, and cheapen silver. An elaborate description of its properties and uses will be the work of a few years to come. Thallium would be useful for stills, or linings of stills, in chemical works, &c.

" Being at present absent from Bolton, I have no opportunity of sending you a piece of metal, but I am making arrangements with some chemical friends in Widnes to do so. I have, through the panic in the cotton manufacture, been prevented several years from bringing this completely before the public; but having some chemical secrets in my mind, I am determined yet to bring out the links in chemical science, and make chemistry one complete and consistent narrative.—Joseph Jones, Lyndhurst Street, Bolton."—*Mining Journal*, June 20, 1863.

Cure of Gangrene by Oxygen.—Additional information on the use of a bath of oxygen in arresting senile gangrene is given in the last number of the *Comptes-Rendus*, by M. Demarquay, who has found it successful in two cases.

Alkali Works Regulation Bill.—This bill has made considerable progress in the House of Commons. The condensation clause was carried without a division. Mr. Bouverie carried a clause limiting the action of the present bill to four years. An attempt was made by Messrs. Cobden and Bright to change the jurisdiction from the quarter sessions to the county courts, which will probably be successful, and with these changes we have no doubt the bill will pass.

Explosion of Turpenzine.—The article sold as a substitute for turpentine is one of the lighter petroleum oils, and its vapour is, as we hope most of our readers know, dangerously explosive. Ignorance of this fact caused a lamentable accident at Devonport a short time ago. A tradesman of that town went down into the cellar with a lighted candle to fetch some turpenzine, and within a minute afterwards a loud explosion was heard, and the place was in a flame. The house was burned down, and the unfortunate man was afterwards found in the cellar burnt to a cinder.

SCIENTIFIC SOCIETIES.

MEETINGS FOR THE ENSUING WEEK.

July 3. *Friday.*

Archæological Institute—26, Suffolk Street, Pall Mall. 4 p.m.

ANSWERS TO CORRESPONDENTS.

Vol. VI. of the Chemical News, containing a copious Index, is now ready, price 10s. 8d., by post, 11s. 2d., handsomely bound in cloth, gold-lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. 6d. if sent to our Office, or, if accompanied by a cloth case, for 1s. Vols. I. and II. are out of print. All the others are kept in stock. Vol. VII. commenced on January 3, 1863, and will be complete in 26 numbers.

⁂ In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

The present number completes the seventh volume of the Chemical News. In the ensuing volumes it will be our endeavour, while not neglecting pure science, to devote as much space as possible to the practical applications of chemistry to the arts, to manufacture, and to pharmacy, and so to increase the value of the Journal to all engaged in such pursuits. With this view it will be our aim to give the earliest information of all discoveries in the science, especially with regard to their practical applications, and so extend the interest to business as well as to scientific men.

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END OF VOLUME VII.

LONDON: BENJAMIN PARDON, PRINTER, PATERNOSTER-ROW.