TRANSACTIONS

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ON THE ANATOMICAL CHARACTERS OF THE SUBSTANCE "INDIAN SOAP."

By Miss M. Dawson, B.Sc. (Lond. and Wales).

(Read 3rd March, 1900).

ON THE STRUCTURE OF "INDIAN SOAP."

In December, 1898, a piece of the material used by the Indians of British Columbia as a substitute for soap, was sent to the Botanical Laboratory, Cambridge, by Prof. Anderson, of the Department of Agriculture, Victoria, B.C.

Enclosed with this was a report upon the material from Dr. Fletcher, who stated that Prof. Macoun had identified it as a Polyporus, allied to *P. betulinus*, which had become changed by its own mycelium into punk. Dr. Fletcher explained that the scroopy feeling upon rubbing the soap between the fingers is due to the presence of quantities of resin, also that it burns freely with a strong resinous odour and much black smoke, in a similar manner to birch bark, which accounts for its use by the children of the district as candles. With hot water, it scarcely produces a lather, but rubs up like clay, and leaves a chalk-like deposit on the hands after drying.

Some of this "Indian Soap" was handed to me last April by Prof. Marshall Ward for a more detailed study of its anatomy. A superficial examination suggests that the substance is either a dead fungus of the larger Polyporus type, or a mass of wood destroyed by a fungus.

It is whitish cream in colour, very friable, breaking into long, thin strands, which seem to be held together by fragments of dead matter. To the touch, it has a curious saponaceous feel, but as Dr. Fletcher remarked, does not produce any appreciable lather when rubbed up with water. If cut dry with a razor, the sections peel off like shavings, and even in very small pieces they are exceedingly impervious to cold water,—they will float on the surface for weeks. Hot water or alcohol, however, wets them readily.

If a small portion of the "soap" be teased out, it is found to consist of a meshwork of hyphæ, covered over with, and more or less bound together by irregular fragments of very varying size. With iodine, or Schultz's solution, the hyphæ stain a faint yellow, the fragments a deeper yellow. Iodine and sulphuric acid at first give a deep yellow colour, but then the sections gradually dissolve up. Phloroglucin or aniline chloride and hydrochloric acid give no trace of colour. The hyphæ stain readily with hæmatoxylin, Congo-red, carbol, fuchsin, aniline blue, etc., either in alcoholic solutions or in water solutions, if the sections have been previously treated with alcohol.

After soaking in alcohol, thin layers of the substance become semi-transparent, and show very clearly longitudinal strands, which in section show irregularly arranged circles, held together by darker portions of the material. Distributed over the "soap" occur deep brown areas, which, to the naked eye, appear like a stain. This colouration is very intense in some places, whilst in others it is scarcely noticeable. The deeper brown areas seem to mark successive layers in a direction at right angles to the longitudinal strands; there is, however, no regularity in the thickness of the layers, and here and there the dark areas cross each other. When examined microscopically, besides these longitudinal strands above referred to, the most striking character is the presence of a double set of hyphæ lying one above the other. Thus, as shown in Fig. 2, the substance is seen to consist of interwoven hyphæ, running in alternating strands longitudinally and transversely, with a more densely matted line separating these strands. The hyphæ arranged thus are colourless, very rarely branched, regular, somewhat thick walled and show but very few transverse divisions. Clamp connections occur here and there. (Fig. 12). Overlying these and very irregularly distributed are numerous dark brown hyphæ, showing frequent swellings and branchings and occasional transverse septa. (Figs. 5 and 11). To their presence undoubtedly the brown colour, mentioned above, is due, since though present throughout the material, they are very much more numerous in the more deeply "stained" areas. With this exception, it is impossible to discover any method in their distribution.

The longitudinal strands of hyphæ are especially clearly seen if a rather thick section is mounted in eau de javelle or potash, and examined under low power. The section gradually clears, owing to the almost complete solution of the coarsely granular substance, which adheres in fragments to all the hyphæ, and seems to be the cementing material which holds the whole structure together. Indeed, thin sections treated with potash are almost immediately disintegrated, breaking first along the boundary lines of the strands of hyphæ, and then the individual hyphæ are separated from one another completely.

Sections cut transversely to the direction of the longitudinal strands show circular areas of loosely interwoven hyphæ, with intermediate layers, consisting of a denser meshwork of hyphæ, running between these areas. As before, the overlying brown irregular hyphæ are visible all over the sections. (See Figs. 1 and 3).

No trace has been found of any remains of woody elements, though a very large number of sections have been carefully examined. Nor has it been possible to obtain any indication of the presence of lignin with any of the usual micro-chemical tests.

In connection with the large colourless hyphæ, one and only one group of spores has been found. They had obviously been considerably compressed and dried up, but were sufficiently clear to show a few still attached at intervals down some hyphæ. The spores are very small, apparently spherical or oval in the fresh, colourless, with slightly thickened walls. The evidence of their mode of formation is not very satisfactory, but they seem to be borne sessile upon the hyphæ in groups of two or more. (Fig. 4).

A second group of spores of another kind was found in connection with the dark brown hyphæ. Like them, they are deep brown in colour, with thick walls, mostly oval in form, and much larger than those described above. Though lying amidst the hyphæ, this group did not show any spores attached, consequently no information could be gained as to their method of formation, though there could be no doubt of their connection with this set of hyphæ. (Figs. 5 and 6). In Figs. 8 and 9 are shown very thin microtome sections, cut transversely and longitudinally as regards the strands of hyphæ.

Staining either with Congo-red or with hæmatoxylin after twenty-four hours treatment with iron alum, showed the existence of a third set of hyphæ, which had not been noticed before. These form a meshwork between the strands of larger hyphæ; they are extremely delicate and colourless, and in fewer numbers can be seen running amongst the other hyphæ, within the strands. This detailed structure was most satisfactorily seen by mounting the sections in glycerine. Dehydration with alcohol, and clearing with either clove oil or xylol, caused the hyphæ to collapse to such an extent that the appearance of the sections is greatly altered. An examination of hyphæ separated out by eau de javelle, under Zeiss Hom. Imm. $\frac{1}{12}$, confirmed the presence of these delicate colourless hyphæ amongst and upon the larger ones. (Fig. 10). This suggests that they are parasitic in nature.

As regards the function of the brown irregular hyphæ, their general characters and distribution suggested, at first, that they might represent a conducting system such as has been described by Istvanffi and Johan Olsen.* (Fig. 11).

This, however, is apparently not their function, for no treatment has been successful in decolourizing them. Even after fixing in alcohol or Rath's solution, embedding, dehydrating and clearing, they retain their brown or yellowish brown colour. They appear, too, to be present throughout the material, and though more numerous in some regions, they do not here exhibit any special characters. Beyond this, moreover, the occurrence of a group of spores, in connection with these hyphæ, makes it impossible for them to be conducting in function, but points rather to their being the hyphæ of a fungus parasitic upon the main fungus, which consists of the large, colourless hyphæ.

We come next to the consideration of the nature of the substance, which causes the saponaceous feel possessed by the material. It has already been suggested that this character is due to the presence of resin, and from all the results, which an examination of its behaviour has given, this seems to be the correct view.

It is insoluble in water but very slowly in strong alcohols, and more slowly in ether or chloroform, in each case resulting in a clear, golden brown liquid. Ether or chloroform does not precipitate it from the alcoholic solution.

* "Über die Milchsaft behälter bei den höheren Pilzen." Bot. cent., 1887.
Istvanffi, "Untersuchungen über die physiologische anatomie der Pilze, etc." Pringsh. Jahrb., xxix.

When these solvents are allowed to evaporate off, a whitish brown amorphous mass remains.

If small pieces or sections of the material be treated with eau de javelle or potash, a bright red colouration is produced, running along the boundary of the longitudinal strands and gradually diffusing over the substance. By degrees a red liquid oozes out, which turns brown in a few hours. (Fig. 7).

If these solutions be treated with acid, a heavy, brownish white flocculent precipitate is produced, probably the resinous acid.

Staining with dry sections of alkanna root and fifty per cent. alcohol gave an intense reddish pink colour, universally over the sections, but somewhat more intense along the edges of the strands. Alcoholic solutions of alkanna or sudan iii.* were obviously useless, owing to the extreme solubility of the substance in question in alcohol.

Some better results were obtained by using a concentrated ethereal solution of sudan. The sections, cut dry, were left in the stain for a few seconds only, then washed in water and mounted in glycerine.

These showed that the hyphæ themselves were quite unstained, whilst attached to them and scattered irregularly over them, were deeply stained fragments and drops ; in addition, along the meshwork of hyphæ dividing the strands of larger hyphæ, a pale pink stain was given. The dark brown hyphæ were still visible, retaining their usual colour. (Figs. 13 and 14). This confirms the view that the resinous substance is present in the innumerable fragments, adhering to all the hyphæ, and the presence of this substance explains the clearing of sections with clove oil, xylol, or potash, when these fragments are almost completely dissolved away.

A spectroscopic examination of an alcoholic solution of the resinous substance shows a complete absorption of the blue rays.

As regards the nature of this substance "Indian Soap," the general arrangement and character of the large colourless hyphæ seem to support Prof. Macoun's conclusion that it consists of a fructification of a Polyporus,—the longitudinal strands representing an incipient stage in the formation of the Polyporus tube. The spores found, however, were not in any definite position relative to these tubes, nor was their arrangement on the hyphæ in accordance with basidial formation. The

* This dye has been described as a test for resins, fats, etc., by Buscalioni. Un nuovo reattivo per l'istologia vegetale. See Bot. Centr., Nr. 22, 1899.

whole structure has obviously been much changed by the action of parasitic hyphæ, so that we may perhaps, with justice, conclude that it consists of some large fungus, probably of the Polyporus type, which has been destroyed by two parasitic fungi, probably also to be classed with the higher forms. As a result, degeneration of some of the interwoven hyphæ seems to have taken place, giving rise to a resinous substance, to whose presence the characteristic saponaceous feeling is due.

EXPLANATION OF PLATE.

FIG 1.—Section at right angles to strands of hyphæ. Treated with potash and mounted in glycerine. Obj. ⅔.

FIG. 2.—Section parallel to strands of hyphæ. Obj. ⅔.

FIG. 3.—Section as in Fig. 1 under 1 inch obj. (Reduced ½).

FIG. 4.—Group of spores in connection with the large, colourless hyphæ. Stained with carbol fuchsin. Oc. 4. Obj. Hom. Imm. $\frac{1}{12}$.

FIG. 5.—Brown hyphæ with group of spores. Oc. 4. Obj. Zeiss D.

FIG. 6.—Spores of the above group. Oc. 4. Obj. Zeiss F.

FIG. 7.—Small piece of "soap" treated with potash (5%). Shaded portions coloured deep red. (Macroscopic).

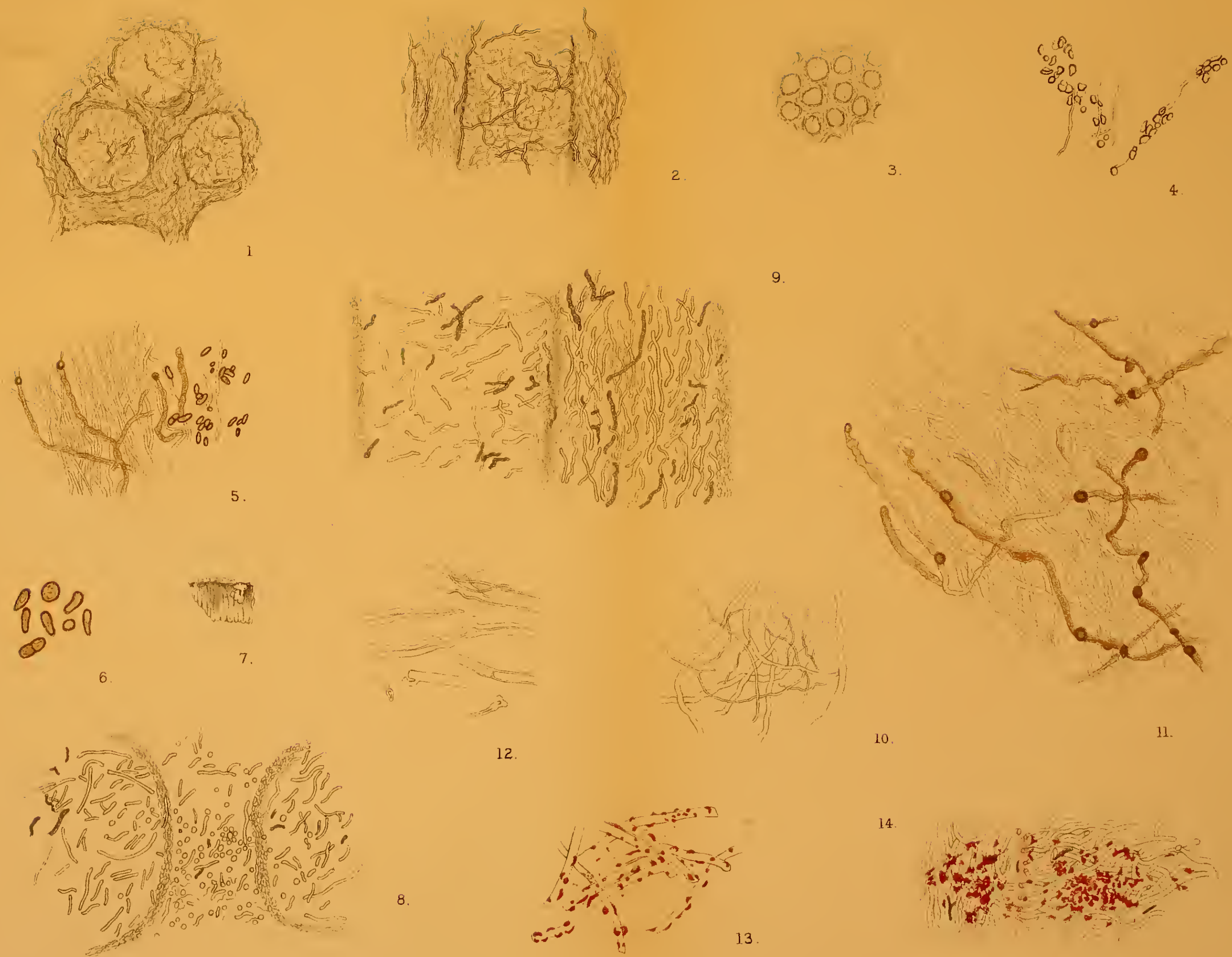
FIGS. 8 AND 9.—Microtome sections showing three systems of hyphæ. Iron alum (24 hours), and hæmatoxylin. Oc. 4. Obj. Zeiss D.

FIG. 10.—A few hyphæ, separated by the action of eau de javelle. Brown hyphæ were present, but are omitted from the figure, which shows the fine colourless hyphæ running amongst the larger colourless ones. Oc. 4. Obj. Zeiss D.

FIG. 11.—Portion of section, treated with potash, showing the arrangement of the brown overlying hyphæ, resembling that of a conducting system. Oc. 4. Obj. Zeiss D.

FIG. 12.—Portions of separated colourless hyphæ, treated with alcoholic potash. Oc. 4. Obj. Zeiss Hom. Imm. $\frac{1}{12}$.

FIGS. 13 AND 14.—Staining of resinous fragments by an ethereal solution of sudan iii. Oc. 4. Obj. Zeiss F.



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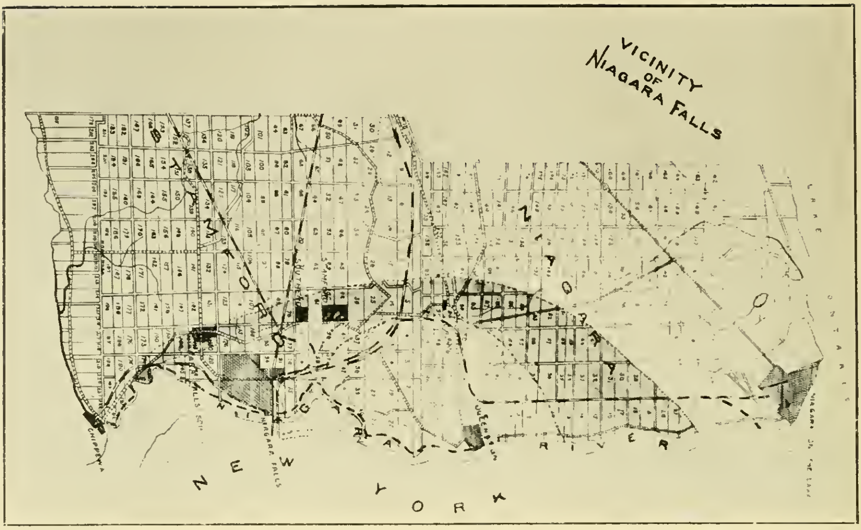

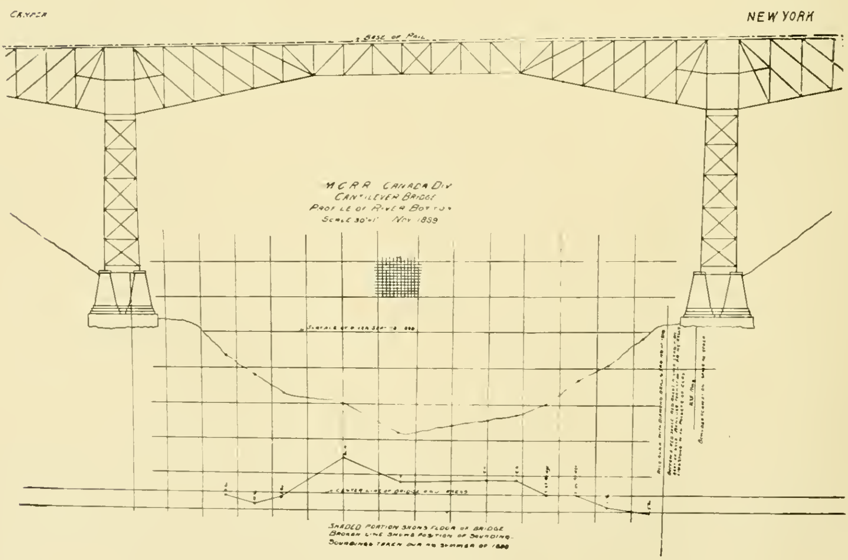

ENGINEER'S REPORT OF SOUNDINGS AT M. C. R. BRIDGE.

ON THE ANCIENT DRAINAGE AT NIAGARA FALLS.

BY P. W. CURRIE.

(Read 9th March, 1901).

IN preparing this paper, the writer has gone over the ground confirming and adding proofs of the statement of others in regard to the old watercourses of the Niagara District.*

THE COURSE OF THE PRE-GLACIAL TONAWANDA RIVER IN CANADA.

The river crossed the present Niagara river nearly at right angles. The edge of the southern bank where it crossed the river is now indicated by the line of breakers above the Falls. The hollow below these breakers is directly due to the erosive action of the ancient stream. Its northern limit was the bank now called Hubbard's Hill, ending on the Canadian side at Mr. Alexander Fraser's house, and shown also on the opposite or American bank of the modern river.

Further evidence of this former channel is found in the character of the soil and of the rock levels in Queen Victoria Niagara Falls Park. At the Dufferin Islands and at Table Rock, the rock level is at the surface. At the Power House where the bed rock is exposed, it is about eleven feet below the level of the water in the channel east of Cedar Island. Opposite the Carmelite Monastery, the rock level is about twelve feet below the water level in this same channel. At the gravel pit rock was found at a depth of about eighteen feet below the same water level. Near the high bank forming the

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* He has to acknowledge the kindness of Dr. J. W. Spencer, whose theory as to the old watercourses is the one accepted in this paper; also many favours from Mr. Wilson, Superintendent of Queen Victoria Niagara Falls Park; Mr. C. H. Mitchell, Engineer at Niagara Falls, and his assistant, Mr. Howard Dixon; Mr. Rothery, Superintendent of the Park and River Railway; and Captain Carter of the "Maid of the Mist," all of whom aided very much in gathering materials for the paper.

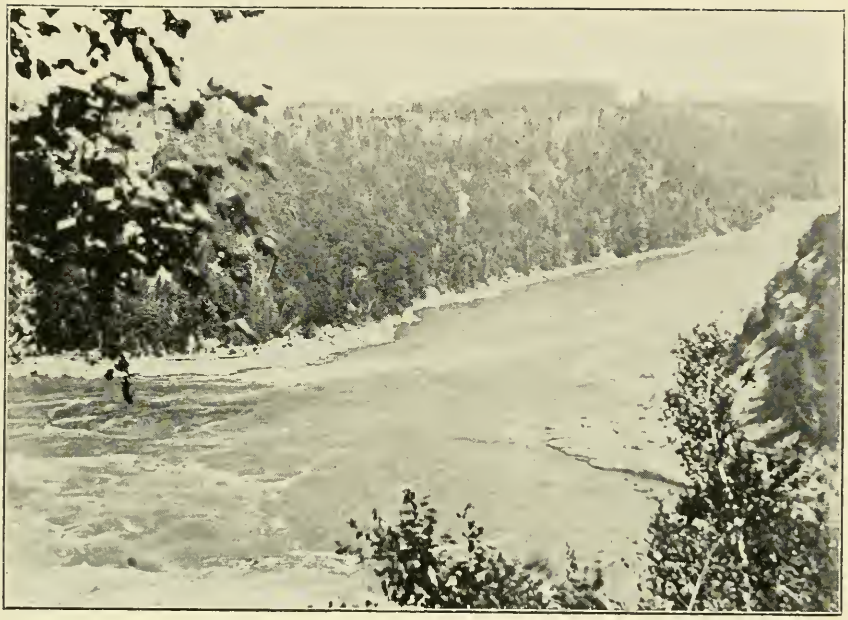
south boundary of the Park, where the Ontario Power Company sank a test pit, rock was not found till the hole was down thirty-three feet in the sand, or between thirteen and fourteen feet below the water in the channel above referred to. The dip in this part of the Park is to the south, but it is too slight to account for these differences in level.

The above facts show the existence of a V-shaped depression in the Park, the rock level sloping downwards north and south, to a point at or near the present gravel pit. The bottom of this depression is lower as it recedes from the river, so that if it were cleared of drift, the water would even now flow down it. It is also generally in a line with the course of the old Tonawanda River across the course of the present Niagara. The course of this channel through Niagara Falls South is not so easy to trace, but what evidence there is, is confirmatory. At the Michigan Central Station at Niagara Falls Centre, the rock is within a foot or so of the surface. In digging the sewer in front of Victoria Hall, the excavation went down nine feet, six inches, but no rock was found. At the line between Niagara Falls and Niagara Falls South, a well was dug over thirty feet without striking rock, and where the street car turns to go to Drummondville, a well thirty-three feet deep is resting on hard pan, that is, it is not yet down to solid rock. Between Niagara Falls Centre and the Niagara Falls South boundary, the land level rises about sixteen feet, leaving sixteen feet fall in rock level in 960 feet, a difference not accounted for by the dip.

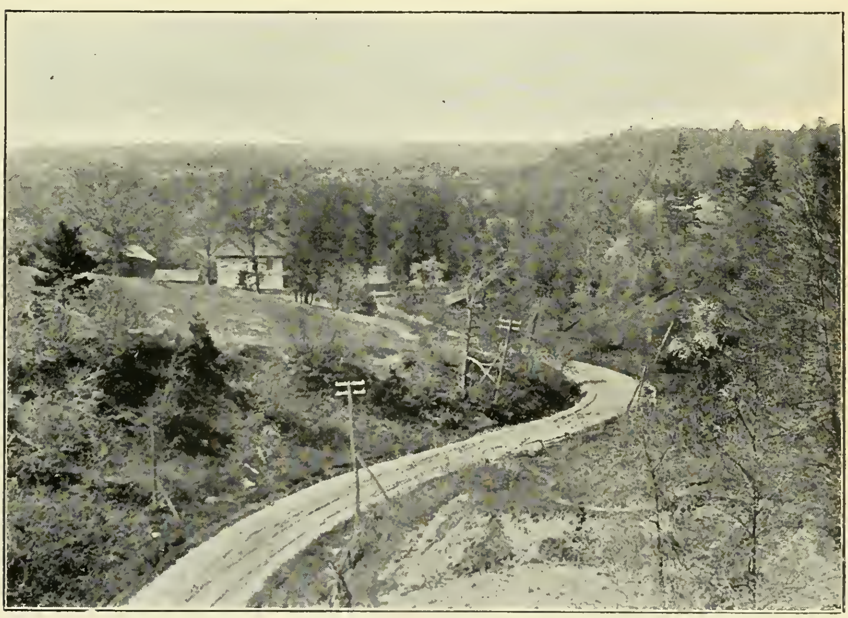
The next place where conclusive evidence of the channel is found is at St. David's ravine. The opening in the escarpment worn out here is about one and a half miles across. The banks slope gradually as do those of all ancient channels, and have not vertical walls as the gorge and other modern cañons have. The old river bed at St. David's is at present used for obtaining the fine sand which is deposited there. These sandpits are very deep, and the arrangement of the sand and gravel in them is very interesting. On the Governor Maitland estate, near them, a well over 150 feet deep was dug through sand and gravel. Mr. Warder, of Stamford, who dug many of the wells of this district, says that a deep deposit of sand and gravel is found continuously from St. David's Ravine to Niagara Falls South. On the way to the latter place, the depth of soil overlying the rock increases, but the increase in depth, he thinks, is due to the greater accumulation of drift in that direction, and not to any

Up the St. David's Ravine from G. T. R.

Whirlpool Rapids Showing Entrance to Pool.



Outlet of the Whirlpool.



Down the St. David's Ravine from G. T. R.

depression of the rock level. The latter, he thinks, remains nearly constant.

In the St. David's valley below the escarpment, are found several flowing wells. Above the escarpment, the surface drainage is either towards the Whirlpool or Thorold. It is possible these flowing wells are caused by the water of the Niagara soaking through the porous soil of the drift.

The course of the channel below the escarpment is harder to trace accurately, but by ascertaining from the farmers the facts in regard to their wells, some very interesting information was obtained, throwing a good deal of light on the subject.

Mr. Warder, whose opinion with regard to rock levels above the escarpment has been stated already, says that in St. David's village the rock is generally sixty or seventy feet below the level of the surface. On the road to St. Catharines, within a mile of the village, wells are found dug in the rock, which is, relatively, close to the surface.

On Lot 54, north of Usher's cement works, Mr. Muir has a good well. In digging it he struck red sandstone rock about twenty feet from the surface. Between Mr. Muir's farm and the lake, as far as Virgil, good springs are very scarce, and those who dig wells do not strike rock. On Lot 58, a well was dug fifty-three feet without striking rock. On Lot 33 they dug thirty-five feet, then bored fifty feet without striking rock. Mr. Harris, west of Lot 61, found rock at one hundred feet below the surface. The northern boundary of this channel seems much more indefinite than the southern one.

Where this channel crosses the Niagara river, is clearly visible on both sides of the river from the deck of the Toronto steamers. Near Queenston the river bank is formed of red shale. At a projecting point about three miles below Queenston, the shale changes to clay, and the bank is formed of clay from here to a place near the middle of Paradise Grove, where shale of the same kind as that at Queenston again forms the bank.

The space between these points, about three miles, was at one time filled with shale of the same kind as that at Queenston and Niagara. This was washed out by the ancient river, and during the glacial period, filled with clay. The map shows the probable course of this ancient stream.

Into the main stream flowed a tributary which joined it at St. David's. Its channel was relatively much narrower than that of the main stream. It appears to have been formed by the union of three small tributaries, whose waters finally came together at the Whirlpool. One followed a buried channel through the Collegiate Institute grounds, in Niagara Falls, Canada, to go by way of the present Niagara to the Whirlpool; on the way it was joined by a tributary flowing on the American side from Eagle Mount; while a third tributary seems to have flowed south from the present Devil's hole to join the combined stream at the Whirlpool.

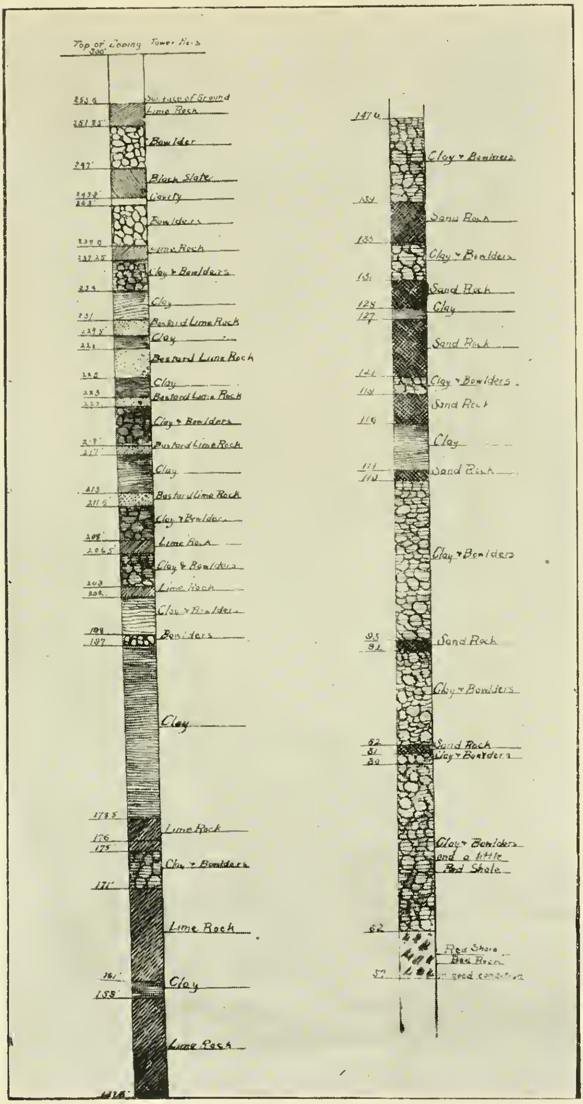
Beyond the Whirlpool, the combined stream followed nearly the course of the present Whirlpool ravine, although the latter seems to be a little to the south of the old channel. This is shown by the rocks, Niagara and Clinton, found in place in the bed of the stream at present flowing through the ravine. The bed of this stream was probably not as deep as the bottom of the present Whirlpool, the latter having its depth increased by the rotary motion of the water.

The Whirlpool ravine crosses the Grand Trunk tracks about a quarter of a mile south of the Stamford station. After this, it crosses a field and road. On the far side of the road is a pond fed by a spring creek, whose source is near the Presbyterian church in Stamford village.

From a little above the pond a wide depression, not very clearly defined, follows back towards Stamford, veering, however, northwards in the direction of the sandpits at St. David's. At the highest point of this depression in swampy ground near the side of the road, is a little grove of trees. From this starts a depression slanting downwards almost without a break to within one hundred yards of the sandpits. Any moisture falling on this depression could easily soak through the intervening gravel into the sandpits and St. David's ravine.

Outlet of Ancient Stream at Devil's Hole. Eroded Bed Points up Present Stream.

Sand Pits, St. David's.



ENGINEER'S REPORT OF TEST-BORINGS AT M. C. R. BRIDGE.

HYPOTHESIS OF THE MODE OF GORGE FORMATION AND CHARACTER OF THE FLOOR OF THE GORGE.

The water wears a large pit in the shale underlying the Niagara limestone, leaving it projecting until, unable to support its own weight, it breaks off and falls into the water. The Clinton limestone is undermined in the same way. After falling, these great masses of rock in front of the centre of the Canadian fall, are used as pestles by the tremendous power of the water, ground against one another and against the floor of the stream, until a great hole or basin is worn out down stream from the falls.

As the water gets down stream further from the fall, it loses energy, and finally a point is reached at which it can no longer move the larger rocks. At this place they are deposited in what formerly was the floor of the basin, and over these larger pieces smaller ones are deposited successively on account of the decreasing power of the water until, at some distance from the stream, the deposit attains a maximum height, acting there as a dam, over which the accumulated water flows. This water, by erosion, wears off the top of the old deposit, so that the position of the shallow dam, of the basin in front of the falls, and of the falls itself, is gradually advancing up stream, leaving behind it, in the bed of the river, the accumulated mass of fallen blocks.

This is the way in which the gorge has been made, at least from Foster's Flats. Below this, there may be part of the stream flowing on a bed of rock in place, as there was a time in the history of the Falls in which there were three separate cascades, and this condition would destroy the power of the water to dig out a deep hole as described above.

The borings made by the Michigan Central Railway to test the foundations of the Cantilever bridge afford a remarkable confirmation of this hypothesis. For 150 feet below the waterline, or nearly 70 feet below the deepest part of the river at this point, before striking rock in place, they bored down through clay and boulders. The rock they found was the red shale of the Medina, which, according to the above hypothesis, was the bottom of the deepest part of the basin worn by the Falls when passing this point.

In his report, the engineer called some layers, lime rock, sandstone, bastard lime rock, etc. Thinking these layers might represent remains of the original hard rock, the shale having been removed by the action of the water, and its place filled by detritus, Mr. H. Dixon, assistant engineer to Mr. C. H. Mitchell, went with the writer to Queenston, where we, using transit and chain, found the thickness of the different layers of rock and shale along the cliff. We tried to correlate the hard layers with those mentioned in the borings under the bridge, but found no relation whatever existing between them. We then decided that these masses of solid rock were simply very large boulders. In his report on the soundings taken under the bridge, the engineer expresses the same opinion.

The Canadian fall at present furnishes very good evidence of the correctness of the above hypothesis. On the margin of the Horseshoe Fall, both next Goat Island and the Canadian shore, great boulders are lying at the foot of the cliff. There is no sign of any such in the centre. Then one of two things must happen,—the rocks sink where they fall at the base of the cliff, or the force of the water carries them down stream to be finally deposited somewhere else. The soundings support the latter hypothesis, for the water is deepest not right under the fall, but a considerable distance down stream. The force of the water carries the rocks forward while their gravity causes them to sink, so they wear out the basin described above.

The American fall has great masses of huge boulders at its base. These, as in the Canadian fall, have dropped from the overhanging Niagara and Clinton rocks, but the water coming over them is not strong enough to displace them. Accordingly they pile up where they fall, and instead of wearing a deeper hole, protect the underlying shale from erosion and stop the wearing action of the fall. This explains the fact that, so far, observations on the American fall can detect but little retreat, as all differences found in measurement are not appreciably greater than the allowance which may be made for errors of observation.

Evidences of a similar state of affairs are found at Niagara Glen. The main stream on the American side was strong enough to move the great rock masses, and the erosion went on continuously, leaving a clear channel, while on the Canadian side a fall of water much smaller in volume and corresponding to the present American fall, descended. During the greater part of its course it was unable to remove the larger rocks, though its central part was stronger than the sides, and in the lower part, at least, left a tolerably clear channel. In the upper part, however, as at present in front of the American fall, the great rock masses are now piled up close to the cliff in such a way as effectually to stop the water from wearing away the soft shale.

The deep channel being in this way partially filled, the action of the weather on the lateral walls gradually causes a talus to be deposited on the sides of the gorge, giving the older parts of the river the rough V-shape shown by the soundings taken at the Cantilever bridge.

One more thing remains to be accounted for, viz., the Whirlpool. Where the current from the Whirlpool rapids enters the Whirlpool, a ledge of rocks projects from the east bank into the Whirlpool. The appearance of the water indicates a shallow part, probably a continuation of the ledge, running clear across to the other side. The Whirlpool basin itself is very deep, probably deeper than it was worn by the Falls or by the pre-glacial stream which formerly passed here. Where the water leaves the pool the passage is very narrow, rocks in place projecting into it both from the Canadian and American sides. Even in the centre of the channel the water appears to be quite shallow.

The depth of the water in the pool is due to the course of the river. Even while it ran on the top of the bank, before the Falls reached the Whirlpool, there would be here a deep pond, in character much the same as the present one. After the Falls passed this point, the same cause would deepen the hole where the change in direction of the stream occurred.

The shallowness of the exit of the Whirlpool is comparatively easy to account for. As the Falls cut their way back from Queenston, a time would come when only a very thin wall would remain between the water of the Whirlpool and that of the gorge. As this partition would break down rapidly in its upper part, the level of the Pool would suddenly lower, leaving the last part of the quartzose sandstone to be eroded only by the mechanical force of the running water—a very slow process.

The ledge above the Whirlpool was left during the time that the thin wall below was being taken out, before the cataract at the upper part had attained power enough to excavate deeply.

The Whirlpool basin was then probably no deeper than the Medina. Since then, the peculiar character of the motion of the water and the erosive action of the stones and pebbles carried by it, have deepened the basin to its present depth.

The following is a list of the works consulted in the preparation of this paper:

"Travels in North America," Lyell.

"Geol. of the Fourth District of New York," Dr. Jas. Hall.

"The Report of the Commissioners of the State Reservation at Niagara, 1893-94," containing Mr. J. W. Spencer's papers on the "History of Niagara Falls."

"Another Episode in the History of Niagara Falls." From "Am. Jour. of Science, Vol. VI., 1898," J. W. Spencer.

"The Tenth Annual Report of the Commissioners for Queen Victoria Niagara Falls Park, 1895," containing Prof. G. K. Gilbert's Monograph on "Niagara Falls and their History."

"Origin of the Gorge of the Whirlpool Rapids," F. B. Taylor.

DÉNÉ SURGERY.

By The Rev. Father A. G. Morice, O.M.I.

(Read 3rd February, 1900).

From the icy wastes of the Arctic circle to the barren borders of Patagonia, under whatever clime and with any environment, or mode of life, the American Indian is more or less shamanistic in his beliefs and practices. To him disease is not that deviation from the normal state of the living organism which is understood among us to result from natural causes. In his estimation, it is mainly due to the ill-will of certain minor spirits whom he generally believes to be under a greater, rather undefined power, and even subservient to the incantations of the conjurer whose role it is to exorcise them out of the patient, free the latter's body of any noxious matter due to their machinations, or otherwise influence them to the extent of restoring him to his former state.

This particularity of the native mind is well known, inasmuch as there hardly ever was a tribe without one or more shamans, or medicine men. What would seem to be less generally understood is the fact that, even in the olden times, the aborigines were far from relying exclusively on the mysterious powers of their conjurers in cases of bodily distress. Either on the advice of the latter or independently therefrom, they frequently had recourse to natural means in order to regain lost health, alleviate temporary ailments, or obviate the result of accidents. The vegetable kingdom furnished them with antidotes against almost any ill that humanity is heir to, and, in several instances too, they resorted freely to external treatment and artificial devices, the most important of which were, among the Northern Dénés, surgical bleeding and burning.

Prof. O. T. Masson has given us, at the end of his paper on "The Ray Collection from the Hupa Reservation," a valuable list of the plants, both economical and medicinal, used by the northern California Indians. Mr. James Mooney has rendered a like service to science in his valuable essay on the "Sacred Formulas of the Cherokees."* I

* VII. Annual Report Bureau of Ethnology, pp. 324-327; Washington.

might also refer the reader to what I have myself written on the same subject in my "Notes on the Western Dénés,* pages 130-132 inclusively. This part of my study, though not intended to be exhaustive at the time of its writing, might for the present, take the place of a complete treatise on Déné medicine. But, though bleeding at least was and remains a very prevalent practice among the natives, I have been unable to discover more than one reference thereto in the whole range of the American ethnographic literature at my command. Very valuable and detailed monographs there are on almost all the chief stocks into which the northern American race is divided, which consider them from every possible standpoint, but, with the one above mentioned exception, they invariably ignore the practice of bleeding so much in vogue among the different tribes.

My attention was lately drawn to this desideratum by an article from the pen of the Rev. H. C. Meredith, published in the December number of the *American Archæologist*.† The cuts which accompany his paper represent stone implements of such unique design as to render them worthy of a moment's consideration.

Those implements have quite a history. Found, close by Indian skeletons, in the vicinity of Stockton, California, by several persons of good social standing, some of them came into the possession of the above-mentioned gentleman. To make a long story short, a few of the latter subsequently passed into the hands of a South California collector of antiquities who, after much hesitation and a consultation with a would-be expert, pronounced them to be frauds and published his opinion to that effect in the *American Archæologist*. One of his main reasons for predicating dishonesty was that he "could not imagine any practical use to which they could have been applied," an excuse which is hardly satisfactory. Thereupon Mr. Meredith came up with a spirited reply containing sworn affidavits and detailed information which leave no doubt that his relics are genuine. A number of similar objects had already, it seems, found their way into the United States Museum, Washington, D.C., and it is probably of them that a Mr. Lyman Belding wrote in *Zoe*,‡ that they "differ from anything" he had "seen elsewhere."

Those implements are chiefly noticeable for three distinct features which are more or less reproduced in all the specimens illustrated,

* Trans. Can. Institute, No. 7.

† *The American Archæologist*, vol. II, p. 319.

‡ Vol. III., p. 200.

Fig. I.

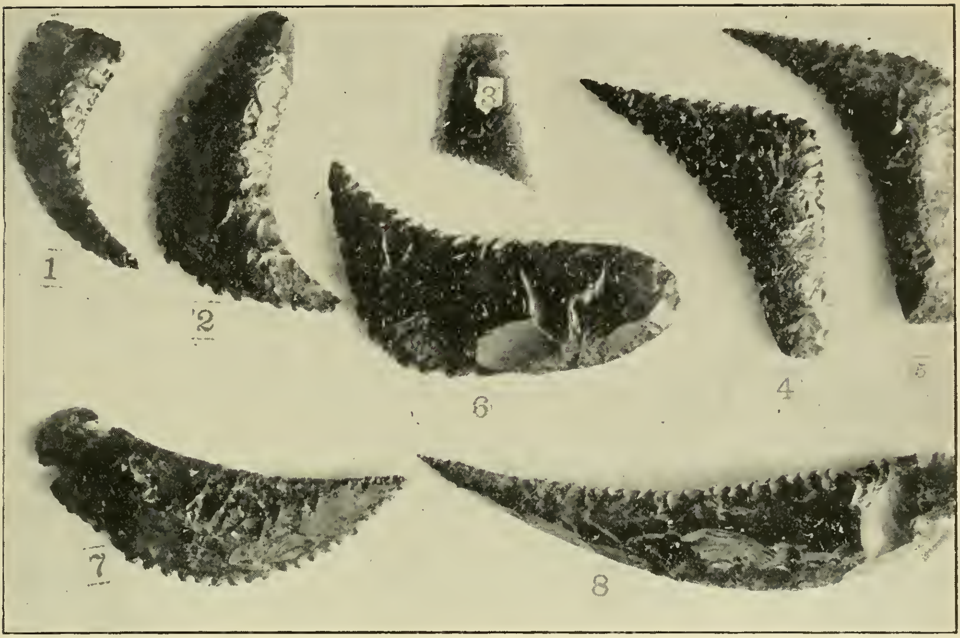
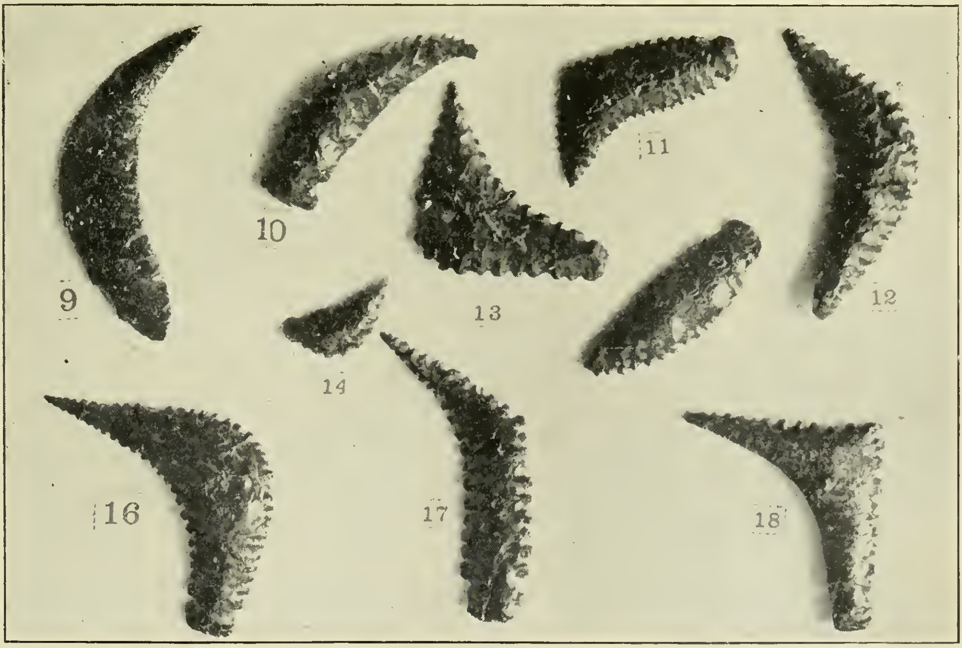


Fig. II.



From "The American Archæologist."

By permission of the Proprietor, Prof. W. K. Moorehead, Saranac Lake, N.Y.

namely, their sharp points, their curved outlines and their serrated edges.

The first particularity is probably what led Mr. Meredith to incline "to the opinion that they were used on occasions of sickness and ceremony for lacerating and bleeding the temples."* My own studies of the Northern Dénés allow me, taking into consideration that analogy found in most things aboriginal, to subscribe to the reverend gentleman's conclusions. But there remain the invariable and apparently unnecessary curvedness and the indentations of the relics. That these two peculiarities were intended for a specific purpose it would be idle to deny. What was that purpose? The idea of their having been designed as saws must be abandoned owing to the nature of the material, soft and brittle obsidian. Both Prof. Wilson and Mr. Meredith are agreed on that point. On the other hand, temple-bleeders do not require to be crooked in outline or serrated on the inner curved edge as is the case with most of the implements figured in Mr. Meredith's article. A brief reference to the custom of blood-letting as it is practised among the Northern Dénés may throw some light on the question.

Among those aborigines, bleeding may be considered under five different heads. There is blood-letting proper, darting, piercing, gashing and scarifying.

The Northern Dénés have always been poor, unæsthetic workmen and, as I have noted elsewhere,† among them "where extra exertion was not absolutely necessary, it was very seldom bestowed upon any kind of work." This explains how it is that none of my informants could remember the use of, or any reference to, anything like a lancet or bleeder by their ancestors. A sharp piece of stone, a flake from one of the few tools or weapons they made, or, more generally, even a common flint or obsidian arrow-head did that duty. Yet it would seem that, in pristine times, they had something like a bleeder, for one of the tribes, the Carrier, has a word, *hokwœlh*, to designate that instrument

The first process, I said, was, or rather is—for in that respect native surgery has not changed with the advent of the whites—*net's'uket* or blood-letting proper. As among us, the operation is performed either on a vein or an artery. In the latter, and by far the commonest, case,

* Loc. cit.

† "Notes on the Western Dénés," Trans. Can. Inst., vol. IV., p. 36.

the chosen subject is the temporal artery. This is slightly cut with as sharp an instrument as can be procured, and the blood is allowed to escape until a rich red colour has succeeded the dark hue of the first flow which is supposed to be the cause of the ailment. The wound is then compressed by the application of a piece of skin or of a green leaf, according to the season. The head is afterwards bandaged so as to ensure the speedy healing of the wound.

In cases of phlebotomy, the vein at the bend of the elbow is the one operated on. Head-ache, general uneasiness, nervous complaints, catalepsy or any accidental stiffness of a limb furnish the usual pretexts for this sort of bleeding. Pains in the legs are, however, more commonly relieved by pricking or darting either side of the knee when fire was not resorted to, as shall be seen further on.

This leads me to speak of the second process, which is darting or thrusting. Though widely different from the first, it is, in the eyes of the Indians, nothing but a modification thereof, and it goes by the same name. The natives have recourse to it mostly in cases of local pains, when it is a question of congested blood either in the course of a malady or as the result of an accident, as in cases of contusions consequent on a blow, a fall, etc. It differs from the first method of blood-letting in having the flesh, not the veins, as its seat of operation. The skin is first thoroughly softened with hot water, the diseased flesh is firmly gripped or pinched out by the left hand while, with the right, the blade of a sharp knife is thrust therein. The escape of the thick, blackish blood affords immediate relief, though the operation may have to be repeated time and again. All these details I know by personal experience.

A modification of this process, the usefulness of which is practically confined to cases of head-ache, is piercing. In connection therewith, the fleshy part of the forehead is grasped as in the previous operation and then several times transfixed with an awl or a like instrument.

The fourth way of using the lancet, *net'sû'tas*, is simply the gashing or cutting open of a swelling, a sore or any unhealthy excrescence on the skin. The Dénés are rather impatient under the stress of long standing ailments. They much prefer undergoing a painful operation to waiting for the natural issue of any complaint.

We now come to the fifth way of using the surgical bleeder which will, I think, explain the curvedness and inner indentations of Mr. Meredith's crooks. It is *neznœlh'tœs* or scarifying. This is very commonly resorted to in all cases of rheumatism, local aching and *mal de raquettes*, or the spraining of the instep resulting from too severe snow-shoeing. It is also regarded by many as a panacea against several other ills of a temporary nature. It consists in scratching numerous lines on the afflicted limb, followed, in many cases, by a liberal application of the bruised root of the hemlock plant (Conium maculatum).

Now let the reader glance at Mr. Meredith's crooks. What is more natural than to suppose that the indentations thereon were designed as so many teeth of a stone currycomb intended to lessen the labour of the native surgeon, as the scratches must be very numerous, while work done with a single point would necessarily result in a useless flow of blood from the first scarifications and unduly prolong the sufferings of the patient? On the other hand, the crooked outlines of the bleeder are easily explained by the use the implement is put to. Its curvedness is simply a means of having it fit the various parts of the arm or leg whereon the simultaneous scarifications are produced.

That this is not a mere fancy of mine can readily be inferred from the fact that this peculiar method of bleeding is not confined to the Déné race. We read in the paper by Mr. J. Mooney already referred to that, among the Cherokees, "there are two methods of performing the operation (of blood-letting), bleeding proper and scratching, the latter being preparatory to rubbing on the medicine which is thus brought into more direct contact with the blood." He then explains that "scratching is a painful process. . . . In preparing the young men for the ball play, the shaman uses an instrument somewhat resembling a comb, having seven teeth, made from the sharpened splinters of the leg bone of a turkey." He further enters into minute details concerning the operation which he says is performed "on each arm below the elbow and on each leg above and below the knee."

So much for the curvedness of Mr. Meredith's implements. The indentations noticeable on some of the outer or straight edges may be explained by the fact that finally "the instrument is drawn across the breast from the two shoulders so as to form a cross, . . . so that the body is thus gashed in nearly three hundred places."*

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* VII. Annual Report, Bureau of Ethnology, p. 334.

Surgical scratching on flat surfaces is also practised, though seldom enough, by the Northern Dénés.

Here are two late instances that will illustrate the circumstances under which bleeding is mostly resorted to here. About two months ago, I returned from a trip of over three weeks duration to an outpost of my central mission. On the way back, one of my companions experienced some physical difficulty with one of his feet which rendered snow-shoeing exceedingly painful, if not quite impossible. Without any loss of time, his instep was duly scarified with a pocket-knife by one of his fellow Indians. Upon my return here, I noticed one of our women who since some time had been suffering from some nervous derangement, possibly catalepsy, bandaged about the head. Enquiry brought out the fact that, during my absence, she had undergone an almost identical operation in the vicinity of the temples with the addition, this time, of a poultice of bruised hemlock roots.

Another surgical practice which was formerly much in vogue among the Carriers, but has now fallen into desultude, was that of burning. It could not properly be called cautery, as its object was not the searing of the flesh as a means of stopping blood or of preventing the extension of a local trouble. It was used mainly against rheumatism or any ill of a cognate nature, and its chosen seat of operation was very generally the joints of a limb, either the wrist, the elbow, the shoulder, or the knee. Sometimes also any part of the spine, and more seldom a spot on the bony surface of the head were likewise experimented on.

This is how the operation was performed. When an Indian had resolved to get rid of an aching pain that had become too acute for patient bearing, he took a round piece of tinder perhaps one-third of an inch in diameter, wetted with his saliva that part of its surface that was to come in contact with the flesh, and then pressed it firmly on the joint the healing of which was deemed most likely to ensure the prompt recovery of the whole limb. Next, he himself, or an obliging friend ignited the top of the tinder, which was suffered to burn down to the very flesh, wherein a corresponding sore or cavity was inevitably produced. The excruciating pain consequent on the slow combustion of the piece of fungus was ordinarly borne in the most stoically indifferent way possible. A moment of the severest anguish is nothing to the Indian, especially if accompanied by the hope of a speedy recovery; but almost any bodily discomfort, if too prolonged, is to him a torment past endurance.

To return to our surgical experiment. Should the tinder totally consume itself on the flesh, the operation was deemed a failure, and it was at once repeated on an adjoining place. Was the result of the second attempt identical with that of the first, a third spot, always close to the joint, was operated on, until, under the effect of a slight explosion due, perhaps, to the sudden contact of the fire with the serous fluid under the epiderm, the burning piece of tinder flew up as a sure token of the disappearance of the cause of pain.

The greatest faith was placed in the efficacy of this operation, and many an old Carrier bears to this day indelible marks which testify to his former trust in that "fire cure."

Another kind of operation in connection wherewith fire figured as a most important factor, was that resorted to as a cure against ear-ache. It has an even more superstitious complexion, and it is likewise on the wane. In such cases, a few hairs picked from the tail of a dog were singed and their extremities introduced, while burning, into the drum of the ear. Should the hair used have been that of a she-dog, the cure was regarded as a matter of course.

Another appliance much in vogue among the Carriers, and which, though taken from the vegetable kingdom, is hardly less effective than fire, is a sort of blister made of the bruised green leaves and stems of a plant called *waltak* in Carrier, and of the botanical identity of which I am not quite sure, though I incline to the belief that it is the *Ranunculus sceleratus.* Its caustic properties are so great that it is seldom applied directly to the flesh, sometimes a thick covering of linen stuff being unequal to the task of rendering its application bearable for more than a few moments. It is used against almost any acute pain of a local character.

Passing now to the various branches of the surgical art such as it is practised among the Northern Dénés, we may come to the setting of broken limbs. This, I am bound to say, is done in a most clumsy way. Though all our Indians, being expert huntsmen and therefore experienced butchers, know and name without the least difficulty any part of the animal anatomy, the most they formerly could do in connection with the injured human body was to try, not always successfully, to reduce dislocations. As to fractures properly speaking, no setting of the broken ends of the bone was attempted. The limb was simply enclosed, with hardly any padding, in several envelopes of birch bark with a few wooden splinters bound around as tight as possible, and then left to heal as best it could. In every instance deformity ensued as a matter of course, and even now more than one crooked leg or stiff arm is a witness to the inefficiency of Déné surgery.

In no case was amputation resorted to, except when it was self-evident that the limb, foot or finger, was too deeply cut to allow of the edges of the wound becoming reunited. In such cases, the bark of the aspen root *(Populus tremuloides)* was much esteemed as an astringent. More than once, too, persons supposed to be endowed with magic powers, and who were on that account styled *uzé hutqai* (he whose mouth effects cure), were in times past, asked to suck the blood out of the wound so as to prevent gangrene or any other undesirable result. Such persons were so much the more inclined to render this service, as they well knew that it would not be left unpaid for. And so it was that even on such occasions superstition claimed rights which, as shall be seen in the course of this essay, affected more or less nearly all surgical operations.

But if the cut did not materially affect the bone, and in other cases as, for instance, that of a serious bite from a vicious dog, the Carriers had, and indeed continue to have, recourse to suture, generally with satisfactory results. In olden times, very often shreds of moose sinews were used as thread, while sharp splinters of bone, commonly of a swan wing, did duty as a needle.

Ligature against hemorrhage was unknown. Applications of the chewed bark of aspen root took its place. When the wound or sore manifested a tendency towards decomposition, a sort of blister of the inner bark of the willow *(Salix longifolia)* and of the outer bark of the bear berry bush* was applied, generally with good results.

All cases of hernia are treated by bandaging. But sometimes the

* Not to be confounded with the Kinnikinik plant or *Arctostaphylus uva-ursi.* The plant I now refer to is a shrub four or five feet high, whose name seems to be unknown to all the English-speaking people I have met, though the plant is very abundant all through my district. French Canadians in the service of the Hudson's Bay Company call it, it would seem, *l'arbe aux sept ècorces*, though it is no tree at all. The medical properties of its leaves, bark and root are highly valued by the Indians. The word I call it by is merely a translation of its Carrier name sœs-mai-tcœn, bear-berry-stick, or bush.

rupture has been so serious that it results in a complete protrusion of the abdominal viscera. Such cases are always fatal here. Unable to effect a cure, the Dénés try to alleviate the sufferings of the patient by means of an eagle or goose quill, slightly cut at the end, and stuck in the outward excrescence. This serves as a duct for the purulent matter which is usually formed in the vicinity of the rupture.

Incredible as it may seem, I never heard of any case of inguinal hernia, and the natives I questioned on that subject profess to be ignorant thereof.

Gunshot wounds are now treated first by sucking out the fine soot-like residue generally concomitant with the tearing of flesh by a projectile from a fire-arm, after which the shots or ball are extracted if possible, the wound carefully washed and finally covered with some emollient medicinal herbs.

I have said that amputation was unknown among the Northern Dénés. This is strictly true as regards surgical operations; but, among the Sèkanais, there was another kind of amputation which was often practised, especially by the women. This consisted in the voluntary cutting off of a finger or of part thereof as an outward sign of extreme grief, anger or resentment. It was resorted to mostly on the occasion of the death of a beloved child, sometimes upon the loss of a kind husband and, more seldom, in cases of disappointed love. More than one mother went even further, and did not hestitate to horribly lacerate the breasts her dead offspring had sucked, as a mark of her disgust that she should be left alive after the object of her affection was gone.* Not long ago there died among the Sèkanais, who trade their peltries at Fort McLeod, a woman who is said to have had but two fingers left intact. Even here, at Stuart's Lake, we have among the Carriers, a Sèkanais woman whose shortened index attests the intensity of her past troubles. On such occasions a common axe replaces the surgical knife.

The treatment of incipient deformities is hardly more serious than that of fractures. As a matter of fact, in most cases it is commenced too late and stopped too soon to be of much benefit. As with fractures, pieces of birch bark, with the addition occasionally of wooden splinters, are kept very tight over the spine or the diseased

* Such outward marks of sorrow recall facts recorded in the history of barbarous nations. Thus we read that, at the death of Attila, his followers manifested their sense of the loss they had suffered by lacerating themselves with knives.

limb by means of stout bandages. But as the patient is generally a child, and as youth has pretty much its own way among the natives, it commonly happens that said patient soon grows tired of the restraint imposed by the apparatus and easily persuades his parents to throw it away before half the time necessary for a cure has elapsed. Thus it is that hunch-backs, mostly females, are not unknown among the Dénés.

Speaking of females reminds me of a circumstance in connection wherewith Déné surgery is, as a rule, more successful. I mean that dangerous complaint known to medical men as *prolapsus uteri*. As most of the drudgery of the daily life, especially when on the wing, still falls to the lot of the woman, accidents, oftentimes quite serious, follow as a matter of course. Heavy packing, stumbling, falling, or the straining of the lower extremities frequently enough occasion the displacement, more or less pronounced, of the womb. In such cases, the treatment observed is quite rational, and therefore, it is ordinarily crowned with success. The patient is immediately laid in a recumbent position with the head rather lower than the womb, and quite commonly, by dint of external manipulation, the injured organ is pressed up to its normal place, after which strong bandages covering a plaster-like padding, added to copious draughts of the decoction of the stems of the raspberry bush, help to neutralize the effects of the accident. But I am bound to confess that, in the more severe cases, sterility ensues even in otherwise healthy women. Some persons of this Mission are quite noted for their skill in connection with all complaints of this and a cognate nature.

Considering the inconstancy of the native temperament, it speaks well for their comprehension of the gravity of such complaints that, in extreme cases, they should keep the patient as long as a full month reclining with the head in a lower plane than the rest of the body after the womb has been duly replaced. I know of one such case which was so serious that the organ had escaped from the body and was protruding almost in its entirety. It happened in a remote village long before I was here, and its treatment may therefore be considered as an instance of unassisted native surgery. It was consequent on a painful delivery, and the mother was so skilfully operated on that she lived to see several grandchildren by the daughter who was the involuntary cause of the whole trouble.

On such occasions, our Carrier women wrap their hand, preparatory to internal manipulation, with a kerchief or some soft material and, of course, perform the operation as gently and as gradually as possible.

Midwifery was formerly unknown among the Carriers as it has remained among the Tsilkoh'tin, another Déné tribe. But since the advent of civilization, it would seem that our women are not half as hardy as they used to be*, and, whenever possible, one or more of their female neighbours are now called in to assist nature in the process of parturition. So far as I know, this aid consists in external pressure only. It goes without saying that none of the various instrumental operations resorted to in grievous cases among civilized people are known among poor children of the forest, whose only cutting tools were, but yesterday, roughly flaked stone implements.

As parturifacients, three plants are chiefly valued and used to this day among the Carriers. They are the horse-tail *(Equisetum hyemale)* which is taken in strong decoctions, the bark of the Devil's bush *(Fatsia horrida)*, and that of the elder *(Sambucus racemosus)*, hot infusions of which are drunk previous to parturition or before the after-birth is expelled.

A particularity subsequent to delivery which is proper to the natives and is based on superstitious notions, is that relative to the placenta. This was formerly wrapped and suspended from a tree at some distance from the village. Should it have come in contact with water, the mother was believed to be doomed to perpetual sterility.

Any reader, ever so little conversant with American aboriginal sociology, knows of the sudatory or sweat-bath wherein the whole naked body is exposed, within an hermetically closed space, to the effect of steam emanating from heated stones. This is quite common among the Northern Dénés. But those Indians have besides a partial or local vapour-bath which is a favourite with lately delivered women. This is called *yœn-dîz'ai* (it—an object long or heavy—lies on the ground), while the regular sweat-bath is known as *tse'-zœl*, or the heat of stones. *Yœn-dîz'ai* consists in a round, shallow hole about one foot in diameter dug in the ground, wherein two or three red hot stones are laid. Across the apex of the cavity, small sticks are deposited gridiron-wise and then covered with moistened grass.

* That this is a common result of civilization over savage populations is shown by the following statement, one of the many which could be adduced: "When they begin to take on civilized habits, the Dakota women find they can not continue to follow the customs of their grandmothers." *Riggs' Dakota Grammar*, etc., p. 208.

Finally some rag, as an old towel just soaked in water—the equivalent of the piece of tanned skin of former years—is thrown on the grass, thus completely closing the aperture of the hole, and the patient steams herself over it in the region of the womb. This contrivance is, it seems, fruitful of the most satisfactory results.

As soon as the new-born child has received the first cares necessitated by its entry into the world, due attention is paid by its mother and her attendants to its future personal appearance. Here long heads do not meet with favour; therefore the head of the infant is frequently compressed or squeezed between the hands applied to its top and to the chin. No mechanical or permanent contrivance is called into requisition. Futhermore, its eyes are time and again opened and the lids pressed asunder, not any too gently, so as to cause generous dimensions for the balls, and the tiny eye-brows* are from time to time manipulated into the most elegantly arched shape possible.

I will now close this review of the Déné surgery by mentioning an operation which the preceding pages have not prepared the reader to anticipate, I mean the extraction of cataract. Few ills are more common here than diseases of the eye. The number of blind people among the Carriers, and the Babines especially, is altogether out of proportion with the population. Snow-haze, accidental blows on the face received in the thickets, smoke from the camp fire or from underneath the fruit-drying or salmon-curing shanties, added sometime to uncleanliness on the part of the old people, are the main causes of this too prevalent complaint. Cataract is easily discerned by the natives who treat it in this wise.

A minute pellicle is torn from a piece of birch bark (*Bitula papyracea*), after which it is doubled up and its extremities firmly held between the fingers. One of the sides of the curve thus formed is then used as the edge of a scraper on the corner of the eye next to the bridge of the nose, and the thin film-like covering on the eye-ball is worked on till part of it is torn asunder, thereby affording a hold for the grasp of the fingers. These now complete the operation by gently drawing off the whole impediment to vision.

Instead of birch bark, others use for the same purpose a piece of calcined bone which, coming in contact with the waste tissue formed

* I suppose I will not teach anything to my readers by recalling the fact that Indian babies are almost always born with a full crop of hair and more than once with several teeth.

on the eye, seems to have the same drawing properties as a magnet has on a bit of iron.

In either case, the eye is left sore and bloody. It is now carefully washed, and, as a final treatment, it is bathed with a cooled infusion of the inner bark of the bear berry bush to which a little woman's milk has been added. The former especially is reputed to be quite a specific against any soreness of the eyes, though its mordant properties render its application very trying at first. With this last preparation the patient is made to retire, and, when he wakes up on the morrow, he generally feels quite well. Such operations are even now quite common and as uniformly successful. But I am inclined to believe that, considering the primitive way they are performed, at least as much credit is due to the endurance of the patient as to the skill of the oculist.

The most common form of ophthalmic trouble among the Northern Dénés is snow blindness and its resulting whitening of the affected pupil. A persistent haziness in the atmosphere and the refraction of a strong light on the water will sometimes have the same effect on persons of a delicate constitution. If allowed to develop itself unhindered, this deterioration of the pupil will completely destroy the sight. The Carriers' great remedy against this complaint, as in all cases of soreness resulting from accidental blows or tearings, is the balsam of young spruce tops *(Abies nigra).* The upper shoots once cut off the sapling are bent and split in two and then left by the fireside. After the resinous liquid they contain has been heated out, the ball of the eye is gently coated therewith by means of a bird quill.

THE CLASSIFICATION OF THE DÉNÉS.

LETTER FROM THE REV. FATHER MORICE.

STUART'S LAKE MISSION, 2 *Jan.*, *1901*.

Editor "TRANSACTIONS C.I.," TORONTO.

DEAR SIR,—In a late communication from Prof. O. C. Mason, of the Smithsonian Institution, I find the following:—

"In the publication which you sent me, you call attention to the list of Athapaskan tribes published in the Standard Dictionary. Supposing me to be the author of that list, you make some just observations about its meagerness. I am happy to tell you that, while I wrote a great number of definitions for the Standard Dictionary, I did not make any ethnological list whatever."

Prof. Mason here refers to remarks published in the latter part of the first of my papers lately printed in the Memorial Volume (p. 82). It is indeed very unfortunate that I should have been guilty of such an injustice with regard to a scientist whom I know by personal experience to be so pains-taking and conscientious in his work. My regret is so much the greater as I more than half suspected the real author of the classification incriminated in my little paper. But the list of the editorial staff of the Standard Dictionary prefixed to that valuable publication left me no choice but to attribute the incomplete Déné classification to Prof. Mason, who is described as having been in charge of the anthropological department, while the actual compiler's name is therein associated with a branch of lexicology which has no necessary connection with ethnography.

Hoping you will kindly give this rectification as mnch publicity as was granted to the statement which has necessitated it, I remain, dear Mr. Editor,

Yours faithfully,

A. G. MORICE, O.M.I.

CRITICAL EXAMINATION OF SPANISH DOCUMENTS RELATIVE TO THE CANARY ISLANDS, SUBMITTED TO THE WRITER BY SENOR DON JUAN BETHENCOURT ALFONSO, OF TENERIFE.

BY JOHN CAMPBELL, LL.D., F.R.S.C., ETC.,

Professor in the Presbyterian College, Montreal.

(Read 26th January, 1901).

A VALUABLE treatise, published in Paris, and bearing date 1629-30, is entitled: "Histoire de la première Descouverte et Conqueste des Canaries. Faite dès l'an 1402, par Jean de Bethencourt, Chambellan du Roy Charles VI., etc." A translation of it is to be found in the publications of the Hakluyt Society, which first appeared in 1872. De Bethencourt was the first European discoverer of the Canary Islands, which passed into the hands of Spain, in whose possession they still remain. A lineal descendant of the Conquistador is Señor Don Juan Bethencourt Alfonso, a doctor of medicine and scholar of note in Tenerife. With a generous confidence in the philological attainments of the writer, he sent him last summer (1900), through M. Henri O'Shea, of Biarritz, member of the Royal Academy of History of Madrid, three important documents presenting problems for solution. These are a printed pamphlet of fifty-six pages octavo, entitled, "Vocabulario del antiquo Dialecto de los Canarios"; a folio manuscript of seventy-seven pages, designated, "Complemento al Vocabulario del antiquo Dialecto de los Canarios"; and a nineteen page manuscript quarto, with many pen and ink illustrations. bearing the heading, "Aclaraciones: Inscripciones de las islas Canarias."

The first contains vocabularies of the Guanche or Canary Island language, now extinct, taken down from the lips of the aborigines from 1503 onward. A few words go farther back, at least as far as 1482. The vocabularies embrace Religious Concepts, Titles, Arms, Clothing and Utensils, Aliments, Animals and Vegetables, Proper Names of Persons and Places, Miscellaneous Words, and a few Phrases or Sentences. They are from Lanzarote, Fuerteventura, Gran Canaria, Tenerife, Gomera, Palma and Hierro. The second implements the first very largely, especially in names of places, and in valuable notes, historical and documentary. The third is virtually a supplement to a manuscript entitled, "Inscripciones de la isla de Hierro," sent to the writer by Dr. Bethencourt in 1898, for translation. A French version of the translation, made by M. O'Shea, appeared more than a year ago in the transactions of the Biarritz Association; and the English version is now in press for the transactions of the Royal Society of Canada. The most important inscription in the present document is that on the statue called "La Virgen de Candelaria," from Tenerife, in apparently Roman letters; others in ruder script are from rock faces in Canaria, Gomera and Hierro. Such then is the material on which the writer is called upon to express a scientific opinion, as a student of language and of ancient Turanian and other inscriptions. The threefold relations of the Canary Islands to Africa, Europe and America, invest the study with special interest.

In allowing Dr. Bethencourt to speak for himself, the writer must crave his and the Institute's indulgence, inasmuch as he has dabbled but little in the Spanish tongue since college days, which lie thirty-five years in the past; and he finds some of the Doctor's rhetorical forms and quaint expressions to transcend the range of the ordinary grammar and dictionary. Unconsciously, at times, he interlards a Guanche term, perfectly significant to the dwellers on the islands, but ignored by the Castilian lexicographer. The first document has no text, being pure vocabulary. The second, or folio manuscript, contains six pages of introduction, which are as follows: "I send as much as is known of the Guanche language and of those spoken in the other islands. The printed document comprises all the words and phrases published up to this date, during four or five centuries, by authors native and foreign; and the manuscript contains what I have been able to bring together in a period of thirty years.

"Either list is replete with errors, not only of orthography and pronunciation, since each collector has gone on accommodating speeches and words to his own age, but also we have arrived at taking as Guanche what is the purest and noblest Castilian. Moreover, there is such a tendency among our people to contraction or apocope, that even speeches reduce themselves to a single common word, as, for example, the word Lerines, of the isle of Hierro, which I am sure, through thorough investigation, arose out of La era de Inez. If, to this ignorance, under the disadvantage of which labour the majority of the words which we have collected, be added the fact that almost all refer to localities and personal proper names, and that there have lived in the islands French, Portuguese, Irish, and natives of many different provinces of Spain, as well as Jews of various origins, and Indians or emigrants from the islands who have returned after many years spent in all parts of America, it will be understood how many foreign words have naturalized themselves in the country.

"However, the difficulties of the problem do not cease here, since it is complicated by the thousands of negro slaves and captive Moriscos, who, in some islands more, in others fewer, have dwelt in all since the time of the first conqueror, Juan Bethencourt, until our day. It is not too rash to assert that, at certain periods, the half of the population of Lanzerote and Fuerteventura was Arab and Morisco. As the clergy and the inquisitors went about, always on the lookout for the filtration of heresy through the chinks of toleration, they employed every kind of means to put an end to it. In the parochial archives of Betancuria (island of Fuerteventura) there may be read in the book of visitation, drawn up for the delegation by the licentiate Aceituno in 1660—'that the Moriscos generally speak the Morisco language, and teach their children to speak it, and not to speak our language; for which reason it has been commanded and is commanded that, from this time forward, no Morisco shall speak the said language nor teach it to his children, under the penalty of 300 maravedis for each offence.' In the instructions of the bishop Zimenez in 1666 (according to the same archives) it is ordained that, 'from this time forward, the said Arabic language shall not be spoken, neither Traigan nor Alquiceles nor Tagolines; that the Moriscos, male and female, and other persons, shall not sing Morisco songs in the Arabic language, such being a scandalous thing and full of suspicion.' The same took place in the island of Lanzerote. In the archives of the Puebla of Teguise, referring to 1665, appears this decision, 'that, being informed that the Moriscos of this island commonly make use of the Algarabian tongue of the Moors, and teach their children to speak it, the evil be not permitted to continue.'

"Until far on in the eighteenth century, down to the reign of Charles III., the freebooters of the islands sustained intimate relations with those of the neighbouring coast (of Africa), visiting each other, inter-marrying, and maintaining amicable and family connections. It is an undoubted truth that the pirates of the nearest coast of the mainland, which is removed by but half a dozen hours of navigation from the islands, had as many Canarians of the isles as Moriscos in the mutual *cabalgadas* and *razzias* which they made. Some years ago I went through these regions, accompanied by certain friends, to examine the scene where were summoned the dwellers in one of the various forts or castlets, who do much to catch little. We went round about among the *Celestinos* or Moors of the coast, inspecting those who exhibited negro, mulatto and Arab types, others indefinable, and many that were ruddy, with blue eyes. While contemplating these last, I asked myself the question: 'Were they descendants of the ancient Guanche family that existed prior to the historic epoch in the remote ages of Atlantis, or of captives taken in the Canaries, from the fifteenth century onwards, or, perhaps, specimens of the Vandal population or of other barbarians who had passed into the north of Africa?' In regard to the islands of Canaria, Tenerife, Palma and Gomera, they contained many Moriscos and many more negro slaves, not only in private service, but also as labourers on the plantations. The first of the islands referred to still has *pueblos*, such as Santa Lucia de Tirajana, in which half of the census is negro shaded or of a significant darkness, an obscurity that extends to several villages. Similar conditions obtain in Tenerife as at Adeje and other points, and the same is the case in Gomera and Palma. As for the island of Hierro, although not altogether free from these extraneous ethnic elements, it was the least contaminated, for a native peasantry lived frugally beside the many waters that irrigated their sugar and other plantations.

" Ignorance of these historical facts, and a superficial examination of the subject, have given occasion to certain writers to state, with a scientific air, that the Guanches were prognathic, not that they could find any living example of an acute facial angle, but, from the discovery of skulls fulfilling the conditions, taken from tombs opened and little studied, they generalized from rare exceptions, being misled by the spirit of novelty. This question led me, as by the hand, to occupy myself, though but lightly, with the subject of the Guanche race, in order to aid in re-establishing historic truth concerning it. Terrified by the assertions of books, the press and other deterrents, both natives and strangers repeat to all new-comers, with affected lamentation, that they possess evidence of the annihilation of the Guanche population by the *conquistadores*. It is this common and vulgar, foolish talk that professes to give knowledge. Guanches are we and all ours, all, even to the foundation of the population of the archipelago, with the foreign elements already remarked. Without doubt, inasmuch as we live, speak, dress and think in European fashion, one would have difficulty in discovering it. Let us turn to our pastoral idylls and primitive skin dress in order to recognize ourselves.

" Until the end of the eighteenth century, none of our chroniclers said, or was able to say, such impostures. Unfortunately, our writers of this age confused their minds with the ideas of the French Encyclopedists, from whose influence escaped no one of the most illustrious historians of the Canaries, not even the most notable, the erudite Viera y Clavijo. This man of positive merit, without knowing how or why, without any foundation, alienating a truth which he ought to have known, first launched his specious falsehood with all the authority with which his reputation invested it. The rest belonged to the romantic school, a couple of generations of satyr-poets, who corrupted our history. The first to protest against these falsities was M. Berthelot in his notable work, and he was followed by others in his laudable purpose. But neither the serious labours of these men, which most people do not read, even including the literati; nor the testimony of the archives, in which appear wills, and contracts of purchase and sale of the aborigines, as well as marriage contracts and judicial procedures; nor the teaching surrendered by the very opened tombs, has sufficed to retard the velocity acquired by the ball, launched forth to roll in time by the unforgotten Viera, meanwhile gliding over the area of superficial ignorance in which we live.

"On the other hand, as before the conquest, so after the fourteenth century, adventurers and pirates came from Europe to capture Guanches and sell them for slaves. From the sixteenth century onwards the natives must of necessity have taken knowledge of the Spanish nobility, not descended from Moor, Jew or Guanche; and from this and a worldwide anxiety after pride of birth, have forgotten their affiliation, of which at the time they had ability to obtain proof. Who knows whether the disappearance and frequent burning of archives and other documents was part and parcel of this foolish pride? As the erudite historian Millares tells us, the inquisitors, certainly without any fraudulent design, made very full lists of Guanche descents.

"I proceed to add the tedious illustrations. I have deemed it suitable to prefix these preparatory considerations for those who are little versed in the internal history of the Canary Island people, a people little known even to those who study uninteresting tribes leading distant and secluded lives. I trust these data will not be lost on him who undertakes the serious but glorious task of making the study of the Guanche tongue. I know of no other materials than those I send; and I do not know if there is sufficient for such an enterprise, in order to re-construct or even make known the affiliation of a language which has entirely disappeared. It is a miracle of mercy to have the subject investigated by a genuine philologist, who alone knows how to exercise a wise discrimination and a useful sifting among the farrago of errors which we hold as vocabularies of the Guanche language. Had the Canary Islanders a common tongue, or, at least, a common origin?"

So far Dr. Bethencourt, who, the writer sincerely hopes, will not be tempted to apply to him the Italian proverb, "Traditore non Traduttore." The foregoing paragraphs present at least the gist of his introduction to the vocabularies, and nothing in it has been omitted. Many books have been written on the Canary Islands, both before and after the publication of the English histories of Glasse and Thomas Nicols. The writer has had access to some of these, among them to the work of M. E. Pégot-Ogier on the "Fortunate Isles." This author and others contend for the Celtic origin of the Guanches, and for their relation with the Berber tribes of northern Africa, whence old Guanche traditions concur in bringing their ancestors. The Berber dialects are much corrupted with Arabic and in part with negro languages, but their substance in vocabulary and grammar is Celtic. While a very considerable body of Celts emigrated from the East through Europe, leaving colonies in Bavaria, the Tyrol and Umbria, and peopling Gaul and the British Islands, a large number of them, even according to British traditions, Welsh, Irish and Scottish, passed westward through Africa and left their name to the province of Numidia. Of these latter some, at least, must have crossed over into Spain accompanied by the Iberic Mauretani, to constitute together the Celt-Iberian population of that peninsula. A smaller, yet not insignificant, emigration took place, at some remote period, from Cape Nun, in Morocco, or some more western point, to Lanzarote and the adjoining islands. Was this last tide of migration purely Celtic or, like that into Spain, was it Celt-Iberian?

Not to speak here of antiquities, such as architectural remains, arms and utensils, manners and customs, of which the writer has treated elsewhere, there remain two sources of information as to the affiliation of the Canary Islanders; the evidences of their written and of their spoken language. Of inscriptions the writer has translated about sixty, thirty and more of which have been already published, leaving twenty-seven, that have not so far seen the light, to illustrate this paper. They are, with no single exception, Iberic, their characters being those of Etruria and Iberic Spain, and the language they yield being archaic Basque. The best Basque scholars of France and Spain have homologated the translations already published, and have thus placed them beyond the reach of cavil. The fact seems, therefore, to be established, that, not only in Hierro, whence came most of the inscriptions, but also throughout the archipelago, a population akin to the Basques of the Pyrenees existed in a state of literary culture, and holding the reins of power. Two of the inscriptions published by M. O'Shea, President of the Biarritz Association, in its "Bulletin Mensuel" of December, 1898, namely, Nos. XX. and XXII., make mention of one Lamia, a Roman functionary. In 23 B.C., Ælius Lamia and L. Apronius were proconsuls in Africa (Tacitus, Annales, IV., 13); and the death of Lamia is noted as occurring in 33 A.D., in Lib. VI., 27, of the same historian. But an Ælius Lamia fought against the Cantabrians, as lieutenant of Augustus (Horace, Odes, III., 17). L. Ælius Lamia again was consul in the year 3 A.D.; and L. Ælius Plautius Lamia was suffragan with Domitian in the year 80 (Fasti Consulares). The Canary Islands were discovered by the Romans before 78 B.C., for Sertorius had the idea of passing his last days in them (Plutarch, Life of Sertorius). While it is hard to decide who the Lamia was that the Hierro inscriptions celebrate, it is probable that his date lies between 23 B.C. and 80 A.D. At least three generations of Iberic kings preceded his advent to the island, thus giving a pre-Christian era for the settlement of the Canaries from Africa.

The spoken language, as represented by the vocabularies which Dr. Bethencourt has, the writer thinks, unduly disparaged, supports, to a certain extent, the written evidence for an Iberic immigration. Among the words found in Fuerteventura occurs *sorrocloco*, which Dr. Bethencourt translates—"consistia en acostarse el marido durante los dias que lo estuviera su muger durante el puerperio, con iguales atenciones." This is "la couvade" of the Basques, which M. Francisque Michel thus describes: "les femmes se lèvent immédiatement apres leurs couches, et vaquent aux soins du ménage, pendant que leur mari se met au lit, prend la tendre créature avec lui, et reçoit ainsi les conpliments des voisins" (Le Pays Basque, p. 201). This custom is mentioned by Apollonius Rhodius, the author of "The Argonauts," as peculiar to the Tibareni of north-eastern Asia Minor; Diodorus Siculus attributes it to the Corsicans; Strabo, to the Iberians of Spain; Marco Polo, to an aboriginal population of China, identified with the Miau-tze, whence Butler in his Hudibras, writes:

"For though Chinese go to bed,
And lie in, in their ladies' stead";

and Du Tertre and Dobrizhofer found it among the Caribs of the West Indies and the Abipones of the Gran Chaco in South America. The whole subject is discussed by the late Max Müller, in his essay on "Manners and Customs," in the second volume of Chips from a German Workshop. The writer, though allowed by competent authorities to be proficient in Basque studies, has sought in vain for the native name of the *couvade*. At length it comes to meet him from far Fuerteventura. M. Michel cites Boulanger, in seeking the origin of this strange custom. He says: "Il semble que l'on doit regarder cette conduite du mari comme une sorte de pénitence, fondée sur la honte et le repentir d'avoir donné le jour à un être de son espece." There is no doubt that the first part of *sorrocloco* is the Basque *sor*, birth or the creature born. If Boulanger is right, the second will be *ahalge*, shame; the whole meaning "the shame of the birth." But the second may be *acholtsu*, in which case the word signifies "the care of the newly born." As the botanist rejoices over a new plant, and the numismatist over a new coin, so will etymologists be delighted with the recovery of the long lost *sorrocloco*.

The "couvade" was an Iberic, and thus a Turanian custom, and there is no evidence that Celts ever practised it. A further examination of the vocabularies, both of common and proper nouns, reveals very few more Basque terms, however. It is possible that *adarno*, a tree, may be the Basque *udarondo*, a pear-tree; *ara*, a goat, the B. *ari*, a ram; while *chede*, a boundary, is pure Basque. Also *estafia*, to beat, may come from the B. *asti*; and *gofio*, porridge of maize, is certainly the B. *sopa*, *zopa*, meaning the same thing. A mace or club, *magado*, seems to connect with the B. *makatu*, to strike with a stick; and *moca*, a javelin, with the B. *moko*, a point. Burnt ears of wheat, *rapayo*, may be derived from the B. *erre-bihi*; and the B. *dupha* is as near to the Canary *tabajo*, a milk-pail, as the Gaelic *tubog*. A flint knife, *tafique*, appears of kin to the B. *epaki*, to cut; and *tamaro*, *tamarco*, denoting a skin cloak and dress, recall the B. *zamarra*, a blouse; while *acichei*, beans, is the B. *ekosari*, *chilate*, a graminaceous herb, the B. *chilista*, lentils, and *morangana*, strawberry, the B. *mariguri*. But these are only fifteen words out of more than 450, for which corresponding Celtic terms have been found. The Basque terms were evidently in the position of loan words. That well-known word *Jainco*, God, does not appear in the lists, but is replaced by *Acoran*, the Celtic Crom; and the same is true of all distinctively Basque terms, which could not be absent if the Guanches had been an Iberic people, and the engravers of the inscriptions.

The writer does not profess to be a Celtic scholar in the best sense of that term, but among the six hundred languages and dialects to which he has given more or less attention in a fairly busy life, he has not neglected the Celtic and their remains. He is fortunate also in possessing the friendship and collaboration of that eminent master of the Celtic languages, the Rev. Dr. MacNish, of Cornwall, who has kindly undertaken a task beyond the writer's powers, that, namely, of assigning Celtic values to the almost innumerable proper names furnished by Dr. Bethencourt, as well as to investigate the construction of larger locutions, phrases or sentences provided by him. In his attempt to explain the remaining terms, chiefly common nouns, with some verbs and adjectives, the writer must apologize for the slender Celtic outfit for the work which his library supplies. The books chiefly drawn upon are O'Reilly's Irish Dictionary, which is admirably full of botanical names, and Edward's English and Welsh Dictionary, which is correspondingly deficient.

The work of comparison had not progressed very far, before the Guanche tongue revealed itself as more Cymric (Welsh and Breton), than Gaelic. The very name Guanche seems to connect with the Welsh *gwyn*, white, and thus to have denoted a white population in the vicinity of African negroes, swarthy Arabs, and red Iberians. Among distinctively Cymric terms appear the Canary Island *guatatiboa*, the national festival, in which it is not hard to recognize the Welsh *eisteddfod; guayafan* and *guayafacan*, co-adjutor of the governor, answering to the W. *cympen* and *cympencun; punapal*, first son, the W. *pen-eppill*, the chief descendant; *malgareo*, rough music, the W. *mawlganu*, to chant; *quevechi*, dignity, the W. *gofyged; titogan*, heaven, the W. *tuddo-cwn*, the covering of the head; *amogante*, berry, the W. *magon; guanoco*, weak, infirm, the W. *gwan;* and *iguanoso*, with the same signification, the W. *egwan*. Compound words are specially valuable as tests of correct or scientific comparison. Take, for example, the Guanche word *valeron*, which denotes, "the cave of the vestals"; it is the Welsh *ffau-lle-rhian*, "the cave apartment of the virgin." The name of a Guanche god was *Atguaychafortanaman*, and this appalling word of seven syllables, means "he who holds the heavens." In Irish Gaelic, it is *adh-se-a-cabhair-t-neamh;* and the latter part "holds the heavens," is in Welsh *cymhorth-nef*. The Guanches had very many words to denote goats and other domestic animals with peculiar markings, and these, as provided by Dr. Bethencourt, so outran the writer's patience, that, after translating a few, he gave the rest up as hopeless, save in the hands of one to the Celtic manner born. Thus *manonda* is a black goat with white feet; but *ban-an-dubh*, Irish Gaelic, and *gwyn-yn-du*, Welsh, mean "white in black." A white and cinnamon goat was called *puipana*, which is Irish *buidhe-ban*, "yellow-white." A male or he-goat was *carabuco*, the Irish *culbhoc*, but the Welsh *bwch-gafr*.

Articles of food come early into the life of a nation and stay long. In the Canaries a butter-cake was called *borondango*, which is just the Welsh *barachdaen*, a slice of bread and butter; *tacerquen*, syrup of mocanes, is the Irish *deasguin*, molasses; *aculan*, fresh fat, is the W. *agalen*, a lump of butter. It has no doubt puzzled investigators of the vocabularies to understand how *asitis-tirma*, *atis-tirma* and *tis-tirma* could mean at once "invocation to God," "cry of surrender," and "a sacred cliff." The Gaelic explains it, for its *aitchim-trom*, "I beg for protection," is alike applicable as a prayer to deity and to a victorious enemy; while *diagha-drim*, in the same language, denotes a sacred ridge or mountain. Take again the Guanche battle-cry, which shows what devout warriors they were, like the crusaders at Jerusalem. Its form, as handed down, is *datana*, which looks dangerous enough for a "Cruachan"; but it is the Irish *deodhann*, "by God's help!" which in Welsh is *a ga gan Dduw*. The lists give *tara*, *tarha*, *tarja* as "sign of remembrance"; it is really the Irish *tarra*, *tarrsa*, and the Welsh *dere*, *dyre*, which mean "come thou!" Some words denoting rank, and which the writer, with smaller vocabularies, once counted to the Iberic Turdetani, who seem to have formed part at least of the Iberic population of the Canaries, are purely Celtic. Achiman, for example, was a famous royal name among the Turdetani, but the *achimenceys*, or nobles of the Guanches, were *acmhaingeach*, which O'Reilly translates "powerful, puissant, rich." So *artamy*, prince, is the Gaelic *ardmhaor*, chief magistrate; *chichiciquico*, a squire, is *gaisgidheach;* and *guaire*, noble, is just *guaire*, "excellent, noble, great," says O'Reilly.

Celtic words beginning with the letter t are often doubtful, for, as O'Reilly remarks, "the letter t is used as an adventitious prefix to all Irish words beginning with a vowel, which are of the masculine gender and are preceded by the article *an*, which in English signifies *the*." The Berbers, with whom the Guanches were most intimately related, make free use of t both before and after words. Thus *medina*, the Arabic for town, they convert into *tamdint; murrah*, "a woman," into *tamraut;* and *dar*, "a house," into *taddert*. It will, therefore, not be a matter of surprise that a comparison of the many Canary words, beginning with this letter, with the Celtic vocabulary, pays but little heed to what is frequently adventitious, and of no root value. In tracing the origin of certain Guanche words, it has been necessary to combine Gaelic and Cymric elements. Such, for instance, is the universal Guanche word *guanil*, denoting "wild cattle." Here the Irish *agh* gives cattle, and the Welsh *anial*, wild. So *magarefo*, a tall thin boy, combines the Gaelic *mac*, a son, and the Welsh *llipa*, lanky.

A peculiar combination, but within the limits of the Irish dictionary, translates the Guanche *omanamastuca*, bright red ; it is *omh-aineamh-dathach*, which means "blood stain coloured." Some very out-of-the-way epithets are well preserved. Thus *babilon* is the nickname given by the inhabitants of other islands to a boy of Tenerife ; and it is the Irish *buibiollan*, a coxcomb, which leads one to infer that the gilded youth of Tenerife prided themselves upon their personal appearance. The son of a plebeian was *achicasna*, the Welsh *gwesin ;* a brave man was *altaha*, the Irish *lath* or *anthaa*, the Irish *niadh ;* an idler, *debase*, was the I. *taimheach ;* a vestal virgin, *harmaguade*, was the I. *er-maighdean*, noble virgin ; a tall vulgar person, *tamarco*, was either the I. *tamhanach*, or the Welsh *amrosgo*, or both ; and a tall slender man, *tigalate*, was the I. *teircfheolach.*

Either the Guanches lost the sound of r in many words, or their reporters omitted to notice it, while, in other cases they intruded that sound, as in *tacerquen* compared with the Gaelic *deasguin.* Among the botanical names of the Guanches that are determined, which are few, occurs that of the Cytisus, which is *tagasaste ;* now *tragasaste* would not be exactly the same as *ddrewgoed*, the Welsh name of a Cytisus, the laburnum, but it is not far from it. Again *faita*, treason, leaves out the r of the Welsh *brad.* A peculiar variation is found in the word which denotes "rod fishing from the shore" ; *jilmero* is the Guanche form, and *genweirio*, the Welsh. Yet they are plainly the same word. In Gomera, Dr. Bethencourt found the term *parano*, which he defines, "especie de armazon o canizo que se pone sobre el hogar para curar el queso, etc.," i.e., a stand or hurdle to place on the hearth for curing cheese, etc. This is just the Irish *brannra*, a stand, a prop, support, doubtless of the same ancient pattern in the old Guanche homes of Gomera, and in the farm houses of the Green Isle. The words signifying man are a fair indication of relationship. Of these, *antraha* answers to the Irish *anra*, common people ; *coran*, to the Welsh *gwr ; cotan*, to the I. *cathaidhe*, warrior ; *guamf*, to the Welsh *ymbaffiwr*, fighter ; *mago*, Guanche, to the I. *mogh*, man ; *mahey*, hero, to the I. *mogan ; teseique*, great man, to the I. *toiseach ;* and *tingalate*, tall thin person, to the I. *tan-cleith.*

The writer has placed the botanical names in a separate vocabulary, both because they constitute a special study, and because of the indeterminateness of most of them, which are simply called, a plant, a herb, a bush, a tree. Already the Canary Island *tagasaste*, Cytisus, has been compared with the Welsh *ddrewgoed.* The two words *chibusco* and *chibusquera*, a berry and the plant that bears it, can hardly be other than the Gaelic *subha*, a raspberry, and *subcraobh*, a raspberry bush, Rubus Idaeus. In *creses*, beech-nuts, appears the Irish *grech*. The two Irish names, *Caorinleana*, Valeriana officinalis, and *Caorogleana*, Lychnis flos cuculi, should between them explain the Canary *girolana*, a bush. *Calgbrudhan*, Ruscus aculeatus or butcher's broom, is hardly so recognizable in *givarvera*, *hivalvera*, the Canary word for it; but *guaydil*, Convolvulus floridus, answers rather to the Irish *codalian*, Mandragora or mandrake, and *codhlan*, Papaver or poppy. Bophthalmum is *joraida* in Canary, the nearest to which in Irish is *ceannruadh*, Chelidonium majus or celandine, which Piny extols as an eye-salve. The Irish *sinicin*, Sempervivum or houseleek, resembles the Canary *sanjora*, denoting the same. The Dracunculus Canariensis is *tacorantia*, and the name of the Irish Arum of the same family is *gacharonda*. A bulbous plant, *tarambuche*, invites comparison with the Welsh *crwnben*, a bulb. Mallows seem to abound in the islands, being known as *aguamante*, *amagante*, *juesco* and *vesto*, with which may be compared the Irish *ucas-fiadhain*, *mil-mheacan*, *ochus*, and *fochas*. The botanical list embraces ninety-three names of plants or their products, of which one only is certainly Basque, namely, the word for strawberry. Had Edward's Dictionary contained any botanical names worth speaking of, greater results might have been obtained, but enough are in evidence to prove the Guanches to be Celts, and Celts, moreover, in possession of some of the plant lore of the Druids.

It is unfortunate that among the Guanche words taken down at various times, the full forms of the personal pronouns do not appear. There is every reason to suppose that they were akin to those of the Berbers, which are:

I, *nekki*.	We, *nekni*.
Thou, *kemmi*.	Ye, *kunwi*, *kunwith*.
He, *netta*.	They, *nuthni*.

The nearest pronouns to these in the first and second persons are, strange as it may appear, the Peruvian of this continent. Such are:

I, *noca*.	We, *nocanchic*.
Thou, *cam*, *chema*.	Ye, *camchic*.
He, *hupa*.	They, *hupanaca*.

The divergence of the third persons is hard to explain, but the Peruvian furnishes the purer Celtic forms, since the Aymara *hupa* and *hupanaca* answer to the Welsh *efe*, *efo*, he, and *hwynt*, they. The Celtic character of much of the Peruvian vocabulary was indicated by the writer as far back as 1879, in the pages of the Canadian Naturalist of

Montreal. Now that no doubt remains as to the language of the Guanches of the Canaries being Celtic, a new interest is created in the Peruvian problem. The Celtic connection of the Peruvians is not a subject confined to the writer. In 1871, V. F. Lopez, published in Paris and Monte Video, a book entitled, "Les Races aryennes du Pérou," in which very learnedly he contended that, in spite of post-positions and other indications of Turanian syntax, the Quichua and cognate Peruvian dialects pertain to the Indo-European family of languages. M. V. Henry, a philologist of some note, made a laudatory, but, at the same time, destructive, criticism of the volume in an article read before the International Congress of Americanists, held at Luxemburg, in 1877. As, however, he therein exhibited no acquaintance with the special features of Celtic speech, his decision is not to be accepted as final. As to postpositions, he ought to have known that they are as common in Sanscrit as prepositions, an indication that the normal prepositional order of Indo-European language had, in its case, been greatly modified by the intimate presence among its speakers of a population making use of a postpositional Turanian language. If it appear that the twin elements, Celtic and Iberic, of the Canary Islands, migrated together to America, finally reaching Peru, and there insensibly fusing the divergent elements of their two distinct forms of speech, the problem of a language which puts Celtic vocables into Iberic grammatical order need no longer present difficulty. It is granted that postpositions to nouns, the postposition of the word governing the genitive, and similar peculiarities, are not Celtic, but Iberic and Turanian. Moreover, there are many Turanian vocables in Peruvian, and the ruling class in Peru, namely, the Incas, was distinctively Turanian. The unanswerable fact, however, still remains that the larger part of the Peruvian vocabularies are Celtic. Before passing to the consideration of these, it may be stated that a German author, Herr Frenzel, has recently contended for the Celtic origin of the Peruvians and of the Aztecs of Mexico.

The writer's Peruvian material has not been sufficiently abundant to enable him, as he would gladly have done, to compare the Canary Island words with it to any extent. An intimate acquaintance with the obscurer forms of Peruvian speech would be necessary for such a purpose. Ordinary vocabularies have little to tell of Eisteddfods, rock-pools, piebald goats, stone implements, and problematical plants, such as fill the Guanche lists. He has, therefore, been compelled to compare the usual vocabulary for comparative purposes with corresponding terms in the Celtic languages to the amount of some three hundred. These appear in the Appendix, after the Canary Island comparative table. Accidental coincidences in the form and sound of words may be found, to a certain extent, in all languages, however remotely disconnected. One wonders, therefore, at the statement in the Peruvian Antiquities of Messrs. Rivero and Tschudi: "The analogy so much relied on between the words of the American languages and those of the ancient continent have induced us to make an approximate estimate, as far as our means would permit, of the numerical value of the idioms of both hemispheres; and the result was, that from between eight and nine thousand American words, *one* only could be found analogous in sense and sound to a word of any idiom of the ancient continent; and that in two-fifths of these words, it was necessary to violate the sound to find the same meaning." The one word evidently that the learned authors have discovered is "the Quichua word for the sun, *Inti*, which unquestionably derives its origin from the Sanscrit root *Indh*, to shine, to burn, to flame, and which is identical with the East India word *Indra*, the sun." The real fact of the case is, that the supposed solitary *inti* is a contraction and attenuation of the Welsh *ganaid*, the sun, and appears to relate to an old Irish title of that heavenly body, which is *ion*. The reason why pretended philologists have not been able to discover relationships in languages is their ignorance of the Old World tongues that are suitable for the purpose. The gospels of St. Matthew and St. John have been translated into African Berber, and that of St. Luke into Peruvian Aymara, without the translators being conscious that they were dealing with Celtic languages, so little are these languages made use of in the sphere of comparative philology.

The three words which first drew the writer's attention to the Celtic element in Peruvian, are the following:

sheep,	ccaura, *Aymara.*	caora, *Gaelic.*
lamb,	una, "	uan, "
goat,	paca, "	boc, "

The Quichua word *llama*, which denotes the diminutive camel of South America, is the old Irish *lumhan*, a lamb, and the Aymara *pilpinto*, a butterfly, is the Welsh *balafen*. A few words in which d or t is the chief factor will exhibit the Celtico-Peruvian connection:

earth,	idatu, *Cayubaba.*	tudd, *Welsh.*
father,	tata, *Aymara*, Etc.	tad, "
father-in-law,	ttosi, *Atacama.*	tadcu, "
house,	uta, ata, *Aymara.*	ty, "
seed,	atha, "	had, "
woman,	tana, *Itenes.*	dynes, "

Here is a series of verbs:

to beat,	panay, *Quichua.*	pwnio, *Welsh.*
	huacta, "	chwatio, "
to bind,	huatay, "	caethiwo, "
to drink,	upiya, "	yfed, "
to enter,	mantana, *Aymara.*	myned, "
to go,	huma, "	imich, *Gaelic.*
to hate,	coysma, *Atacama.*	casau, *Welsh.*
to heal,	callana, *Aymara.*	gwellau, "
to kiss,	quischama, *Atacama.*	cusanu, "
to know,	yatina, *Aymara.*	adwaen, "
to learn,	yaticha, "	dysgu, "
to load,	penaclo, *Atacama.*	pynorio, "
to love,	qquipi, "	hoffi, "
to run,	huayra, *Quichua.*	gyru, "
	paway, "	ffoi, "
to sew,	chucuna, *Aymara.*	gwnio, "
to sleep,	iquina, "	huno, "
to speak,	arusi, "	areithio, "
to teach,	yatichana, "	addysgu, "
to wash,	harina, "	glanau, "
	maylla, *Quichua.*	ymolchi, "

Here are eighteen common verbs perfectly corresponding, after centuries have separated the branches of the parent stock that speak them in the New World and in the Old. Like the Guanche tongue, the Peruvian is Cymric rather than Gaelic; and, like the former, the Peruvian frequently omits the liquids r and l, as in the following:

angry,	pina, *Quichua.*	ffrom, *Welsh.*
to break,	pakiy, "	bregu, "
door,	puncu, "	porth, "
feather,	puyu, *Aymara.*	plu, "
flower,	pucher, *Atacama.*	fflur, "
hail,	chijchi, *Aymara.*	cesair, "
hot,	capi, *Atacama.*	craf, "
house,	puncu, *Aymara.*	ffronc, "
jaw,	kaki, *Quichua.*	cargen, "
night,	haipu, *Aymara.*	gosper, "
strong,	capac, *Quichua.*	cryfach, "
thigh,	changa, *Quitena.*	clun, "
throne,	tiana, *Quichua.*	tron, "

This species of phonetic decay is a common feature in the history of language all the world over, and finds its most familiar illustration in the Italian and other Romance languages as compared with the original Latin.

The English word "writing" is rendered by the Quichua *quippu.* As a matter of fact, so far as is known, the Peruvians possessed no system of writing, but preserved their records and tallies by collections of knotted cords of different colours, to which they gave this name. It is the Welsh *coffau*, to record, a term that exactly expresses the object of the cords. The word *hualpa* or *atahualpa*, which enters into the composition of the titles of the last three Incas, often written *hualppa*, denotes a fowl, and is a common Celtic designation for many kinds of birds, such as the Gaelic *gealbhan*, sparrow, *gealbhan-cuillion*, bulfinch, *gealbhan-garaidh*, hedge sparrow, *gealbhan-lion*, linnet, *gealbhan-sgioboil*, bunting, and the Welsh *golfan*, sparrow, *golff*, swallow, and *gylfinog*, curlew, whence the lowland Scottish "whaup," which is just *huallpa*. While on the subject of birds, it may not be amiss to remark that English dictionaries set down *condor* as Spanish, and some Spanish lexicons at least claim it as such. It is really the Peruvian name of the vulture of the Andes, and is a corruption of the Welsh *gwylldyr*, a vulture, to which the Gaelic *gairrfhiach* only half corresponds. The Latin *vultur* would thus appear to be of Celtic origin. The combination of the guttural and the labial in the Welsh *gwi* explains the rise, out of the same common original, of such apparently divergent forms as *condor* and *vultur*. A corruption of another kind appears in the Atacamena *quelechar*, truth, as compared with the Welsh *gwirder;* and in *ualcher*, bad, wicked, in the same dialect, in comparison with the Welsh *ysgeler*. A tendency to replace dentals by sibilants is found in the Aymara *cachomasi*, friend, and *arusi*, to speak, corresponding with the Welsh *cydymaith* and *areithio*. Many other points of comparison are worthy of note, but the vocabulary must speak for itself This much is certain, whatever syntactical modifications have supervened in the original language, by virtue of Iberic or other Turanian admixture, the bulk of Peruvian speech is Celtic, and that almost exclusively, yet not completely, Cymric.

The problem remains, how and when did Cymric Celts find their way to the far western shore of South America? The Peruvian annals, preserved by Montesinos, Garcilasso de la Vega, and other historians, give no credible account of the advent to the vicinity of Cuzco of the ancestors of the present native population. But in both the histories named, the year 1062 is affirmed to have been the beginning of a new order of things, subsequent to a period of great corruption and decline of royal authority. Then Inca-Rocca or Sinchi-Rocca founded the dynasty which continued in power till the time of the Spaniards. This date of 1062 is very significant, for in that year, Huemac III., the last Toltec king of Tollan, in Mexico, began to reign. Two years later, according to the Mexican historians, he and his Toltecs fled before their Chichimec enemies into the south. Some records affirm that he was pursued and put to death ; others that he escaped, and, in a far off land, established a new kingdom of the Sun. The only empire in the south rivalling that of Mexico, with the exception of the states of Oaxaca, Yucatan and Guatemala, which were no doubt in existence in 1062, would be that of Peru, which is then said to have had its beginning. Had the Peruvians called themselves Toltecs, the migration might be taken for granted, but there is no evidence that they did so.

Anahuac was the name of the Mexican region in which the Toltecs founded, in 717 and 752, the kingdoms of Culhuacan and Tollan. Shortly before the first date, they and the Olmecs, who have no separate history, came to Potonchan on the east coast, from a region far beyond the sea, called Chicomoztoc or the Seven Grottos. They are said to have passed through the channels of the Bahamas, to have left some of their seven crews on the shore of Florida, and to have coasted along the Gulf of Mexico till they came to their landing place and permanent settlement. The Toltecs were large, well-made men, almost as white as Europeans, and fully clothed. They were sun-worshippers and offerers of human sacrifices. In the arts of architecture, metal-work, the manufacture of cloth and many other useful articles, they excelled, and were skilled in music and in medicine. They possessed monastic institutions for men and women, had a great variety of religious festivals, and a class of learned men called *amoxoaquis*. Their history, however, as related by Aztec writers, is so corrupted by the large infiltration of very ancient traditions, such as that of Quetzalcoatl, which belongs to a period thousands of years in the past, as to be almost incapable of disentanglement, save in its chronological outline.

Apart from the matter of physical stature and complexion, for which the present Peruvian, in a state of subjection and degradation, furnishes no trustworthy data to compare, the above description of the Toltec is applicable to the subjects of the Incas. They were great masons, which as a rule Turanians are not, being carpenters instead, and built both enormous megalithic structures, and edifices of hewn stone, besides constructing admirable roads and bridges. They excelled in the textile and metallurgic arts. They worshipped the sun, and had monasteries and vestal houses devoted to that deity. Their Amautas or wise men correspond to the Toltec Amoxoaquis, and these cultivated music, astronomy and medicine, in the first and last of these far excelling the Mexicans. Their year consisted of twelve months, while that of the Aztecs and Central American peoples contained eighteen. The names given by the Amautas to some of the plants of their pharmacopœia have survived, but their botanical names are only known in a few cases, and these to South American botanists. There is a valerian among them, the *Valeriana coarctata*, and it is called *Huaritura*. The European species, *Valeriana officinalis*, bears the Irish name *Carthan-curaigh*, out of which *Huaritura* may have been evolved. A euphorbia, species not mentioned, was *Huachancana*. The Irish title of the *Euphorbia tithymalis* is *Buidhe-na-ningean*, in which the final *ningean* answers to the *ancana* of the Peruvian word. The uncouth term *Llamapnahui* denotes the *Negretia inflexa*, a plant unknown to the writer. It is not unlike the Irish *Lion-an-abhain*, the "Ranunculus aquatilis," but its fuller form, and its use in medicine, suggest the *Lion-na-mbean-sighe*, which is the name of the "Linum catharticum," or purging flax. The "Krameria triandria" is called *Maprato* and *Ratana*, and these names suggest the Irish *Bior-nam-bride*, the dandelion, and *Liathan*, the not-unsimilar marigold. The Peruvian title of the "Molina prostrata" is *Parhataquia*, and *Baladh-chnis* is the Irish yellow ladies bed-straw, or "Galium verum." The plant *Chinapaya* is not identified, but it is probably the *Chenipa* of the Canary Islands, the Irish *Cnaib*, and the well-known "Cannabis sativa," or hemp. Another unidentified plant is the *Chenchelcome*, the nearest to which is the Irish *Samharcain*, the primrose, "Primula veris." The *Panqui* may be the *Fanaigse* or dog violet, "Viola canina"; the *Fuinseach* or enchanter's night-shade, Circœa lutetiana; or *Puineoga*, sorrel, "Rumex acetosella." The names, *Checasoconche* and *Chucumpa*, invite comparison with the Irish *Sgeach-chumhra*, sweet-briar, and other *sgeachs* and *sgeachanachs*, which are thorn bushes. Another plant *Mulli* may be the *Mol* of the Canaries, an aromatic shrub, or the *Amuley* of the same, an herb, for which equivalents are offered in the vocabulary. The *Matecllu* may be the Irish *Meastorc-caoil*, St. John's wort or "Hypericum androsaemum"; or the *Bodan-na-cloigin*, yellow rattle or "Rhinanthus cristagalli." The nearest equivalent of *Tasta* is the Irish *Saisde* which denotes sage, "Salvias" of many species. The name *Masca* suggests many Irish botanical words, such as *Masog*, a small red berry; *Miosach*, another name of "Linum catharticum"; *Measog*, the acorn; *Meascan*, butterwort, "Sanicula"; and *Feusogach*, the bearded capillary or "Adiantum." The same is the case with *Chillca*, for the Irish *Caolach* denotes fairy flax, *Ceolagh*, purging flax, *Cloch*, henbane, *Sailchuach*, violet, *Salchuach*, "Viola odorata," *Seileach*, willow, etc. Finally *Huacra-huacra* resembles *Eochair*, a sprouting plant, *Ioclus*, healing herbs, *Oighreog*, wild strawberry, and *Uachdar*, "Sanicula montana."

The Peruvians called their kings Tahuantin-Suyu-Capac, or Lords of the four quarters of the earth. The title is a very old one, as certain ancient Babylonian monarchs termed themselves kings of the four regions, and others commemorated their victories over the four races or *kiprat arba*. It is to be remembered also that there was a Kirjath Arba in Palestine, it being a name of Hebron, in what afterwards became the domain of the tribe of Judah; but it is to be noted that, while *arba* is Semitic, and even Mongol for the number four, it is not so in Hittite. The Basques are familiar with the term, as appears in the Rev. Wentworth Webster's Basque Legends, one of which represents Mr. Laur Cantons as seeking a vine-dresser's daughter in marriage. Laur Cantons is the Four Quarters. There was an ancient hero called Arba, the father of Anak, whose sons, the Anakim, were Sheshai, Ahiman, and Talmai (Joshua xv., 13, 14). They were Hittites, and pertained to the Gesshurite branch of the Zerethite, Cherethite, or Dardanian family. Talmai, king of Gesshur, the father of Absalom's mother, was of this race. The name Anak became a title, and was borrowed by the Greeks in the form *anax*, to denote "a prince." In the New World it became Inca, and the Inca was suitably Lord of the Four Quarters. Arba is a common name in the Iberic inscriptions of the Canaries, and Telama or Talmai is also found in them as a princely name. Jackson, in his version of Shabeeny's Travels, says: "The opinion of the author of the History and Conquest of the Canary Islands, is, that the inhabitants came originally from Mauritania, and this he founds on the resemblance of names of places in Africa and in the islands": "for" says he, "Telde, which is the name of the oldest habitation in Canaria, Orotaba, and Tegesta, are all names which we find given to places in Mauritania and Mount Atlas. It is to be supposed that Canaria, Fuertaventura, and Lancerotta, were peopled by the Alarbes, who are the nation most esteemed in Barbary; for the natives of those islands named milk *Aho*, and barley *Temecin*, which are the names that are given to those things in the language of the Alarbes of Barbary." The Al-Arbes as founders of places called Telde seems to suggest the presence of the Toltecs in Barbary and the Canary Islands. Immediately opposite the African coast in southern Spain dwelt the Iberic Turdetani in the days of the classical geographers. Strabo calls them "the most intelligent of all the Iberians; they have an alphabet, and possess ancient writings, poems, and metrical laws, six thousand years old, as they say." Whether

Pliny's four jurisdictions of the region in which they dwelt was native, or the creation of the Roman conquerors cannot be decided.

The epomym of the Turdetani was an ancient Hittite prince named Zereth, from whose appellation, Zareth Shachar, Zaretan, and Zartanah, in Palestine were called. The doubtful nature of the initial letter as mediating between a sibilant and a dental, led the Egyptians, who had good knowledge of his descendants, so to write their name upon the monuments that decipherers have variously rendered it by Sardinian, Dardanian, and Cretan. In the Old Testament, they are called Cherethites; they colonized and named Crete; and some of their descendants are the Kurds of what was once Assyria. It is also more than probable that Sardinia received its first colony from this adventurous race; and the Chronicon Paschale states decidedly that the Dardanians were descendants of Heth. The Turdetani were Spanish Dardanians. The final *ni* of Dardani was the old Hittite plural; the name of the people was Zereth or Zareth, Dart or Dard, Telt or Teld. Hence arose the words Telde and Toltec, identified in various quarters with the names Arba and Anak. Another word that accompanies this race is the ancestral Hittite name Ashchur, that of the father of Zereth. The *anaktes paides* of the Greeks were the Dioscuri; the Basque language is the Euskara; and a frequently recurring Inca name is Huas-car. The Umbrian Engubine Tables speak of the *trifor Tarsinater, Tuscer, Naharcer, Iapuscer*, or the threefold Tyrrhenians, Euskara, Navarrese, and Guipuscoans. The men of Navarre or the Naharcer, became in part the Nahuatl or Navatl of Mexico. It is necessary now to consider the Celtic element in relation to the Iberic Toltecs.

From an early period, yet by no means so far back in the past as some capricious German explorers would have the world believe, the Sumerians occupied a position on the page of history. Urukh or Orchamus was king of Sumir and Accad, as was his son Dungi or Tarkhun-dara, together with Burna-Buryas, Ulam-Buryas, and many more. The Sumerian name is Zimri in Jeremiah xxv., 25, where it is united with Elam and the Medes, and it is well placed in historical time, in spite of adverse forms of biblical criticism, as that of the descendants of Zimran, the eldest son of Abraham by the Perizzite princess Keturah. The Hebrew Zimran or Zimri, "celebrated in song," stands in intimate philological relation with the Gaelic *amhra, amhran*, "a poem or song." The Zimri are mentioned along with the Elamites on the monolith of Samas-Rimmon of Assyria, a contemporary of Ahab, Jehu, and Hazael of Syria; and on the Black Obelisk of Shalmanezer

II. The Persians knew them as the Gimiri, the Greeks as the Cimmerians, and they are in part represented by the Cymri or Welsh people of the present day. Dungi or Tarkhun-dara, the son of Urukh, wrote a letter to Amenhotep IV., the Pharaoh of Tel-el-Amarna, in pure Celtic, asking for the hand of his daughter, the princess Akh, called in Egyptian Ankh-nes-paaten, whom he afterwards married, and by whom he became a Pharaoh under the title of Tutankh-Amen. The Celtic tongue in which he wrote was the Sumerian or Zimrite. A descendant of Zimran, and father of Urukh, or more properly Orchamus, was Peresh, the Buryas of the Babylonian monuments, and the brother of Orchamus was Ulam-Buryas, or Ulam, the son of Peresh. This Ulam figures in Irish legendary history as Ollamh, whom the old chronicles call Ollamh Fodhla, and represent as a great lawgiver and patron of learning. The word *ollamh* came to mean a doctor or professor of any kind of learning, as it does to the present day. A son of Ulam was Bedan, the Phaethon of the Greeks, fabled to have been drowned in the Eridanus or Padus named after him, the Eridanus being the Jordan of Palestine. This name so far has not been clearly identified on the oriental monuments, but it survives in Greek story. The second Ilus of the Dardanian line was his father Ulam, who had married into the Zerethite family of Zareth-Shachar on the Dead Sea, and had thus acquired supremacy over the Dardanians. The son of Ilus was Laomedon, which truncated and apocopated word represents Ulam-Bedan, in an inverted or Turanian order, meaning Bedan of Ulam, but which in Celtic syntax should read Bedan-Ulam. As a geographical term, it survives in Bodon-hely of Hungary, known to the classical geographers as Ulmum of Pannonia, through which country the Boii and other Celts passed on their way to the west and north ; and also in the more western combination of Baden with Ulm of Wurtemburg. Potonchan, where the Toltecs and Olmecs landed in Mexico, Peten, and the Votan of legendary American history, all have reference to the ancient fame of Bedan, son of Ulam ; and the Bladud of British history, who flew like Phaethon and was dashed to the earth, but who built Caer-Badus or Bath, is a corruption of Bedan, since Nennius places Badon hill at Bath. The Ulams or Ollamhs were the Olmecs of Mexican story, who were confederate with the Toltecs. Did we know more of Guanche history, their name and that of the Bedanites might appear in the Canary Islands. The Celtic Vettones in Strabo's time dwelt side by side with the Iberic Turdetani in Spain. It is not beyond the reach of possibility, that the Latin name of the Canary archipelago, "Fortunatæ Insulæ," may be a corruption of "Fotunatæ Insulæ," or the Islands of Votan or of the Votanides.

A larger name, however, marks this branch of the Celtic family. It is the Zimrite, Sumerian, Gimirian, Cimmerian, or Cymric. As the Gaelic *amhra*, *amhran* represents exactly the Hebrew *Zimri*, *Zimran*, one may expect to find the initial sibilant or guttural absent at times. The Berbers of north-western Africa, with whom it is now generally agreed that the Guanches were most intimately related, had, and probably have to-day, tribes called Zimuhr and Amor. Of the Zimuhr, it is said: "They are a fine race of men, well grown and good figures; they have a noble presence, and their physiognomy resembles the Roman." And of the Amor it is recorded: "When the Sultan Muhamed began a campaign, he never entered the field without the warlike Ait Amor, who marched in the rear of the army; these people received no pay, but were satisfied with what plunder they could get after a battle; and accordingly, this principle stimulating them, they were always foremost in any contest, dispute, or battle." Gomera, the name of one of the Canary Islands, favours the connection of the Guanches with the Zimuhr of Africa and the Cymri, as the language of their vocabulary has already done. In Peru the tribe whose form of speech most closely approaches the Welsh is that of the Aymaras. It almost follows that the Peruvian Aymaras are the Mexican Olmecs under a larger designation. The Aymaras, according to Forbes, claim to have had an older and more advanced civilization than the Incas, and they were undoubtedly the masons to whom Peru owes its massive stone remains. Dr. Tschudi erroneously supposes the Aymaras to have been the tribe with whom the Incas originated. He says: "The crania of these people present differences equally remarkable, according to their respective localities, and particularly in the contour of the arch of the cranium. It is proper here to remark that there is a very striking conformity between the configuration of this race and that of the Guanches, or inhabitants of the Canaries, who used also the same mode of preserving the bodies of their dead." The latter reference is to mummification, common to the Guanches and the Peruvians. According to Forbes, the Aymaras wear their hair very long, the men plaiting theirs into one pig-tail and the women into two. This was a Guanche custom as Pégot-Ogier remarks. He also says that, "the oven of the Guanches was a hole under ground like that of the Peruvians." This writer compares a Guanche temple with similar remains at Carnac in Brittany as proof of their Celtic origin. Megalithic structures of the same character have been found throughout the Berber area, such as that at Bless in Tunis, described by Mr. Frederick Catherwood in the Transactions of the American Ethnological Society. The chief seat of the Aymaras was about Lake Titicaca, and a short distance from its shores stand the ruins of Tihuanaco, consisting of a large group of immense stones, each from six to seven yards high, placed in lines at regular intervals. It has been fitly termed "a Peruvian Stonehenge," and a tradition prevails concerning it identical with that which ancient chroniclers preserve regarding the famous English structure, namely, that it was erected in a single night by an invisible hand. Another historical parallel, that no longer seems strange, occurs in the Peruvian story of the war between the Inca Yupanqui and his warlike subject Ollanta, in which the Inca's General acted the part of Sextus Tarquinius in Livy's account of the taking of Gabii, and that of Zopyrus in Herodotus' relation of the capture of Babylon. As the original Ulam was the uncle of Dungi, who calls himself Tarkhun-dara, or Tarquin the second, he may have been the Ollanta of the legend. But there remain to this day a town and the ruins of a strong fortress called Ollanta-Tambo, the latter perched high up in a narrow tract on the banks of the river Urubamba. At any rate, in the word Ollanta survives the Olmec name. The final *ta* of Ollanta is a dialectic variation, corresponding to that which changed the Welsh *balafen*, butterfly, into the Aymara *pilpinto.*

Having thus cleared the way for explanations, it is time to indicate traces of the Olmecs in the vicinity of Mexico. Referring to the statue of Chaac-Mol at Chichen-Itza in Yucatan, Professor Short says, in his "North Americans of Antiquity": "he is adorned with a head-dress, with bracelets, garters of feathers, and sandals similar to those found upon the mummies of the ancient Guanches of the Canary Islands." And again: "Dr. Le Plongeon observed that the sandals upon the feet of the statue of Chaac-Mol, discovered at Chichen-Itza, and of the statue of a priestess found at the island of Mugeres, are exact representations of those found on the feet of the Guanches, the early inhabitants of the Canary Islands, whose mummies are occasionally met with in the caves of Tenerife and the other isles of the group." Now, the Mayas, Pokomams, and other Yucatecs, belong to a race entirely distinct from both the Iberian and the Celt, being of Malay-Polynesian origin. Brasseur de Bourbourg, quoting a Quiche document, informs us that there was in the vicinity of Yucatan a little kingdom of Peten, the name of which is neither Maya nor Quiche, but recalls Bedan and Poton-chan. The chief of this principality was Canek, a handsome and warlike young monarch, beloved by the daughter of a king and the most beautiful woman of her time, but who, against her will, had been betrothed to the king of Chichen. While the chief nobles of the latter's court were bringing home the bride in joyous procession, Canek fell upon them and carried off the princess. Then gaining the sea shore, he embarked with his prize and bore her away to his kingdom of Peten. This is the Celto-Dardanian story of Helen, taken by Paris from Menelaus, the aged bridegroom, and carried to Troy by way of Sidon. In the Celtic or British story, as told in the Mabinogion and by Geoffrey of Monmouth, the lover of Helen was Conan Meriadoc, who would have taken her from Maxen Wledig, to whom her father Eudav or Octavius, had married her. This Conan is the Conn of Ossian and of Campbell's Tales of the West Highlands; and, in Irish story, is Conn of the hundred battles, the father of an Art or Arthur. As Paris was called Alexander, so in the Indian Puranas he bears the name Harischandra, and his son that of Rohita, the Irish Art. Whatever truth may lie in the varying details of his story, this hero was a historical personage, being Baal-chanan, the last but one of the ancient line of kings, who, before the time of Moses, reigned in what subsequently became the domain of the Edomites. From the Chanan part of his name came the British Conan, the Gaelic Conn, and the American Canek. The British addition Meriadoc, like the Gaelic Murdoch and Murtough, is a Turanian or Hittite word, Merodach or Berodach, meaning the son of Beor, who was Bela or Baal, whence Baal-Peor; Merodach, therefore, is a synonym of Baal, and Conan Meriadoc is virtually Hannibal. The Greeks cut down the full name Baal-hanan to the form of Priamos, and made him the father of the handsome libertine instead of himself, and they represented him as the son of Laomedon or Ulam-Bedan, while his true father's name was Achbor, perhaps, although this is not settled, a brother of Bedan. He reigned over the Dardanian region in which lay Zareth-Shachar, and of his race were the Celtic army leaders of the Hittites; for Achbor was the Saprer of the Egyptian monuments who ruled in the time of Ramses I., and his son was called Mauro-sar. But the sons of the latter, one of whom gave his daughter in marriage to Ramses the Great, were Mauthanar and Khetasar. The first the Greeks called Antenor, and the second, Ramses' father-in-law, receives but scant mention as the Cytissorus or Cytorus of Herodotus and Strabo. Baalhanan, as the son of Achbor or Saprer, must have been the elder brother of Mauro-sar, and thus the uncle of Mauthanar. With inversion of parts, for these compound names, as on a treaty of peace with the Hittites, are in Turanian order, Mautha-nar would read Nar-mautha. This is Brugsch's form of the name; Lenormant's is Maut-nur. Now this Nur-maut is Celtic, being Near-mada, "the boar pig," and, with a change of the initial dental, is Diarmaid, the ancestor of the Campbells, and the slayer of the mighty boar, by which he was himself slain. As the son of Mauro-sar or Sar-mauro, perhaps a Gaelic Ceir-bheoil, the name of the father of the first Irish Diarmaid, he could not claim the Campbell name, which was that of his uncle Chanan-baal, or Baal-hanan, with inversion of parts. They were, therefore, pure Celts who carried the story of Canek or Conan to Central America. The exigences of the Celtic proof, and no desire to refer to the origines of the family to which he has the honour to belong, have led the writer to what may seem to some a genealogical excursus. The Bu-chanans and Bu-chans are very probably of the same Baal-chanan ancestry.

The burden of proof the writer lays on the vocabularies, which present incontrovertible evidence that the language of the Guanches was, with the exception of a few loan words of Iberic origin chiefly, purely Celtic, both in vocabulary and in grammatical construction, and that that of the Peruvians, and in particular of the Aymaras, though Iberic in grammar, was very largely Celtic in vocabulary. He has also presented evidence of various kinds for the advent of an Olmec or Celtic people to the shores of America, for their presence in the vicinity of Mexico, and finally for their existence at the present day in Peru. And in many ways he has shewn that these Celts came from the Canary Islands, where they and Iberians once dwelt side by side, and from which, as Olmecs and Toltecs, they migrated in company. As to the period of that migration, there is nothing to proceed upon but the statements of the Mexican historians as to the foundation of the Toltec monarchies. That of Culhuacan began, under the King Nauhyotl, in 717, and that of Tollan, under Mixcohuatl-Mazatzin, in 752. Are these Toltec names or Aztec disguises? Nauhyotl means in Aztec "the four quarters," and answers to the Peruvian title of the Incas. The old Basque term for *laur*, four, was *nora*, as several Etruscan records testify, perhaps the original of the Aztec *nauh* or *nahui*. To make the four quarters, the Basque offers the addition of *alde*, *une*, *gune*, *tegi*, *toki* or *ziri*, each meaning "a place, region, or quarter." A name in the eleventh inscription of Hierro, published by Mr. O'Shea, is rendered provisionally Notara. As the Basques use *lau* as often as *laur* to denote four, the Iberians of the Canaries may have abbreviated *nora* to *no*. Then *tara* or *tari* would represent the present B. *ziri*, and the Japanese *atari*, a region; the whole as *no-tari*, giving "the four regions." Mixcohuatl or "the cloud burst" is a purely Mexican word: *buhumba* would be the Basque equivalent, and *bofu*, the Japanese.

The land of Chicomoztoc or the Seven Grottos, whence came the Toltecs and Olmecs, admirably describes the Canary Islands both as to number and the peculiarities of their rock formation. These the emigrants sailed from some time in the early part of the eighth century. Now 714 was the year of the conquest of Spain by the Arabs, and before this they had taken possession of Northern Africa, whence it was but a short voyage to the Canaries. Not having access to any of the Tarikhs or Chronicles of Maghreb and Andalus, or Western Africa and Spain, the writer is unable to state when the Canary Islands were invaded, and Sir William Muir's admirable work on "The Caliphate" makes no mention of them; but Sir William Ouseley's statement, in the preface of the anonymous translator of "Sadik Isfahani," that the Mahometan geographers calculated their longitude from the Fortunate Isles eastward, would evidently indicate an ancient acquaintance with them as the world's Ultima Thule in the west. Whether the Arabs were the invaders, or the Berber tribes that refused to obey the authority of the Koran, fleeing before their arms, sought refuge in the islands, a pressure of a hostile people took place some time between the years 700 and 717. The result in any case was the westward migration of, in all probability, the whole of the Iberic population, and of a very considerable number of the Guanches. If any of the former, who, in the time of their inscriptions, were the dorminant race, remained behind, the vocabularies both of proper names and common words, as well as what chronicles survive, indicate that they lost their identity, and forfeited their authority to the Celtic Guanches.

What were the circumstances of their long voyage straight in the line of the tropic of Cancer, will probably never be known. The reason why, on reaching the American islands and coasts, they did not take up their abode on them, was, perhaps, the same that made them leave their beautiful homes of many centuries, the presence, namely, of a hostile population on these, and their desire to lead a peaceful life in the New World. From at least 717 they built up their Toltec empire in Mexico, pressed upon from time to time by new immigrants from the north and west, until, after more than three centuries, they could bear the pressure no longer, and took up their weary travels again. Neither the Mexican account of their flight, nor the Peruvian story of their sudden appearance at Cuzco, favours the idea of a second voyage from the west coast, followed by a landing at a Southern Pacific port. It is more likely that they made their way overland, through Guatemala and the deadly Isthmus of Panama, helped here and there by lakes and rivers, until, traversing the mountains of Colombia, they found and named the Ucayali river, against whose tide they steered their way to Cuzco, and Lake Titicaca. There they soon forgot the stirring events of former national history in the task of founding a new empire. Four centuries and a-half of Peru followed the three hundred and fifty years of Mexico, as these had succeeded seven hundred and more of Canary Island life; and then the Spaniard came to conquer these brave wrestlers against adverse fate, who had never really been conquered before, and to abase their pride in the degradation of hopeless servitude. It is a pathetic story, made tenfold more so by the knowledge that they were, and are to-day, more than half of them, our kindred Celts, who, under better conditions might have emulated the best achievements and lives of Wales and Brittany.

THE CANARY ISLAND INSCRIPTIONS.

Of these Dr. Bethencourt writes as follows: "As far as my information goes, there are no other inscriptions than those already sent, and those which I now send. As regards those of the island of Hierro, it may be that some are here repeated, but I prefer this tiresome redundancy to the fear that there should be any incompleteness of material for elucidating the subject. If I do not remember amiss, M. Berthelot generalized the idea that the written characters of Hierro were Libyan, founded on the opinion of General Faidherbe. Later, some have begun to doubt the truth of this assertion, but nobody has interpreted them. The studies of Mr. Campbell not only confirm the conclusions of anthropologists, but also open up unsuspected horizons, and point out new departures in the history of the Guanche population, along highways almost closed for about four centuries. May it please God to deign to direct him in fixing his attention upon so interesting a problem.

Prehistoric Inscriptions of the Canary Islands.
Island of Tenerife.

"In reality none are known here, unless we count among them the Roman letters on the image of the Virgin of Candelaria, probably of scant historical interest, and the inscription of Anaga of our illustrious friend, Dr. Manuel Osuna, to my mind of doubtful existence as an inscription.

The Virgin of Candelaria.

"According to our chronicles, this image suddenly appeared on the shore of the little kingdom of Guimar, about the year 1390, over a century before the conquest of the island; and, in the course of the 15th, 16th and 17th centuries, they repeat with much frequency miracles wrought by it and similar objects throughout the archipelago, as well as in the said island of Tenerife; so that, after dispassionate critical judgment, there is nothing to do but accept the historical fact of the presence of the effigy during these centuries. There are those who observe that, in the fourteenth century, reports were current of the presence of vessels of many different nationalities in these seas, some pursuing legitimate commerce, but most entering them in order to perpetrate all sorts of piracy; and it is quite possible that some one of these barks may have lost the said image, may have bartered it as an article of trade, or have made a present of it out of a spirit of religion. Some of our people have given heed to the version of a friar, who asserted that the Virgin was the nymph or figure-head from the prow of a ship, through observing in the hinder part of the figure the marks of the rings whereby it was fastened. This image disappeared, being carried out to sea in consequence of a terrible inundation that visited the island in the twenty-sixth year of last century. As the theogony of the Guanches was abundantly complex, and they, beside being Sabaeans, were also idolaters, they supplanted one of their idols by this new sculptured figure which had fallen into their hands, giving to it special worship; what, perhaps, contributed to this being the counsel of a certain Guanche that, after a time of captivity and civilization, it would bring them their own land, as the chronicles relate. This is the foundation of the pious religious legends of the Virgin of Candelaria, which our people have preserved, and of its subsequent exaltation by the Catholic clergy.

"The appearance of the image, according to the ancient historians who had seen it, was as follows. It was of painted wood, compact but not very heavy, and about five hands high, along with the pedestal which was two fingers in thickness. Its colour was brown, the face of a fair size, and the eyes large and full. The head was bare, with the hair spread out on the shoulders and braided in six plaits. The female figure carried a naked infant on the right arm, which in its time grasped with both hands a little golden bird, and in the left hand was a taper painted green, with a hole in the lower part for the purpose of increasing it at will. It was fully clothed from the throat to the feet, without any opening whatever. Its cloak was of blue and gold, with much golden flower work behind, and, falling in front over the shoulders, was attached at the breasts by a coloured cord of a span wide. The left foot, a little uncovered by the skirt, was shod with coloured serge. The dress and cloak were adorned with Roman letters of green colour upon a gold ground, which nobody so far has been able to interpret. On the collar are the following :—*See Inscription I. a.*

"On the border or margin of the dress below are these :—*See Inscription I. b.*

"All are not here, for in order to give away as a relic, some one had broken off a piece of the skirt and of the pedestal. In the part of the sleeve near the left wrist are :—*See Inscription I. c.*

"The robe was girt about below the breasts, which on both sides had a very graceful effect, with a girdle of blue, on which were the following :—*See Inscription I. d.*

"The border of the cloak was of burnished gold, and the right side contained these letters :—*See Inscription I. e.*

"The letters on the border of the left side were :—*See Inscription I. f.*

"On the lower part of the cloak at the back were these letters :—*See Inscription I.g.*

"The scientific importance of deciphering these letters may limit itself to an acquaintance with one of the nations that navigated these seas in antiquity.

(The description of the supposed inscription of Anaga is omitted, since it presents no definite trace of phonetic writing).

Island of Canaria.

"Some inscriptions have been publicly talked of, as found in the ravine of Los Balos in the pueblo of Santa Lucia, and, as far as I can remember, the subject was treated either by Dr. Chil or the Señores Millares, all illustrious historians of our archipelago, but the first to make them known through the press was Dr. Verneau, about 1882, in the 'Revue d'Ethnographie' of Paris. Up to this time in which we find ourselves no one has deciphered them.—*See Inscriptions II. and III.*

"Inscriptions of the ravine of Los Balos in the pueblo of Santa Lucia in Gran Canaria. (Dr. Bethencourt's notes on the supposed written remains of Gomera are omitted, because his illustrations are mere fragments, conveying no information. His statement regarding Fuerteventura is, that no real inscriptions have come to light there.

And his carving from the island of Palma is an obscure pictograph, of which the writer has no solution to offer).

Island of Hierro.

" Besides the inscriptions which I had the pleasure of sending you, and which you have so brilliantly interpreted, in turning over some papers which I had forgotten, relating to different excursions made to this island, I came across some that I do not remember to have mentioned. In this state of doubt I take the liberty to send them.

Inscriptions of La Dehesa.

" These are engraved on strata of lava, some of a dark brown colour, others of a reddish yellow, those of the latter tint presenting themselves first when the superficial layer broke or peeled away. These same characters are either in a dark brown or in a light gray ground, which makes one suspect they were engraved at different periods, given the uniformity of the state of the rock, and the depth of the layer. But this is no place for premising, without serious foundation to support it. The characters have a very marked savour of antiquity. and we are disposed to believe that in order to trace them, stone chisels were made use of, perhaps of phonolite (clinkstone) which abounds in those regions, having been brought from other parts, and also hammers, likewise of stone. Our people are under the impression that the etchings were engraved with such instruments, and the sight of them justifies the conjecture. Moreover, we have observed no bold strokes nor sharp cuts, such as would be made by a metal chisel. These sites present all the characteristics of having been inhabited in remote ages, by the remains of curious edifices of a primitive type, which we do not describe, so as not to overload the notes, like *kitchen-middens* in increasing strata, by little altars or 'pireos,' on which the natives sacrificed live ewes and kids. In the different mounds of lava, more or less cleft by the wear of time, which has torn away from the foot of the outside slope that forms, from east to west, as it were, a semicircle, and which are found laid out on a ground more or less inclining to yellow and grayish brown, consisting of granulated lava, mixed with hillocks of sand, we copied the following inscriptions :—*See Inscriptions IV., V., VI.*

(There are over twenty altogether, but the rest are either mere useless fragments of phonetic writing, or pictographs, which the writer does not profess to decipher).

Inscriptions of Tejeleita.

"They are sculptured on prisms of basalt which form a steep rock five or six metres high. Some of them have given way. The rock faces the west, being thus protected from rain and the prevailing winds. The characters have been also traced with a stone chisel, like those of La Dehesa, though basalt is much harder than lava :—*See Inscriptions VII., VIII., IX., X.*

Inscriptions of the Port of La Caleta.

"These are also engraved on prisms of basalt which form a wall about three metres high, as well as on others fallen away from this to the sea-shore. In these sites are vestiges of the habitations of the Bimbapes or aborigines of Hierro, shell-heaps and *kitchen-middens*, small altars, stew-holes or 'pireos' for sacrifice. Many of the prisms have fallen away :—*See Inscriptions XI. to XXVI.*

Inscriptions of La Candia.

"They are engraved in a cave which was formed at a *salto* or jutting rock at the base of the ravine (*barranco*)."—*See Inscription XXVII.*

There are two more in this group, but they are too imperfect to admit of satisfactory decipherment. Such then is the material which Dr. Bethencourt has furnished to shed light upon the ancient history of the Canary Islands. Having studied the Northern Turanian characters as found in many lands and ages, from the Sinaitic of hoar antiquity to the Etruscan and Celt-Iberian of a pre-Christian century or two, and from the Buddhist Indian of the fourth century B.C. and the Siberian of the fifth A.D., to those of the American Mound-Builders as late as the thirteenth century, the writer had no difficulty in identifying the lines of the Virgin of Candelaria, and the ruder outlines of the rock-faces, with what is best known as Etruscan script, although it is morally certain that its writers in the Canaries never saw Etruria. Copies of the interpretations now submitted have been sent not only to Dr. Bethencourt, to whose courtesy the writer is indebted for his knowledge of the inscriptions, but also to Mr. O'Shea, the well-known author of "La Maison Basque," "La Tombe Basque," and many other valuable works in English, French and Spanish, who will submit them to the critical judgment of the best Basque scholars.

This it is necessary to state, because, while there are many in Canada who can pass a pertinent opinion upon the Celtic side of the argument presented in this paper, it is doubtful if there be one possessed of sufficient knowledge of Basque to appreciate the simplest and most evident coincidences between that language and the subject matter of the inscriptions. The writer may, perhaps, be permitted to insert here one of the many flattering testimonials that have come to him, alike through printed publications and literary correspondence, as to his proficiency in Basque studies. Mr. O'Shea, after other kind things, remarks: "Our native Basque scholars cannot account for your thorough acquaintance, not only with the modern forms of their language, but with that also of the primitive roots." And yet the writer was once publicly taken to task in this Institute for presuming to know Basque!

The old Turanian characters are not alphabetic, but constitute a more or less imperfect syllabary, imperfect because in many cases one character represents all the powers of a consonant, for instance, Λ, which may be ra, re, ri, ro or ru. In transliterating, the equivalents of the characters are grouped, as nearly as convenient, first in the order in which the characters appear in the inscriptions, and afterwards in their order of modern reading. A table of phonetic values of the characters, and a grammatical analysis of the texts, is appended to the paper, so that those who are curious to examine the method of interpretation may have every facility for so doing. Of the former there are necessarily two parts, inasmuch as Dr. Bethencourt's Roman letters on the Virgin of Candelaria present that Graeco-Roman aspect of the Etruscan characters, which has misled almost, if not all, interpreters to assign to them the phonetic equivalents of the European alphabet, which naturally has led to no results.

INSCRIPTION I.

The Virgin of Candelaria.

Line a.—ko i en tu po no en tu me ne ra au.
koi entu pono entu Menera au.
desire hear grief hear Menera this.

"Let this (goddess) Menera hear the prayer, hear the sorrow!"

(The ornamental cross at the end of this line and in the following lines is a mere punctuation mark).

Line b.—ni ar ba mi, au ra ne ka i ka i, ba me ne ra er en ai.

ni Arba imi, aur ne Kai Kai, ba Menera errunai.

I Arba place, child to Caius Caius, if Menera will pity.

"I, Arba place (this) for the child Caius, if Menera will compassionate Caius."

Line c.—so to be ri u ga ne ka ai ta en tu ka i ba ra ka ko.

Sotoberri uga neke aita entu Kai barka ka.

Sotoberri mother weary father hear Caius forgiving by.

"Hear the mother Sotoberri, the weary father, by forgiving Caius."

Line d.—mi ra er mi to ri se me ma gu re er en.

mira erimi etorri seme ema gure erren.

spectacle cause place come son give our compassion.

"Coming to cause to set up a spectacle, to give the son our compassion."

Lines e.—ma sa mi, u ga ra er ka an re, au ra ne la ka tu ne mi, ar ba be ne ka.

emaitsa imi, uga ra erruki anre, aur ne lekatu ne imi Arba be neke.

gift place mother to pitiful lady child to please to place Arba under weary.

ga be au ka ri di o me te ba hi ga be ai ta au ka i di o er ka.

gabe au ekarri dio ematu bahi-gabe aita au Kai dio erruki.

deprivation this to bear he calm pledge deprived father this Caius he pities.

"Placing a gift to the mother (and) to the child of the compassionate lady, to be pleased to put (strength) under Arba this weary loss to bear. Let him calm this pledge-deprived father; let him compassionate Caius."

Lines f.—pa be tu mi au ra ka ri, ni ga be tu mi koi, en tu ka mi ra au ra, be re be ha be ha ka er ka.

pabetu imi aur ekarri, ni gabetu imi koi, entuka mira aur bere beha behaka erruki.

to help place child carry I bereaved place desire in hearing regard child own behold in beholding pity.

ar tu be ha me ne ra au do i be kai er ka ga go ra te ka.

artu beha Menera au doi be Kai erruki gogoratu ka.

take glance Menera this justice under Caius compassionate remembering by.

"I, the bereaved, place the desire to have help to carry the child (in remembrance). In hearing, look at the child; behold (thine) own (and) in beholding, pity. Take a look, thou Menera; with justice compassionate Caius by remembering.

Line g.—ka ol au mi ni o, er ka ka ni o du en ar ba mi o bi ne au pa be ba.

achol au imi nio, erruki egi nio duen Arba imi obi ne au pabe ba.

care this place I to him, pity make I to him it is who Arba places tomb to this help place.

"I place this attention to him; I make pity for him; it is I, Arba, who set at the tomb a place of help."

This inscription, or series of inscriptions, is as Etruscan as if it had come from a Tuscan cemetery, in which the bones of many a Caius lie. The image is that of the goddess Menera and her son, the first of whom can hardly be Minerva, a virgin deity, but some mother goddess, whose name is compounded of the Basque *men*, "power, authority." The name of the father of Kai or Caius, namely Arba, is, as has already appeared, one of the chief personal designations of the royal line of the Canary Island Iberians, who named the Teldes, and in migration became the Toltecs. There is, therefore, no reason to suppose the image foreign to the islands, but rather is there reason to regard it as a survival of the mortuary votive offerings made by their Iberic inhabitants in ancient times. The image of Menera and her son, with the inscribed prayer, was originally attached to the sepulchre of young Kai or Caius by his father, Arba, and his mother, Sotoberri, as a phylactery. Wherever the Celtic Guanches first obtained it, there seems to be little doubt that they were ignorant of its real nature, and regarded it as one of their mother-goddesses, that the Abbé Banier, in his "Mythology Explained by History," and other writers, show to have been common throughout the Celtic area of Europe. Judging it alike by the form of its characters and the simplicity of its language, the image and its inscription must have been of much antiquity, perhaps a century before the Christian era. The grammatical forms *dio*, *nio* and *duen* denote attention to literary style such as does not characterize many of the inscriptions of the same region.

The next inscription is from Canaria, and reads in Japanese order from top to bottom, but, unlike Japanese, the columns begin at the left.

INSCRIPTION II.

ge	u	go	ko	go
ma	tsi	ma	i	ma
ma	ta	ta	ta	ma
si o	ta	ya	ya	ma
	ya ba	so	go	ma
			be	

Age Mama zio utsite Taya ba Goma Taya so,
indicates Mama to him to leave Taya if Goma Taya regard.

koi Taya jabe Goma ema Mama
wishes Taya lord Goma gives Mama.

"Mama indicates to him (that he will) evacuate Taya, if Goma desires the regard of Taya. Mama gives Goma (to be) lord of Taya."

As the Iberic name Telde remained in the islands after the departure of the Toltecs, it is probable that they left other names, which the Guanches did not supersede, just as many Pictish and other Iberic names survive in the British Islands. Such in Scotland are the Goldenberry hills, whose true name was the Basque *golde-nabara,* "the ploughshare." Places called Taya and Taha are not uncommon in the Canaries. The inscription records the cession of an inhabited tract, so called, by a chief named Mama to another bearing the designation Goma, or perhaps *gomu*, the remembrance. That it was inhabited is indicated by the *so* or regard of its people as a factor in the cession. Taya may be the modern *itai*, a scythe, reaping hook, so called on account of its appearance, like the Greek Zancle or Messana. The prefix of vowels, such as the *i* of *itai* seems to be a modern feature of language; in Etruscan days, the verbs "to give" and "to place" seem to have been *ma* and *mi*, not *ema-n* and *imi-ni*, as now.

INSCRIPTION III.

This is to be read in the same order as No. II.

pi		
mo	ra	
ta	au	
ya	do	de
au	i	be
bisi	ta	sisa
ta	ka	be
te	ma	hi
	tu	

Pimo Taya au bizitate arau doi Taka ematu debe esatz behi.
Pimo Taya this inhabitant right just Taka gives forbidding word cows.

"Pimo, this inhabitant of Taya, according to law, gives Taka notice, forbidding cows (to trespass)."

This also is an inscription of Taya belonging to a different period from that of the first Pimo, which is the Etruscan or old Basque numeral "one," and may here mean *princeps*, replacing the former Mama and Goma. The inscription is imperfect, the word "to trespass," "to pasture," "to seek shelter," being doubtless defaced as is often the case with prohibitory notices. This is not the only inscription relating to cattle, which appear, in ancient times to have constituted the chief wealth of the islands.

The remaining documents that are legible are from the island of Hierro, the smallest and most westerly of the group, where the Iberic element seems to have been in greatest force, and whence, in all probability, it migrated to America.

INSCRIPTION IV.

This is to be read in the same way as the foregoing:

ma	bi	
sis-a	ma	al
la	kü	

Machisala Bimaku al.
Machisala Bimaku power.

"Machisala, the potentate of Bimbachos."

Dr. Bethencourt says that the aborigines of Hierro were called Bimbapes. In the inscription, one Machisala, perhaps *motz-zale*, "the Shearer," is made Lord of a place called Bimaku. Such a name as Bima or Bimaku would have no chance of surviving as such on the lips of Latin peoples, but would undoubtedly be strengthened into Firma or Palma.

INSCRIPTION V.

This brief document is to be read horizontally, from left to right:

ga no be ta.
Ganibeta.
"Knife."

In the writer's article on "The Oldest Written Records of the League of the Iroquois," in Vol. VI. of the Transactions of the Institute, p. 260, he has translated a Sinaitic inscription of the nineteenth century B.C., which reads: "Hadad, lord of the whole earth, son of the metallurgist, the noble Bedad," in which "metallurgist" translates *ganibeta*. Never dreaming of finding the name or title in the Canaries, he wrote: "This is undoubtedly the Hadad, son of Bedad, of Genesis xxxvi., 35, 36, who succeeded Husham in the range of Hor, and smote Midian in what afterwards became Moab. The name of his city was Avith, that is to say, Abydos in Egypt. His father, Bedad or Beda, he calls the metallurgist, as one who was among the first to work the mines of Arabia Petraea. The modern Japanese name for a metallurgist is *kane-fuki*, but the ancient Hittite term for smelting was *beta*. The remarkable thing, however, about the word *kanebeta* is that it is the original of the English *knife* and French *canif*, which were derived from the Basque *ganibet*, a knife, the meaning of which in old Hittite days was simply 'smelted or manufactured metal.'" M. Van Eys suggests a derivation from the Provençal *canivet*, but the debt is plainly the other way. As the writer has indicated elsewhere, (The Nations of Canaan; *Presbyterian College Journal*, November, 1900, pp. 10-13), the Hebrew Hadad is an attempt to render the Basque *Otadi*, which means a field of gorse, broom, or whin. In Egyptian, an equivalent leguminous plant was called *usert*, the *osiritis* of Pliny, and, with the addition of *sen*, a tree or shrub, gave name to the Usertsens of Abydos, famous Pharaohs of the so-called twelfth dynasty (Brugsch, Egypt under the Pharaohs). These Usertsens were Hadads or Otadis. A body of their tribal descendants arrived in Britain at some pre-Christian period, and were known to the classical writers, drawn upon by Richard of Cirencester, as the Ottadini, who dwelt along the borders of England and Scotland. Their ancient traditions formed the subject of the "Gododin" of Aneurin, a famous Welsh bard. A lordly offshoot of this family remained behind in Anjou in France, till, in the twelfth century, their chief, Geoffrey, married Matilda, Queen of England, and brought into that country the royal line of the Otadis, Usertsens, or, in Latin speech Plantagenistas (which are words that perfectly translate the former) to become the Plantagenets, from whom, in the female line of John of Gaunt, His Gracious Majesty King Edward in part descends. In translating two of the already published Hierro inscriptions, Nos. XVI. and XXI. of M. O'Shea, the writer mistook the value of two characters and rendered by *Osata* what should have been *Otadi*. The person so called in No. XVI., is termed "the Son of Tane, King of Amahetzio."

In the persons of Ganibeta and Otadi, this family must have been fully domiciled in Hierro, and this will account for the number of inscriptions in that island. Ganibeta and Otadi belonged to the Hamathite, Beerothite, or Kenite stock, who were, *par excellence*, scribes (I. Chronicles II., 55), and who wrote the inscriptions of Arabia Petraea called Sinaitic. As Hamathites, they are represented by the greatly cherished name of the Japanese, *Yama-to*, or "The Mountain Door," as well as by the Amoxoaquis of the Mexican Toltecs, and the Amautas of the Peruvians, who were their wise men, for the synonymous word Kenite is derived from the Japanese *Ken*, "intelligent, clever, wise." These scribes must in part have accompanied the Zerethites or Toltecs of the Canaries, both in their migration thither, and afterwards to America ; for, not only were the sages of Peru called Amautas or Hamathites, but also the word Amauta enters into the composition of no fewer than eleven names of the Incas given by Montesinos. While the original word Hamath undoubtedly meant the same as the Japanese Yama-to, the Mountain Door, from the application of the term to scribes, it came to denote a book or library, as in the Akkadian forms *sumuk*, *samak*, the Japanese and *Loo-Chooan shomotsu* and *shimutzi*, and the Mexican *amox*, whence the wise men or Amoxoaquis.

It is worthy of note in this connection that Berothai, the Syrian capital of a Hadad-ezer or Ben-Hadad of this race, leads back to an ancient Hittite Beeroth, named evidently after Beeri, a father-in-law of Esau, whose wife was Bashemath, and his daughter Judith or Yehudith. Homer and the Greek dramatists have preserved the eponym of Beeroth as Proteus, the old man of the sea ; his wife Bashemath as Psamathe, and his daughter as Eidothea. Beeri, the father of Bedad and grandfather of the first Hadad, or Usertsen of Abydos, gave name to the Bharatan race of India, celebrated in that famous epic, the Maha-Bharata, of which Yudhish-thira or Hadad-ezer is the principal hero. The Parthians of the Persian Empire were the same race, and their kings, Tiri-dates, bore the name with inversion of parts. In Welsh history the well-known word Brython has nothing to do with the Cymri or any other Celtic people, and as certainly has no connection with the Sassenach. The Brython were Iberic Picts, in other words, the Ottadini. There are two curious passages, in the poems of Taliesin, the Welsh bard, and in one by an anonymous author, which seem to point, not only to an Iberian connection of the Welsh, but to the fact that the Iberians were their instructors in mythology and many things beside. In his Angar Cyvyndawd, Taliesin says :

"Traethator fyngofeg,
Yn Efrai, yn Efroeg."

Davies translates this in his "Mythology of the British Druids": "My lore has been declared in Hebrew, in Hebraic." The other poem, entitled The Praise of Lhudd the Great, contains the following passage in a foreign tongue, which Davies thought might be Phoenician :

"O Brithi Brith oi,
Nu oes nu edi,
Brithi, Brith anhai
Sych edi edi eu roi."

This the writer turns into more modern Basque form as follows:

"O Brithi, Brith oi,
Nu o-etsi, nu adi :
Brithi, Brith anai
Zac adi, adi au arau."

This is more Etruscan than modern Basque, and means:

"O Brithi, associate of Brith,
Pay attention to me, hear me :
Brithi, brother of Brith,
Do thou hear, hear this measure."

Either Brith or Brithi, besides being Proteus, the sea-deity, and the Indian Bharata, is the Brutus of Geoffrey of Monmonth, and of the Brut d'Angleterre. His original sanctuary or oracle was called Beeroth. The Viscomte Chasteigner applied to the writer last winter (1899), for the derivation of the name Biarritz, which he had traced back in various forms of orthography as far as 1186. After much study, its original was found in Beeroth, derived from *Be-ur-i*, or "he of the great water or the sea," as *Be-ur-ots*, "the sound of the great water," or as *Be-ur-itz*, "the speech of the great water." It was at first, doubtless, an oracle of the Ottadini, whose name Pliny disguises as Oscidates, and places in the vicinity of Biarritz. Such is the long excursus to which the simple mention of Ganibeta has, it is to be hoped, not unprofitably led. It remains to observe that Amahetzio, the city of Otadi, son of Tane, has a Hamathite, or, Peruvianly speaking, Amauta look. In the Etruscan inscriptions is found the equivalent of the Japanese *shomotsu* and Mexican *amox*, "a book," which is wanting in modern Basque, namely, *ezaumeka*, in the compound word *egin-ezaumeka*, which translates the Latin Volumnius, "a book maker." This is virtually the Akkadian *sumuk*, *samak*, "a library."

INSCRIPTION VI.

This also reads horizontally from left to right; it is of no historical importance.

be ha tu de be
Behatu debe
to look forbid

"It is forbidden to look."

INSCRIPTION VII.

It follows the order of Nos. II. and III.

au	ai
arbe	ta
ma	arbe
	ma

Au Arbema aita Arbema.
This Arbema father Arbema.

"Arbema, the father of this Arbema."

The name of father and son may be *Arpimo*, "the first in front." Hittite names generally descend from grandfather to grandson.

INSCRIPTION VIII.

It reads like No. VII.

arbe	ta	ma
sis-a	bera	
sama		

Arbe esatz asma Tabera ema.
Arbe spoken indication Tabera gives.

"Tabera gives a sign of speech to Arbe."

M. O'Shea's No. XVII. mentions Tabera, whose name, accompanied by the figure of a turtle, suggests the modern Basque *chaberama*, the turtle or tortoise, and the Iroquois *anowara*. He is made the father of three chiefs, Ola, Mamaye, and Machi. Ola's son was Temane; his, Maneta; and Maneta's Olaochita or Ahaluste, in whose time the Roman envoy Lamia visited Hierro. This lapces Tabera four generations before that event, and if Arbe be his progenitor, as is probable, this must be a very ancient monument indeed.

INSCRIPTION IX.

This reads from left to right in both lines, save in the case of the solitary subscribed character at the end of the first:

ma mu ta ma i ta ta mo
be
si ta si ta tu
Mamuta mai Tatamobe Chitachi edatu.
funereal tablet Tatamobe Chitachi erects.

" Chitachi erects a sepulchral tablet to Tatamobe."

A name like Chitachi is Chisetachi of M. O'Shea's No. XXIII. It may connect with *chichtatu*, *sistatu*, *sistatze*, "to pierce, strike with a pointed weapon." Chisetachi erected a monument to Chioko. Tatamobe may be *edat-ambe*, "great extent," or it may connect with *tumpa*, " the sound made by a slight blow."

INSCRIPTION X.

This is to be read perpendicularly:

be ma
la ka
Bela-maka.

"Bela-Maka."

Bela-Maka or Maka-Bela is a common Hittite name, found on the Mound-Builder tablets of Davenport, Iowa, as Wala-Maka and Maka-Wala. Its first appearance in history is in Genesis xxiii, 9, 17, 19, where it has the form Machpelah. It thus appears to have been a Zocharite or Teucrian name, rather than Zerethite or Dardanian. In the form Belamaka it may possibly relate to that strange Basque word *palanka*, *palenka*, "bar of iron." The Japanese *maki-wari*, an axe, suggests some form of the Basque *maka*, *makatu*, to strike, in connection with *makilla*, a stick, as if it had been originally *mak-pilla*, a striking instrument.

INSCRIPTION XI.

To be read perpendicularly and from the left:

sipisai		
si	ma	3
bara	ma	u
at	ma	ma
ta	te	ri
	mara	

Ichpichoi Sibara atita Mama ema Temara 3 umerri.
In tribute Sibara holder Mama gives Temara 3 young cattle.

"Temara gives Mama, the lord of Sibara, three young cattle in tribute."

Mama has appeared in No. II. as ceding Taya to Goma. Here he occupies the position of a chief of feudal rank, to whom Temara is subject. The estate or kingdom of Mama was Sibara, perhaps derived from *sapar*, a bush, *zembera*, thickets, with reference to the nature of the land.

INSCRIPTION XII.

This is read in the same way:

te	go
ra	sas-a
au	ma
ra	i
ma	pi
	mo
	i

Tera aur ema kosatze mai Pimoi.
Tera child gives inscribed tablet Pimo to.

"The child of Tera gives an inscribed tablet to Pimo."

Tera may be *Ateri*, the sheltered or serene. Pimo is the Etruscan numeral one. It may be that Tera or Ateri's son's name was also Pimo, and that the Pimo of the inscription was his grandfather.

INSCRIPTION XIII.

Reads as the preceding:

	o
	pisa
pi	ma
mo	bi
	sisa

Pimo Opisama bizitza.
Pimo Opisama inhabiting.

"Pimo, the inhabitant of Opisama,"

It is more than probable that this is the Pimo of No. XII., and that Opisama should be read *obi samatz*, the vault, or literally, "the court of the grave."

INSCRIPTION XIV.

Follow the same order, but see final u-ma-ri.

		ma	bi		
	be	masi	si	3	ri
o	al	au	sa	u	ma
si	ar	ka	te		
ko i	te	ra			
ai ta					

Otsekoi aita be Alarte ema emaitz Aukara bizitzate 3 umerri.
Otsekoi father under Alarte give present Aukara inhabitants 3 young cattle.

"The inhabitants of Aukara give a present of three young cattle to Alarte, under the father of Otsekoi."

It is possible that *aitabe* is one word and the same as the modern *aitaba*, grandfather, in which case the Inscription would read "to Alarte the grandfather of Otsekoi," The names are all significant. Aukara is evidently *Aukera*, the choice, rather than *okher*, oblique, or *ukhur*, leaning forward. An instance of the use of the word "choice" in geographical nomenclature is Rogelim in Gilead (II. Samuel, xvii, 27, etc.), which is the Gaelic *rogh-eallamh*, "the choice of the flock." Alarte is the holder of power or authority, and Otsekoi is the ambitious, literally, "desirous of fame."

I

a. TIEPFSEPMERI ✠

b. EAFM ✠ IRENINI ✠ FMEAREI ✠

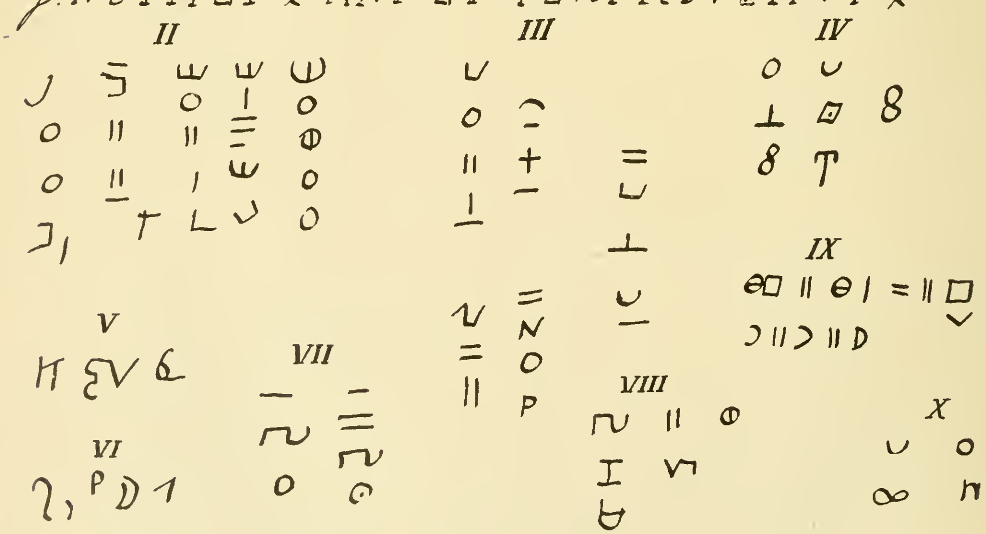
c. LPVRINENIPEPNIFANT ✠

d. MARMPRLMOTARE.

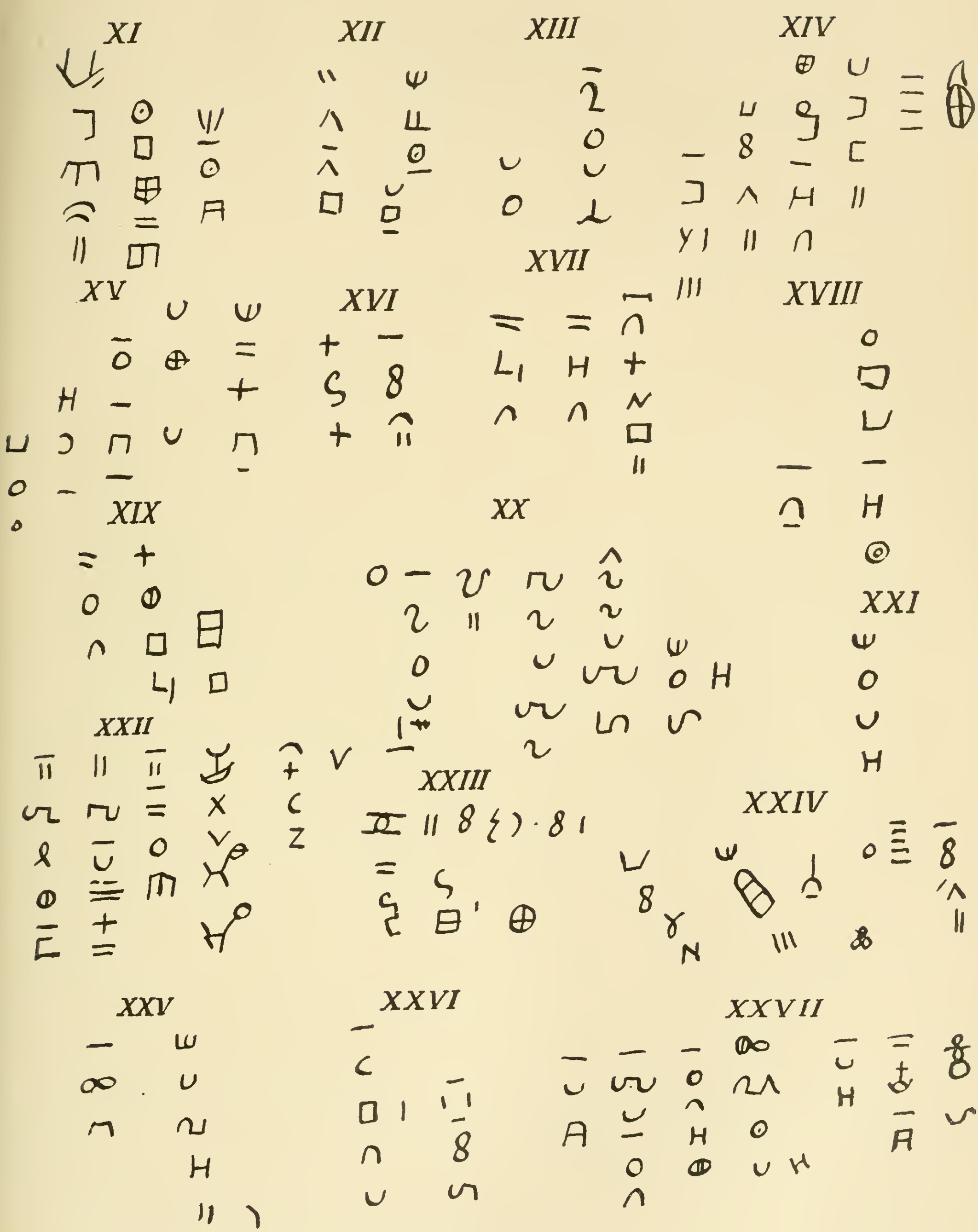
e. OLM ✠ INRANER ✠ IAEBNPEM ✠ RFVEN ✠ NV-
-INA PIMIIFINV IPI ✠ NIPIAN ✠

f. FVPMIRNA ✠ ENVPMTI ✠ EPNMRIR ✠ VRVIVIN-
-RN ✠ APVIMERI ✠ PIVNIAN ✠ NTRIIN ✠

g. NBIMEI ✠ ANNEI PERFMIV IFVF ✠



INSCRIPTIONS.



INSCRIPTIONS.

INSCRIPTION XV.

Follows the same perpendicular order:

				go
			pi	ti
		au	mo	tu
	ka	ma		
pi	si	i	be	
mo	i	ar		ar
ma		au		au

Pimo ema Kasii au mai arau: Pimo be goititu arau.
Pimo gives to Kasi this tablet suitable: Pimo under erects suitably.

"Pimo gives this fitting tablet to Ikasi: the underling of Pimo correctly sets it up."

This is evidently an earlier inscription than No. XIII., which commemorates the death of Pimo or the first. Ikasi, the learner, has no other memorial. The postposition *be*, under, below, is probably the shortest name in the world for a servant.

INSCRIPTION XVI.

In the same order:

da	au
no	la
da	ra
	te

Danda au Alarte.
Tribute this Alarte.

"The tribute this Alarte (gives, receives, etc.)."

In No. XIV. Alarte, the grandfather of Otsekoi, receives the tribute of Aukera. This may be another record of such feudal dues, paid on a different occasion. The document is imperfect.

INSCRIPTION XVII.

The order of reading is the same:

		au
it	te	ra
saha	ka	de
ra	ra	ka
		ma
		te

Itzahar Tekara aur Deka mate.
Itzahar, Tekara child Deka King.

"Itzahar, son of Tekara, King of Deka."

The word for king is the equivalent of the Japanese *mi-to*, *mi-kado*, the honourable door, or sublime porte. In Basque it would be *mi-ate*, an abbreviation of *mira-ate*, the admirable door. The Basque *ate* and the Japanese *kado*, are probably the original of the English "gate," and cognate words in other languages, including the Gaelic *geata*. Lexicographers are almost absolutely ignorant of the extensive Iberic element in all Indo-European and even Semitic languages. There are also debts the other way, as in the Basque *pan-toka*, a pile of stones, and the Japanese *ban-jaku*, a boulder, in which *pan* and *ban* are not native words, but ancient survivals of the Semitic *eben*, a stone, denoting former intercourse with Hebrews, Assyrians and similar orientals. Itzahar means "the old ox," which, in Turanian nomenclature, is not remarkable. Sitting-bull belonged to the same race. Even in Celtic, the Babylonian Sin-Gasit, who is the original of the British legendary Hen-gist, is *sean-gaiscidh*, the old warrior, a name which he no doubt received as a child. Tekara may be the Basque *zigora*, the rod, scourge, etc. Deka again may stand for *ideki*, open.

INSCRIPTION XVIII.

In perpendicular or Japanese order:

	ma
	ma
	be
au	ha
ra	ka
u	ma

Au arau Mama beha Kama.
This right Mama regards Kama.

"Kama thus suitably shews regard to Mama."

The name Mama has already appeared in No. II., an inscription not of Hierro, but of Canaria, where it is combined with that of Goma, a word not unlike Kama or Gama. Canaria is a considerable distance from Hierro, but the multitude of its inscriptions, as compared with other islands, suggests that Hierro may have been chosen by the Iberic aborigines as their place of sepulture, and thus that the Mama and Gama of this inscription are the Mama and Goma of No. II.

INSCRIPTION XIX.

Read in the same way:

te	de	
ma	ma	
ra	ma	al
	sai	ma

Temara Dema Masai al ema.
Temara tribute Masa-to power gives.

"The tribute of Temara gives sovereignty to Masa."

The name of Masa does not occur elsewhere, unless it be in M. O'Shea's XXII., in which the Roman Lamia is called a Masa, Basque *mesu*, *mezu*, envoy. Instead of Masa as a proper name, one might read *mezui* "to the envoy" or Lamia, which would relegate the inscription to the early days of the Roman empire. Temara has already found mention in No. XI., as the tributary of Mama, lord of Sibara. Temara may be *zamari*, the horse, or *zamar*, the crab. In Japanese *temari* is a hand-ball.

INSCRIPTION XX.

Reads like the preceding, but has lacunae or partial defacements:

				ra		
ma	i	bi	rabe	no		
	no	te	no	no		
	ma		be	be	go	
	be		berabe	berabe	ma	ka
	ha		no	sari	be	
	au					

Mai anoma beha au bite * * * * *rabe no Beberabe no ranono Beberabe zari Goma beka.*
Tablet contributed regards this envoy * * * * *rabe* of Beberabe of towards Beberabe commander Goma chief.

"The contributed tablet regards this envoy (bitezar) * * * * (a tribute) towards Beberabe: the commander Beberabe, chief of Goma."

This imperfect inscription seems to relate to a military man and an envoy, so that, instead of, "the chief of Goma" standing in apposition to him, the words may denote the giver of the tablet. The writer knows of no Basque or Etruscan name or word like Beberabe, but as Bibi-rube, it is just what an Etruscan document would turn the Latin Vibius Rufus or Rufinus into. There was a C. Vibius Rufinus in the Roman consulate 22 A.D., to whose family the supposed *bitezar* or envoy may have belonged, although his date would suit the time of one at least of the Lamias.

INSCRIPTION XXI.

go
ma
be
ka

Goma beka.
Goma chief.

"The chief of Goma."

Unfortunately, the name of the chief is lost.

INSCRIPTION XXII.

u	at	u	be	ar	be
da	erbe	da	hei	di	
beri	o	ha	tu	za	
al	bi	te	de	no	
ma	a	am	be		
u	te	bera	mopira		
sa	da		mopira		
	te				

Udaberri al ema osa atherbe obi ate edate: udahate ambera beheitu debe mopira mopira ardizain be.

Spring power gives overseer shelter cave door extend: summer more than lower forbids eight eight shepherd under.

"In spring, the overseer gives authority to open the door of the cave shelter: in summer he forbids to lower (into it) more than eight (times) eight, under a shepherd."

The inscription, which does not contain a single proper name, is the best test of the correctness of the method of interpretation.

INSCRIPTION XXIII.

This reads horizontally, from left to right, with a slight variation :

simasa	te	la	no	si	le	ya
te						
	no					
no	ma i		ma			

Chimasa Tela nausi, Leyate non mai ema.
Chimasa Tela lord. Leyate, who tablet gives.

"Chimasa, lord of Tela, Leyate who gives the tablet."

Talaya, by some derived from the Arabic, denotes "a look out on the coast;" Chimasa may be a form of *zematu*, to threaten, meaning "the menacer," and Leyate signifies "the zealous."

INSCRIPTION XXIV.

This irregular inscription is to be read in the main perpendicularly :

be	go		am 5	a
la	ramama	o		hal
ma		ro		ure
ka	ai ta	i	3	te

Belamaka Goramama aita oroi 5 amar 3 ahal urte.
Belamaka Goramama father remembers 5 tens 3 power year.

"Belamaka, the father of Goramama, remembers fifty-three years of authority."

The name of this aged sovereign appears alone in No. X. His son's may be *gora-mama*, "the exalted spirit."

INSCRIPTION XXV.

o	go
le	be
ro	arpe
	ka
	te i

Olero jabe Arpekatei.
Olero lord Arpekate to.

"To Arpekate, lord of Olero."

Olero or Oloro invites comparison with Oleron or Oloron in the Lower Pyrenees. Arpekate is, perhaps, a verbal form of *erpeka*, a stroke of the claws, meaning "to claw."

INSCRIPTION XXVI.

Also, with one variation, perpendicular:

o	
sa	ai
ma i	ta
ar	i
be	al
	beri

Osa mai Arbe aitai Alberri.
Pays-attention tablet Arbe father to Alberri.

"The tablet honours Alberri, the father of Arbe."

Arbe appears in No. VIII., as one of the oldest of the kinglets of Hierro. Alberri, his father, furnishes a still higher antiquity. The name may mean as it stands "new authority." It is not at all likely to be *alfer*, lazy, *alabere*, similarly, etc., but it might easily be *elbarri*, the crippled, lame, *ilberri*, the new moon, or *ilbera*, the waning moon.

INSCRIPTION XXVII.

Perpendicular, like the last:

a	a	u	le	o	te	
be	berabe	me	rosari	be	da	al
re	be	ri	mo	ka	tu	me
	u	ka	pi ka		a	na
	me	ma			ri	
	ri					

Abere aberabe be umerri: umerri kama lerrozarri mopika: obeka athedatu ari almena.

Cattle tread under lambs: lambs shepherd place-in-order by twos: better take away ram virility.

"Cattle tread under the lambs. The shepherd will place the lambs two in a rank. It is preferable to deprive the rams of their virility."

This last inscription is worthy of comparison with No. XXII., both denoting, not only the existence in Hierro of a pastoral Iberic population, but also that of a population whose humble class of shepherds was able to read such engraved notices. This seems to indicate that education was general in the islands, or at least among the Iberians in them, before the Christian era, and in the early Christian centuries. Can it be that all their writing was confined to rock faces; or had they, as Strabo asserts regarding their congeners, the Turdetani of Spain, books and parchment documents, containing, among other things, an account of their eventful history? Everything tends towards the suspicion that they once had such memorials, which may not all have perished. Librarians and similar custodians pay little attention to documents which they cannot read, and can, therefore, neither class nor catalogue. The question is worth asking, not only of archivists in the Canary Islands, but also of the same in Spain, southern France, Italy, and north-western Africa, whether a little research may not bring to light important historic facts concerning a race that has played no small role on the stage of the past.

GENERAL VOCABULARY.

COMPARING THE CANARY ISLAND DIALECTS WITH IRISH-GAELIC, WELSH AND BASQUE: I., W., B.

aala, alamen,	water,	lo. lua, uaran, I. ur B.
abara, abora,	god,	adbar, adbal I. *cause*; peryf W. *creator*.
abimbar,	to throw stones,	beim-bair I.
acaman, achaman,	highest god,	acmhaing I. *puissance*.
acano,	lunar year,	eigh I. *moon*, cann I. *full moon*, eang I. *year*.
acaman,	sun,	huan W., samh I., shems Arabic.
achacae, chacais,	rock pools,	cuas I. *cavity*.
achahuaran,	great god,	aigh-urraim I. *upholding power*.
achemen,	milk,	segh I. *milk*, seghamhuil I. *milky*.
achic,	son, descendant,	ae I., esil W.
achicaxna,	peasant, clown,	gwasan W.
achicuca, aguahuio,	illegitimate son,	ac-cuig I. *secret son*.
achijerres,	trifles,	ceirriach W.
achimaya,	mother,	iog, iogain I.
achimencey,	noble,	acmhaingeach I.
achjucanac,	supreme god,	uchaf, uchbenaeth W.
achjuragan,	god and lord,	uachdarach I., udd-dragonoll W.
achormaza,	green figs,	boccore Arabic.
acoran, achoran,	god,	crom I.
acucanac,	highest god,	aige-ceannach I., *the upholder of authority*.
aculan,	fresh fat,	agalen W. *lump of butter*.
adago,	goat's milk,	at I. *milk*.
adarg,	shoulder (of rock),	otir I.
adargomo,	arm of rock,	otir-gob I.
adarno,	tree,	udarondo B. *pear-tree*.
aemo,	water,	aw W., amh, amhan, en I.
afaro,	corn, grain,	bar I.
aguayan,	dog,	cian W.
ahicasna,	son of plebeian,	gwesyn W., oganach I.
ahico,	dress, skin shirt,	gwisg W., haik Arabic.
ajerjo,	torrent,	easar, easard I.
aho, ahof,	milk,	as, ceo I.
alcorac, alcoran,	god,	uileghlic I. *all-wise*, uilecoireach I., ollgwyr W. *all-just*.
algarabana,	wheat and barley,	iolach-arban I. *mixed grain*.
alio,	sun,	haul W.
aljereque,	narrow wall,	cul-gwyrch W.
almogaren,	temple,	armighthear I. *sanctified*, airmhidh I. *vow*, airmhidin I. reverence.

almogaroc,	adoration,	ermygiad W., urnaighe I, *prayer*, iarrata I. *asked.*
altacayte,	brave man,	lath-cathach I. *war-champion.*
altaha,	brave man,	lath I., lluyddwr W.
althos,	god,	alla, alladh, alt, art I.
amago, umiago,	sacred mount,	myg W. *sacred.*
amodagas,	sticks sharpened with fire,	maide I. *stick*, miodog I. bidog W. *dagger.*
amolan,	butter-cake,	eim-aran I.
ana,	sheep,	oen W., uan I. *lamb.*
anepa,	lance,	omna I.
anthaa,	brave man,	niadh I. *hero.*
antieux,	house,	anedd, W.
antraha,	man,	anra I. *mean men.*
ara,	goat,	ari B. *ram.*
aramatonaque,	barley cakes,	eorna-taoisnighthe I.
archormaze,	green figs,	bokkore Arabic.
arguna,	saddle bag,	bolgan W.
aridaman,	flock of sheep, etc.,	airmheadh I, *herd of cattle*, altain I, *flock*, porthmon W. *drover.*
artamy, arteme,	chief, prince,	ardmhaor I. *chief magistrate.*
asidir-magro,	invocation to God,	magh-adraidh I. *field of adoration.*
asitis-tirma,	invocation to God,	aitchim-trom I., *I beg for protection.*
atguaychafortanaman,	he who holds the heavens,	adh-se-a-cabhairt-neamh I. cymhorth-nef W.
atinavina, atinaviva,	hog,	aitheach-ban I. *sow*, hob W.
atis-tirma,	cry of surrender, invocation to God,	aitchim-trom I., *I beg for protection.*
auchones,	connections of caves,	uaigh, uaighneach I, *cave.*
auchor,	cave dwelling,	ogof W., coire I.
azamotan,	barley bread,	haidd-miod W. *barley cake.*
axa,	goat,	seaghach I. *he-goat.*
axo, xayo,	mummy,	ecc, echt I. *dead.*
azarquen, tacerquen,	syrup of mocanes,	deasguin I. *molasses, etc.*
babilon,	nickname of Tenerife child,	buibiollan I. *coxcomb.*
baifo,	kid,	beag-boc I., bitika B.
balma,	cloud, veil,	beala I.
belete, beleten,	first milk,	flaith I.
benesmen,	harvest,	pen-cywain W. *first harvest.*
benesmer,	August,	beinn fomhar I. *first harvest.*
bimba,	round stone,	beim I. *to strike.*
bochafisco,	roasted grain,	poeth-plisgo W., bocht-blaosg I.
borondango,	butter-cake,	barachdaen W. *bread and butter* barantionsan I.
bosigaiga,	el pene (Spanish),	biach I., potzuak B.
burgado,	shell-fish,	bylchiad W., murac I.
cancos,	priests of medium rank,	carnach I. *heathen priest.*
cariana,	rush basket,	crannog I.
cariano,	large bag,	crannog I.
carabuco,	earthen jar with handle,	cailphig I.
carabuco,	male goat,	culbhoc I., bwch-gafr W.
casjua,	the cud,	athchagnadh I.

cel,	moon,	gealach I.
cela,	month,	gealach I.
chabogo, chaboigo,	cavern,	ogofawg W.
chabor,	royal palace,	sabhal I. *granary, storehouse.*
chacanisos,	feet,	cos I.
chafija,	debility,	afiechyd W.
chacares,	castanets,	cliciwr W.
chafariles,	rock pools,	tiobhar I.
chajaco, chajasco,	litter, bier,	caiteach I. *winnow sheet.*
chajaija,	dark colour,	gwywgoch W.
chajajo,	corpse, dead,	aisc, eag, oighidh I.
chajinasco,	cumular clouds,	eachanach I. *stormy.*
chalafusco,	crevice in mountains,	scalp I.
chamato,	woman,	cyffoden, cymones W., gamh, caomhog, coint I.
chayofa,	nose,	comar, comhor I.
chede,	limit, boundary,	chede B.
cherga,	belly,	croth W., cilfin I.
chescaro,	mean, penurious,	cagaltach I., cyrrithus W.
chibichibi,	a game,	gogampau W., subha I.
chibusco,	rope,	suag, sioman I.
chichiciquico,	squire,	gaisgidheach I.
chihisquico,	cavalier,	gwych W.
chilhisquizo,	squire,	giollasguain I.
chinea,	lower hell,	anwn W.
chinichibito,	change of pasture,	cyfnewydiad W. *change.*
chiribito,	ear mark on cattle,	cearbhach I. *ragged, torn,* clustnod W.
chirrimile,	small helix,	cregyn W., creachan I.
chirripota,	pubescent girl,	gwryf W.
chiscano,	bone,	seic, asna I., asgwrn W.
chiscanado,	bony,	asnach I., esgirnig W.
chivato,	kid,	gabhar I. *goat,* giden W.
ciguena,	female goat or ewe,	ceathnaid I, *sheep.*
chocos,	chips of wood,	casnaig I., coed W. *wood.*
coran,	man,	gwr W.
coruja,	red owl,	sgreachog I.
cotan,	man,	cathaidhe I. *warrior.*
creses,	beech-nuts,	grech, creachach I.
cuna,	dog,	cu I., cian W.
cuncha, cancha,	little dog,	cynos W.
cuteto,	piebald animal,	caideacha I. *spotted.*
datana,	war cry,	deodhann I. *by God's help,* a gagan Dduw W. *god grant.*
debase,	idler,	taimheach I.
eccero,	limit, boundary,	cwr W.
echeyde,	hell,	avagddu W.
efequen,	place of worship,	impuighim I. *pray.*
embroscar,	poison water,	amh-briosog, rossachd I.
enac,	night,	nos W.
ere, eres, erales	fresh water holes,	feirsde, earc I.
esmira,	bee-hive,	smeraighe I. *swarming of hive.*
esquen,	house,	iosdan I.
estafia,	to beat,	asti B.

fagayo,	teat, udder,	boig I., piw W.
faira,	round stone,	bair I., bwrw W.
faisca,	spark of fire,	bacht I.
faita,	treason,	brad W., fionaih I.
fayacan,	governor,	pencun W.
faycao,	priest,	faigh I.
faysage,	councillor,	feasach I., *knowing*, *skillful*. fasuigheadh I., *knowledge of law*.
fe,	crescent moon,	fas I., *crescent*, *growing*.
firancas,	gray,	bracach I.
firanque,	beetle,	primpiollan I., chwilen W.
fol, fole,	big bag,	bolg I.
furna, furnia,	abyss,	uffern W., ith frionn I.
gabio,	evil spirit,	siabhra I.
gabeit, gabiota,	devil,	siobradh I.
gagames,	appetite,	geogamhail I.
gahuata,	devil,	sighidh, siogidh I.
gaire, gayre,	war-councillors,	gearait I., *prudent*.
galiot,	devil,	goilline I.
gama,	enough,	cmyhwys W.
gambuesa,	shed for wild cattle,	gabhann I., *a pound*.
ganigo, guanigo,	earthen pot,	cunnog W., cuinneog I.
ganofa,	generous,	hynaws W.
gara,	island, rock in sea,	sgeir I.
garajao,	water fowl,	curcag I., gwyach W.
garepa,	chip, shaving,	sgealp, sgolb I.
garepa,	spark,	gwraich W., caor I.
garfa,	lance,	geurgath I., gwaywffon W.
gocho,	little yard,	cata I.
gofio,	porridge,	sopa, zopa B.
gongo,	hole,	ionga I.
goran,	yard,	cro, cru I.
goro,	circus, arena,	chwareufa W.
gouro.	little yard for kids,	cal, corlan W.
groja,	laughter,	gaire I., chwardd W.
gua,	son of,	ua I.
guacacque,	jug of measure,	cuachog I.
guacaros,	bettle,	chwil W.
guachafisco,	reduce to powder,	creafog I.
guaclo, juaclo,	natural cave,	ceule W., iseal I.
guague,	measure,	cuachog I.
guaire,	noble,	guaire I., gwerlin W.
guamf,	man,	ymbaffiwr W., *fighter*.
guan,	son of,	gein I., cenaw W.
guanac,	estate,	ceannas I.
guanac,	republic,	ceannach I.
guanaja,	devil,	einioes W.
guanarteme,	king,	ardmhaor, airdimnhe I.
guanhot,	favour, gift,	cymhorthi W.
guanoco,	weak, infirm,	gwan W.
guanoth,	steward of estate,	ceannart I.
guanil,	wild cattle,	agh I., *cattle*, anial W., *wild*.
guapil,	skin cap,	cwflen W., caba, caibin I.

guardaseme,	king,	airdcheim I., *eminence.*
guarirari,	who anchors the world,	accaire I., heor W., *anchor.*
guatatiboa,	national festival,	eisteddfod W.
guaya, iguaya,	spirit,	sia I.
guayca, guaycos,	buskins,	asachcos I., gwentas W.
guaycas,	sleeves,	cuachog I.
guayafacan,	co-adjutor of governor,	cympencun W.
guayfan,	co-adjutor of governor,	cympen W.
guayere,	populace,	gwerin W.
guayoto, huayota,	devil,	sigidh, gosda I.
guijon,	ship,	cwch W.
guirre,	vulture,	buri W., gairrfhiach I.
guisne,	pudenda,	caise I.
gurancho,	cave for animals,	gurna I., cor-ychain W.
gurgusiar,	to cry,	gairgala, golghair I.
gurgusiar,	to examine, pry,	chwilgar W., cuartughadh I.
hana,	help,	anaice, congain I.
hara,	ewe,	caora I., *sheep.*
harba,	loan,	airle I.
harhuy,	sheep skin,	caorach I.
harmaguade,	vestal virgin,	er-maighdean I., *noble virgin.*
hecheres hamanates,	councillors,	agarach comchaint I., uchreithwr cymanfa W.
herguele,	shoeing,	archeniad W.
hero, herez,	cistern,	fuaran I.
hirahi, hiragi,	heaven,	earc I.
huirmas,	large sleeves,	llawẹs W.
ife,	white,	aoibhe I., *fair*, abead Arabic.
iguanoso,	weak, infirm,	egwan W.
iguaya hiraji,	god of heaven,	sia erc I.
ilfe,	hog,	lia I., llwdn W.
irichen,	wheat,	rhygen W., *rye.*
iruene,	devil,	iarog I.
irvene,	apparition,	airidh, arrach I.
jameo,	water hole in lava,	uam, uain I., *a hollow.*
jao, josio,	term of endearment,	cu, W.
jarco,	dead,	erca I.
jeren,	shaft of mill,	garan W., seaghlan I.
jilmero,	rod fishing from the shore,	genweirio W.
jucancha,	god universal,	sia-ceannach I.
jurnia,	abyss,	fuirne I., uffern W.
juvague,	fat ewe,	mamog W.
leren,	irrigation ditch,	llyr W., flirim I.
lia,	summer sun,	les, leus, lo I., *light.*
lion,	sun,	laom I., *blaze of fire.*
machafisco,	object of little value,	meas-beag I.
magado,	mace, club,	piocaid, picidh, fascut I., makatu, makilla B.
magarefo, magarejo,	tall thin boy,	mac I., *boy*, llipa W., *lanky.*
magido,	fire wood,	fagoid I., ffagod W.
mago,	Guanche,	mogh I., *man.*
maguas, magada,	vestal,	maighdean I.

mahey,	hero,	mogan I.
maho, maxo,	shoe,	mogan I., amgoesan W.
majec,	sun,	mais I., mychedin W. *sunshine.*
malgareo,	rough music,	mawlganu W.
maniota,	little bag,	mang I., amner W.
manonda,	black goat with white feet,	ban an dubh I., gwyn yn du W. *white in black.*
marona,	fried meat,	mollwyn W. *mutton*, bruin I. *stew.*
masiega,	thatch,	imscing I. *covering.*
maxio,	enchanted spirit,	mwci W.
mayan,	piece, part,	men Arabic.
menceit,	heir apparent,	fineachas I. *inheritance.*
mesdache,	relaxation,	feisteas I.
minaja, minajo,	goat,	meann, mionnan I. *kid.*
misgan, misgano,	cat-hole,	musgan I.
moca,	javelin,	meas I., moko B. *point.*
moneiba, moreiba,	female idol,	menerbh I. *goddess of dyeing.*
malan,	buttermilk fat,	mehin W. *fat*, molchan I. *buttermilk cheese.*
naguayan,	animal, insect,	ednogyn W., snagan I.
oche, hoche,	grease, fat,	usg, iach I.
omanamastuca,	bright red,	omh aineamh dathach I. *blood stain* coloured.
oranjan,	god,	arnaigh I.
orduhy,	court, hall,	ard-tig, alladh I.
parano,	a stand,	brannra I.
puipana,	white and cinnamon goat,	buidhe-ban I, *yellow-white.*
punapal,	first son,	pen-eppill W.
quebehi, quehibi,	dignity,	ceap I.
quevechi,	dignity,	gofyged W.
quevihiera,	greatness,	gwchder, cynghori W.
rapayo,	burnt ears of wheat,	erre-bihi B.
reste,	support, defence,	airchiseacht I.
sabor,	counsel, advice,	cyfarwyddiad W.
sabuco,	sharpened stick,	yspig W., cipin I.
safiro,	insect,	gwiban W., giuban I.
samarin,	priest,	seanmoirighe I.
sera,	cheese-hoop,	cor, cylch W.
serfacaera,	priestess,	seirbhiseach I. *attendant.*
sigone,	noble, leader,	seighion, soichinealach I.
sisa,	yard to attract wild cattle,	gaiste I. *trap.*
sorrocloco,	the *couvade*, (This is not a Celtic custom).	sor-acholtsu, sor-ahalge B. *care of newly-born, shame of newly-born.*
sunta,	war fleet,	uiginge I.
suzmago,	dart,	saeth W., sais-macha I.
tabajo, tabajoste,	milk-pail,	tubog I., duphe B.
tabese,	cooked whey,	chwig W. *whey.*
tabite, tebite,	handled pot,	poite, poitin I.
tabona,	stone knife,	deimhne I. *edge tool.*
tabor,	royal palace,	sabair I., ysgubor W. *granary.*
tafiaque,	lancet shaped flint,	twca W., diobadh I.
tafique,	flint knife,	samhagh I, *edge*, epaki B. *cut.*

tafrique,	stone knife, lancet,	spealg I. *splinter.*
tafugada,	much, abundant,	hafog W.
tagoro, tagoror,	town council,	tagra I., dadllewr W. *discuss.*
taguacen,	hog,	arcain I.
taguado, taguao, taguas,	squeezing ladle,	guasg W. *a squeeze.*
tahatan,	ewe,	dafad W.
tahuyan,	skin petticoat,	hugan W., *gown.*
tajalaque,	palm leaf,	duilleog I. *leaf.*
tajorase,	goat under one year,	aos arraise I., oes cyrhaedd W. *age attaining.*
tajos,	night-bird,	eos W. *nightingale.*
tajuco,	milk-pail,	dabhach I.
talabordon,	slope against the sea,	tuilemara don I. *on account of the tide.*
tamaide,	fountain,	tiobraid I.
tamaite,	big, swollen,	ymchydd W.
tamaismia, tamasma,	sparrow,	camhuin, scamoghuin I. *wry-neck.*
tamaro,	short fur cloak,	zamarra B., ionar I.
tamarco,	skin dress,	zamarra B., sgabul I.
tamarcano,	violent blow,	tuargain I. *beating.*
tamarco,	tall vulgar person,	amrosgo W., tamhanach I.
tamarco,	large fat adder,	diamhar I.
tamasaque,	lance,	amusadh I. *attack.*
tamazen,	hog,	mochyn W.
{tamogantacoran, tamogante en acoran,	house of God,	tamhait an crom I.
tamonante,	inspired priesthood,	dewin W., deamhnoir I. *prophet, conjurer.*
tamogitin, tamogantin,	house,	tamhaighim I. *I dwell.*
tamozanona,	fried meat,	mochyn W., *pig,* muc anong I. *fried pork.*
tamozen,	barley,	tumdhias I. *bushy ear of corn.*
tano, taño,	straw basket,	toin, tonna, tonnog, tunog I.
tara, tarha, tarja,	sign of remembrance,	dere, dyre W., tarra, tarrsa I. *come thou (here).*
tarhais,	tree,	dair I., derw W. *oak.*
taro,	stone granary,	daras I. *house.*
tarqui,	call to speak,	*see* tara.
tarquis,	certainly,	deigh I.
tarute,	ambassador,	treithu W.
tasaigo, teseque,	corpse,	taise I.
tasasa,	shaft of mill,	deatachan I. *chimney.*
tasorma,	flat stone,	sarn W.
teberite,	cattle mark, ear clip,	diobhaladh I. *mutilation,* gofyriad W. *clipping.*
tebija,	little handled earthen pot,	pig, pigin I.
tefene,	roasting grain,	teibidh I. *harvest making.*
tegala,	shepherd's enclosure,	teagair I.
tazufre,	goat skin bag,	tais-cofra I.
tegue,	yellowish chalk,	dathach I. *coloured, chromatic.*
teguevelt,	ewe,	othaisg-allaidh I. *wild sheep.*
teguevita,	goat,	othaisg-fiadha I. *wild sheep.*
tehuete,	small skin bag,	tiach, tiochog, I., cod W.

teigue,	hard land,	tingh I., tew W. *dense.*
teique, teseique,	argillaceous earth,	toes W., doigh, taos I. *dough.*
tejuete,	shepherd's bag,	tiach I.
teniques,	three stones of hearth,	teinngha I. *relating to the fire,* teinntein I. *the hearth.*
teofuivite,	goat or ewe skin,	fetha-boc I.
testadal,	coloured chalky earth,	des-dathach I.
teste,	trace of animal,	deisidh I. *it sat, rested.*
teseique,	great man,	toiseach I.
tezerez,	cudgels,	tagar I. *fight,* zigor B.
tezezez,	sticks of wild olive,	zotz B. *small sticks.*
tibiabin,	priestess,	dewin-ben W.
tibibeja,	small handled earthen pot,	pibcyn W.
tifia,	correction,	cosp W.
tifiar,	to steal,	cipio W.
tigalate,	tall slender man,	teircfheolach I.
tihagan, tihaxas,	ewe,	othisc, othaisg I.
timargo,	invocation,	diomrac, I. *temple,* diamaireachd I. mystery.
timixiraqui,	measure, weight,	tomhas, toimhseacan I.
tingalate,	tall thin person,	tan-cleith I.
tirma,	sacred cliff,	drim I, trum W. *cliff.*
tisamago,	sacred cliff,	diagha-magh I. *sacred field.*
tistirma,	sacred cliff,	diagha-drim I.
titogan,	heaven,	ditiu ceann I., tuddo cwn W. *covering of the head.*
tofio,	handled pot,	cib W., tupin B.
tofio, tabajoste,	cattle trough,	dabhach I.
tolmo,	land slide,	deillion I. *slide away.*
tozio,	crockery,	toes W., taos I. *dough.*
trichen,	wheat,	triosg I. *grain.*
trifa,	corn, grain,	arba I.
tufa,	ewe,	dafad W.
tujite,	little purse,	tiach, tiachog I.
urraja,	cry to scare hawks,	oergri W.
valeron,	cave of vestals,	ffau-lle-rhian W. *cave-room-virgin.*
varode,	lance,	bruidh, bioradh, morgha I.
verdone,	big stick,	buailtin, baircin, farachdach I.
xerco,	shoe,	cuarog I., archen W.
ynbaque,	reed mat,	beach I.
zeloy,	son,	gille I.
zonfa,	hole, centre, navel,	ion, ionga I.
zucaha,	daughter,	oghachd I.
zucasa,	legitimate son,	ac-aicsi I. *heir.*

NAMES OF PLANTS.

aceben,	plant,	easping-ban I. *ox-eye daisy*, Chrysanthemum leucanthemum.
acichei, haquichey,	beans, vetches,	ekosari B.
aderno,	hardwood tree,	derwen W. *oak*.
afaro, ofaro,	grain,	bar I., brachtan I., *wheat*.
agonan,	plant,	ccnamh I., *samphire*, Crithmum maritimum.
aguamante,	mallow roots,	ucas-fiadhain I. *mallow*, Malva sylvestris.
aijara,	bush or bramble,	casair I. *thorn*.
aijota,	mushroom,	caochog I. *puff-ball*.
aites,	plant,	uath I. *white thorn*, Crataegus oxyacantha, iodha I. *yew*, Taxus baccata, ote B. *broom*, Genista tinctoria, aiteann I. *furze*, Ulex europæus.
ajinajo, jinajo,	bush,	conasg I. *furze whins*, Ulex europæus.
alcaritofe, algaritofe,	Cedronella canar,	lus-grafandubh I. *horehound*, Ballota niger.
alchofe,	yellow flowered thorn,	oirchiabhach I. *golden haired*.
algafita,	Agrimonia,	leatach-buidhe I. *lady's mantle*, Alchemilla vulgaris.
algarfe,	Cedronella trip,	lusgrafanban I. *horehound*, Ballota alba.
amagante,	mallow,	mil-mheacan I. *mallow*, Malva sylvestris.
amogante,	berry,	magon, bacon W.
amuley,	herb,	amharag I. *mustard*, Sinapis arvensis ; amaraich I. *scurvy grass*, Cochlearea officinalis.
anarfeque,	wormwood,	mormont I. *wormwood*, Absinthium latifolium ; norp I. *houseleek*, Sempervivum tectorum.
aromatan,	barley,	eorna I.
balo,	Plocamia pendula,	bhalla I. *wall pellitory*, Parietaria officinalis.
bejique, bequeque,	Sempervivum,	beidiog W. *evergreen*.
bequeque,	*ib*,	feusogach I. *bearded capillary*, Adiantum capillus veneris.
berode,	*ib*,	bythwyrdd W. *evergreen*.
beya,	plant,	bihi B. *corn*, fead I. *bulrush*, Typha latifolia ; fiag I. *rushes*, Juncus ; feith I. *honeysuckle*, Lonicera periclymenum.
bicacaro,	Canaria,	bascart I. *cinnamon*, Laurus cinnamomum.

bubango,	gourd,	pepog I., pompiwn W.
cadil, cail,	food plants,	cadhal, cal I. *coleworts.*
carisco,	grapes,	caora I.
chabora,	plant,	seamar, seamrog I. *clover*, Trifolium repens.
chajil,	plant,	cagal I. *cockle*, Agrostemma githago; cuisle I. *hepatica*, cucuillean I. *bed-straw*, Galium verum ; casair I. *thorn.*
chajinate,	plant,	casmhoidhach I. *haresfoot.*
chaguira,	plant,	chosgair I. *laurel*, Laurus nobilis ; cocoil I. *burdock*, Arctium lappa.
chenipa,	herb,	cnaib I. *hemp*, Cannabis sativa.
cheremina,	plant,	chermen B. *pear*, goirmin I. *woad*, Isatis tinctoria ; coireaman I. *coriander*, Coriandrum sativum ; guirmin I. *indigo*, gorman I. *bluebottle*, Centaurea cyanus ; surramont I. *southernwood*, Artemisia abrotanum; searbhan I. *dandelion*, Leontodon taraxacum.
chibusquera,	plant,	subcraobh I. *raspberry*, Rubus idaeus.
chibusco,	berry of same,	subha I. *rasp-berry.*
chilate,	graminaceous herb,	chilista B. *lentils*, seirg I. *clover*, Trifolium pratense.
cofecofe,	Chenopodium,	pigogo W. *spinach*, Spinacea oleracea ; gabaisde I. *cole-worts.*
cosco,	herb,	cusag I. *wild-mustard*, Sinapis arvensis.
creses,	beech-nuts,	grech I.
garao, garse,	sacred tree,	caorran I. *mountain-ash*, Pyrus aucuparia.
garasera,	plant,	glasair I. *betony*, Betonia officinalis.
girolana,	bush,	caoirinleana I. *valerian*, Valeriana officinalis ; caorogleana I. *meadow-pink*, Lychnis flos cuculi.
givarvera, hivalvera,	butcher's broom,	calgbrudhan I., Ruscus aculeatus.
golgora,	plant,	zurchuri B. *poplar*, culuran I. *birth-wort*, Aristolochia ; galluran I. *angelica*, Angelica sylvestris ; glasair I, *betony*, *see* garasera.
guasimo,	plant,	gaoicin I. *arum*, Arum maculatum ; hasuin B. *nettle.*
guaydil,	convolvulus floridus,	codhlan I. *poppy*, Papaver ; codalian I. mandrake. Mandragora.
gurman,	plant,	corrman I. *wall pennywort*, ? Sedum ; gorman I. *bluebottle*, Centaurea cyanus, *see* cheremina.
haran,	fern,	ira B., chorrain I. *Asplenium.*
iguaje,	plant,	cuigeag I. *cinquefoil*, Potentilla reptans ; cusag I. *wild mustard*, Sinapis.

irama,	shrub,	arne I. *sloe*, Prunus spinosa; oruin I. *beech*, Fagus sylvatica; uillean I. *honeysuckle*, Capritolium periclymenum.
irichen,	wheat,	ceirchen W. *oats*.
jarjado,	shrub,	groisaid I. *gooseberry*, Ribes grossularia; curcais I. *flag*, *bulrush*.
jirdana,	shrub,	caorthainn I. *quickbeam*, ?. Pyrus aucuparia; crithean I. *aspen*, Populus tremula; cailtin I. *hazel*, Corylus avellana; scrutan I. *hawkweed*, Hieracium; sraidin I. *shepherd's purse*, Capsella bursa pastoris.
jopebe,	herb,	sobha I., hebog W. *sorrel*, Rumex acetosa; copog I. *dock*, Rumex obtusifolius; fib I. *bilberry*, Vaccinium myrtilus.
joriada,	Bophthalmum,	ceannruadh I., *celandine*, Chelidonium majus.
jorjal,	plant,	crostal, crutal I. *moss*.
juesco,	mallows,	ochus I., Malva vulgaris.
loro,	tree,	llawryf W. *laurel*, Laurus nobilis.
marmojarse, marmojoce,	herb,	barbog, barbrog I. *barberry*, Berberis vulgaris; borramotur I. *wormwood*, Artemisia absynthium; feoran-curraigh I. *water horehound*, Lycopus europæus.
marmolan,	mountain tree,	malabhar I. *dwarf elder*, Sambucus humilis.
mocan,	Visnia mocanera,	meacan I. *taprooted plants*, meangan, maothan I. *osier*.
mol,	aromatic shrub,	marros I. *rosemary*, Rosmarinus officinalis.
morangana,	strawberry,	mariguri B.
name, niame,	plant,	noinin I. *daisy*. Bellis perennis.
orijania,	plant,	oragan I. *wild marjoram*, Origanum vulgare.
orixama,	Cnecrum pul,	ragaim I. *sneezewort*. Achillea ptarmica.
pirguan,	plant,	fraochan I. *bilberry*, Vaccinium myrtilus; bairgin I. *buttercup*, Ranunculus repens, mearacan I. *foxglove*, Digitalis.
romame,	fruit of thorn,	romhan I. *French wheat*, ? Polygonum fagopyrum.
sajira,	plant,	seichearlan, seicheirghin I. *primrose*, Primula veris.
sanjora,	Sempervivum,	sinicin I. *houseleek*, Sempervivum.
saquitero,	tree,	sgeachmhadra I. *wild rose*, Rosa canina.
sorame,	little bush,	surrabhan I. *southern wood*, Artemisia abrotanum.

tababoire, tabajoiri,	aromatic herb,	bofulan I. *mugwort*, Artemisia vulgaris ; biorfheir I. *water-cress*, Nasturtium officinale.
tabaibo,	Euphorbia,	dathabha I. *hellebore*, Helleborus niger.
tacorantia, taragontia,	Dracunculus canar,	gacharonda I. *arum*, Arum maculatum.
tadaigo,	bush,	sceitheog I. *hawthorn*, Crataegus oxyacantha.
tagasaste,	Cytisus,	ddrewgoed I. *laburnum*, Cytisus alpinus.
taginaste,	bush,	giogun-ard, oigheannach I. *thistle* Cirsium lanceolatum.
tajose, tajarnollo,	plant,	cuigeag, cuigmhearmhuire I. *common cinquefoil*, Potentilla reptans.
tamozen,	barley,	tumdhias I. *bushy ear of wheat.*
tanjose,	plant,	caineog I. *barley and oats.*
tarabaste,	herbaceous plant,	treabhach I. *winter rocket*, Eryssimum barbara ; trombhod I. *vervain mallow*, Malva ? gropis I. *mallow.*
tarambuche	bulbous plant,	crwnben W. *bulb.*
tarhais,	tree,	darach I. *oak.*
tasaigo,	plant,	sgathog I. *cotton grass*, Eriophorum polystachion : sgathog I. *trefoil*, Trifolium.
tebete,	mountain tree,	eabhadh I. *aspen*, Populus tremula.
tinanbuche,	bryony,	cnaibuisge I. *water neck weed.*
titimalo,	purgative plant,	taithfhuillean I. *wood-bine*, Lonicera periclymenum.
togia,	sand plant,	taga I. *teazle*, Dipsacus.
trichen,	wheat,	rhych W. *rye.*
trifa,	wheat,	arba I.
vesto,	mallow roots,	fochas I. *mallow*, Malva.

COMPARATIVE VOCABULARY OF PERUVIAN.

(Q. QUICHUA, QT. QUITENA, A. AYMARA, AT. ATACAMA, I. ITENES, C. CAYUBABA, S. SAPIBOCONO, AND Y. YURACARES), WITH CELTIC (A. ARMORICAN, G. GAELIC, E. ERSE, AND W. WELSH.)

	Peruvian.	*Celtic.*
above,	araja A.,	goruch W.
after,	ucata A.,	gwedi W.
air,	huayra Q.,	awyr W.
all,	taque A.,	gac E.
angry,	pina Q.,	ffrom W.
arm,	hicani A.,	cainc W.
armpit,	huallhuancu Q.,	cesail W.

	Peruvian.	*Celtic.*
arrow,	micchi A.,	picell W.
ashes,	quella A.,	ulw W.
ask,	isquina, mayina A.,	gofyn, ymofyn W.
bad,	valchar, ualcher At.,	ysgeler W. *wicked.*
basket,	sappa A.,	cawell W.
beard,	tironcayu A.,	rhawn W. *hair.*
beat,	panay Q.,	pwnio W.
	huacta Q.,	chwatio W.
belly,	puraca A.,	bru, bolg G.
below,	urac Qt.,	goris W.
	ichen At.,	is, isod W.
bind,	huata, huatay Q.,	caethiwo W.
bitter,	haru A.,	chwerw W.
black,	chamaka A.,	much W.
	coca A., hachi At.,	cuchiog W.
blood,	huila A.,	fuil G.
	yahuar Q.,	crau W., cru G.
blue,	selqui At.,	glas G. W.
body,	hanchi A.,	neach G.
bone,	cchaka A.,	seis E.
bow,	picta Q.,	bwa W.
branch,	ali A.,	osgl W.
bread,	tanta Q., ttanta A.,	teisen W. *cake.*
break,	pakiy Q.,	bregu W.
breast,	haiti At.,	uchd E.
	pivur At.,	afell W.
bring forth,	sarma At.,	esgor W.
butterfly,	pilpinto A.,	balafen W.
buy,	rantiy Q.,	prynydd W. *buyer.*
cheek,	buca I.,	boch W.
choice,	ahllay Q.,	ethol W.
clear,	illan, illari Q.,	glain, eglur W.
cloak,	iscallo A.,	casul W.
clothes,	sau A., acsu At.,	gwisg W.
cold,	chiri Q., serar At.,	oer, goroer W.
corpse,	aya Q.,	ec E.
cut,	cuta A.,	cat W., *a cut.*
dance,	raymi Q.,	llemain W.
dark,	kata Q.,	caddug W. *darkness.*
dead,	hinata A.,	ymado W.
death, die,	huanhu, huanuy Q.,	angau, angeu W.
	amaya A.,	mas W.
	mulsi At.,	marw W.
deep,	ccorahua A.,	craff W.
demon,	supayu A., zupai Q.,	siabhra E.
	huantahualla A.,	enaidmall W.
deer,	lluchos, taruco Q.,	
	taruja A.,	cellaig W. *stag.*
dew,	sulla A.,	gwlith W.
dice,	huayru Q.,	ffrist W.
do,	rurani Q.,	llunio W. *make.*
dog,	anu A.,	cain W.
	alljo Q., locma At.,	llechgi W. *cur.*

	Peruvian.	*Celtic.*
door,	puncu A., Q.,	porth W.
dress (woman's),	anoco A.,	gynog W. *gowned.*
drink,	upiya Q.,	yfed W.
dry,	chaki Q.,	sych W.
dust,	turo Q.,	stur G.
ear,	iradike C.,	clust W.
earth,	idatu C.,	tudd W.
	hoire At.,	daiar W.
	lacca A.,	llwch W. *dust.*
eat,	oloma At.,	llewa W.
end,	ccorpa A.,	gorphyn W.
enter,	mantana A.,	myned W.
equal,	cusca A.,	cystal W.
eye,	nairi A.,	meilyn W.
	iyocori C.,	suil G., crai W.
face,	akanu A.,	gwyneb W., cainsi E.
faggot,	picho A.,	ffasg W.
fall,	tincuna A.,	disgyniad W.
falsehood,	selima At.,	celwydd W.
	karina A.,	creinio W. *to lie.*
fat,	lanccu A.,	bloneg W.
	huira Q.,	gwer W.
father,	tata A., S., Q.,	tad W.
father-in-law,	ttosi At.,	tadcu W.
fear,	ajsarana A.,	dychryn W.
feather,	puru Q., puyu A.,	pluen, plu W.
field,	cancha Q.,	caint W.
	vaca At.,	maes W.
	yapu A.,	ceufaes W.
	tunar At.,	cymle W.
figure, form,	culam At.,	eilun W.
fire,	humur At.,	ufel W.
	nina A., Q.,	tan W.
	cuati S.,	goddaith W.
flesh,	aicha A., aycha Q.,	hig A., eig W.
	sabur At.,	ymborth W. *meat.*
flower,	pucher At.,	fflur W.
fly,	cuspi Q.,	gwiban W.
foot,	kayu A., cuchi At.,	cas G.
	ebbachi S.,	ped W.
fountain,	puquio Q.,	ffynon W.
fowl,	hualpa A.,	golfan W. *sparrow*, gylfinog W. *curlew*, etc.
fox,	atoc Q.,	gwyddgi W.
friend,	cachomasi A.,	cydymaith W.
frog,	hampatua A.,	llyffant W.
	ccaira A., kayra Q.,	creiniog W.
ghost,	llantu Q.,	gwyllon W. *plural.*
girl,	ppucha A.,	bachgenes W.
	tahuaco A.,	hogen W.
	imilla A.,	plah A., merch W., *daughter.*
give,	huiti I.,	dodi W.

	Peruvian.	*Celtic.*
go,	humi A. Q.,	imich G.
goat,	paca A.,	boc G.
	sila, telir At.,	llill W.
gold,	ccori Q., coori A.,	aor W.
good,	asque A.,	gwiw W.
	alli Q.,	llesol W.
granary,	coptra Q.,	ysgubor W.
great,	capur At.,	syberw W.
green,	ccari, khal At.,	cri, glas W.
	komer Q.,	gorm G.
hail,	chijchi A.,	cesair W.
hand,	tachlli A., arue C., uru I.,	llaw W.
harness,	recau At.,	trec W.
hate,	coysma At.,	casau W.
have,	tausi At.,	dygyd W.
he,	hupa A.,	efe W.
head,	dala Y.,	talcen W.
	ppekei A.,	pen W.
	lacsi, hlacse At.,	llyw W.
heal,	callana A.,	gwellau W.
	hampi Q.,	cymodi W.
health,	ccaya At.,	iechyd W.
heart,	haiti At.,	uchd E. *breast.*
heaven,	urajpacha A.,	goruch W. *above.*
horn,	huakra Q., quajra A.,	adharc G.
hot, heat,	cambs At.,	twym W.
	capi At.,	craf W.
	huntu A.,	chwantus W.
	conic Q.,	cynhesu W. *to heat.*
house,	uta, ata A.,	ty W.
	puncu A.,	ffronc W. *hut.*
	turi, t'huri At.,	twlo W. *hut.*
	huasi Q.,	ios-da G.
in,	na A.,	an G.
increase,	aliyani A.,	helaethu W.
iron,	quella A.,	caled W.
jaw,	kaki Q.,	cargen W.
king,	capac Q.,	ceap E.
	curaca Q.,	goruch W. *supreme.*
kiss,	quischama At.,	cusanu W.
know,	yatina A.,	adwaen W.
lamb,	una A.,	uan G., oen W.
	chita Q.,	gid W. *kid.*
lance,	chuqui Q.,	gwayw, ysgeth W.
laugh,	teshma At.,	dychwardd W.
leaf,	llakka A.,	duilleag E.
learn,	yaticha A.,	dysgu W.
leg,	chara A.,	esgar W., cara E.
life,	haka A.,	bwch W.
light,	ccana A.,	cain, cynneu W.
lip,	uirpa, sirpi Q.,	gwefus W.

	Peruvian.	*Celtic.*
load,	penaclo At.,	pynorio W.
love,	qquipi At.,	hoffi, cudeb W.
louse,	lappa A.,	lleuen W.
male,	orko Q.,	gwryw W.
man,	chacha A.,	cia G.
	kkari A., Q.,	gwr W.
	huataki I.,	cathaidhe E., *warrior.*
medicine,	ccolla A.,	iachaol W.
meet,	tinquy Q.,	cynghyd W.
middle,	chaupi Q., taipi A.,	cefnaint W.
moon,	irare C.,	lloer W.
	quilla Q.,	gealach G.
	ccamur At.,	eighmor E.
morning,	ccara A.,	gwawr W. *dawn.*
mother,	mama A., Q., At.,	mam W.
	cua S.,	iog E.
mountain,	monono Y.,	mynydd W.
	kkollo A., iruretui C.,	gallt, garth W.
	pata Q., pico I.,	ponc, bre W.
mouth,	quaipi, khaipe At.,	safn W.
	simi Q.,	genau W.
much,	alloja A.,	lliaws W.
nail,	khin, qquini At.,	ewin W.
	sillu A.,	hoel W.
neck,	cunka Q., conka A.,	cegen, W. *throat.*
night,	haipu A.,	be E., gosper W. *evening.*
no,	hani A.,	chan G.
nose,	ibarioho C.,	ffri W.
	cenca Q.,	comhor E.
old,	ucuti I.,	gwth W.
open,	istorana A.,	egori W.
paint,	llampi Q.,	lliw W.
palace,	inca-pillca Q.,	plas W.
peace,	tecum At.,	tangnef W.
pigeon,	culcataya A.,	colom-cuddan W.
pike,	tupina Q.,	gwaywffon W.
plant,	liga A.,	llys W.
pot,	payla A.,	paeol W.
	potor At., ppucu A.,	pot W.
priest,	pachacuc A.,	faigh E.
rabbit,	cuys Q.,	cwning W.
race,	ayllo A.,	hil W.
rain,	hallu A.,	gwlaw W.
reed,	curcura A.,	corsen W.
rest,	sama A.,	seib W.
red,	pako A., Q.,	basc E.
rich,	capac At.,	cyfoethog W.
	quaraj Q.,	goludog W.
ripen,	poccoy Q.,	ffaethu W.
river,	hahuiri A.,	suir G.
	mayu G.,	afon W., amhain G.
road,	peter At.,	fford W.

	Peruvian.	*Celtic.*
round,	moyoc Q.,	amgant W.
run,	huayra Q.,	gyru W.
	paway Q.,	ffoi W.
see,	ulla A.,	seall G., gweled W.
	ricu Q.,	edrych W.
	unjana A.,	cenio W.
seed,	atha, sata A.,	had W.
servant,	yana A.,	gweinydd W.
sew,	chucuna A.,	gwnio W.
shadow,	chitua A.,	cysgod W.
sheep,	ccaura A.,	caora G.
shoe,	usuta, ojota A.,	esgid W.
sick,	onchok Q.,	gwanychu W. *sicken.*
sin,	chata A.,	gwyd W.
sister,	collacha A., turay Q.,	chwaer W.
sit,	tiyay Q.,	eistedd W.
skin,	ccara Q.,	croen W.
sleep,	punu Q., iquina A.,	hun A , huno W.
	iqui A.,	cysu W.
small,	huchuy Q.,	ychydig W.
	hisca A.,	bach W.
smoke,	heuque A.,	mygu W.
snake,	katari A.,	nathair G., neidr W.
sour,	callcu A.,	sarug W.
speak,	sana A.,	cynanu W.
	ni Q.,	yngan W.
	arusi, arusina A.,	areithio W.
spread,	takay Q.,	teddu W.
star,	sillo, huarahuara A.,	seren W.
	coyllur Q., halar At.,	reult G.
stone,	ccala A.,	careg W., gall G.
string,	lica Q.,	llin W.
	chanca A.,	tant W.
strong,	capac Q., At.,	cryfach W. *stronger.*
sun,	inti A., Q.,	ganaid W.
	villca A.,	haul W.
	puine Y.,	huan W.
	camosi S.,	samh E.
swallow,	reganama At.,	llyncu W.
sword,	calhua Q.,	claiseach E., cledd W.
tail,	chupa Q.,	cynffon W.
take,	hapi Q.,	tybio W.
teach,	yatichana A.,	addysgu W.
thigh,	changa Qt.,	clun W.
throat,	etippi S.,	gwddf W. *neck.*
thorne,	tiana Q.,	tron W.
throw,	toccnaclo At.,	tawlu W.
tie,	chinuna A.,	cynhas W. *mutual tie.*
trumpet,	cqueppa Q.,	cadbib W. *fife.*
trunk, stock,	capintin Q.,	cyff W.
truth,	quelechar At.,	gwirder W.
village,	lican At.,	llan W.

	Peruvian.	*Celtic.*
vulture,	condor At.,	gwylldyr W.
wall,	percca Q.,	bwrch W.
wash,	maylla Q.,	ymolchi W.
	harina A.,	glanau W.
water,	eubi S.,	aw W.
	puri At.,	mer W.
	yaku A., Q.,	uisge G., gwy W.
	sama Y., como I.,	
	huma A.,	amh E. *ocean.*
weave,	tilana A.,	eilio W.
well,	pucyo A.,	pydew W.
white,	hanco, hancona A.,	guen A., gwyn, can W.
	yurac Q.,	pur W.
wife,	liqui At.,	gwraig W.
wild,	kita Q.,	chwidr W.
will,	muna A.,	myn W.
	chicatha A.,	gogwydd W.
	orichuhuenhua C.,	gorchymyn W. *to will.*
winter,	casac-puchu A.,	gauaf W.
wizard,	pachacuc Q.,	faigh E.
woman,	rakka Q.,	gwraig W.
	marmi A.,	merch W.
	tana I.,	dynes W.
word,	aru A.,	gair W.
worm,	lacco A.,	llyngyr W. *worms.*
writing,	quippu Q.,	coffau W. *to record.*
young, youth,	huaina A.,	ieuaint, ieuanc W.
	iroco I.,	ir W.

GRAMMATICAL ANALYSIS OF THE INSCRIPTIONS.

No. I. *a.* koi *Basque*, desire.
entu *Basque*, to hear.
pono *Basque*, root of pontsu, *humeur sombre.*
Menera, a goddess, composed of B. *men*, power.
au B., this.

b. ni B., I.
Arba, proper name masculine, probably the root of the B. *arrapatu*, to seize.
imi B. imi-ni, ipi-ni, to place.
aur B., child.
ne B., n, an, en, to.
Kai, proper name latinized as Caius.
ba B., if.
erru-nai, compound of *erru*, root of B. *erruki*, compassion, and *nai*, will.

c. Sotoberri, proper name feminine, compounded of B. *soto*, vault, cellar, and *berri*, new.
uga, old B. word for mother, survives in *ugazama*, second mother *ugazaita*, second husband of the mother, and *ugatz*, mother's milk.
neke B., difficult, fatiguing, trouble, poverty.
aita B., father.
barka, root of B. *barkatu*, to pardon.
ka B., postposition, by, with.

d. mira B., astonishment, admiration from *miratu*, to behold, but here employed as a substantive, a spectacle or object to be seen.
erimi, from B. *er*, cause and *imi-ni* (see above, *b*).
etorri B., to come.
seme B., son.
ema, root of B. *ema-n*, to give.
gure B., our.
erren, peculiar form of erruki (see *erru-nai*, *b*).

e. emaitsa B., present, gift.
imi. see *b*.
uga, see *c*.
ra B., to.
erruki B., compassion, here an adjective.
anre B., lady.
aur, see *b*.
ne, see *b*.
lekatu B., to please.
be B., under.
neke, see *c*.
gabe B., without, deprivation.
ekarri B , to carry : original of the English word.
dio B., he does it to him.
ematu B., to calm.
bahi B., pledge.
gabe, see above.
erruki, see above.

f. pabetu B., to help.
imi, aur, ekarri, see above *b* and *e*.
gabetu B., to deprive.
entuka B. *entu*, to hear, and *ka*, by.
mira B. *miratu*, behold, see.
bere B., own.
beha B. *behatu*, behold.
beha-ka B., by beholding.
artu B., take, hold.
doi B., just, right.
gogoratu B., to remember.

g. achol B., care.
nio, Etruscan for *diot*, I do it to him, but B. imperfect of the same is *nion*.
egi B. *egin*, to do.
duen B., who has, for B. *den*, who is.
obi B., tomb, grave.
pabe B., help.
ba, Etruscan and Japanese, place.

No. II. age B. appearance, *age-ri*, declaration.
zio, Etruscan for *dio*, he does it to him; but the B. imperfect of the same is *cion* or *zion*.
utsite B. *utzi*, *utziten*, to leave.
ba B., if, see No I. *b*.
so B., regard.
jabe B., lord.

No. III. bizitate B. *biztandu*, *biztanle*, inhabit, inhabitant.
arau B., rule, night.
doi B., see No. I. *f*.
ematu B., to give, fuller form of *ema-n*.
debe B., prohibition, B. *debekatu*, forbid.
esatz B. *esan*, *esaten*, to say.
behi B., cow.

No. IV. al, ahal B., power.

No. VI. behatu, see No. I. *f*.
debe, see No. III.

No. VII. au, aita, see No. I. *a*, *c*.

No. VIII. esatz, see No. III.
asma B., a sign, indication.
ema, see No. I. *d*.

No. IX. mamuta B. *mamu*, phantom, *mamutu*, act a ghost.
mai B., table, tablet.
edatu B., to stretch, to extend.

No. XI. ichpichoi B. *ichpicho*, "pari, gageure," in dative.
atita, obscure form of *atchiki*, *itcheki*, to hold.
umerri B., lamb, small cattle.

No. XII. kosatze, now B. *koskatzen*, to carve.
mai, see No. IX.

No. XIII. bizitza, see No. III., *bizitza* now means "la vie."

No. XIV. aitabe B. *aitaba*, grandfather.
emaitz B., see No. I *e*.
bizitzate, see No. XIII.
umerri, see No. XI.

No. XV. goititu B., to raise.

No. XVI. danda B. "pact, obligation."

No. XVII. mate, explained in text *ad loc.*

No. XVIII. No new words.

No. XIX. dema B., "gageure."

No. XX. anoma B. *ano*, portion and *ema*, given.
bite-zar B., envoy.
no B., sign of genitive.
ranono B. *rano*, towards.
zari in B., *agint-zari*, *buru-zari*, a captain ; Japanese *kashira*, Semitic *sar*, Sclavonic *czar*, Teutonic *kaiser*, etc., etc., a universal word.
beka, form of B. *bekoki*, front, forehead, a chief.

No. XXI. beka, see above.

No. XXII. osa, old form on Iberic inscriptions of the Isle of Man. Its root is o as in B. *o-artu*, to give attention. In Japanese it is *uya-mai*, and also means "to give reverence." Here it denotes, *sa*, the person giving *o*, attention, to the flocks.
atherbe B., shelter.
obi B., grave, pit, cave.
ate B., door.
edate B. *edatu*, extend.
udahate is not B. *udahaste*, a synonym of *udaberri*, spring, but, as *uda-berri* is literally "new summer," so *uda-hate* will be "end of summer."
ambera, now B. *ainbertze*, as many as.
beheitu, B. *beheiti*, to lower.
mopira, Etruscan 8. See the writer's Etruria Capta.
ardizain B., shepherd.

No. XXIII. nausi B., master, more commonly *nabusi*.
non, Etruscan "who."

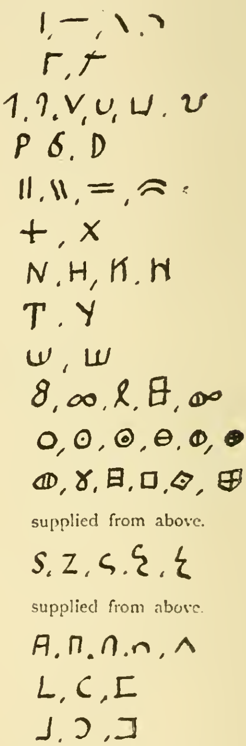
No. XXIV. oroi B., to remember, fuller *oroitu*.
am for *amar* B. ten.
ahal, al B., power, authority.
urte B., year.

No. XXV. jabe, see No. II.

No. XXVI. osa, see No. XXII., here employed as verb ; perhaps it should be written *o-tsu*.

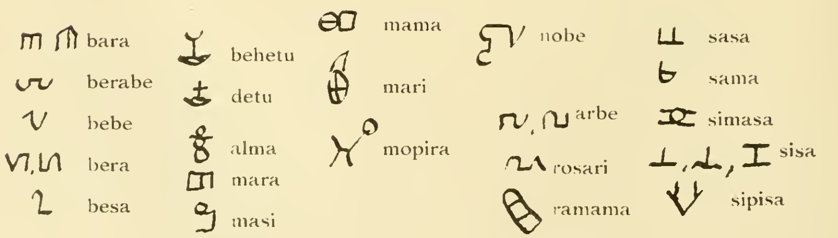
No. XXVII. abere B., cattle.
aberabe, survives in *apurtu*, *apurtzen* "baisser."
kama, evidently denotes one who has the care of sheep and other cattle. The writer does not know it as Basque. To keep domestic animals in Japanese is kai-oki.
lerrozarri B. *lerro*, a rank, and *ezarri*, to place.
mopi-ka, Etruscan *mopi*, 2, and B. *ka*, by.
obeka B. *obeki*, better.
athedatu for B. *itoitea*, or *ateratzen*.
ari B., ram.
almena B., power, vigour.

PHONETIC VALUES OF THE CANARY ISLAND CHARACTERS.



	Virgin.	*Rocks.*
vowel, diphthong, aspirate syllable, . .	I	
b, p syllables with a, o, u, ...	F	
" " e, i, ...	V	
to, tu, syllables, ...	P	
ta, te, ti, "	II	
da, de, di, syllables, ...		
ka, ga, "	N	
ko, ku, go, gu, syllables, ...	T	
go, syllables, ...		
l, "	B	
ma, mo, mu, syllables, ...	O	
me, mi, "	M	supplied from above.
na, no, nu, "	S	
ne, ni, "	E	supplied from above.
r, syllables, ...	A, R	
sa, so, su, syllables, ...	L	
se, si, chi, zi, syllables, ...		

COMPOUND CHARACTERS ON ROCKS.



THE RIPENING OF CHEESE AND THE RÔLE OF MICRO-ORGANISMS IN THE PROCESS.

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(Read 23rd March, 1901).

DURING the last twenty-five years the ripening of cheese has been the subject of numerous investigations, and although the problem has been attacked in many varied ways, it cannot be said, even now, that the changes which cheese undergoes from the time it is made until it is ready to be eaten have been fully explained.

The task of the investigator is, no doubt, a difficult one, owing to the many different kinds of cheese manufactured, the various ways in which they are made, and the diverse methods used to ripen them. The difficulties do not by any means end here. No two cheeses are exactly alike; the bacterial flora of the milk changes constantly; the methods of manufacture differ slightly from day to day, and more so from season to season; the temperature and humidity of the curing room usually alter with the outside temperature; and lastly the difficulty of sampling and the methods of analysis leave much room for improvement. The constant publication of some new culture method for lactic acid bacteria suggests that as yet no completely satisfactory method of cultivating them has been discovered.

Ferdinand Cohn, in 1875, declared that the ripening of cheese was a fermentation due to the influence of fermenting organisms. He microscopically investigated rennet, and finding bacteria present in this substance concluded that the ripening of cheese was due to bacteria introduced into the cheese from this source. He considered that the milk sugar underwent butyric fermentation. He also found *B. termo*, *micrococci*, and the *Hay bacillus* present in the cheese, and came to the conclusion that these were introduced with the rennet, because Remak had found *B. subtilis* in the stomachs of calves.

A few years later Duclaux published his researches upon Cantal cheese, a soft cheese manufactured in France. He isolated a number of micro-organisms, six of which he supposed were of special importance, —an alcoholic, a lactic, a butyric, and a ferment acting upon casein and forming alkaline nitrogenous compounds of simple composition. Of the remaining two ferments, one was a vibrio, which preferred an optimum temperature of 75° to 80° F., formed spores, and caused the development of carbonic acid and hydrogen gas when grown in milk. The casein was transformed into an albuminous substance, soluble in water; small amounts of butyric acid and sodium butyrate were formed.

Duclaux attributed the matting together of the cheese after it was cut, to the action of this vibrio, which he thought caused the parts of the coagulum to stick together and form a solid mass of cheese; consequently the presence of this germ was desirable, but unfortunately should the germ enter the coagulum itself gas was produced; and, as a consequence, the cheese became puffy or swollen.

The other ferment was more objectionable because it formed acetic acid, and a substance of an intensely bitter taste.

In conclusion, he considered the ripening to be caused by the butyric ferment, because under its influence the casein was precipitated, but afterwards gradually dissolved or digested. This ferment was probably helped by others which acted upon the albuminoids so as to split them up into compounds of a less complex nature; ammonia being the simplest.

Benecke made a microscopical analysis of Emmenthaler cheese of different ages. He found Cohn's rod-like bacteria, which were probably identical with *B. subtilis*, and also yeasts. In consideration of the circumstance that the formation of peptone like bodies took place chiefly at the beginning of the ripening process, and that at this time an increase of schizomycetes, probably identical with *B. subtilis*, was noticed, Benecke arrived at the conclusion that the ferment which brought about the peptonization of the fresh curd was *B. subtilis*. The objection that *B. subtilis* was an aerobe and could not live in the interior of hard cheese, he set aside and followed the observation of Liborius, according to which *B. subtilis* retained its peptonizing qualities even though the air was shut off, provided that some kind of sugar was available. The gradual disappearance of the milk sugar was accountable for the diminishing of the rod-like forms in cheese which had reached a more advanced stage of ripeness. In conclusion, Benecke believed that the formation of amides (Leucin, etc.) was not due to the action of the Schizomycetes.

In 1887, Duclaux published the results of further studies upon

Cantal cheese, and was able to study them in pure culture. The dilution method of culture was then in vogue, and some writers have criticised the accuracy of his work on this account. Not only did Duclaux isolate a large number of species, but he was able to contribute other interesting details as to form, spore formation, aerobic and anaerobic characters, physiology, and the nature of the fermentation products of the different micro-organisms, that he studied. To all species isolated, he gave the generic name Tyrothrix (cheese-threads), and of these seven were aerobic and three anaerobic. All but one of these germs possessed the ability of coagulating the casein ; and, subsequently digesting the coagulum. From one of the most energetic of these bacteria *(Tyrothrix tenuis)* Duclaux isolated a ferment which was able to convert the casein into a soluble peptone. This he called casease and the product of its action on casein he named "caseone"; this latter substance might be even further split up into other substances, as leucin and tyrosin. Several of the other species isolated also produced the last named substances.

Adametz, working at Sornthal, in Switzerland, in 1889, upon Emmenthaler and Cottage cheese (a soft variety) isolated nineteen different, well characterized schizomycetes and three yeasts. Of the first, seventeen were new species and were supposed to influence the ripening process. Contrary to previous investigations, neither *B. subtilis* nor *B. butyricus* was found. He divided these bacteria into three groups.

(a). Such as dissolved the paracasein, or changed it to a peculiar spongy condition. Soluble albuminoids and peptones were produced in greater or less quantities at the same time, and these were accompanied by traces of smelling (*e.g.*, Butyric acid) and tasting (*e.g.*, bitter extractive matters) substances.

(b). Such as developed slowly in sterilized milk, and for which unchanged paracasein was not a favorable soil, but they easily assimilated the substances produced by the first group.

(c). Such as had no appreciable effect upon any of the nutritive substances herein concerned, and whose presence or absence made no difference to the ripening of the cheese.

The Cottage cheese was distinguished bacteriologically from Emmenthaler by the following points :

1. The larger bacterial content (in one gram of Emmenthaler 850,000 germs, and in one gram of Cottage cheese 5,600,000 bacteria).

2. The greater number of species.

3. The relation of the peptone gelatine liquefying to non-liquefying colonies (1:300 to 1:600 in Emmenthaler against 1:90 to 1:200 in Cottage cheese).

The bacterial content of Emmenthaler was shewn to grow during the ripening process from 90,000 to 850,000 and finally he ascribed to the liquefying germs the rôle of ripening the cheese.

Adametz also demonstrated that when disinfectants like Thymol and Kreolin were mixed with the curd, the ripening process was totally checked. The same result ensued from attempts to ripen Hauskäse (Cottage cheese) in an atmosphere of Carbon disulphide.

De Freudenreich by the use of better methods, such as the employment of whey peptone gelatine, and more accurate triturations obtained much higher figures than Adametz. He followed, step by step, the ripening of a single cheese, in order to see if the changes the cheese passed through were the work of special microbes, and to see if the species present at the commencement of ripening continued active until the end of the process. The cheese was analysed at intervals of eight to fifteen days, and like Adametz he found in fresh cheese many different micro-organisms which quickly disappeared as the cheese aged, so that at the end of eighteen days, a microbe called *Bacillus x*, by the author, predominated in the culture plates; and at the end of sixty-four days, only this bacillus was found. The analyses were continued until the 155th day when the ripening was perfect. The cheese at this time contained 1,662,500 bacteria per gramme, all being *Bacillus x*. The highest number counted was 8,975,000 when the cheese was fifty-two days old, but there were considerable fluctuations in the numbers found.

The *Bacillus x* was a true type of lactic acid germ, somewhat similar to Adametz' No. xix, and forming, like it, lactic acid.

De Freudenreich described minutely the morphology, physiological properties, and the resistence of this germ to desiccation and chemical agents, and in the end came to the following conclusions:

1. The ripening of cheese was the work of bacteria; without bacteria there was no ripening.

2. Two periods could be distinguished in the ripening: the first characterized by the presence of many species, and the second distinguished by the predominance of one bacterial species. In most cases

this single microbe was *Bacillus x ;* when it was absent, it was replaced by other bacteria belonging to the same class of lactic acid ferments.

Adametz had also found in predominating numbers a germ very like *Bacillus x*, and which also behaved as a true lactic ferment.

It did not appear probable that these germs alone produced all the phases of ripening without the co-operation of other bacteria.

Lloyd, who was appointed by the Council of the Bath and West of England Society to make investigations upon Cheddar cheese making, did much work on this subject ; and, although no numerical data are given, he stated after making over 100 separate cultivations that, "In the manufacture of Cheddar cheese, one and only one organism plays an important part up to the time the curd is put into the vat, and that organism is *Bacillus acidi lactici.* Further, when the cheese was ripe, this bacillus was most abundant, and that the ripening during the first few months depended mainly on *B. acidi lactici*, supplemented as the cheese grew older by the growth and action of *B. amylobacter.*"

De Freudenreich in 1895 followed up his first work by a very extensive contribution to the subject. He analysed bacteriologically five Emmenthaler cheese made at the Rütti Dairy School. By means of special methods, such as the use of milk serum gelatine, milk agar, anaerobic culture methods, and partially sterilized cheese emulsions, he endeavoured to give the best opportunities for the liquefying species to grow, especially Duclaux's Tyrothrix forms. The latter were, however, found only a few times. The principal liquefiers present were bacilli belonging to the *subtilis* and *mesentericus* groups. These researches corroborated his former work.

The lactic acid species, abundant from the start, increased greatly during the ripening process, whilst the liquefying germs were few in number and decreased rapidly.

The most complete numerical data given of a single cheese were as follows :

Age in Days.	*Number of Bacteria per Gramme.*
Fresh Cheese 1	750,000
9	15,000,000
33	20—30,000,000
42	40,000,000
65	20,000,000

Forms resembling Tyrothrix were found only four times out of sixteen analyses.

Hay bacillus types were found only six times out of eleven analyses. Agar surface plates were used for isolating these germs as they favoured the growth of this group of micro-organisms.

The remaining analyses agreed very closely with the above; on one occasion, however, the large number of 100,000,000 lactic acid bacteria per gramme were found in cheese ten days old.

In addition to these analyses, a number of cheese were made with starters made from the microbes isolated during the investigation, as well as many of Duclaux's Tyrothrix forms. The experimental cheese were compared with control ones; and in most cases the ripening was not normal. In conclusion, de Freudenreich drew the following conclusions from his observations and experiments:

1. Those often looked upon as prime factors in the ripening of cheese—the gelatine liquefying bacilli (Tyrothrix, Potato, or Hay bacillus) are not numerous in cheese, and generally not in milk.

2. Far from multiplying in the cheese, they seem, even if added to it in great quantities, to die off rapidly, unless when added in the spore form, in which case they remain alive for a longer time, but without multiplying.

3. Added to milk set for cheese, they seem neither to produce fermentation nor favour it.

4. Probably various lactic ferments play the principal, if not the only part, in the ripening of Emmenthaler cheese. In the soft cheese, on the contrary, *Oidium lactis*, and also yeasts, take part in the ripening.

Under the heading of "Character and Variability of species of Tyrothrix," W. Winkler found after an examination of species of Tyrothrix that, "whilst some, as *T. tenuis*, were more allied to the hay and potato bacilli, others as *T. urocephalum* and *T. filiformis* were more nearly connected with the granulo-bacteria. They adapted themselves with great ease to different nutrient media, and their characters thereby became altered. In milk, they were more or less peptonizing. Butyric acid was only produced by a few of them. Milk sugar favoured their growth, but seemed to interfere with their peptonizing power." Three varieties of Tyrothrix tenuis were cultivated: (1) a form which peptonized milk and liquefied gelatin; (2) a form which produced lactic acid, but did not liquefy gelatin; (3) a fluorescing type which formed a red pigment on potato.

Winkler stated that, "*Bacillus xvi*, *Adametz*, which was undoubtedly a species of Tyrothrix, was an example of the conversion of a lactic acid bacterium into a peptonizing organism. *T. urocephalum* and *T. tenuis* were found to aid the ripening of cheese, and there were grounds for believing that in this ripening, peptonizing bacteria played the principal part. A bacteriological examination of hard cheese always shewed a greater preponderance of lactic acid bacteria, and this might possibly be explained in this way, that certain peptonizing bacteria changed in the cheese to lactic acid bacteria, especially strongly developing the property of producing lactic acid. Aside from the behaviour of *T. tenuis* and *T. urocephalum* that of *Bacillus xvi, Adametz* in Emmenthaler cheese confirmed this view."

These results of Winkler were subsequently made the subject of a special research by Wittlin working under von Freudenreich. The experiments were made with a culture obtained originally from Duclaux, and after many cultivations on gelatin, no evidence was forthcoming to support the conversion contended for. Wittlin failed to be convinced that *Tyrothrix tenuis* could be converted into a lactic acid bacterium ; and the author supposed that Winkler's results were due to contamination.

By making emulsions of various cheeses and inoculating milk therewith, De Freudenreich was able to obtain absolutely anaerobic bacilli. These bacilli probably formed butyric acid. One of the two isolated, formed spores and from its shape, it was evidently a clostridium form. He named it *Clostridium foetidum lactis*. This germ, as its name implied, imparted a disagreeable odour to the milk, did not grow on gelatin, but on agar developed slowly giving rise to a cheesy odour. It apparently entirely dissolved the casein of milk, this medium turning yellow, only a slight sediment remaining. Later de Freudenreich became convinced that this clostridium was identical with the bacillus of malignant oedema. I have also found this bacillus in two small experimental cheese, made from Swiss milk, its pathogenicity and cultural characteristics leaving no room to doubt its identity.

In a Holland cheese, Weigmann found two aromatic bacilli. These gave the milk cultures a cheese aroma. When pasteurised milk was inoculated with these forms, and cheese made. it ripened and resembled Swiss cheese. These germs were gelatine liquefiers and digested the casein of the milk.

A very systematic study of the rise and fall of the bacteria in Cheddar cheese was made by Russell and Weinzirl. Six cheeses were analysed at various periods, and the qualitative distribution of the bacteria found at the different stages is thus graphically delineated by the author's diagram.

The general results are thus summarized :

1. There is at first a marked falling off in the number of bacteria in green curds for a day or two. (Period of initial decline).

2. This is followed by a very rapid increase in numbers, in which the bacteria reach scores of millions of organisms per gram. (Period of increase).

3. This period is followed by a diminution in numbers at first rapid but later more gradual, until the germ content sinks to insignificant proportions. (Period of final decline).

4. The time necessary to reach the maximum development (second period) is hastened or retarded by such external conditions as temperature, etc.

5. The second period also marks the beginning of the physical change that occurs in the cheese in the earlier part of the breaking down of the casein.

6. The bacterial flora of cheese differs markedly from that of milk. In milk, the lactic acid bacteria predominate, but accompanying them are always liquefying or peptonizing organisms, and as a rule bacteria capable of developing gaseous bye-products.

In the ripening cheese the peptonizing or casein digesting bacteria are quickly eliminated ; the gas producing bacteria disappear more slowly, sometimes persisting in very small numbers for a long time.

The lactic acid bacteria on the other hand develop enormously for a time until the cheese is partially ripened, when they too begin to diminish in numbers.

7. The generally accepted theory that the peptonizing or digesting bacteria are able to break down the casein in the cheese as they do in milk is improbable because this type of bacteria fails to increase in the cheese and usually disappears before there is any evidence of physical change in the condition of the casein. The same is true where cheese is made from pasteurised milk to which copious starters of these peptonizing organisms have been added.

8. The coincidence existing in point of time between the gradual ripening of the cheese and the marked development of the lactic acid

bacteria seems to indicate that these phenomena are causally related. This view is further strengthened by the fact that cheese made from pasteurised milk in which the lactic acid bacteria have been destroyed fail to ripen in the normal manner, while the addition of a pure lactic acid ferment to the pasteurised milk permits the usual changes to occur in a probably normal way.

Schirokich took up the study of this problem by preparing in milk pure cultures of some peptonizing bacteria as well as of lactic bacteria and then investigating by means of chemical analysis the changes which took place in the milk in the course of the development of the micro-organisms in it. The change in the composition of the milk was studied as to (1) the quantity of the casein of the milk converted into soluble form ; (2) the amount of ammonia found in the cultures, and (3) the amount and kind of fatty acids produced by the micro-organisms.

With reference to the first point, it was found that while the peptonizing bacteria converted during the first fifteen days of their culture almost all of the casein of the milk into proteids soluble in water, and the remainder into products of decomposition, the lactic bacteria did not alter in the slightest, the amount of nitrogen in the soluble protein matter after thirty days of culture. In other words, while the bacteria of the former group acted very energetically on the casein, those of the latter group did not affect it at all. The fungus *Oidium lactis* was also found very active in changing the casein, although in a lesser degree than the peptonizing bacteria.

Further, the author found in the cultures of the *Oidium lactis* less ammonia than in those of the peptonizing bacteria, and none whatever in the cultures of the lactic bacillus.

Finally, on comparing the nature of the fatty acids formed in cheese (the author experimented with hard Gruyére and soft Brie cheese) and those produced by the bacteria in pure cultures, he found that the mixture of the volatile acids caused by the bacteria not liquefying gelatine did not correspond to those which are formed either in the hard or in the soft cheese. On the contrary, the volatile acids produced by the peptonizing bacilli were found to be very similar to the mixture of these acids produced in the ripening of Gruyére cheese. And, lastly, great similarity was observed between the volatile acids of the soft Brie cheese and those produced by the fungus *Oidium lactis*.

Thus, all three lines of investigation pursued by the author lead to the conclusion that the bacteria of lactic fermentation, though present in milk and cheese in very great numbers, do not induce the changes in the casein in the process of ripening, and if they exert any influence at all, it is only indirect, since these bacteria do not dissolve casein, do not give off ammonia, and do not form the volatile acids characteristic of ripened cheese. The peptonizing bacteria and the fungus *Oidium lactis*, on the other hand, produced all the changes of casein which take place in the ripening of cheese; they yield soluble proteids and decompose albuminous compounds with the formation of ammonia and volatile acids corresponding to those occurring in the cheese.

It is pointed out that the peptonizing bacteria would appear from the foregoing to play an exclusive part in the ripening of cheese, but such a conclusion would overlook the important fact established by the analysis of Bondzinsky, viz.: that there is in cheese only a small quantity of peptone which is not precipitated by ammonia sulphate. In opposition to this fact, the author found while investigating the nature of the soluble albuminous bodies in pure cultures of peptonizing bacteria that, under the influence of these micro-organisms, the casein is converted almost entirely into peptone. In view of these opposing facts, the author concludes that the joint action of the peptonizing bacteria and the lactic acid bacteria must be considered as essential to the ripening of cheese, and that this should serve as a starting point for future investigations of the process.

The lactic acid bacteria are not capable of producing this process, while the peptonizing bacteria, when they multiply without any check, carry on the decomposition too energetically and to an undesired extent, but in the presence of lactic bacteria, which in a measure restrict and regulate the development and activity of the peptonizing bacteria, the joint effort of all these micro-organisms gives the desired results.

From this point of view, the chief care in the production of cheese should be that both the peptonizing and the lactic bacteria are in the curd, and that the proper conditions for their life activity are provided. But the peptonizing bacteria, especially the *Bacillus subtilis*, are very widely distributed and multiply with extreme ease; therefore from a practical standpoint, no provision need be made for their presence, and attention should be confined to the lactic acid bacteria.

Having defined the part which the peptonizing bacteria play in the ripening of cheese, the question still remains unsettled whether these bacteria, which are according to de Freudenreich present in hard cheese in small numbers, act as such in the process of ripening, or by means of diastase secreted by them at the beginning of the process. The author states that in experiments made by him, it was shown that the diastase in question, named by Duclaux, casease, acts just as energetically in the absence of the bacteria by which it is secreted as in their presence. From this, it would follow that if casease is a factor in the ripening of cheese, it would have to be present only in a small amount.

Von Freudenreich's results in two series of experiments made in 1897, considerably strengthened the theory that the lactic acid germs were the chief factors in the ripening process. He grew a number of lactic acid bacteria, isolated from cheese, in sterile milk to which chalk had been added to neutralize the acid formed by the bacteria. These bacteria were thus able to continue their growth, and at the end of two or three months a portion of the casein was found to be converted into soluble products. Thus cultures of three different species of lactic acid germs gave 5.1, 6.4, and 2.4 times as much soluble nitrogen as there was present in the original milk. The reaction of these cultures was not acid, but neutral or slightly alkaline.

Von Freudenreich concludes his second paper by stating that "It appears from my experiments that the lactic acid ferments, especially those isolated from cheese, are endowed with the power of rendering the casein soluble and decomposing it. Emmenthaler cheese, as compared with the results of Bondzynski, gives even more conclusive results. Thus, the latter author found in the filtrate of two emulsions made from ripe Emmenthaler cheese 1.44 and 1.51 per cent. of soluble nitrogen. The nitrogen of the amides gave 0.93 and 0.82 per cent. These figures are nine to ten times higher than mine, but Bondzynski analysed cheese and I milk. But it takes eleven kilograms of milk to make a kilogram of cheese and the agreement is as perfect as can be when we consider that the experimental conditions (temperature, etc.), were not identical. These results, proved by my numerous experiments, show that the lactic acid ferments are in enormous numbers in ripening cheese, whilst other species, as the Tyrothrix class, are relatively rare, and this fact permits us to affirm that the microbic agents in the ripening of cheese ought to be looked for among the lactic acid ferments."

A new factor in the ripening of cheese was the discovery of an unorganized ferment, or enzyme, in milk by Babcock and Russell. These authors kept milk in contact with an excess of chemical substances that destroyed the metabolic activity of bacteria, but which did not suspend entirely the action of the organized ferments. Under these conditions the milk coagulated, and there was a progressive formation of soluble proteids (albumoses and peptones) comparable to the breaking down of the casein in the normal ripening of cheese. Bacterial life was not absolutely excluded from the milk with which the experiments were performed, but by elaborate precautions, their numbers were minimized as much as possible.

Cheese was also cured under anaesthetic conditions. A cheese kept under chloroform and heavily saturated with this anaesthetic, even when more than a year old was physically thoroughly broken down and resembled a well cured cheese. Chemically, more than fifty per cent. of the casein was converted into soluble products, which amount is about the same as that found in normal cheese of the same age. Bacteriologically it was sterile.

These experiments seemed to the authors to indicate that the inherent enzymes in the milk played a very important rôle in the breakdown of the casein.

The year following this discovery, 1897, the authors published additional studies, and named the ferment *galactase*, on account of its presence in milk. It was found that this enzyme was allied to trypsin, the digestive ferment of the pancreas, and in this connection, it is interesting to note that Jensen independently discovered that cheese made from pasteurised milk with the addition of ether to prevent bacterial action, and a certain amount of pancreas to furnish the trypsin, cured more quickly, and contained nearly fifty per cent. more soluble nitrogen than cheese made without the addition of pancreas.

This ferment, however, differs from trypsin, in that it gives rise to a certain amount of free ammonia. It also differs with regard to the temperature at which its action is most energetic.

Storch's test for determining whether milk has been heated to a temperature exceeding 80° C. depends on the presence of galactase, the activity of which is destroyed by this temperature.

Babcock and Russell also made extensive researches on the distribution of galactase in different species of mammalia, in individual milks at the same or different periods of lactation, etc.

As to the structures in the body in which the enzyme is found, the authors have not yet examined the mammary glands for its presence, but suggested the close relationship between the blood and milk, as seen in the production of immunizing substances in the milk of animals rendered artificially immune to bacterial poisons.

Barthel has recently pointed out that normal cows' milk contains large numbers of leucocytes, and attributes Storch's test to their presence in the milk.

He even considered the leucocytes, or an enzyme secreted by them, as the cause of the phenomena observed by Babcock and Russell and by them attributed to galactase. The leucocytes also behave in the same manner towards anæsthetics as galactase, and another indication that the colour reaction obtained in Storch's test is due to the presence of leucocytes, is that whey gives a reddish-brown and not a blue colour as in the case of milk. The latter colour has been shown by Storch to be due to the casein of the milk.

Schirokich, in 1898, experimented on the diastases produced by *Tyrothrix tenuis.* He grew the bacillus for four days at 35° C., and then filtered the culture through a porcelain filter, and added the germ free filtrate to sterilized milk. The milk was digested and the casein became soluble in water, but the liquid had not the odour of cheese. He then tried another method. A pure culture of a lactic acid ferment was made in sterilized milk, and, as soon as complete coagulation had occurred, five per cent. of the sterile diastase from Tyrothrix was added, and the mixture kept at 35° C. The diastase acted slowly on the casein, and at the end of fifteen days the mixture had a typical cheesy smell. This experiment was repeated several times with like results. Further experiments showed that the intensity of the cheesy smell depended on the amount of acid present in the milk when the diastase was added.

In conclusion, Schirokich suggested the use of pure cultures of lactic acid together with digesting bacteria and casein, as the ripening seemed to be a special kind of symbiosis.

In a series of articles in the Milch Zeitung (1899) Weigmann discussed the rôle of the lactic acid bacteria in the ripening of cheese, especially referring to the work of de Freudenreich. He drew the following conclusions, from his own experiments and also those of other investigators:

1. The special lactic acid bacteria are not cheese ripening bacteria, the form used by de Freudenreich in his experiments being only facultative, or more probably degenerate lactic acid bacteria.

2. Lactic acid bacteria have an important rôle in cheese ripening, not in actually taking part in the ripening, but by directing the process in the right direction.

3. This function consists in eliminating certain forms of bacteria and fungi by the lactic acid formed, and providing an acid nutrient medium upon which only such bacteria and fungi can thrive as can withstand the acid or consume it. The micro-organisms which consume the acid prevent its accumulation in too strong a degree, take part in the peptonizing and flavour producing processes, and enable other bacteria or fungi, whose activity was weakened by the acid, to continue their work.

4. The specific character of a particular kind of cheese depends upon the predominating form of micro-organism, which the manner of preparation and the handling of the cheese have brought about.

Boekhout and Vries made Edam cheese from pasteurised milk inoculated with a lactic acid ferment isolated from Edam cheese, but this cheese failed to ripen.

Cheese made from pasteurised milk and inoculated with a plug of fourteen day old cheese did not ripen, and the same result took place with cheese made from pasteurised milk and mixed with five per cent. market milk.

Better cheese was made from milk heated to 55° C. for half an hour and treated exactly as in the above experiments, but even this cheese could not be called normal.

Then these investigators attempted to get milk as germ free as possible, by washing the hind quarters of the cow with soap and water, followed by three per cent. boracic acid. The milker's hands were similarly treated. The milk obtained was not quite sterile, but so poor in bacterial content that a portion remained a long time in the incubator at 22° C. without change.

This milk was divided into two equal portions without the addition of any cultures, and this part acted as a check on the other portion with which the following three experiments were made:

1. Milk inoculated with fourteen day old cheese.

2. Milk inoculated with a lactic acid bacterium isolated from Edam cheese.

3. Milk inoculated with market milk.

In numbers one and three experiments, there was a normal ripening, but the cheese made from milk inoculated with the lactic acid bacillus failed to ripen.

The cheese made from the control milk did not ripen at all.

From these experiments, the authors concluded, that:

1. The heating of milk changes the casein and prevents ripening.

2. If we look for the ripening organisms among the lactic acid bacteria, we must remember that not all the lactic acid bacteria are able to cause the ripening.

3. The theory of Babcock and Russell is incorrect, otherwise the control cheese would have ripened.

4. If the theory of Weigmann should be confirmed, it must be qualified in so far that the organisms are still alive on the fourteenth day, for the cheese used was inoculated with fourteen day old cheese.

De Freudenreich and Jensen's experiments on the relation between lactic acid ferments and the ripening of Emmenthaler cheese were very extensive and thorough. They conclude that the Tyrothrix bacilli take no part in the ripening. They did not multiply in normal cheese, and even when added in large numbers they exerted no influence on the decomposition products, in fact their influence was harmful.

The natural enzymes (galactase) perhaps participate in the ripening, by rendering the casein soluble, and thus facilitate the operations of the lactic acid ferments. Pasteurising deteriorates the quality of Emmenthaler cheese. Another fact brought out was the loss of the soluble constituents of cheese during ripening.

The results of de Freudenreich's experiments in 1900 confirmed the work of Babcock and Russell upon galactase. Several new facts were also demonstrated. The presence of 0.3 and 0.5 per cent. of lactic acid considerably decreased the action of the diastase.

Twenty per cent. of ether was added to milk sterilized at 120° C. and this was then inoculated with a few drops of an emulsion of spores of *Tyrothrix tenuis*, and kept at 35° C. Another lot was similarly treated, except that the emulsion of spores was previously heated to 100° C. to destroy the enzymes present. At the end of three months, the latter sample had undergone no change and contained 0.053 per cent. of soluble nitrogen ; and in the first sample, a change commenced at the end of four weeks, and progressed rapidly during the next two months. At the end of that time, there was some digestion of the casein, and the chemical analysis showed 0.098 per cent. of soluble nitrogen. This experiment showed that not only were the diastases produced by bacteria capable of action on casein, but even bacteria themselves or even their spores might contain digesting enzymes. De Freudenreich does not believe with Babcock and Russell that galactase plays the principal rôle in the ripening of cheese, but that in rendering the casein soluble, it probably prepares for and facilitates the work of the bacteria which cause the ripening, and the special taste of cheese.

Jensen studied the origin and properties of the enzymes found in cheese, both hard (Emmenthaler) and soft (Limbourg) varieties. To determine if galactase played any part in the ripening, he examined the following four points :

1. Is the galactase of milk in sufficient quantity in the curd to be able to produce an appreciable transformation of the casein ?

2. How long does galactase remain in the cheese ?

3. Are the natural conditions met with in cheese such, that the galactase can exercise its action ?

4. Does the pepsin of the rennet take part in the ripening of cheese?

By a series of analyses, too long to quote in this paper, Jensen partially answered the above questions. Thus he concluded :

1. As cheese is made with rennet, galactase and the pepsin in rennet are present in sufficient quantities to produce changes in the casein.

2. Soft cheese is richer in enzymes than hard cheese.

3. The quantity of free lactic acid in soft cheese is sufficient to hinder the action of galactase, and consequently favour the action of the pepsin. In hard cheese, on the contrary, the quantity of free lactic acid present is smaller and only helps in a slight degree the action of pepsin at the expense of the galactase.

To determine which of the two factors, the lactic ferments or galactase, plays the principal rôle in the ripening of Emmenthaler cheese, Jensen compared the changes which naturally occurred in the casein of cheese with those caused by *each* of the two factors above mentioned. The action of galactase was rendered insignificant by using one per cent. of Formalin to prohibit bacterial action. Whilst to show the action of this enzyme, fifteen to twenty per cent. of ether was used to destroy the bacterial life. Again, the chief function of galactase is in its rendering albuminoid substances soluble, and the principal rôle of the lactic ferments is to form decomposition products. Thus, at the commencement of the ripening of Emmenthaler cheese, soluble albuminoid substances are first formed and only traces of decomposition products; and as soon as the free lactic acid is neutralized, the lactic acid bacilli commence their work, and immediately there is a considerable increase in the decomposition products. The quantity of these latter products constantly increases during the rest of the ripening process, whilst the augmentation of soluble albuminoid substances diminishes. In other words, the lactic acid bacteria or their enzymes become more and more the only factor in the ripening, probably because the galactase becomes gradually enfeebled.

In a few words, the changes that occur in the casein during the ripening of Emmenthaler cheese seem to consist of a metabiosis between galactase and the lactic acid bacteria.

From the results of his researches, Jensen thus describes the process of ripening in Emmenthaler cheese:

The curing is due to different fermentations accompanied by two simultaneous processes—salting and drying. The term fermentation is used to signify the chemical decomposition, due to organized ferments or to unorganized ones.

Salting helps to keep the cheese, moistens the crust, and facilitates the drying. These two actions (salting and drying) delay fermentation, especially at the outside of the cheese.

Before transferring the cheese to a warmer room, salting and drying continue for some time, during which interval the galactase begins its action and dissolves or renders the casein soluble. At the same time, the amount of free lactic acid gradually diminishes.

As soon as the cheese is placed in a warmer room, the lactic acid bacteria commence to increase, the temperature of the interior of the cheese probably rises, and the greater part of the soluble albuminoid substances form and the production of the holes finishes. The drying out of the cheese also occurs more quickly at this temperature. Finally, the cheese is transferred to a cooler room, in which the cheese age, until they are considered ripe. During this time, the bacteria slowly die out, but their enzymes continue to act, and increase the quantity of the products of the decomposition of casein. During the last fermentation, the final salting is given, and drops of liquid (tears) form in the holes in the cheese.

Babcock and Russell, in 1900, experimented upon the action of rennet on cheese, and concluded from a number of experiments:

1. That an increase in the amount of rennet extract used in making cheese does increase the amount of soluble nitrogenous products, which measure the progress of cheese ripening.

2. Increase in amount of rennet used does not increase the water content of cheese; and, therefore, the ripening of cheese cannot be indirectly affected in this way.

3. The products of peptic digestion in milk and cheese are confined to the higher decomposition products, viz., albumoses and peptones precipitated by tannin.

4. The increase in soluble nitrogenous products and also in milk due to an increase in amount of rennet extract used are confined to those bye-products that are peculiar to pepsin, thus indicating that the digestive action of rennet extract is attributable to the action of the pepsin incorporated with rennet extract.

5. The crucial test of this conclusion was made by adding purified pepsin to milk and making the same into cheese, where rennet extract was or was not added to curdle the milk. In such cheese digestion has been increased in those cases to which pepsin has been added, and this increase has been confined to those bye-products that are characteristic of pepsin, and which also appear in cheese made with high quantities of rennet.

6. The digestion in cheese incident to pepsin is determined mainly by the degree of acidity developed in the milk and curd. In Cheddar cheese, peptic digestion probably does not begin until the acidity of the milk is approximately 0.3 per cent. lactic acid.

7. Acid salts as phosphates, etc., favour peptic digestion in milk in a manner comparable to free acids.

8. Free acid does not normally exist in Cheddar cheese, the apparent acidity being due to acid salts.

The results of the first researches of Chodat and Hofman-Bang were published in 1898, and their conclusions were as follows:

1. A single species of bacterium can produce the digestion of the casein and the characteristic odour of cheese.

2. That contrary to the opinion of de Freudenreich but in agreement

with that of Duclaux, bacteria which are not lactic acid producers can ripen cheese.

3. The acidity at the commencement of ripening was not necessary to bring about the solubility of the casein.

In their second paper (1900), they criticised de Freudenreich's work and report fresh results. De Freudenreich had shown that lactic acid bacteria could attack and render soluble portions of the *casein of milk*, but Chodat and Hofman-Bang point out that it has not been shown that the lactic acid bacteria can attack *coagulated casein;* and these two substances are so different that the results which have been obtained with the casein of milk cannot be applied *a priori* to coagulated casein.

These authors then experimented to see if the lactic acid bacteria were capable of dissolving coagulated casein. They isolated five different germs from Emmenthaler cheese, all of which produced lactic acid, and others volatile acids, as formic, acetic, and valerianic. These germs were grown on coagulated sugar-free casein, which had been previously sterilized at 120° C., for three successive days. At the end of three months, the acidity of the lactic ferments and the percentage of soluble nitrogen were determined, and it was found that the bacteria were well developed and were not contaminated, and that the quantity of soluble nitrogen had not increased. All the cultures had a feeble butyric odour. From this experiment, the authors concluded that de Freudenreich was wrong when he said "That the lactic acid bacteria play the principal rôle in the ripening of Emmenthaler cheese." Further, they thought the lactic acid bacteria in their cultures grew at the expense of the casein dissolved in the water used for moistening the curd in the culture flasks.

The lactic acid bacteria were also sown in flasks containing casein modified by casease obtained from a species of Tyrothrix. The casein was not dissolved by the Tyrothrix, and after the lactic germs had grown on this substance for two-and-a-half months, there was no increase in the percentage of soluble nitrogen.

Again, they seeded sterilized casein with living Tyrothrix, allowed it to grow until the curd was softened, without becoming liquid, and then sterilized germs and casein together at 120° C. Upon the casein thus prepared they placed a lactic acid bacillus, but with negative results, no increase of soluble nitrogen was demonstrated, which showed that the lactic acid germ had not been able to attack the casein.

Klein and Kirsten rendered heated milk suitable for cheese-making by adding calcium chloride. In their experiments they were able to produce normal cheese of several varieties (Bachstein, Spitz, Remoudou, Kloster, etc.) by heating the milk to temperatures varying from 85° C. to 100° C. for different periods of time, treating it with calcium chloride (twenty-five grams of calcium oxide per litre) and adding either cultures or starters. Cheese made by this method not only ripened normally but also gave a larger yield than cheese made from non-heated milks.

My own studies on the bacterial content of cheese were commenced in 1896 at the Bacteriological Laboratory of Dr. Russell, at the University of Wisconsin. During that summer and the following one of 1897, many analyses of Canadian Cheddar cheese were made, the methods of analyses being similar to those already published in the various reports of the Wisconsin Experiment Station. Briefly described, they are as follows :

A sterilized test-tube was sent to a factory with the request that a plug of cheese be placed therein, and asking that the cheese trier be sterilized with steam before use. A typewritten form accompanied each tube, upon which the cheesemaker filled out particulars as to the age of the cheese, condition of manufacture, amount of rennet used, etc. From one to five days elapsed between the taking of the sample and the making of the analyses ; and, on many occasions, the cheese received was very greasy or had otherwise deteriorated owing to the very hot weather, and the length of time taken in transit. Doubtless these facts have contributed to bring about the diversity of the results shewn in my first table.

On arrival at the laboratory, one gram of cheese was weighed out and triturated with ten grams of sterilized sugar, sand or powdered glass. Sterilized water was then added and various dilutions made, differing with the age of the cheese. The medium used at Wisconsin was the ordinary beef peptone gelatin, with or without the addition of milk sugar. From two to five plates were made.

Subsequently this method was improved on, by using sterilized warm water (37° C.) for the dilutions, and yeast water lactose gelatin for medium. To this a small quantity of precipitated chalk was usually added. This medium gave excellent results compared with the ordinary nutrient gelatin or whey peptone gelatin, the colonies of the lactic acid bacteria were larger, and the dissolving action of these germs on the chalk materially aided the labour of counting.

Following de Freudenreich's plan of obtaining the liquefying germs, surface cultures were occasionally made. This method also helped in the isolation of yeasts, the surface colonies of which were far easier to spot than those deep in the gelatin.

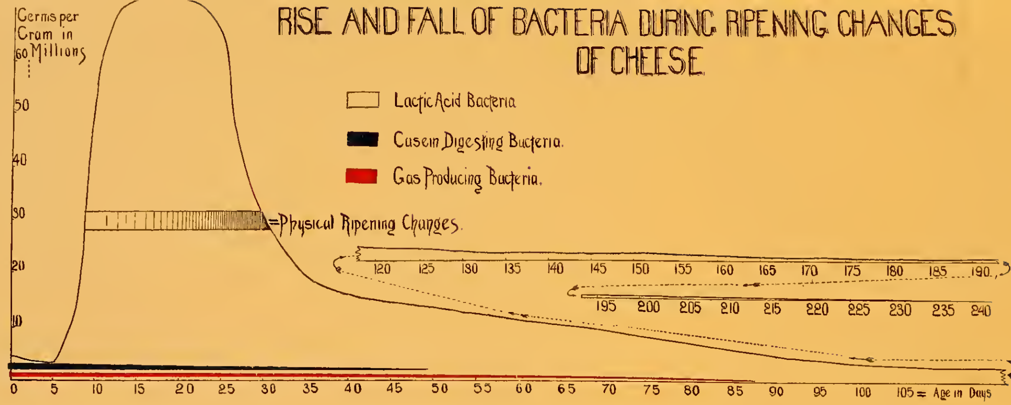
Anaerobic methods of culture failed to show any obligate anaerobes.

Pake's apparatus was used for counting the colonies, but when these were too numerous, a low power of the microscope was employed and from five to ten per cent. of the total number of microscope fields in a Petri dish of eighty m.m. diameter were counted, and computations made therefrom.

TABLE I.

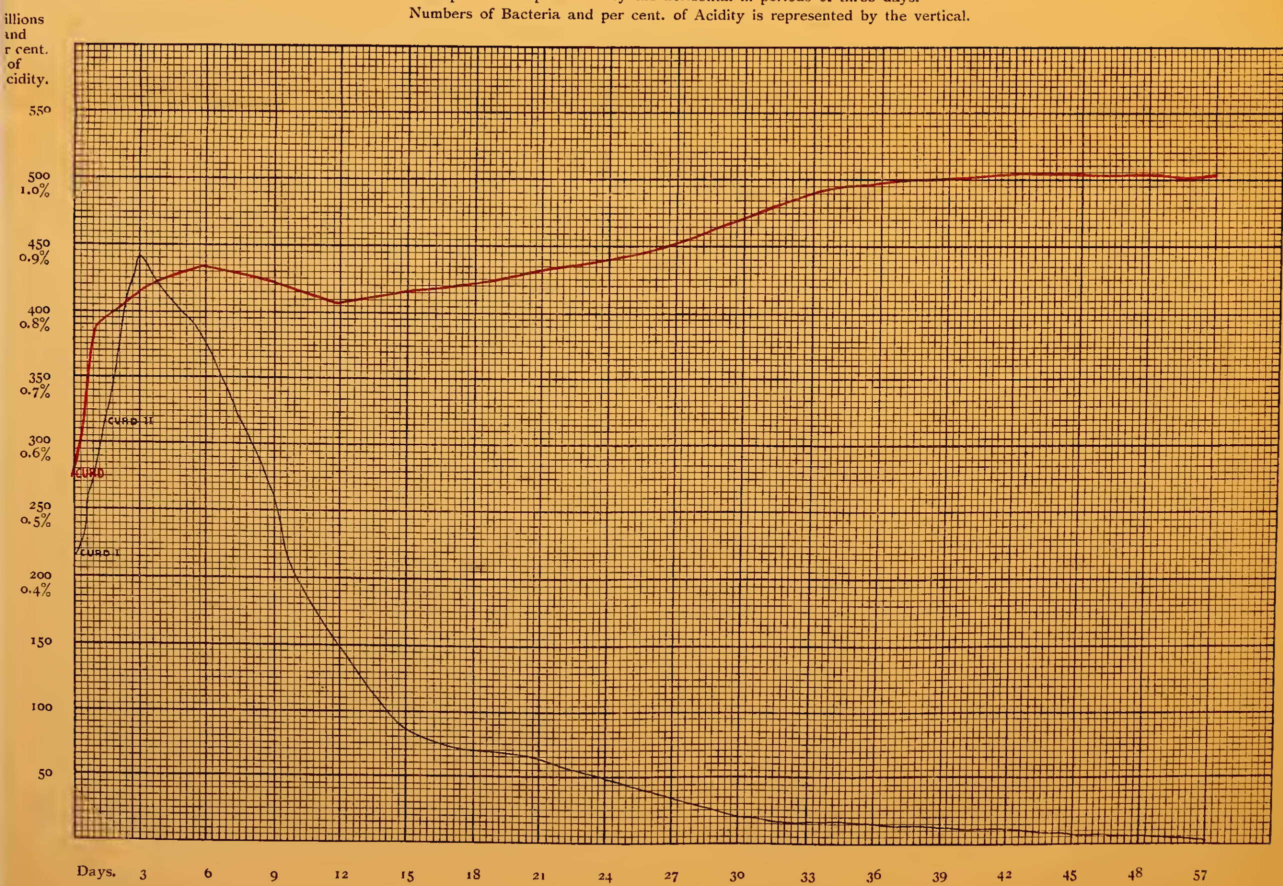
Age of Cheese.	Month Cheese was made in.	Average temperature of curing room.	Rennet.	No. of germs per gram.	Lactic Acid Bacteria.	Digestors.	Gas Producing.	Yeasts.	REMARKS.
6 Days.	August.	70° F.	3 oz.	152,000,000	150,000,000	....	500,000	1,500,000	
7 "	July.	65° F.	$3\frac{1}{3}$ oz.	66,600,000	64,225,000	175,000	2,200,000	....	
11 "	August.	75° F.	3 oz.	28,800,000	28,200,000	1,500	120,000	400,000	
14 "	August.	75° F.	$3\frac{1}{3}$ oz.	74,800,000	71,300,000	......	3,500,000	3,500,000	
17 "	August.	75° F.	3 oz.	20,000,000	17,000,000	......	2,125,000	875,000	
22 "	August.	60-75° F.	$3\frac{1}{2}$ oz.	26,000,000	25,410,000	882,500	500,000	7,500	Cowy flavour.
23 "	July.	70° F.	3 oz.	64,000,000	57,600,000	500	200,000	6,000,000	Milk very gassy.
24 "	July.	70-78° F.	$2\frac{1}{2}$ oz.	33,600,000	33,100,000	100	50,000	400,000	
24 "	June.	65-70° F.	3 oz.	44,800,000	44,700,000	....	....	50,000	
25 "	July.	70° F.	$3\frac{1}{3}$ oz.	44,800,000	44,700,000	....	80,000	....	
29 "	June.	50-75° F.	3 oz.	4,500,000	4,500,000	....	.....	15,000	Plug received in a very greasy condition.
30 "	July.	74° F.	$3\frac{1}{2}$ oz.	5,500,000	5,400,000	....	100,000	......	Plug received in a very greasy condition.
30 "	July.	70° F.	3 oz.	17,750,000	17,700,000	......	......	30,000	
31 "	July.	72° F.	4 oz.	5,000,000	4,900,000	......	....	100,000	
33 "	June.	74° F.	$2\frac{1}{2}$ oz.	8,500,000	8,500,000	.....	......	....	
34 "	July.	70° F.	3 oz.	7,500,000	7,000,000	500,000	....	....	Willow flavour (maker s remark).
36 "	July.	65° F.	$3\frac{1}{4}$ oz.	8,600,000	8,600,000	....	....	25,000	
37 "	July.	75° F.	3 oz.	18,000,000	18,000,000	15,000	....	5,000	
37 "	June.	77° F.	$3\frac{1}{2}$ oz.	3,500,000	2,800,000	....	....	700,000	
40 "	July.	65-70° F.	3 oz.	5,800,000	5,800,000	......	....	....	
42 "	June.	65° F.	$3\frac{1}{3}$ oz.	4,900,000	4,850,000	......	....	49,000	
45 "	July.	70° F.	3 oz.	12,600,000	12,600,000	150	....	....	
50 "	June.	70° F.	$3\frac{1}{3}$ oz.	10,500,000	9,750,000	....	....	750,000	
51 "	June.	75° F.	$3\frac{1}{3}$ oz.	3,150,000	3,090,000	....	....	63,000	
51 "	June.	75° F.	$3\frac{1}{3}$ oz.	1,050,000	939,000	.....	....	111,000	Acid did not develop in milk.
58 "	June.	70° F.	$3\frac{1}{3}$ oz.	630,000	598,000	....	....	31,500	
68 "	May.	60° F.	$2\frac{3}{4}$ oz.	16,800,000	16,800,000	......	....	7,500	
403 "	July.	60-70° F.	......	1,400	1,350	.....	....	50	Taken out of curing room when matured.

RISE AND FALL OF BACTERIA DURING RIPENING CHANGES OF CHEESE



Rise and Fall of Lactic Acid Bacteria during the Ripening of Canadian Cheddar Cheese.
Rise and Fall in the amount Acidity during the Ripening of Canadian Cheddar Cheese.

Time period is represented by the horizontal in periods of three days.
Numbers of Bacteria and per cent. of Acidity is represented by the vertical.



NUMBER AND KIND OF BACTERIA IN CHEESE AT DIFFERENT STAGES OF RIPENING.

CHEESE I.

Age in Days.	No. of Bacteria per gram.	Lactic Acid Bacteria.	Gas Producing Bacteria.	Casein Digesting Bacteria.	Yeasts.
Curd.	290,000,000	289,700,000	....	50,000	250,000
2 Days.	430,000,000	429,599,000	....	1,200	400,000
5 "	387,000,000	386,849,000	....	1,200	750,000
10 "	217,000,000	219,599,000	....	700	40,000
15 "	85,000,000	84,818,000	....	50	182,000
20 "	64,000,000	63,800,000	....	......	200,000
25 "	38,000,000	37,810,000	....	.....	190,000
30 "	22,000,000	21,760,000	....	.....	240,c00
40 "	7,000,000	6,867,000	....	25	133,000
50 "	3,500,000	3,430,000	....	......	70,000

REMARKS.—The temperature of the curing room was from 65° F. to 75° F.
The ripened cheese was of good flavour and texture.

CHEESE II.

Age in Days.	No. of Bacteria per gram.	Lactic Acid Bacteria.	Gas Producing Bacteria.	Casein Digesting Bacteria.	Yeasts.
1 Day.	520,000,000	514,900,000	5,000,000	100,000	....
3 "	480,000,000	477,000,000	3,000,000	25,000	2,000
7 "	275,000,000	274,250,000	700,000	2,000	45,000
12 "	270,000,000	269,900,000	5,000	......	27,000
17 "	130,000,000	129,000,000	....	500	70,000
24 "	83,000,000	82,900,000	....	100	32,000
31 "	31,000,000	31,000,000	....	......	14,000
51 "	6,700,000	6,700,000	....	......	0,500

CHEESE III.

Age in Days.	No. of Bacteria per gram.	Lactic Acid Bacteria.	Gas Producing Bacteria.	Casein Digesting Bacteria.	Yeasts.
2 Days.	340,000,000	340,000,000	....	5,000	10,000
9 "	214,000,000	214,000,000	....	......	70,000
16 "	134,000,000	133.750,000	....	......	250,000
23 "	88,000,000	86,500,000	....	......	1,500,000
30 "	37,000,000	35,780,000	....	......	1,220,000
37 "	27.500,000	26,700,000	....	......	800,000
45 "	12,250,000	11,950,000	....	......	300,000

Both of the above cheeses were of good flavour.

ACID IN CHEESE.*

Method.—Five grams of cheese and an equal amount of glass were ground together in a mortar; 100 c.c. of water was then added and well mixed with the cheese. After standing fifteen minutes, the mixture was filtered through a dry filter paper, and 25 c.c. of the clear filtrate taken for the determination of the acidity. Phenolphtalein was used as indicator.

Where the acidity was determined in an unfiltered portion, as much as two per cent. of acid was found in the older cheese, probably due to the casein neutralizing the alkali.

Curd at Milling showed .54 per cent. acid figured as lactic acid.
" Salting " .76 " "

Cheese	6	days old from	Salting	showed	.86	per cent. acid.
"	6	"	"	"	.86	"
"	13	"	"	"	.81	"
"	13	"	"	"	.79	"
"	20	"	"	"	.86	"
"	20	"	"	"	.90	"
"	27	"	"	"	.90	"
"	27	"	"	"	.86	"
"	34	"	"	"	.93	"
"	34	"	"	"	1.02	"
"	41	"	"	"	1.08	"
"	41	"	"	"	1.08	"
"	48	"	"	"	1.08	"
"	48	"	"	"	1.08	"
"	55	"	"	"	1.08	"
"	165	"	"	"	1.08	"
"	225	"	"	"	1.08	"

The work of Russell and Wenizirl on the normal cheese flora of Wisconsin cheese, during the entire history of the cheese from the time it was made until it was consumed, has been duplicated in my laboratory, but with these differences:

1. Canadian Cheddar cheese was the subject of study.

2. The culture medium used was somewhat different; yeast-water, lactose gelatine, whey peptone gelatine, and ordinary beef broth lactose gelatine were employed. From six to ten Petri dishes were poured for each analysis.

*These determinations were made by R. Harcourt, Associate Professor of Chemistry, Ontario Agricultural College.

3. The oldest cheese analyzed was fifty-one days old. Russell gave the result up to 108 days for two cheeses, and up to 237 days for one.

The general results of these studies on Canadian cheese are shown by the diagram and tables, and demonstrate the enormous increase of the lactic acid bacteria in the initial stages of cheese-making. From the moment the milk is placed in the vat, every condition favourable for the growth of this class of micro-organism is carefully fostered. The greatest number found was in cheese two or three days old; at this age, one cheese contained 520 millions bacteria per gram, nearly five times as many as Russell observed in Wisconsin cheese.

From this point the numbers decline at first rather rapidly, but subsequently the decrease is more gradual.

Comparing my results with Russell's and summarizing them, I might state that in Canadian Cheddar cheese there is:

1. *Period of increase* in which the bacteria develop most rapidly in the curd, and in the cheese up to the age of two or three days, followed by

2. *Period of rapid decline*, in which the numbers fall away somewhat rapidly until about the thirteenth day, when the cheese may be said to enter the final

3. *Period of slow or general decline*, in which their numbers slowly decrease, at the 430th day but 1,400 per gram were found.

The results, whilst agreeing with Russell's, as to the enormous development of the lactic acid bacteria in cheese, differ in that there is no period of "Initial Decline," and that the "Period of Increase" virtually takes place in the curd. The maximum number are present in the cheese at the moment it is taken out of the press. The "Period of Final Decline," I have subdivided, although my two divisions may be quite well taken together.

If in connection with this remarkable increase of lactic acid bacteria, we examine the amount of acid present in the curd and cheese, we find that the greatest increase in acidity occurs between milling and salting. From this point until the cheese is forty days old there is a gradual and progressive increase. Thus, the increase in acidity from the time of milling until the cheese was six days old was 0.32 per cent. calculated as lactic acid, and the increase from the latter age to forty-one days old was exactly the same, 0.32 per cent.; but in the former case, the increase was accomplished in six days, and in the latter thirty-five days.

Whilst the increase in numbers of the lactic acid bacteria can fully account for the initial increase in acid, no ready explanation can be given for the gradual and progressive development of acidity from the sixth to the fortieth day. The acidity is due more to acid salts than to free acid, and it may be that some change occurs in the acid bases, and perhaps some of the fatty acids are liberated.

The "Period of Rapid Decline" is practically synchronous with the gradual increase of acidity, and the "period of gradual decline" is coincident with the maximum amount of acidity. This increase, followed by an almost permanent amount of acidity, may possibly explain why the bacteria die out at first rather rapidly and subsequently somewhat more slowly, the least resistant germs being killed off quickly by the slow accumulation of acid followed by the gradual death of the stronger individuals.

Lactic acid bacteria.—The prevalent lactic acid species present in all samples was undoubtedly the *B. acidi lactici* of Esten. This author stated that this bacterium is identical in every particular with Günther and Thierfelder's organism. In most of the samples of cheese it was the predominating lactic acid bacterium present. Next in numbers was *B. lactis aerogenes*, or a form closely allied to it. This microbe curdled milk into a soft curd, and after some time gas bubbles appeared. When cultures of the above two organisms were seeded together in sterilized milk, a firm curd was produced with little or no gas. Occasionally a micrococcus producing a buff-coloured colony, and also torulae were found. Both of these turned litmus red and coagulated it into a solid curd with no separation of whey.

Gas producing bacteria.—The gas producing bacteria belonged usually to either the *B. coli* group or else to the *B. lactis aerogenes* group. In the former group many varieties have been cultivated, showing considerable differences as to motility, indol and gas production. Nearly all writers on dairy bacteriology blame varieties of this bacillus for producing gassy milk and bad flavours. In several instances the cheese was mottled when this germ was present and this result might be possibly brought about by the bleaching action of the hydrogen liberated in the cheese by this ubiquitous microbe.

According to Weinzirl the "huffing" of cheese results from the activity of a large number of Hueppe's *B. acidi lactici*. Cohn considers this germ to belong to the aerogenes group, but the forms of this bacterium that I have met with in Canadian cheese show considerable variations from the type, especially with regard to the appearance of the gelatine colonies which simulate rather that of B. acidi lactici (Esten).

This group does not increase in cheese and I have not found them in cheese older than thirty days.

The liquefying bacteria found in Canadian cheese were not numerous, and their numbers decreased as the cheese ripened, so that in three weeks old cheese they were seldom found. A constant endeavour was made in order to isolate micro-organisms belonging to this class, but usually without success. On one occasion the presence of a larger number of digesting bacteria than usual was associated with a bad flavour in the cheese.

Some seven different species of liquefying germs have been isolated. Probably the commonest species were forms almost identical with Hueppe's *Bacillus butyricus*. This germ was several times found in fair numbers, from cheese taken from very warm curing rooms. The next most common species was a clostridium form, which grew in long threads. On gelatine, it formed a brown granular colony, and milk was completely digested. It might belong to Duclaux's Tyrothrix group.

What was evidently a variety of Conn's *B. varians lactis* was also isolated.

Two or three forms met with seemed allied to the Proteus group, and seemed to be closely related to *B. fulvus* (Zimmermann). These were found only in young cheese.

Practically the whole of this group gave rise to bad flavour or odours in milk. Butter has been made from cream ripened with some of these bacteria, and invariably a bad product was produced.

YEASTS.

I have found yeasts quite commonly present in Canadian cheese, and frequently in large numbers. This fact was first noted whilst studying at Wisconsin University in Professor Russell's laboratory, but no special endeavour was then made to give them the best conditions for growth. Since, I have used yeast water gelatine, and ale wort gelatine, for their isolation. The latter medium is excellent, as other bacteria that may be present in the sample do not find it a suitable pabulum.

So far as my experiments are concerned, they are the only micro-organisms that actually increase in cheese. Cheese No. 3 illustrates the increase of yeasts that may take place in cheese, and some of the analyses in Table 1 support this conclusion.

Whilst I have applied the name yeasts to this class, most of them are species of *Torula*, as they form no spores, even under the conditions prescribed by Hansen. They may be classified roughly here as beneficial and injurious. The former either act like the lactic acid bacteria in milk, produce acid, and give a firm curd, or they may make but little acid and after considerable length of time digest the casein. The injurious species have more diverse habits. Thus some are able to ferment lactose, form gas, and give rise to bad flavours in the cheese. A species of torula isolated in 1900, besides causing gas formation, produced a bitter flavour, which gave much trouble in as many as four factories in the same district.

Another species of torula caused no perceptible change in milk, but when grown with a lactic acid bacterium it produced a mottled appearance in the cheese. I was able to produce such mottles in milk containing a little cheese colour, and seeded with a lactic acid bacillus, or a drop of lactic acid and the torula.

The great difficulty which a cheese maker experiences when working with yeasts is the remarkable tolerance they shew to acidity, so that a maker is unable to repress an undesirable yeast fermentation by the addition of a vigorous lactic acid starter. Some of the torulae I have isolated grew luxuriantly in peptone solutions containing 2.25 per cent. of lactic acid. This fact undoubtedly explains their increase in cheese.

Nothing can be said as to the place of origin of these yeasts.

Other bacteria in Cheese.—During these investigations a number of other forms, not falling into any of the classes I have mentioned, have been isolated. Some of these produced undesirable flavours and others were inert. Nothing need be said of these in this paper, on account of their occasional occurrence, or unimportance to my subject.

My experiments and results are perhaps rather few, and too little chemical work has been done to justify much theorizing on what causes the ripening of cheese, but from a review of the works of others and my own results, I may perhaps be justified in making a few remarks.

Three most important facts seem well supported by good evidence and trustworthy experiments :

1. The enormous number of lactic acid bacteria in hard cheese, and the very small numbers of liquefying or digesting bacteria.

2. The existence of galactase, a natural enzyme inherent in fresh milk.

3. The ability of rennet to cause the change of non-soluble nitrogenous products to soluble ones.

If we grant that these three facts are proved, and we may safely do so, our inquiry into the cause of the ripening of cheese will be somewhat simplified.

The lactic acid bacteria seem to be able to cause an increase in the amount of soluble nitrogenous products in the casein of milk (de Freudenreich). Klein and Kirsten also state that normal cheese may be made from pasteurised milk (hence free from enzymes) with the aid of starters. Russell, before the discovery of galactase, stated "that the addition of a pure lactic acid ferment to the pasteurised milk permits the usual changes to occur in a perfectly normal way."

On the other hand, Boekhout and Vries were unable to produce normal cheese (Edam) made from aseptic milk with the addition of a culture of lactic acid bacteria, but at the same time they admit that perhaps some other variety of lactic acid bacteria might bring about the ripening changes.

Chodat and Bang grew lactic acid bacteria on coagulated casein, but the quantity of soluble nitrogen in this mass did not increase ; so that taking into account these facts, we are bound to admit that there exists more or less doubt as to the ability of the lactic acid bacteria to alone bring about an increase in the amount of soluble nitrogen.

Babcock and Russell's discovery of galactase led them to consider that the "breaking down of the casein was due in larger part to the action of this enzyme" ; in fact, they attribute to galactase the principal rôle in the ripening of cheese. Both de Freudenreich and Jensen confirmed the presence of this enzyme in milk, but they do not consider that it is the all-important factor in the curing process. Boekhout and Vries completely deny its ability to ripen cheese, and Klein and Kirsten's experiments show that soft cheeses ripen normally, even when made from milk in which the enzyme has been destroyed by heat.

My experiments show that the amount of acid present in Canadian Cheddar cheese is sufficient to inhibit and perhaps altogether stop its action in cheese ; for, if as shown by de Freudenreich, 0.5 per cent. of lactic acid can considerably enfeeble its action, then the amount of acidity in normal Canadian Cheddar cheese might still more diminish the action of galactase, as the percentage of acidity in the manufactured cheese varies at different ages from 0.76 per cent. to 1.08 per cent.

Thus, we are bound to conclude that, as far as Canadian Cheddar cheese is concerned, the presence of galactase is of little importance.

There now remains the question of the ability of rennet to cause the ripening changes.

Jensen was the first to show that curing might be accelerated by incorporating pancreas with the curd, and subsequently Babcock and Russell and Jensen simultaneously proved that the pepsin in rennet increased the higher decomposition products, as albumoses and peptones in cheese.

There is also the well-known fact that cheese-makers increase the amount of rennet when they want a fast curing cheese.

Now rennet acts more quickly and better in acid solutions, and it seems that the rôle of the lactic acid bacteria, whose growth in the milk is so carefully fostered by the cheese-maker, is to bring about or create the requisite acidity so that the pepsin of the rennet can exercise its digestive action on the cheese.

There is practically no increase in the number of lactic acid bacteria after the cheese have been taken from the press, but the amount of acidity increases and Schirokich's experiments proved that the diastase of Tyrothrix (which is similar to rennet in its action) was able to act on the casein when the requisite amount of acid was present.

This connection, for we can hardly call it symbiosis, between the action of rennet and lactic acid bacteria will serve to harmonize the results of the experiments of other investigators. Thus, if we substitute the enzymes of rennet for Schirokich's bacterial enzymes, the curing process may thus be explained, and Weigmann's theory that the lactic acid bacteria direct the process of curing in the right direction by eliminating undesirable forms of bacteria by the lactic acid formed, is quite in accord with my proposal.

Summarized, the ripening of cheese may be said to be caused by the digestive action of the rennet on the insoluble nitrogenous matter of the cheese, in the presence of acid formed by the lactic acid bacteria. The large amount of acidity also prevents or inhibits the growth of other (and perhaps undesirable) species of bacteria.

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GŒTHE'S FAUST.

BY PROF. L. E. HORNING.

(Read 13th April, 1901).

The following tabular comparison of the three stages in which Gœthe's Faust, Part I. is known, has been drawn up by Prof. Horning, of Victoria University, Toronto. It was submitted in the first of a series of studies in Gœthe's masterpiece.

The numbering of the lines is according to Schmidt's Edition of the *Urfaust*, Seuffert's reprint of the *Fragment* and the Weimar Edition of Part I.; where advisable totals of lines in the corresponding scenes are indicated.

URFAUST, 1773-1775?	FRAGMENT, 1790.	PART I., 1808.
		1. Zueignung, 1–32.
		2. Vorspiel auf dem Theater, 33–242.
		3. Prolog im Himmel, 243–353.
1. Nacht, 1–248.	1. Nacht, 1–248. Urf. 122, 123=122. " 194, 195 replaced by 195. 155, 194 are added.	4. Nacht. *a* 354–605 (252 lines). Urf. 122, 123=475. " 194, 195 replaced by 548. 507, 547, 598–601 are added. *b* 606–807.
		5. Vor dem Thor, 808–1177.
		6. Studierzimmer I., 1178–1529.
2.	2.	7. Studierzimmer II. *a* 1530–1769.
	a 249–346 (98 lines).	*b* 1770–1867. (98 lines).
Schülerscene, 249–444 (196 lines).	*b* 347–529 (183 lines) Schülerscene.	*c* 1868–2050–Schülerscene. (183 lines).
249–262=1868–81 Pt. I.	......347–360.	Result=70 lines omitted.
263–266=1882–95 "	361–374.	57 " added.
267–332	....	22 " " as *d*.
333–340=1896–1903 Pt I.	......375–382.	
1904–1909 "	.. ...383–388.	Changes ?
341–394=1910–1963 "	.. ..389–442.	
1964–2000 "	.... 443–479.	*d* Faust tritt auf, 2051–2072.
395–444=2001–2050 "	......480–529.	
	c Faust tritt auf, 530–551.	

URFAUST, 1773-1775?	FRAGMENT, 1790.	PART I., 1808.
3. Auerbach's Keller. *a* 445-452. *b* 25 lines of prose. *c* Rattenlied. *d* 48 lines of prose. *e* Flohlied. *f* 83 lines of prose. (In all 210 lines).	3. Auerbach's Keller. 552-815 (264 lines). Changes?	8. Auerbach's Keller. 2073-2336 (264 lines). Changes?
4. Landstrasse, 453-456		
	4. Hexenküche, 816-1067. (252 lines).	9. Hexenküche, 2337-2604. (268 lines). 2366-77 } = additions. 2390-93 }
5. Strasse, 457-529.	5. Strasse, 1068-1140.	10. Strasse, 2605-2677.
6. Abend, 530-656.	6. Abend, 1141-1267.	11. Abend, 2678-2804.
7. Allee, 657-718 (62 lines).	7. Spaziergang, 1267-1327. (60 lines). *Note.*—Between 1277 and 1278 two lines omitted =667-668 of Urfaust.	12. Spaziergang, 2805-2864. (60 lines). *Note.*—Between 2814 and 2815 two lines omitted= 667-668 of Urfaust.
8. Nachbarinn Haus, 719-878. *Note.*—726 and 727 blank.	8. Der Nachbarinn Haus, 1328-1487. *Note.*—Two lines inserted =1356-7=between 748 and 749 of Urfaust.	13. Der Nachbarinn Haus. 2866-3024. *Note.*—Two lines inserted= 2893-94=between 748 and 749 of Urfaust.
9. Faust and Mephistopheles, 879-924(46). Wie ist's?	9. Faust and Meph., 1488-1535 (48). *Note.*—Lines 1509-10 added, = between 899 and 900 of Urfaust. Changes?	14. Faust and Meph., 3025-3072. (48). *Note.*—Lines 3046 and 3047 added=between 899 and 900 of the Urfaust. Changes?
10. Garten, 925-1053. (129).	10. Garten, 1536-1664. (129).	15. Garten, 3073-3204. (132). *Note* 1.—"Er liebt mich" of line 3184 is counted as one line in Fragment=1643. Is this a printer's error? *Note* 2.—Lines 3149-52 added =between 1611 and 1612 of Fragment.
11. Ein Gartenhäuschen, 1054-1065.	11. Ein Gartenhäuschen, 1665-1676.	16. Ein Gartenhäuschen. 3205-3216.

Urfaust, 1773-1775?	Fragment, 1790.	Part I., 1808.
See 18 *b*.	See 15.	17. Wald und Höhle. *a* 3217-3341 (125 lines). *b* 3342-3369 (28 lines). *c* 3370-3373 (4 lines). *Note* 1.—*b* = Urfaust 18 *b*. " 2.—This scene = Fragment 15. Changes? Espec. ll 3346-48. 3363. 3366.
12. Gretchens Stube, 1066-1105.	12. Gretchens Stube, 1677-1716.	18. Gretchens Stube, 3374-3413.
13. Marthens Garten, 1106-1235.	13. Marthens Garten, 1717-1846.	19. Marthens Garten, 3414-3543.
14. Am Brunnen, 1236-1277 (42 lines)	14. Am Brunnen, 1847-1889. (43 lines). *Note*.—Ach, 1853, counted as one line.	20. Am Brunnen, 3544-3586. *Note*.—Ach, 3550, counted as one line.
See 18 *b*.	15. Wald und Höhle, *a* 1890-2014 (125 lines). *b* 2015-2042 (28 lines). *c* 2043-2046 (4 lines). *Note*.—*b* = Urfaust 18 *b*. *Note position of scene in Part I.*	See 17.
15. Zwinger, 1278-1310.	16. Zwinger, 2047-79.	21. Zwinger, 3587-3619.
See 17 for Part I. *a*. See 18 *a* for Part I. *c*.	*Dropped.*	22. Nacht—Strasse vor Gretchens Thür. *a* 3620-45. (26 lines) *cf.* Urfaust 17. *b* 3646-49. (4 lines). *c* 3650-59. (10 lines) *cf.* Urfaust 18 *a*. *d* 3660-3775 (116 lines).
16. Dom, 1311-71 (61 lines).	17. Dom, 2080-2137 (58 lines). *Note* 1.—After 2094 line omitted = Urfaust 1326. *Note* 2.—2124-27 = 5 lines in Urfaust = 1356-60. *Note* 3.—2131-35 = 6 lines in Urfaust = 1364-69.	23. Dom, 3776-3834 (59 lines). *Note* 1.—Line 3789 added after 1323 of Urfaust. *Note* 2.—After 3791 a line omitted = Urfaust 1326. *Note* 3.—Lines 3821-24 = 5 lines in Urfaust, 1356-60. *Note* 4.—Lines 3828-32 = 6 lines in Urfaust, 1364-69.

URFAUST, 1773-1775?	FRAGMENT, 1790.	PART I., 1808.
17. Nacht — Vor Gretchens Haus, 1372–97 (26 lines) *cf.* Part I. 22 *a.*	*Dropped.*	See 22 *a.*
18. Faust and Mephistopheles (vor Gretchens Haus).		
a 1398–1407 (10 lines) *cf.* Part I. 22 *c.*	*Dropped.*	See 22 *c.*
b 1408–1435 (28 lines) *cf.* Part I. 17 *b.*	See 15 *b.*	See 17 *b*
		24. Walpurgisnacht, 3835–4222.
		25. Walpurgisnachtstraum, 4223–4398.
19. Faust and Mephistopheles. *Prose.*	*Dropped.*	26. Trüber Tag. *Prose.*
20. Nacht, 1436–1441.	*Dropped.*	27. Nacht, 4399–4404
21. Kerker. *Prose.*	*Dropped.*	28. Kerker, 4405–4612. *Verse.* Other Changes ?

PHYSICAL GEOLOGY OF CENTRAL ONTARIO.*

BY ALFRED W. G. WILSON.

(Read 20th April, 1901.)

CONTENTS.

* This paper was written as a thesis for the Doctorate of Philosophy at Harvard University, and was presented in May, 1901.

INTRODUCTION.

LOCATION.—That portion of the Province of Ontario designated CENTRAL ONTARIO is a triangular area with its base on Lake Ontario to the south ; the western arm is formed by the Niagara escarpment in its extension from Hamilton to Collingwood ; the northern boundary follows the edge of the crystalline rocks from Georgian Bay to a point on the St. Lawrence river a short distance east of Kingston.

Historical References and Sources of Information.—Previous to the institution of the Geological Survey of Canada, in 1843, there had been no systematic studies of the geology of Upper Canada, now the Province of Ontario. Before that date much even of the then unsettled parts of the Province had been surveyed into townships, and more or less accurate maps prepared. Admiral Bayfield's surveys of the Great Lakes were the most important work upon the shore-lines of the Province. The present available maps, though in part corrected by more recent work, are based largely upon these early surveys. Dr. J. J. Bigsby had published (**1829**) a few papers in which reference is made to certain features of the area under discussion. After the institution of the Survey, the most important work is that of Alexander Murray. Between 1843 and 1856 Murray had explored and mapped a large portion of the present Province. His work in 1843 in the western portion of this area, and in 1852 in the eastern portion, forms the basis of our present knowledge of its geology. The first systematic account, in which all of Murray's work is summarized, was published by Sir William Logan in 1863. This volume, entitled "The Geology of

Canada," is still the standard work of reference for the geology of "Old" Ontario. Since 1863 there has been little work in the area under the auspices of the Survey, except some work in 1886, the results of which have not yet been made public. Both previous and subsequent to Logan's summary there have been many shorter papers published upon various topics. Some of these will be noted in the text.

In the preparation of the present paper the writer has made use of many sources of information, and due acknowledgment will be made in the appropriate places. During the last few summers, as opportunity offered, the greater number of localities referred to in the context have been visited, and use has been made of the writer's own observations in the field.

The writer wishes to acknowledge his indebtedness to Mr. J. M. Clarke, of Albany, for the identification of a number of fossils; and to Professor W. M. Davis, of Harvard University, for advice and criticism while this paper was in preparation.

Résumé.—The area comprises, in all, about 6,500 square miles of territory. Within its limits are found rocks ranging in age from the Archean to the Niagara. These are overlaid by a great complex of deposits dating from the Pleistocene epoch. Everywhere along the northern boundary the various members of the crystalline series are found passing beneath the Cambro-Silurian sediments; in some localities outliers of the sediments are found upon the crystallines; again, inliers of the latter are found wholly or partially surrounded by the former. At one time the sediments extended much farther towards the north; their removal has revealed ridges, valleys, and residual monadnocks, the sub-mature topography of a well-dissected plain of denudation, a plain long antedating the Cambrian.

The basal members of the sedimentary series are destitute of fossils, and consist of more or less coarse detritus; above them thick deposits of fossiliferous limestone were formed, and in many localities this limestone rests directly upon the crystallines. These limestones are in turn overlain by bituminous shales.

A second cycle of slow depression, much greater than the former, resulted in the formation of a similar series of deposits, the upper members of which lie beyond the area under consideration.

In the long interval from the close of the last period of deposition within the area until the beginning of the Pleistocene epoch, during

which the northern portion of this continent is supposed to have, at different times, stood at different but undetermined elevations above the then sea-level, the rocks of this area were exposed to the atmospheric agencies of disintegration and degradation. The result was the development of a topographic system whose remnants, though partly obscured by the deposits of the Pleistocene epoch, are still recognizable.

Extensive climatic changes, by some supposed to be the product of, or accompanied by, elevation of this and adjacent portions of North America, interrupted these processes of dissection ; ice, in the form of *sheet-glaciers,* modified the topography produced in previous epochs, and introduced large amounts of material from the adjacent crystalline area. During the close of the epoch, the time of melting of the glacier, the clay, sand, gravel and boulders which it carried were deposited. The waters collected in great lakes in front of the retreating ice. Around their shores deltas were built, beaches formed, and benches cut ; and a new system of drainage lines was instituted.

Again, however, changes in relative elevations of different parts, and the withdrawal of the ice, led to the partial dismemberment of the drainage systems, to the definition of the present lake basins, and to the development of new lines of drainage, which are essentially the same to-day, though these and the lake levels are being slowly modified by secular changes of elevation.

TOPOGRAPHY OF THE PRE-SEDIMENTARY FLOOR.

Diverse Character of the Crystallines.—The crystalline series along the northern boundary of the area comprise rocks in greatest variety, crystalline limestones, micaceous and hornblendic schists, and gneiss, the latter very abundant. Associated with these are plutonic and volcanic rocks, acid, basic, and of intermediate varieties. The whole region has been one of complicated folding and intense metamorphism. The schistose structure of the rocks, throughout the area, is nearly vertical, and has a northeast southwest trend, with local variations from this general direction.

This great variety of rocks would necessarily offer different resisting powers to erosive agencies, and give rise to very diverse topography. In travelling through the region on foot one is continually ascending or descending. Even then he cannot fail to note the many small tarns, muskegs, and beaver-meadows found so frequently upon the upland areas.

Even-topped Character of the Uplands.—Almost anywhere in the region the ascent of a height, from which a good view can be obtained, will disclose a remarkably even sky-line, indicative of the even character of the upland surface, with occasional greater elevations standing out in relief. One of the best localities to see this is from the crest of the divide between Deer bay and Stony lake, almost the middle point of the northern boundary of the area. The waters of Deer bay lie 120 feet below ; to the north-east is the small Lovesick lake ; to the east, three miles away, is the basin of Stony lake, the water-level being thirty feet below that of Deer bay. The sky-line of the upland upon the opposite side of these basins is remarkably even. Almost directly east, twelve miles in an air line, are the Blue Mountains at the other end of Stony lake, rising above the general level. These ridges, locally called mountains, are syenitic masses which stand out nearly 200 feet above the rolling surface of the surrounding district.

A most striking view over the upland is that obtained from the summit of the cliff near the narrows of Haliburton lake, in the township of Harburn, forty-five miles north of Stony lake. Here the observer will be standing about 175 feet above the lake, and over 1,000 feet above Lake Ontario, this being one of the highest points in Central Ontario. The waters of Haliburton lake flow southerly. Within a radius of ten miles are a number of small lakes and streams whose waters flow to the west, north or east, eventually reaching the Georgian Bay or the Ottawa river.

Looking towards the east, south or west, the even upland plain appears to have a slight inclination to the south. Towards the north the direction of inclination is not so evident. Over the upland there are sometimes large, nearly flat areas of muskeg, a feature in which it is comparable to the uplands of Norway.

Dissection.—Though still in an early stage of the cycle, the region as a whole is much dissected. Minor ridges and valleys trending prevailingly northeast and southwest are the dominant topographic features of the upland areas ; deep, steep-sided valleys, due apparently to later dissection, interrupt the continuity of the upland surface ; slopes, frequently of almost bare rock, are common, and steep cliffs not infrequent. At the borders of the sedimentary series the difference in level between the general upland surface and the bottoms of the larger valleys would average about 150 feet ; further north, in areas which have perhaps been much longer denuded, this difference is much greater. All the deeper valleys are now lake basins ; many of the larger basins in the vicinity of Haliburton are deep, so that the depth of the valley bottoms below the level of the upland plain is frequently as much as 400 feet. The less-deep lateral valleys seem frequently to be graded with respect to the lake surfaces. In some localities, in small areas upon the upland, the topography is rolling, with only occasional low ridges and shallow valleys, except close to the present lake depressions.

Gradients.—The general even character of the skyline throughout the crystalline area justifies a comparison of the arithmetic gradients of the surface upon which the sediments rest, as ascertained by the differences between the elevations of a number of localities within the area. Data to institute a comparison along a series of parallel lines, outside of the sedimentary area, are not available. Radially from the upland surface in the vicinity of Haliburton lake to a number of points along the base of the Cambro-Silurian escarpment, between Georgian Bay and Kingston, the average gradient is nearly nine feet per mile.

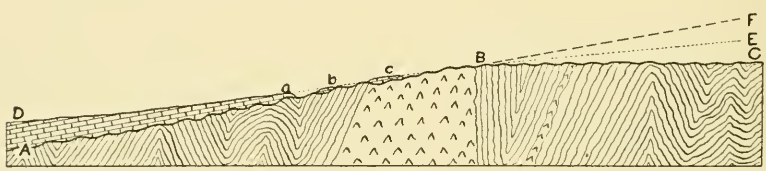


FIGURE 1.—AB represents the plain beneath the sedimentary cover ; BC, the plain north of the edge of the cover ; DB, the plain over the surface of the sediments ; *a*, the escarpment, *b* and *c*, outliers. Vertical exaggeration about forty times.

At Toronto the crystallines are known to be about 1,100 feet beneath the present surface. Two other borings, one at Cobourg, and the other in the township of South Fredericksburg, indicate that the floor is over 500 and over 600 feet, respectively, below the surface at these localities. The average gradient beneath the sedimentary cover along a series of lines from the foot of the Cambro-Silurian escarpment to the bottoms of these borings indicates that the gradient beneath the cover varies from twenty-two feet per mile in the western portion of the district to over forty-one feet per mile at the eastern end. The relative attitudes of these two surfaces are represented in figure 1, where AB represents the edge of a cross section of the plain beneath, and BC the edge of a cross section of that outside the sedimentary area.

Upon the surface of the sediments toward the eastern part of the area, the gradient appears to lie between that beneath and that without the cover (figure 1, DB). In the vicinity of Toronto it, in part, approximately coincides with that upon the surface of the crystallines to the north. The surface is too irregular to justify any general comparison. In the area studied, that portion lying east of a line running a little to the east of north from the west side of Balsam lake is inclined towards the southeast. West of this line the inclination is towards the west.

The meagre data available thus indicate that beneath the sedimentary cover towards the western end of the area the average gradient is more than double that of the uncovered portions, while at the eastern end it is about four times as great. Near the western end, the gradient from the summit over the crystallines to the Black River escarpment, and over the surface of the Palaeozoic strata, is nearly the same. This gradient is less than half the average gradient of the northern side of the basin of Lake Ontario (23.7 feet).

The relative positions of the three plains suggest certain problems which may be summarized thus :—

1. Do these three plains represent three distinct periods of planation?

2. Are AB and BC of the same age, but now discordant by warping?

3. Did the plain AB formerly extend upward in the direction BF; is BC of the same age as DB, or is it younger?

4. Is the discordance between AB or DB and BC produced by warping?

The accordance of the plains DB and BC towards the western end of the area is suggestive of warping elsewhere. Data of a detailed character as to the gradient upon the uncovered crystalline areas and upon the sedimentary outliers of the plain AB between the point represented by B and the front of the escarpment *a* have not been obtained. Without them the evidence available is inadequate to solve the problems.

It should be added that the relative arrangement of the three plains, represented as meeting in a broad angle at B, is purely fortuitous. The data in hand are not enough to determine whether AB and BC represent two intersecting plains, or portions of a continuous arc, and whether all three have a common point of intersection.

Similar relations between two plains of denudation upon crystalline rocks, meeting at low angles, have been found by Van Hise in Wisconsin (**'96**), and by Smyth in the region south of Lake Superior (**'99**).

Darton has described a somewhat similar case in Virginia ('94, 582). In the Grand Cañon of the Colorado we have an actual transverse section of two such intersecting plains, both older than the Cambrian, but meeting at a much higher angle.

THREE PROBLEMS STATED.—Among the many problems which present themselves for consideration, three, which have reference to the character of the sedimentary floor, seem worthy of special attention :—

1. Have we here an ancient sub-maturely dissected plain of denudation, a kind of geographical fossil, or is this topography the result of post-sedimentary causes?

2. In either event is there any possibility of approximately dating its origin?

3. Is the plain wholly the product of sub-aerial processes, or have we here a plain of submarine abrasion, and subsequent dissection?

FIRST PROBLEM, PRE-SEDIMENTARY TOPOGRAPHY.—Turning now to a consideration of the first of these problems, it will be necessary to describe, with some detail, a number of special localities which seem to afford evidence for its solution.

Inliers.—In the township of Verulam, about midway between Sturgeon Point and Bobcaygeon at the foot of Sturgeon lake, conspicuous among the hills just north of the lake, is a ridge of aplitic granite known locally as Red Mountain. The exposed base is about sixty feet and the crest one hundred and ninety feet above the level of Sturgeon lake. The ridge itself is about 2,000 feet in length and 600 in breadth; the longer axis strikes N 23°E. The crest is rounded, but falls off at the northwest corner very abruptly, at an angle of about 80°; on the east the inclination, though less, is still too steep for a person to descend in safety. At the south end the descent on both sides, though steep, is less precipitous. The crest and sides, especially towards the north, are free from boulders; but the southern end, where the crest is lower, is strewn with large and small sharply angular fragments derived from the ridge itself, together with some large blocks of limestone. Forming a belt one hundred yards in width is marshy ground, beyond which are lower ridges of morainic material. Half a mile to the west of this ridge, occurs a second much smaller granitic ridge trending in the same direction. The deposits of drift seem to obscure any limestone deposits which occur in the immediate vicinity. Four and a half miles to the west, at Sturgeon Point, thin-bedded fossiliferous Trenton limestones are found dipping north at less than one degree. On the south shore of the lake other outcrops of limestone occur, with very light southerly dip. To the north, the edge of the Cambro-Silurian escarpment, looking out over the main body of the Archean, is found at a distance of about 11.5 miles (as measured on the maps).

Is this the summit of a monadnock, buried when the sediments were deposited, and since uncovered in the progress of denudation; or have there been granitic intrusions since the formation of the stratified deposits? In this locality positive evidence either way seems to be lacking.

Further east, just north of Varty lake, and four miles from the edge of the escarpment, is a small oval dome of pink gneiss. Towards the north end of the dome four shafts, on a line transverse to the longer axis, penetrate the overlying limestone and show that the dip is nine degrees east on one side, and very much less on the other. A short distance away from the dome the strata have a dip of less than one degree. Near the southern end, where the gneiss is exposed, the limestone strata, quite close to the contact (the last few feet are covered with sod) are in an attitude which indicates that they abut against the gneiss. The higher strata, which once must have overarched the dome, have been eroded away. Here then we have beds of limestone strictly conformable with each other, and parallel to the surface upon which they rest, where seen in the shafts, arching over a dome of gneiss. So far as could be ascertained there is no evidence of post-sedimentary elevation.

In the valley of Mill creek, a small stream, the outlet of Sydenham lake, about five miles from the main area of the archean, is a small ridge of gray micaceous gneiss. The valley of the creek is about one mile in width, and flat floored; the nearest of the two bounding escarpments is 400 yards away, and the crest is 105 feet above the valley floor. The small crystalline ridge has evidently been exposed by the agency which carved the deep broad valley. Similar exposures of gneiss are found in the depressions occupied by many of the lakes of the Trent river system, on the Moira river, and elsewhere in like situations.

Still further east, at Kingston Mills, just west of the bridge across the gorge of the Great Cataraqui creek, there is a railway cut transverse to a granite ridge. The west end of this cutting passes through a small mass of calcareous quartz conglomerate, lying in a hollow upon the flank of the granite. The contact between granite and conglomerate shows in cross section on both sides of the cutting. There are, in all, about twelve feet of strata, dipping lightly to the west. The contact plane between these strata and the granite has, for the upper two-thirds of its length, a dip of about thirty-five degrees to the west. Towards the base this dip flattens out, and just where the line of contact passes beneath the material of the railway bed, the bedding of the sediments and the surface of the granite seem to be parallel (figure 2).

In this calcareous conglomerate are found fragments of crinoid stems and the casts of a Cameroceras. Several specimens of the latter, composed of white crystalline calcite, were taken from one of the lower beds at a point six inches from the granite. Other specimens more than five feet distant from the granite are identical in appearance with

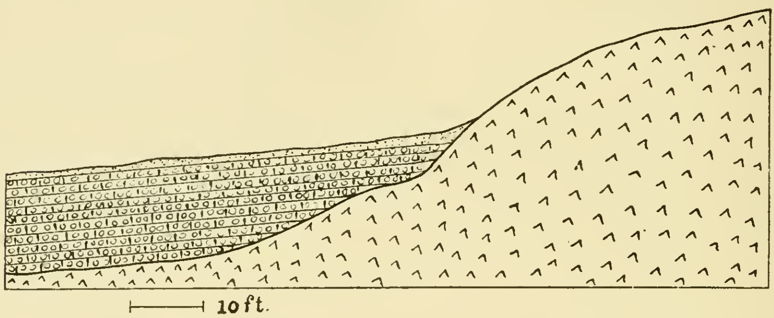


FIGURE 2.—Diagram to show the relative positions of the calcareous quartz conglomerate and the granite at Kingston Mills railway cut. Horizontal and vertical scales equal.

those obtained close to it. Neither rock has undergone any changes such as might be expected were the granite a post-sedimentary intrusion.

On the southwest flank of the same hill, at a slightly higher elevation, is a small exposure of a compact, fine-grained, gray limestone, with a conchoidal fracture, and in close proximity to the granite, which can be followed around it. A quarter of a mile west the limestone beds in the valley are fifteen feet in thickness.

Three and two-thirds miles almost directly south of this, at Fort Hill, on Barriefield common, midway between the Gananoque road and the river shore, occurs an ovoid quaquaversal dome with a gneissic core. The direction of the main axis of the dome is about northeast. The strike of the gneissic structure is about east and west, while the dip is almost vertical. The limestone forms a low infacing ridge, in places broken down. The maximum dip, sixteen degrees, occurs on the southwest side of the dome, but rapidly becomes less as one recedes from the central core. On the northeast side the dip is five and a-half degrees to the south of east, but away from the core, this also diminishes. The limestone is compact, fine-grained, and fossiliferous. In texture it much resembles that found upon the higher exposures on the southwest flank of the granite ridge at Kingston Mills. (figure 3.)

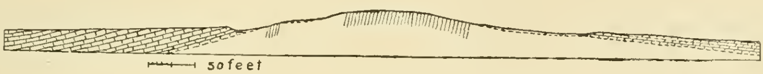


FIGURE 3.—Transverse section of the quaquaversal dome at Fort Hill, Kingston. Horizontal and vertical scales equal.

One-third of a mile east of the dome, the nearly horizontal limestones form an easterly-facing escarpment, talus covered, facing a large area of crystallines, partly gneiss, but mainly a dark red granite. The valley between is about one hundred yards in width, but towards the northeast the depth and width diminish, and the limestones, still almost horizontal, outcrop near the granite (figure 4). This granite is itself a

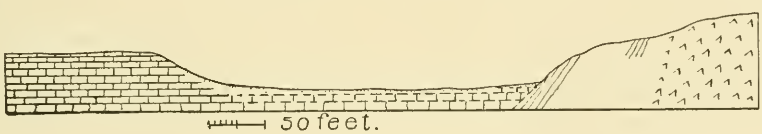


FIGURE 4.—Diagram to show the apparent relative positions of the limestone and crystallines east of Fort Hill. Horizontal and vertical scales equal.

large inlier from the western side of the arm of the crystalline series which connects the Canadian archean with that in New York. East and north, through the township of Pittsburg, there are many ridges of gneiss with a general northeast trend, thes trikes being sympathetic with the direction of the ridges, and the dips nearly vertical, or when inclined, the inclination is generally the same on both sides of the ridge in question. Between some of the ridges long tongues of horizontal strata extend northeastward, frequently, though not always, with an escarpment facing the gneiss. In no place does the limestone show a dip sympathetic to the inclination of the ridge adjacent, though there are cases where the relative positions of the two are such that the dip ought to be nearly thirty degrees, if the gneissic ridge were elevated after the deposition of the sediments.

With reference to these ridges of gneiss, and to the unroofed dome at Fort Hill, Dr. Drummond writes: "The Laurentian strata have been here elevated into these great ridges at a period subsequent to the

Black River times" ('92, 110). So far as indicated in the context, the only evidence upon which this deduction is founded, is the occurrence of the limestone strata "at a high angle," dipping outward from the central dome of gneiss. The evidence, as stated, seems inadequate to admit of the broad conclusion reached.

If the ridges were elevated subsequently to the deposition of the strata, *i.e.*, were ridges of deformation, certain necessary results would follow. Over large areas the gneissic structure is frequently parallel. If, in such an area, a ridge is formed by elevation without faulting, in a region where the dips are not exactly vertical, the inclination on the side of the new ridge towards which the structure hades would be steepened, and that on the side opposite lessened. A traverse across the ridge in the direction of hade would show a decrease to a minimum where the grade was greatest, followed by an increase through the normal at the crest, to a maximum where the grade was at a maximum upon the opposite side, and then decrease to the normal (figure 5). In a series of such ridges this *diversity of dip* would occur *uniformly* across the ridges.

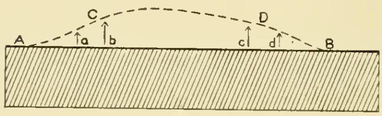


FIGURE 5.—If AB represent the position of the surface of the gneiss before upheaval, and ACDB the position of the surface of the dome after upheaval, then that portion that moves upward at *a* will have a less distance to rise than that at *b*. Therefore the dips between *a* and *b* will be steepened. Similarly, on the opposite side the dips will be lessened. Beneath the crest the uplift will be uniform, and hence there will be no alteration of dip. Where the grade of the new ridge is steepest the change from the original dip will be greatest.

Secondly, strata adjacent to *all* the ridges would be inclined in sympathy with the elevation, the greatest inclination being nearest the steepest and highest ridges; where the inclination of the ridge is very steep the strata would be affected for a longer distance away from the centre of elevation, or if the uplift did not affect the strata at any great distance, faulting and slipping would occur.

If the uplift took place gradually, or rapidly, during the period of deposition of the strata, the *uniform diversity of dip* in the structure of the ridges would be as in the first case. The strata would tend to thin out over the crest, if the material were somewhat coarse. If the uplift were very great some of the beds might even end in wedges against the sides of the ridge. The other necessary results would be as in the former case, though faulting and slipping are less likely to occur, owing to the softness of the beds. In the sections as usually exposed it would be very difficult to distinguish between intersedimentary and post-sedimentary uplift.

Had the ridges existed prior to the deposition of the sediments, having been the product of erosion, the structure of ridge and valley would frequently have the same dip; in cases where the dips varied the diversity would not be systematic. The position assumed by the sediments would depend upon three factors, two of which are, in practice, determinable by observation, the third by inference only. The steepness of the grade of the ridge would necessarily be an important factor. Where the grade was light the sediments would be deposited evenly over the inclined floor. As the inclination of the floor increased the tendency would be for the beds to wedge out and finally to abut against, rather than to rest upon the incline. The fineness or coarseness of material and the degree of agitation of the water would be important varying factors. Where the waters were quiet, and the sediments coarse, the deposition could take place upon slopes down which the materials would readily move if the waters were agitated. If the sediments were fine, the slopes upon which they could rest would be much steeper. The angle of repose for the sediments will then vary as each factor varies, and hence numerous variations are possible and many of these are also probable.

The deduction leads to inquiry as to what is the maximum angle of repose at which, under what may be called normal conditions of deposition, strata of different compositions may be deposited, for obviously this must be known before we can determine, from the dip alone, whether strata were deposited in an inclined position. The number of variants is too great to permit of a complete reply to the question; it seems advisable rather to apply the criteria already deduced to the facts under consideration, and indirectly to obtain a partial answer to the last problem.

In the areas where the gneiss is not obscured by a cover in the bottoms of the valleys, we frequently find the dips the same over large areas of ridge and valley; sometimes there is diversity, in the valley it may be vertical while in the ridge it is inclined, or vice versa, but *uniform diversity* is not found. This *uniform continuity* and lack of *uniform diversity*, particularly in areas where the structures are inclined away from the vertical, would alone indicate that these are not ridges of deformation, but on the contrary, would lead us to infer that they are the result of erosion.

Attention has already been called to the lack of sympathetic dip in limestones adjacent to steep gneissic ridges. At Kingston Mills cut the upper part of the granite face was too steep for the coarse sedimentary deposits to rest upon it under the then existing conditions. Consequently they moved down, as deposited, to a position of stable equilibrium, producing in the slipping, the slight upward drag seen in the beds just at the contact. At the ridge near Varty lake, and at that one now fronting the escarpment east of Fort Hill, similar conditions probably prevailed. The steep face of the ridge in the latter case has a slope of about thirty-five degrees, in some places it is even steeper, yet the limestones show no sympathetic dip, (figure 4). At Fort Hill, where the grade indicated by the strata is *now* sixteen degrees, the water would probably be somewhat deeper, and the calcareous sediments were very fine grained. The inclined position of the much coarser sediments at Kingston Mills, and the state of preservation of the fossils in these coarse sediments, indicate that the water was moderately quiet so there seems no adequate reason why the finer material should not have been deposited in its present inclined position at Barriefield common, (figure 6). With reference to the other criteria, thinning out of

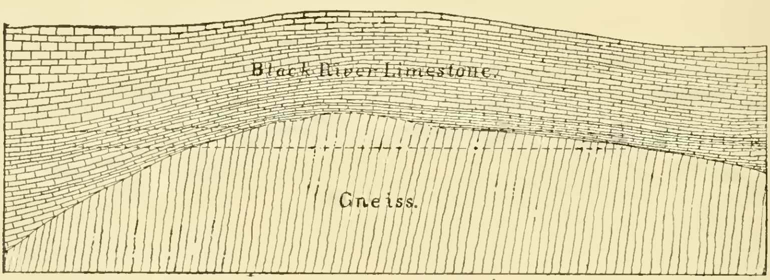


FIGURE 6.—An ideal section to show the probable conditions at Fort Hill before degradation and denudation. The dotted lines show the bottom and top of figure 3.

beds, faulting and slipping, so far as known the two latter are absent, and the first can only be applied rarely.

The balance of evidence thus seems to indicate that the ridges are of pre-sedimentary origin, and that the sedimentary strata were deposited essentially in the positions in which we find them to-day. It is interesting to note that at the base of the cliff on Deer bay, in the distance of a little over a mile, there is a continuous transverse section of no less than five light anticlinal domes in strata which are only removed a few feet from the gneiss, here below the water level. The arch dies out in about the first twenty feet of strata. Above that, so far as the eye can judge, they are nearly horizontal.

Logan ('63, 98), refers to strata near Millburn (then, Daly's Mills) having comparatively high angles of dip near the junction with the

Laurentian, where they seem "almost always to be slightly accommodated to the worn surface." Laflamme ('84, 15; '86, 43) has noted similar features in the Lake St. John region; and Adams ('93, 338) has drawn attention to the fact that the *roche moutonnée* character of the Laurentian rocks was impressed upon them in pre-Cambrian times.*

Outliers.—Similar evidence as to the character of the pre-sedimentary surface is offered in the vicinity of the numerous outliers upon the crystallines.

Conclusions.—In Central Ontario, from the evidence afforded by the inliers of Archean within the area of Palæozoics, and of the outliers of Palæozoic strata upon the Archean, it seems that the sediments were laid down upon an uneven floor essentially the same as that presented at the present day by the Laurentian areas along the borders of the Cambro-Silurian escarpment.

Examples elsewhere.—These buried oldland surfaces are found in many other localities. Sometimes the eroded surface is almost a plane, as seen in the Grand Cañon section of the plain upon which the lower Palæozoic deposits rest. Again the surface may have had irregularities many hundred feet in height, as shown by the Baraboo ridge in Wisconsin, or as seen in an area in the Scottish Highlands, described by Geikie. (See Newberry, '58, 57; Irving, '72, 99, and '77, 427; Dutton, '82, 209; Geikie, '88, 400; Bell, '94, 362; Keyes, '95, 58; Van Hise, '96, 59).

SECOND PROBLEM, DATE OF EROSION.—The second and third problems for consideration have reference to the time and conditions of erosion by which this pre-sedimentary topography was produced.

From the writings of the earlier geologists the prevailing view seems to have been, that the Palæozoic sediments were laid down upon a rising sea bottom, and that the Archean areas in Canada represent the first emerged land. The more recent view is that the sediments were accumulated on a sinking land surface.

In this area, the granites, gneisses, and schists date from Archean time. The evidences of a vast amount of denudation afforded by the truncating surfaces, and the absence of lower and middle Cambrian sediments from every portion of the area, make it improbable that during the deposition of these sediments elsewhere the land here was below sea level. The consensus of opinion, based upon the study of

* See also Lawson, 1890.

the conditions of the pre-Cambrian floor, and upon the distribution of the Cambrian sediments over North America, is that during the interval of the deposition of early Cambrian sediments there was a great interior continent, of which Central Ontario would form a part.

Walcott (**'91**, 567) thus sums up the conclusions from his studies on "North America during Cambrian time":—1. "The pre-Cambrian Algonkian continent was formed of the crystalline rocks of the Archean nuclei, and broad areas of superjacent Algonkian rocks that were more or less disturbed and extensively eroded in pre-Cambian time. Its area was larger than at any succeeding epoch until Mesozoic time."

* * * * * * * * * * * * *

4. "The interior continental area was, at the beginning of Cambrian time, an elevated, broad, relatively level plateau between the Paleo-Appalachian sea on the east, and the Paleo-Rocky Mountain barrier on the west."

* * * * * * * * * * * * *

7. "The Cambrian Sea began to invade the great Interior Continental area in late Middle Cambrian time, and extended far to the north toward the close of the period."

8. "The depression of the continent in relation to sea level began in pre-Cambrian time and continued with a few interruptions until the close of Paleozoic time."

Many conclusions with reference to events which occurred so long ago must necessarily be somewhat uncertain. With our present knowledge of the evidences, it seems that during Archean and early Cambrian times this area formed part of a continental area. The processes by which the even-topped upland was produced operated so long ago that it is impossible to determine their precise nature. The character of the subsequent dissection appears to indicate that the present topographic features of the uplands were, in the main, the product of subaerial erosion during a pre-Potsdam period of elevation.

The balance of evidence thus leads to the conclusion that the present surface features of the crystalline area, at least along the borders of the Palæozoic sediments, are essentially the same as they were in pre-sedimentary times. The problem now arises as to the process by which the degradation and the denudation produced the even-topped upland and the varying features of the present topography.

It would be well to note with reference to the term *even-topped*, that a personal equation must be considered. The expression is used here to describe the sky-line of the upland plain, where for long distances, so far as the eye can judge, it appears with no marked irregularity. In many parts of the area the surfaces of large lakes offer horizontal lines for comparison. Occasional irregularities occur, and these frequently make abrupt changes in the otherwise even line. *Upland plain* has been used to indicate that imaginary surface whose elevations accord with the elevations of the even sky-lines, as seen in many parts of the area. *Upland* indicates portions of the present land surface whose elevation accords closely with the upland plain, and whose surface presents only minor irregularities, as compared with the greater irregularities of the surface of the region as a whole (figure 7). The present

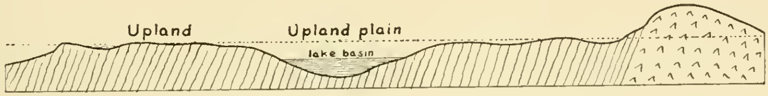


FIGURE 7.—Diagram to illustrate the definitions of terms.

topography is such that although the slopes are frequently graded, there are few areas to which the corresponding term lowland should be applied. The change in gradient from the valley side to the upland is frequently so marked as to justify the use of the term *shoulder* to describe the place where the change occurs.

THIRD PROBLEM, CONDITIONS OF EROSION.—Two hypotheses have been offered to account for the origin of topography of this nature. The one would consider it as the product of a single cycle, the other as the product of two or more (n + 1) cycles. The first, the "bevelling" hypothesis, would consider the present features as those of an ancient mountain system reduced to maturity and possibly re-elevated and made more rugged. The second would consider that the even uplands (produced during a long interval of time, at a period when the land stood relatively near base level) are remnants of the upland plain, and that the present valleys and lowlands were due to an increased activity of the agencies of degradation and denudation because of subsequent elevation.

If the area is part of an old mountain system reduced to its present form by bevelling, the present elevations must have once been higher and more rugged than they now are (figure 8, ABCDEF). In the process of degradation the ruggedness would be reduced and the slopes become graded by the removal of waste from the mountain sides and its transportation to the valleys, where it would either remain or be removed according to circumstances. Eventually, there would be a uniformly graded slope from stream bed to mountain top (AGCDHF). After the production of this slope the process will continue, but more slowly, with the gradual reduction of the crest, and corresponding

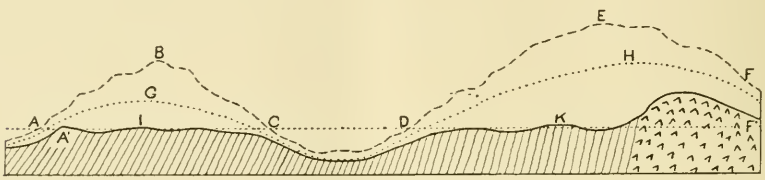


FIGURE 8.

decrease in grade, approaching but never reaching complete horizontality. Of necessity there will always be one point, or a series of points in line, higher than all the rest. From here the gradient would be downwards in all directions. In the late mature stages, when there is some approach to a nearly smooth surface over the whole mountain, there will be no abrupt change in slope.

Such an explanation of the process of degradation makes no provision for the occurrence of areas of greater or less extent with almost identical elevation, (A′ IC, DK) ; nor for the abrupt changes in gradient such as occur at the shoulders before mentioned (A′, C, D.) Hence, in the writer's conception of the process, it seems inadequate to explain significant facts of the present case.

The two-cycle (n + 1 cycle) hypothesis would explain these peculiarities by postulating a previous cycle (or cycles) of erosion in which the land was cut down to a surface of very faint relief and subsequently elevated and dissected, the new valleys not having yet extended their graded slopes far enough to completely obliterate the plain of the former cycle. The shoulders were produced where the change in gradient from the present valley side to the older plain occurs.

In almost every locality where the Cambro-Silurian sediments are seen in contact with the crystallines, the surface is seemingly quite fresh. Except in one known locality, where a distinct arkose of angular material is found, the old soil cover seems to be completely gone. The process by which this cover was removed and the surface of the Archean freshened, is at present undetermined.

SUMMARY.—The present topography of the pre-sedimentary floor may be regarded as the product of a degradation which produced a planation surface, and the residual monadnocks, as indicated by the even-topped upland. This surface was uplifted to permit of the renewed activities that carved and denuded the ungraded, or partly-graded slopes of sub-maturity, now presented wherever it has been freed from the Palæozoic cover. This latter dissection and denudation antedates the sediments, within this area, commonly called Potsdam, and may well have taken place in early and middle Cambrian times.

The ancient pre-sedimentary surface may be conveniently described as a sub-maturely dissected and denuded peneplain dating from before early Cambrian times.

THE PALÆOZOIC SERIES.

A Question of Correlation.—In tracing the geological history of this area, by means of the nature and relations of the different deposits found adjacent to and within its boundaries, there is a question that must not be disregarded, as to the correlation of partly eroded stratified deposits, at a low angle of dip, around the margin of an oldland area. In the formation of a series of deposits upon a slowly sinking land surface, the normal distribution of material is the formation of sand and pebble beds at the shore line, grading gradually into clays and muds, and thence into calcareous deposits (figure 9). Any given stratum must have three synchronous members, each merging gradually into the

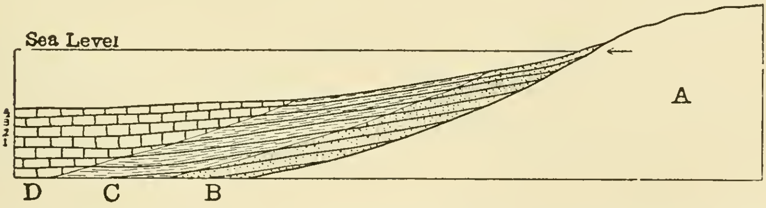


FIGURE 9.—Diagram to represent the normal distribution of sediments. A, oldland ; B, sandstone and conglomerate zone ; C, shale zone ; D, limestone zone. Transition zones are indicated by lines.

adjacent member. The beds composed of strata which have been deposited successively must also each consist of these three members. During the time of the formation of any given bed the forms of life existing at that time will be distributed over the surface of that bed, each in its appropriate locality. The sand-loving forms will be near the shore, the mud-loving forms in the areas which afterwards become converted into shale, and the forms which thrive best in deep clear water will be found further seaward. At the transition zones where there is a merging of conditions there will be a merging of forms. Accidents may happen by which the normal distribution is slightly disturbed ; and some few forms may exist in all three zones.

Since the production of the deposits, their thickness, and their other relations depend upon the two factors, *rate of depression* and *rate of supply of detritus*, with varying conditions as to depth of water, there are many possible variations from the normal arrangement. The result of one such variation is represented diagramitically in figure 10, where the rate of supply of detritus has been sufficiently in excess of the rate

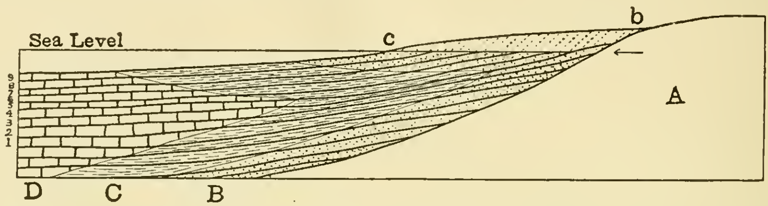


FIGURE 10.—Diagram to represent a special variation in the distribution of sediments. A, oldland ; B, sandstone and conglomerate zone ; C, shale zone ; D, limestone zone. Transition zones are indicated by lines. *bc*, sub-aerial deposits.

of depression of the land, to permit of the transgression of the sands over the seaward zones. If a rapid variation had taken place in the opposite direction, the muds and sands might become mingled along the shore, and eventually the limestones might rest directly upon the oldland surface. Irregularities in the variations of each factor will lead to many irregularities and overlaps along the zones of transition.

By subsequent processes, after a greater or less interval of time, these deposits will become indurated and form sandstones and conglomerates, shales, and limestones. If, after uplift, the greater part of the sediments are eroded away, and small remnants, perhaps as outliers, remain in protected areas, we may find limestone in one place which is contemporaneous with sandstone in another, though the fossils in each are wholly unlike. Moreover, deposits, of entirely different epochs, may be almost identical because derived from the same source.

SANDSTONES.—In Central Ontario, particularly towards the eastern end, where the sediments occur in ellipsoidal basins, and with a very slight and irregular dip, the sandstones and some of the other beds entirely without fossils, there is a strong a priori argument for considering that the *sandstones* and *some of the limestones* are contemporaneous. The oldest sediments, within the area, which can be identified by fossils, are the Black River limestones. Conformably below these are beds, which in some cases carry fossils that are supposed to mark a transition vertically from the Chazy at least, but which equally well may mark a transition horizontally. Chiefly as outliers, but occasionally passing beneath the non-fossiliferous beds below the identified beds of the Black River formation, are a series of sandstones usually termed Potsdam. (The maximum thickness is, locally, sixty feet). As no fossils, except some very obscure Scolithus borings, have, so far as the writer knows, ever been found in these beds; and as *Potsdam* is a term introduced to describe rocks where certain definite stratigraphic and faunal relations hold, it is inadvisable to apply the term until these well defined relations are proven to exist here. Even were fossils found in the sandstone, the possibility of their being, *in this locality*, contemporaneous with the lower limestone beds, would not be diminished.

Much of the sandstone of these deposits, in this area, occurs in depressions between pre-sedimentary ridges. There are many angular fragments of gneiss and quartz, both large and small, included in the sandstone. In many places there are no known beach-worn pebbles, and no fossils, even in very thick beds; the material has been well sorted and consists almost wholly of quartz grains; the beds are frequently very massive and obscurely cross-bedded; ripple marks are absent or very obscure in many localities. There thus seems to be good reason for thinking that much of this sandstone may be waste which was laid down here, possibly by streams, after the crystalline surface had been smoothened and freed from its residual soil, if such ever existed, but before the advent of the sea, and that the shoreline of that time is now concealed by the overlying deposits. Subsequently, in the rapid depression of the land immediately preceding the time when the limestones overlapped upon the crystalline area, the surface of these sands may have been evened off, and perhaps a small amount of new material was added.

ARKOSE.—There is one known locality in which the non-fossiliferous sediments beneath the identified Black River limestone are especially interesting. At the foot of the escarpment on Deer bay, just at water level, a few well marked beds of arkose are exposed. The beds average about ten inches each, the whole deposit being an unknown amount over six feet in thickness. This arkose consists of translucent partly worn crystals and fragments of quartz and angular fragments of pink orthoclase feldspar, cemented with a dark reddish-purple feldspathic and calcareous cement, with occasional patches which resemble the argillaceous portions of some of the succeeding beds. The rock is readily friable and forms a beach of small gravel just at the foot of the cliff. The constituents of this arkose are distinctly different from those of the adjacent gneiss. The nearest outcrop of gneiss is one hundred yards away, and the water between reaches a maximum depth of nine feet, but, from the conformation of the bottom, the exposed portion of the deposits must evidently be within a very few feet of the gneiss immediately below it. The deposits may be regarded as a remnant of the old soil cover of pre-sedimentary times, slightly rearranged.

BLACK RIVER AND LATER FORMATIONS.—Succeeding these unidentified beds are the Black River limestones, which form a cuesta, whose northern boundary forms an escarpment extending from Georgian Bay to Kingston. The Black River beds are succeeded by the Trenton limestones with a thickness, as calculated from the dips near the eastern end across Prince Edward county, of over 1,400 feet. These are overlain by about 100 feet of Utica shales. Above this are nearly 800 feet of Lorraine shales and sandstones, overlaid in turn by 545 feet of Medina marls and sandstones. The upper bed of the Medina is, in Central Ontario, a heavy gray sandstone, about twelve feet in thickness, but occasionally thicker. The beds above this, found in the Niagara escarpment, consist of Clinton dolomitic limestones and shales, overlaid by the Niagara limestone. Throughout the region, so far as known, there is no observed unconformity between the beds of the various formations.

SUMMARY OF THE PALÆOZOIC HISTORY.—The geologic history of the area, subsequent to the period of denudation and dissection of the crystallines, was begun by a depression of the land, during which some small amounts of sand were deposited along and near the shores, with deposits which formed shales and limestones in the deeper waters. This depression continued somewhat faster than the rate of supply of detritus, and finally limestones, which, however, contain siliceous material, were deposited over the whole area. The waters were "richly tenanted by a great variety of forms of invertebrate life, and representing the culmination of invertebrate animals in the Lower Palæozoic" (Dawson, '89, 73). The great thickness of the deposits indicates that the Trenton epoch was of considerable duration. Towards the close of the limestone-forming epoch a variation took place, the new material supplied to this area was in the form of clays and muds. The change in the character of the deposits was accompanied by a change in the types of animal life here present. This change, marked by the Utica shales, was probably caused in part by a decrease in the rate of depression of the land. That it did not cease is shown by the thickness of the deposits.

Throughout the Lorraine epoch the area has been one of large sandy mud flats, alternately bare, exposed to the sun and rains, and submerged. The shallow sea appears to have endured for some time, since these deposits gradually give place to the sandstones of the Medina epoch. During the Medina there has been an alternation of the depth of the ocean here, as evidenced by the mingled sandstones and marls, the former with mud cracks, ripple and current marks. The final stage of the Medina led to the accumulation of a broad thick band of fine-grained siliceous sandstone, free from ferric oxide, in marked contrast to the majority of the lower beds.

The succeeding epoch must have begun with a relatively rapid depression of the land, since the overlap of the Clinton dolomitic limestone upon the upper sandstone of the Medina is abrupt. The depression seems to have continued rapidly enough to permit of the overlap of the succeeding Niagara rocks upon the crystalline areas far to the north. From the purity of the limestone, and from the types of organic remains, and their abundance, it is inferred that the waters of this epoch were clear and warm. The materials from which the limestone is made were probably drawn from the sea water by the invertebrate animals in the making of their hard parts.

This second great limestone-making epoch was followed by a gradual shallowing of the water, during which the Guelph dolomites were formed. Eventually the water became very shallow ; enclosed lagoons, occasionally flooded, were numerous ; in these lagoons the salt and gypsum beds of the Onondaga were formed by the evaporation of the water and the concentration and precipitation of the saline compounds in solution.

The sandstones of the succeeding Devonian period are now many miles distant from the front of the Niagara cuesta. They may at one time have reached out and overlapped it, but if so, what their north-eastern extension may have been is unknown. During the period of their formation the central portion of the Archean area may have been above water, and the denudation which has subsequently removed all the Niagara limestone, with a very few small protected areas excepted, could then have already begun. It is interesting to note that the peneplain represented by BC (figure 1, page 144) may date its beginning from this Devonian degradation.

The area had thus taken part in three great cycles of deposition concomitant with three great continental oscillations, or a long continued single oscillation of varying rate. During two of the cycles great limestone deposits were made within its boundaries. The nearest known areas of Lower Carboniferous are in Michigan, 140 miles away, and their composition is such that it is usually inferred that ever since the close of the Devonian period this area has been above sea level and exposed to denudation and dissection.

POST-CARBONIFEROUS HISTORY.

MESOZOIC, CAINOZOIC AND EARLY PLEISTOCENE EPOCHS.—There is little or no direct evidence of the history of the area during Carboniferous and Mesozoic time. The late Mesozoic was a period of extensive peneplanation throughout most of North America. In Wisconsin and Michigan to the west, and in New York and Pennsylvania to the south, the remnants of the planation surface have been recognized. It seems probable that the same planation processes, working northward from these areas, and southward from the Arctic region, may have, in part, produced the younger of the two plains upon the Archean areas in Canada. It is true, this plain may be of pre-Palæozoic age. Whether it is such, and yet younger than that beneath the sediments cannot be shown until it is proven that the sediments once actually rested upon it, and not upon a surface now eroded away. This latter would be the former northward extension upon which they now rest (figure 1, BF p. 144). The study of the isolated outliers, such as those of the small areas of limestone in the Lake Nipissing region and elsewhere, may show that they are preserved because thrown into their present protected positions by the downthrow of a fault block. If so, the probability of this plain being of Cretaceous age will be strengthened. By way of comparison it may be noted that a series of faults dislocated the early sedimentary rocks of Sweden and Norway. Later planation left only a few small patches at baselevel, upon the downthrown blocks. Subsequent elevation of the whole area, and erosion of these softer remnants produced a series of depressions, in some of which are still found isolated patches of the soft rocks. The lower portions of these depressions frequently form lake basins, the most noted of which are Boren, Roxen, Glan, and Braviken.

The period of Cretaceous planation was followed by an undetermined amount of elevation of portions of the continent, probably including this area. The immediate effect of such an uplift would be the active

MAP I.


MAP
of the
RIVER SYSTEMS
of
CENTRAL ONTARIO
by
A.W.G. Wilson
Scale
0 10 20 30 40
GEORGIAN BAY
Nottawasauga Bay
Owen Sound
HALIBURTON
ADDINGTON
VICTORIA
HASTINGS
FRONTENAC
PETERBOROUGH
SIMCOE
GREY
BRUCE
DUFFERIN
ONTARIO
DURHAM
NORTHUMBERLAND
PRINCE EDWARD
LENNOX
YORK
WELLINGTON
PEEL
HALTON
WENTWORTH
LINCOLN
NEW YORK
LAKE ONTARIO
TORONTO
Lake Simcoe
Severn R.
Orillia
Barrie
Collingwood
Nottawasauga R.
Balsam L.
Sturgeon L.
Pigeon L.
Stony L.
Chemong L.
Trent R.
L. Scugog
Croydon
Belleville
Trenton
Napanee
Kingston
K. Mills
Palgrave
Humber R.
Whitby
Port Hope
Cobourg
Presqu'Isle
Pt. Peter
Stony Pt.
Milton
Hamilton
81°30' 81° 80°30' 80° 79°30' 79° 78°30' 78° 77°30' 77° 76°30'
45° 44°30' 44° 43°30' 43°

renewal of the process of subaerial degradation, and the development of topographic forms and an adjusted drainage system appropriate to a region underlain by alternate series of strong and weak rocks at low angles of dip. During the extensive Pleistocene glaciation the topographic features, the product of the preceding cycle, may have been largely modified, destroyed, or otherwise obliterated, and new forms produced.

Measure of erosive work.—Our measure of the work performed during these two periods must necessarily be derived from a knowledge of the present features, and of the conditions existing before the operation of the erosive agents. The proportion to be assigned to either period depends upon a knowledge of the relative competence of the processes of degradation, and of the time during which they were operating. The amount of work performed by either process, and by both, will vary with the locality, and with the conditions under which the process is in operation, *e.g.* geographical position, elevation, position with reference to baselevel, character of the rocks, relation to the ice front and to the névé of a glacial lobe. At present the knowledge of the total effects of both processes, and of the method of operation of sheet-glacier ice, seem too limited to warrant the assignment of a definite portion of the work to either, except in local cases.

PRESENT FEATURES.—*General Description.*—The Niagara cuesta is a prominent topographic feature extending along the south shore of Lake Ontario from east of Rochester to Hamilton, thence northward across Ontario to the Manitoulin Islands, thence curving southwestward to the east of Green Bay and across parts of the States of Wisconsin, Illinois and Iowa. Lakes Erie, Huron, and Michigan are situated upon the outer lowland; Lake Ontario, Georgian Bay, and Green Bay lie upon the lowland in front of the cuesta; Lake Superior lies in a position outside of both lowland and cuesta.

The cuesta-front forms one boundary of a great inner lowland. The southwestern loop of this lowland is best developed in the State of Wisconsin, and may thus be appropriately designated the WISCONSIN LOWLAND. The eastern part, the ONTARIO LOWLAND, includes the basins of Lake Ontario and Georgian Bay, as well as the adjacent land areas. The two parts of the inner lowland are connected by a narrow, more or less submerged belt, passing across the Manitoulin Islands. It has been found convenient to refer to the present unsubmerged part of the Ontario lowland, within the Province of Ontario, as the CENTRAL ONTARIO LOWLAND. (Map I.)

The northeastward extension of the Ontario lowland merges gradually with the cuesta formed by the Black River strata. The escarpment-front of the Black River cuesta extends from the vicinity of Kingston northwestward to Georgian Bay, and thence across the bay, beneath whose waters it seems to be still traceable, to the Manitoulin Islands. The unsubmerged portion of the escarpment averages about ninety feet in height, and locally is occasionally much higher. In the region west of Lake Simcoe, and in northern parts of Hastings and Addington counties, it is partly obscured by drift deposits.

The fronts of both cuestas present many irregularities appropriate to development under subaerial processes. The principal physical features of "Old" Ontario are those characteristic of an ancient coastal plain which has passed through a period of planation followed by one of uplift, dissection, and the development of an adjusted drainage system. Similar topographic forms have been developed, also with varying strength and expression, in Middle England, in the Paris Basin and elsewhere near oldland areas.

The drift deposits in Central Ontario form a prominent ridge, or series of ridges, the Oak Ridges, of varying breadth, lying at an average distance of about ten miles north of Lake Ontario, and extending eastward to the vicinity of Trenton. At the western end, near Palgrave, the thickness of the deposits is sufficient to almost obliterate the escarpment of the Niagara cuesta. A number of spurs extend southward and northward from the main ridge.

This morainic ridge divides Central Ontario into two drainage slopes, a northern and a southern. The Trent river, the largest stream within the area, conveys a large percentage of the drainage from the northern slope, and from the southern slopes of the crystalline area to the north, across the ridge to the Bay of Quinte in the vicinity of Trenton. The remaining portion of the drainage of this northern slope reaches Georgian Bay, chiefly by the Severn river from Lake Simcoe basin, and by the Nottawasaga river. The waters from the southern drainage slope reach Lake Ontario by a number of small streams. East of Trenton the drainage, which is across the area from within the Black River cuesta, is controlled almost wholly by the rock topography.

The present features of Central Ontario, as a product of the operation of the two processes, Pliocene and early Pleistocene subaerial erosion, and Pleistocene erosion by sheet-glacier ice, are of special interest, not only in themselves, but because of their relation to the

PLATE I.

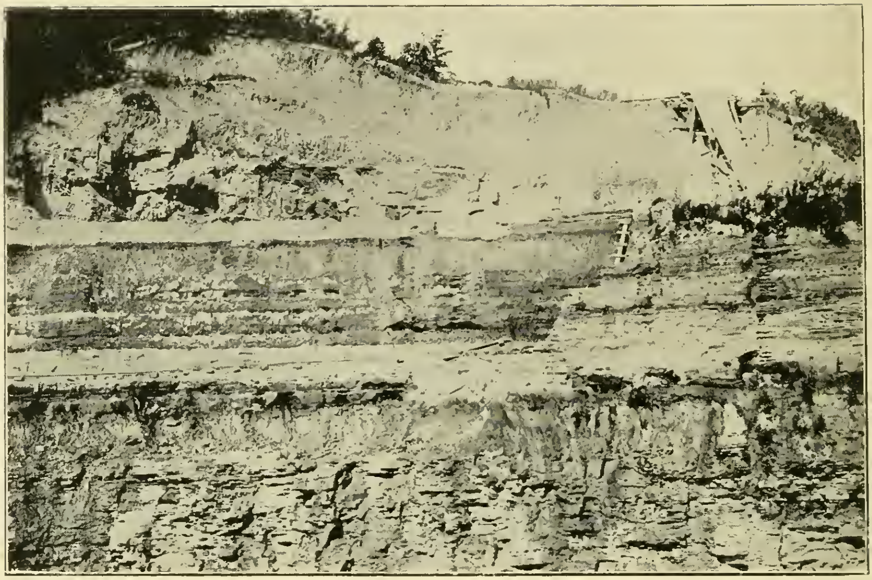


Figure 1.—Pleistocene deposits at Taylor's Brick Mills, Toronto. The lowest beds are Lorraine shales; these are overlain by a thin sheet of till, and this in turn by the beds of the first Interglacial epoch. (Photo. taken 1895).

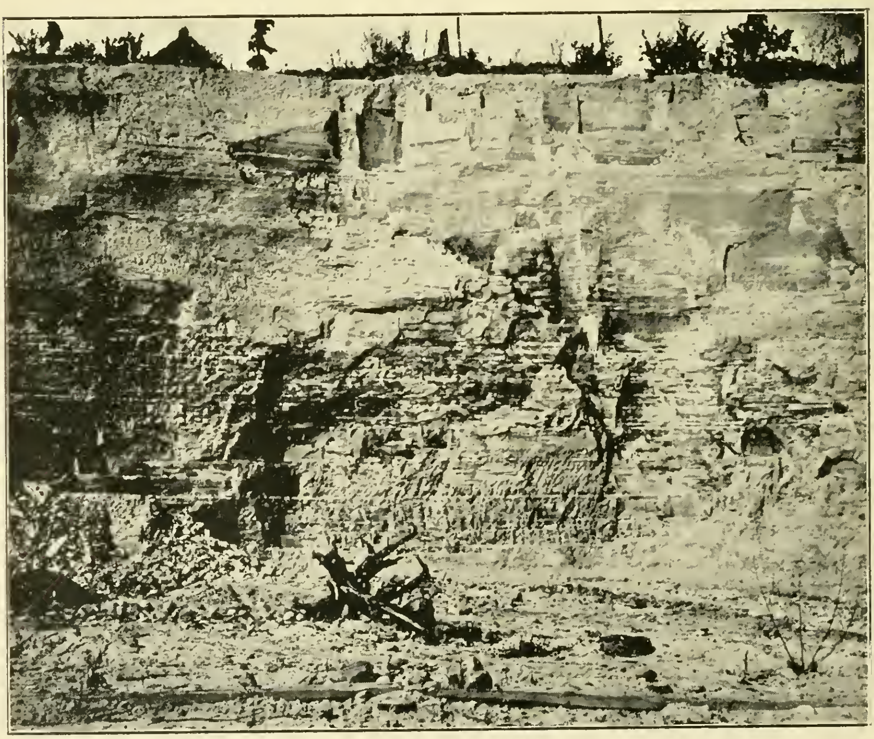


Figure 2.—Pleistocene Deposits at Taylor's Brick Mills, Toronto. Beds of the second Interglacial epoch. (Photo. taken 1895).

question of the origin of the basin of Lake Ontario. Although over large areas the topography of the rock floor beneath the Pleistocene deposits cannot be ascertained, there are also large areas in which there is no difficulty in determining its essential features, often even to minute details. Since the interpretation of the features of this rock topography depends upon its relation to the overlying Pleistocene debris, it has been thought best to first describe in outline, these latest deposits.

Pleistocene Deposits.—Hinde ('77), Coleman ('94, '95, '97, '98, '99, 1900), and others, have described Pleistocene deposits occurring in the vicinity of Toronto, notably at Scarboro' Heights, describing three sheets of glacial till. The two upper sheets overlie thick deposits of stratified and finely laminated clays and stratified fossiliferous sands and gravels. (Plate I.) One hundred miles east of Toronto, just north of Trenton, occur series of deposits in which is cut a sea-cliff attributed to Lake Iroquois. The crest of the Iroquois sea-cliff is 718 feet (bar.), and the rock surface just east of this, along the Trent River is about seventy-eight feet above Lake Ontario.* The total thickness is thus 640 feet. These deposits show three till sheets alternating with two series of stratified beds, chiefly sands and gravels. The precise thickness of each of the five series of beds has not been ascertained as yet, but the till beds certainly, and the stratified beds probably, are much thicker than the similar beds at Toronto.

Between this locality and Toronto, in each of four other transverse sections northward from Lake Ontario, the three till sheets have been encountered by the writer. In a trip, on foot, along the lake shore from Presqu' Isle to Burlington beach, the two lower of these three till sheets have been traced for a long distance. East of Port Hope, between Bowmanville and Whitby, and west of Toronto, a till sheet rests in many places directly upon the rock surface. Sometimes only the upper portion of this sheet is visible, and occasionally it passes wholly below the lake level. Provisionally this lowest sheet may be considered as the equivalent of the lowest till sheet at Toronto and at Trenton.

From Port Hope westward a second till sheet, with varying thickness, resting upon stratified deposits, both sands and laminated clays, and once (near Oshawa) upon the lower till sheet, can be followed along the lake shore almost continuously to Scarboro'. At Scarboro' there is a nearly continuous section about nine miles in length. Between Port Hope and Trenton the edge of this sheet lies from one to four miles

*There is a continuous exposure of rock surface along the Trent, transverse to the ridge, and in a number of localities to the eastward the general topography of the rock surface can be well established.

back from the lake shore. Provisionally this bed, from its position, not from any identification of underlying beds, may be correlated with the middle till sheet, the lower of the two sheets exposed at Scarboro'.

Except in the Scarboro' section, the edge of the third till sheet is found at a varying distance back from the lake shore. At Trenton it is about three miles from the Bay of Quinte ; in Northumberland county it is about six miles north of the lake. Its extent northwest of Toronto has not been traced. The upper till in these localities is thus provisionally correlated with the upper till in the Scarboro' section and in the vicinity of Toronto. In no place, so far as the writer is aware, is it known to rest upon the middle till sheet, but always upon stratified sands, gravels, or clays. In Northumberland and Durham, and elsewhere, the upper till sheet is overlain by a series of stratified sands and gravels.

In the districts around Lake Scugog and around Lake Simcoe, and for some distance on either side of these areas, till sheets, overlying sands and gravels also occur. From their relative position and other relations, there is reason to think that the upper one of these is the equivalent of the third till sheet on the Lake Ontario side of the ridge.

The middle till sheet rests unconformably upon the beds of the first interglacial epoch ; the amount of erosion which preceded its deposition cannot at present be determined because the necessary data are not all collected. Obviously the amount of erosion to be attributed to the ice sheet is also, at present, indeterminate. A maximum limit of less than five miles may be assigned in one case for part of the underlying deposits, because of the fragments of Utica shale in the middle till. It may be possible to define the upward limit later when the precise relations of the stratified beds are worked out.

In the Scarboro' section this till sheet fills an old erosion valley in the underlying stratified deposits. Hinde ('77, 402), who first described the depression, regarded it as a result of glacial erosion, but recent investigators, because of its form, location transverse to the direction of the ice movement, and the absence of any evidence of violent erosion, consider it an old river valley. Similar but smaller depressions, some of which even Hinde regarded as stream channels, are found elsewhere in the Scarboro' section, and more rarely in sections to the east. At the eastern end of the Scarboro' section, where the ice *ascended across the beds*, the stratified beds, which underlie the till sheet, are very much contorted and plicated. Westward from this there is little or no

FIGURE 1.—Plications in stratified sands of the first Interglacial epoch, Scarboro' Heights.

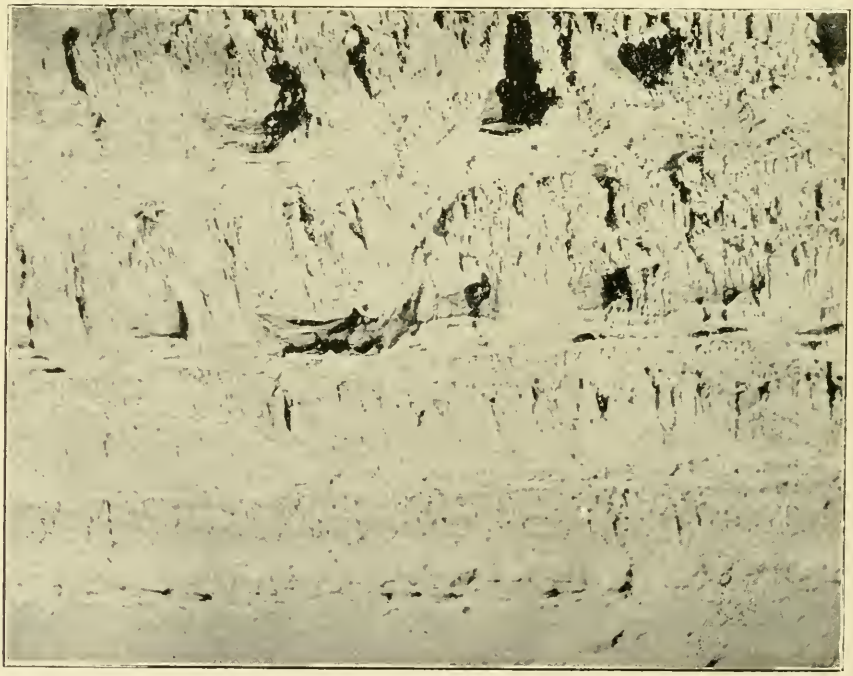


FIGURE 2.—Laminated clays and sands slightly folded by the overriding ice sheet, near Newtonville.

plication, not even at the crossing of the old depressions, and the till sheet descends at the opposite side of the section. Some twelve miles further west it again ascends over plicated beds. In some sections east of Scarboro' the till sheet is seen in clear cut cross section ascending across the beds with almost no plication of the underlying deposits (Plate II).

Whether the three periods of glacial transgression and retreat, marked by the three till sheets and the intermediate deposits in Central Ontario, are to be correlated with similar periods as determined to the south of the lake, or represent local variations in the later positions of one ice sheet, it is at present impossible to say. The correlation of the deposits in the two localities, for lack of sufficient knowledge of intervening areas, is not yet definitely determined. Professor Chamberlin has provisionally classified the fossiliferous beds beneath the middle till sheet as contemporaneous with the interval preceding the Wisconsin formation, regarding the middle till sheet of the Toronto sections as equivalent to the Wisconsin till (**'95**b, 273). He suggests that the Toronto beds might lie in a position at least one hundred miles back from the front of the ice sheet whose till deposits overlie them (**'95**a, 768; see also, Coleman, **1900**).

A feature of particular interest is the fact that here are two sheets of glacial till, overlying still soft sands and gravels, over which the ice that deposited the till sheets must have transgressed. In its transgression the ice sheet has passed over large areas without leaving any mark of disturbance in the underlying beds. In some cases, not in all, where it ascended, the beds on the side from which the ice came are very much disturbed, but the disturbance is confined to the place of ascent. Many instances of modern glaciers over-riding soft deposits have been cited as evidence of the inability of the ice to do significant erosive work. To this the principal objection has been that this inability is shown only at the edge of the sheet. In Central Ontario, whatever the distance between these beds and the edge of the ice sheet which overlay them may have been, it is extremely improbable that at its maximum extension they were just at the margin. There were two periods when the ice overran obviously very incoherent deposits, and there is no known evidence of great erosion by these ice sheets *alone*, over a distance of more than one hundred miles in length, and of a width undetermined, but more than six miles for the middle till sheet, and over an area very much larger for the upper, and perhaps for both. Whatever may have been the amount of material eroded by the ice during these two advances, there is an enormous amount still in place, lying between the rock floor and the youngest till sheet. The inability of the sheet-glaciers, under certain unknown conditions, to remove this material during the two last periods of transgression, raises the question as to whether these conditions may not have existed also during the period of operation of the first ice sheet, when it was overriding bed rock instead of soft sands and clays. This question can in part be answered by a study of the bed rock features.

Eastern Rock-Valleys.—The eastern boundary of the moraines is approximately a line running northeast from Trenton to a point just south of Croyden in the northern part of the township of Camden, Addington county. East of this line in the southern part of the county of Hastings, in Prince Edward, in Lennox and Addington, and in Frontenac, the topography of the underlying rock surface is but little concealed, large areas of almost bare limestone are quite common. This is also essentially true of the rock topography along the margin of the Black River cuesta between Mud Turtle lake and Kingston, with the exception of a narrow belt in part of Huntingdon and Hungerford townships.

In the eastern counties the drift cover is very thin, and rock valleys, now occupied by streams, can be followed readily from their outlet on Lake Ontario or the Bay of Quinte, more or less completely across the limestones towards the Archean. In these counties there are six at least which can be followed all the way across, each to a long narrow lake whose limestone scarped basin is floored with crystalline rocks. There are many more which reach nearly across (Map II). From a map study of Jefferson county, New York state, it seems probable that at least some of the streams in that state belong to this category. The whole series of valleys, some twenty-five and more, is remarkable for its parallelism, the general direction being southwest, and for the regular spacing of the streams. The valley depressions of some are readily traceable under the lake waters, with some complications, to a line running between Stony Point and Point Peter, and in some cases beyond. Where these valleys are unsubmerged, their sides, at the lower ends, are generally steeper on the southeast, towards which the rocks dip, and less steep, sometimes broadly open, on the northwest. Towards the upper end, especially in the case of those which reach the Archean highlands, the valleys are sometimes still broad, but both sides are of about equal altitude and steepness. The average depth is about one hundred feet, locally often much more, and rarely less, except in the smaller valleys. Towards the lower end the width varies to about five miles, while at the upper ends they are usually much narrower.

MAP II.


MAP
showing the streams
in the vicinity of
Kingston.
Scale in miles.
0 1 2 3 4 5 6 7 8
TAMWORTH
Salmon R
Napanee R
NAPANEE
Big Cr.
Mill Cr.
Collins Cr.
Cataraqui
K. MILLS
Gt.
KINGSTON
BAY of QUINTE
R. St. Lawrence
AMHERST ISLAND
WOLFE ISLAND

Near the edge of the cuesta, the breadth is sometimes over a mile, and some of the valleys are remarkably flat-bottomed, occasionally with gneiss outcropping in the floor, the sides being limestone. Very frequently the bounding walls in the upper reaches are so steep that they are in places unscalable.

The intervalley spaces are flat topped, inclined gently southward at a less angle than the dip, and have a thin, more or less discontinuous covering of drift, rarely enough to significantly change the flat upland topography. Some few of the intervalley areas, though flat topped, are very narrow in parts, even to one hundred yards in width.

The drift blocked equivalents of these valleys are found all the way to the vicinity of Lake Simcoe and perhaps beyond. The upper reaches along the Black River cuesta, are generally occupied by streams or lakes. The Trent river, through part of its course, occupies portions of several of these. The Bay of Quinte, itself a complex, may be a member of the series.

The lower courses of all lie below the level of the first interglacial deposits, and in some cases the lowest till sheet, overlaid by some of the interglacial beds, is found within the valleys. They are thus either of glacial or of preglacial origin.

The axial direction of the drumlins in Hastings county, corroborated by the direction of striæ upon the inter-valley upland surfaces, indicates that the direction of ice flow sometimes made an angle of about fifteen degrees with the direction of these valleys. Sometimes, just at the edge of the escarpment, striations are found on a curved rock surface bending down obliquely into the valley. The best example is on Mill creek, about two miles west of Sydenham. Occasionally in the valley bottom striæ are found which are not accordant in direction with those upon the adjacent upland, but which nearly accord with the direction of the valley sides, suggesting in some cases, local oblique motion beneath the general ice stream. In other cases the direction of ice motion and that of the valley coincide. As a rule the escarpments and valley-sides, where the rock is exposed, are little, and generally not at all, scoured. On the other hand, where there is a change in the direction, and the valley is bounded by a steep rock wall, that cliff face is sometimes polished smooth on the thrust side, but not elsewhere, in one case, near Napanee, for over one hundred feet below the crest. The postglacial retreat of the escarpments has been very small in some cases, and in others nothing at all, there being no talus in some places, in others striæ rounding over the edge, or, again, the cliff presents a polished face. In one

PLATE III.

FIGURE 1.—Long Reach, on the Bay of Quinte—a drowned rock-valley.

FIGURE 2.—Benches on the lower Trent, north of Trenton.

case a broad open valley (Great Cataraqui) suddenly turns slightly and narrows to a gorge cut in granite, through which the ice has passed. In another case (Consecon creek, Prince Edward county) the present creek heads on the upland and runs southwesterly, the valley gradually broadening and deepening. About a mile to the east of its head the Bay of Quinte valley, (Plate III., fig. 1) whose depth below the upland at this place is 185 feet, cuts across at an angle of about fifty-five degrees.

These valleys are all, with the single exception of the gorge at Kingston Mills, carved out of homogeneous limestones, lying in a nearly horizontal position. Before the carving of the valleys the country must have been one of almost no relief. The adjacent region, from which the ice came, is also one of low relief. There are thus no topographic features which would cause the action of the sheet-glacier to be concentrated along certain lines which are oblique to its own general direction of motion, and there is no reason why these lines should sometimes unite into one trunk valley. The expectation is that a sheet of ice would under such circumstances tend rather to reduce than to accentuate topographic features. This was true in this area in the case of the second ice sheet, and has been shown to be true elsewhere, and therefore is not an assumption as to a method of *sheet-glacier* action. It is known that an *ice stream*, which invades a valley of subaerial erosion tends to destroy the systematic arrangement of spurs and re-entrants. That a sheet-glacier in a less confined area would tend to erode systematic valley-systems more or less athwart its course seems highly improbable.*

On the contrary, their form and adjustments are appropriate to stream erosion. Loose debris in the bottoms of the valleys near their heads, pinnacles, and isolated outliers along the valley sides, are, however, almost completely wanting. Occasionally the present stream is held back, forming a small pond, by the accumulation of a little drift debris across a portion of the valley, or by a rock obstruction. Where the tributary valleys join a main valley there is no discordance, or as Playfair puts it, there is "such a nice adjustment of their declivities, that none of them join the principal valley, either on too high or too low a level; a circumstance which would be infinitely improbable, if each of these valleys were not the work of the (predecessor of the) stream that flows in it." ('02, 102).

The fact that these valleys are broadly open towards the southwest,

*Compare with the valley of the Rhue, Davis, 1900, p. 275.

and are narrow and steep-walled towards the northeast, indicates that the streams which carved them flowed towards the southwest. These streams may have been initially consequent on a plain inclined towards the southwest, but whose inclination has since been altered by secular uplift or depression, so that the present St. Lawrence flows over the lowest portion of the sag. The direction of the streams has undoubtedly also been controlled by the direction of the master joints of the limestone, and the valleys may have been developed by headward growth of streams guided by these joints. To the writer this latter alternative seems the more probable, though additional field work is necessary before a definite opinion can be expressed. The outlet to the present St. Lawrence seems to be a complex of several of these valleys in which the water is now flowing in a reversed direction owing to secular changes in elevation.

Jointed and Fissured Uplands.—Another feature of the rock surface of the limestone uplands, found upon the intervalley ridges, along the Black River escarpment, and upon the many outliers in front of the escarpment, is the joint structure, which has split the surface layers into rhomboidal blocks of various sizes. Subsequent weathering of the upper blocks especially, has widened the fractures and rounded the edges of the blocks more or less. In some cases we find till and pieces of gneiss in these widened fractures, and in others the glacial striæ bend obliquely downwards in crossing the curved surfaces near the open fissures. Again, over wide areas of almost bare rock, the joints occur, but the blocks are close together and there is no weathering or rounding of the edges, and the striæ cross the joints without deflection. These features occur sometimes within short distances of each other on limestones that are identical in texture, and so far as known, identical in composition. They are found both at the edges of the upland and some distance back from them ; unfissured areas are sometimes found close to the edge of the escarpments.

The jointing which produced the rhomboidal blocks preceded the earliest ice advance. The relation of the ice-scoured surface to the open fissures shows the existence of these fissures before the advent of the ice which planed that surface. The low temperature of the subglacial water, and the absence of organic matter in solution, except the small amount derived from the preglacial soils, render it improbable (but not impossible) that the subglacial waters could have materially widened them. During interglacial times, at least portions of the area were below the level of standing water, and were possibly covered with ice, so that it seems very probable that much of the weathering pro-

PLATE IV.

FIGURE 1.—Blocks on the Black River cuesta, about one-third of a mile from its edge, near Stoco Lake.

FIGURE 2.—The front of the Black River cuesta, Deer Bay. Talus nearly to the crest.

cesses which opened the fissures must have operated during preglacial time. In any event ice which was capable of scouring passed over the area after the fissures were opened, removed some of the blocks from small areas, but left still larger areas with the blocks still in position, even on narrow ridges.

The topography of these uplands is in many places similar to that peculiar to a limestone region undergoing the process of subaerial degradation. A comparison with ridges in like situations in the unglaciated area of Wisconsin shows that the similarity is very striking. In central Ontario, however, there are no pinnacles nor small prominences in front of the escarpments. Many of the larger outliers still remain as such, generally each with a steep cliff and talus slope in the direction from which the thrust of the ice-sheet came. On the lee side there is a long trail of rhomboidal blocks from the rear slopes of the outlier. (Plate IV, fig. 1.)

Along the escarpments where the old cliff faced the ice thrust there is always a well defined talus slope, sometimes right up to the crest. (Plate IV., fig. 2.) When the direction of the cliff approaches parallelism with the direction of ice motion, the talus is frequently much smaller and occasionally nearly wanting. Where the valley sides are graded to the edge of the upland, loose blocks usually seem to be altogether wanting in the valleys. Whether any of the original soil cover is still in situ it is at present impossible to say. Certainly much of the present soil is imported.

Gorges and Valleys of the Niagara Cuesta.—Along the Niagara cuesta from east of the Dundas valley, described by Spencer (**'81**), to Cabot's Head on Georgian Bay, are a number of incisions transverse to the escarpment, varying from deep and narrow gorges to deep but broadly open valleys, sometimes as much as ten miles across the mouth, whose bottoms are occupied by obsequent streams flowing to the inner lowland. There seem to be three types of these valleys; first, narrow short and deep gorges, which in some cases might almost be described as hanging gorges, since they are not yet cut down to grade with respect to the rock floor in front of the escarpment. Second, narrow steep-walled gorges, which so far as known appear to be graded with reference to the frontal rock floor. Third, deep broadly open valleys, whose upper reaches may become gorges. They are graded with respect to the rock floor of the inner lowland some distance, sometimes a number of miles, away from the immediate vicinity of the escarpment.

The gorges of the first type seem to be free from drift debris, and their immature form would indicate that they are largely of postglacial origin. The second and third types are usually more or less drift filled, especially in the upper reaches of the third type. The valleys of the second type are relatively narrow and steep walled. The level to which their mouths are graded is not known. The valleys of the third type are broadly open at the point of exit from the cuesta, and some of them penetrate ten to eighteen miles back from its front. Occasionally they are indicated by topographic depressions beyond the point where the bounding rock scarps can actually be followed, though the amount of drift material upon the cuesta has usually obliterated all rock-surface features beyond the limits already mentioned. The rock-walls of each valley (except the Dundas valley as far as can be traced at present) tend to converge, but convergence to a point of union has only been demonstrated for the walls of some of them. Some have also tributary lateral gorges. Spencer has described several entering the Dundas valley. In these tributaries the walls usually unite and the present stream falls over a cliff. The tributary gorges may belong to any one of the three types.

Owen Sound, sometimes wrongly designated a fiord, Colpoy's bay, and other bays upon the Georgian Bay coast, may serve as illustrations of the type (Map III). There are, however, between Owen Sound and Burlington, a number of valleys, not submerged, and equally typical. The north shore of Manitoulin Island seems also to possess many comparable with these, but developed on Trenton and older strata.

As in the case of the rock-sided valleys at the eastern end of the area, we lack an accurate knowledge of the precise form of valley which a sheet-glacier, acting on homogeneous rocks in a region of very low relief, might possibly be capable of eroding, and of the form of escarpment-front, which it might, acting alone, produce. It is necessary then to make the partial assumption, that if the sheet-glacier were capable of producing such topographic features, the products would bear a definite relation to the direction of ice advance, and would, in homogeneous rock, assume forms less tortuous than those carved by the more mobile erosive agent, running water charged with sediment.

The direction of the valleys as a whole is entirely independent of the general direction of the ice movement, whether it be determined from the evidences out upon the lowland or from those upon the crest of the cuesta at the edges of the valleys. They lie in all positions through an angle of about 180° ; all but one (that at Dundas) in such a position

PLATE V.—VALLEYS IN THE NIAGARA CUESTA.

FIGURE 1.—View across the unsubmerged valley of the Bighead river. Cape Rich in the right background. Looking west.

FIGURE 2.—View across a portion of the unsubmerged valley of the Beaver river. Blue Mountains in the distance. Looking east.

FIGURE 3.—Fisher's Gully, a tributary of the Dundas valley, showing systematic arrangement of spurs and reentrants.

that any water which formerly flowed through them must have reached the lowland in front of the cuesta. In many of them the rock scarps which form their sides show no evidences of glacial action. Had the ice advanced up or down them we would expect to find ascending or descending glacial striæ. In places there is a systematic arrangement of alternate spurs and re-entrants, producing a tortuous channel, eminently characteristic of stream erosion, but, if we may judge from existing examples elsewhere, such as no ice stream could have passed through. (Compare with the valley of the Rhue, Davis, **1900**, 275.)

The Owen Sound valley, and several others along the Georgian Bay shore, both northwest and southeast of this, in their lower reaches, flare broadly open towards the direction of the ice advance. Striæ show that in part they controlled the direction of the ice motion, diverting it, in the Owen Sound case, about fifteen degrees to the east of its general direction. This broadly open portion of the valley was certainly modified by the ice. Along the eastern side of Owen Sound, and similarly in some of the other embayments in the escarpment, there are spurs which have not been removed, while upon the western sides, which received the thrust of the ice, the escarpment presents a much more even face.

North of Owen Sound in Colpoy's bay, and between Lion's Head and Cape Croker, there are a complicated series of channels, irregular bays, and islands in front of the escarpment. The different channels bear no definite relation to the direction of the ice movement in adjacent regions, some being even transverse to it. There is no evidence of discordance where the smaller side channels join the principal channel.

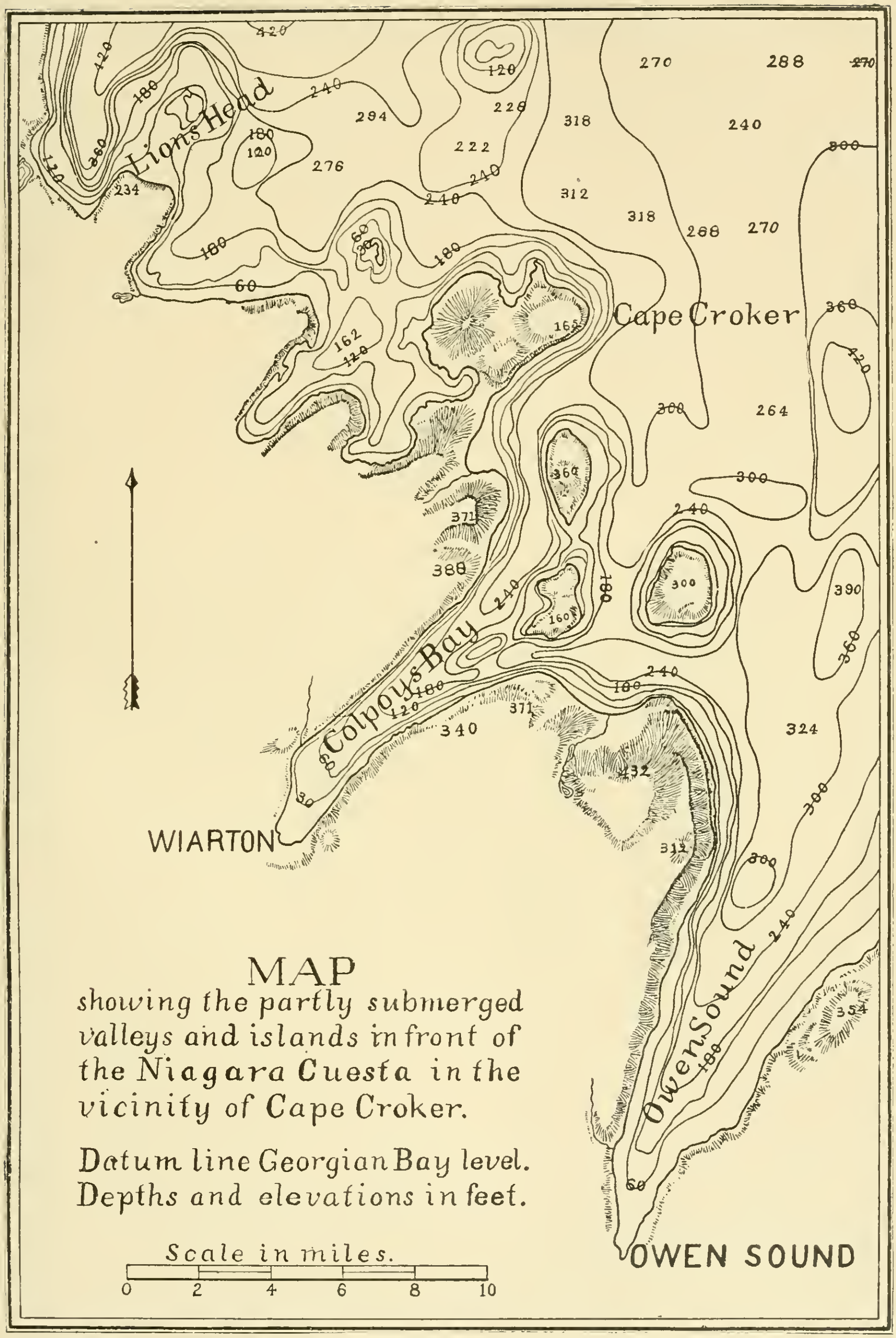
Between Owen Sound and Collingwood there are two unsubmerged sinuses extending far inland. Through one of these the Bighead river enters Georgian Bay at Meaford. The other, which reaches back for more than fifteen miles, over eight miles in breadth at the mouth, and about 1,000 feet in depth, is now the valley of the Beaver river, which enters the bay at Thornbury (Plate V, figs. 1 and 2). Between Collingwood and Hamilton there are a number of similar valleys. The most important of these are those now occupied by the Noisy, Mad, Nottawa, Nottawasaga and Credit rivers, Sixteen-Mile creek and Twelve-Mile creek. A branch of this latter heads on the outlier west of Milton, and through its upper course passes between it and the main escarpment. The largest of all the valleys is that at Dundas, described by Spencer (**'81**). (Plate V, fig. 3.)

In most of these the mouth of the valley is more or less drift-encumbered, but it can be shown in several cases at least that they are graded with respect to some level lower than that of the Medina sediments immediately in front of the escarpment. This is definitely proven for those which lie northwest of Collingwood, for the Dundas valley, and for some others north of Burlington.

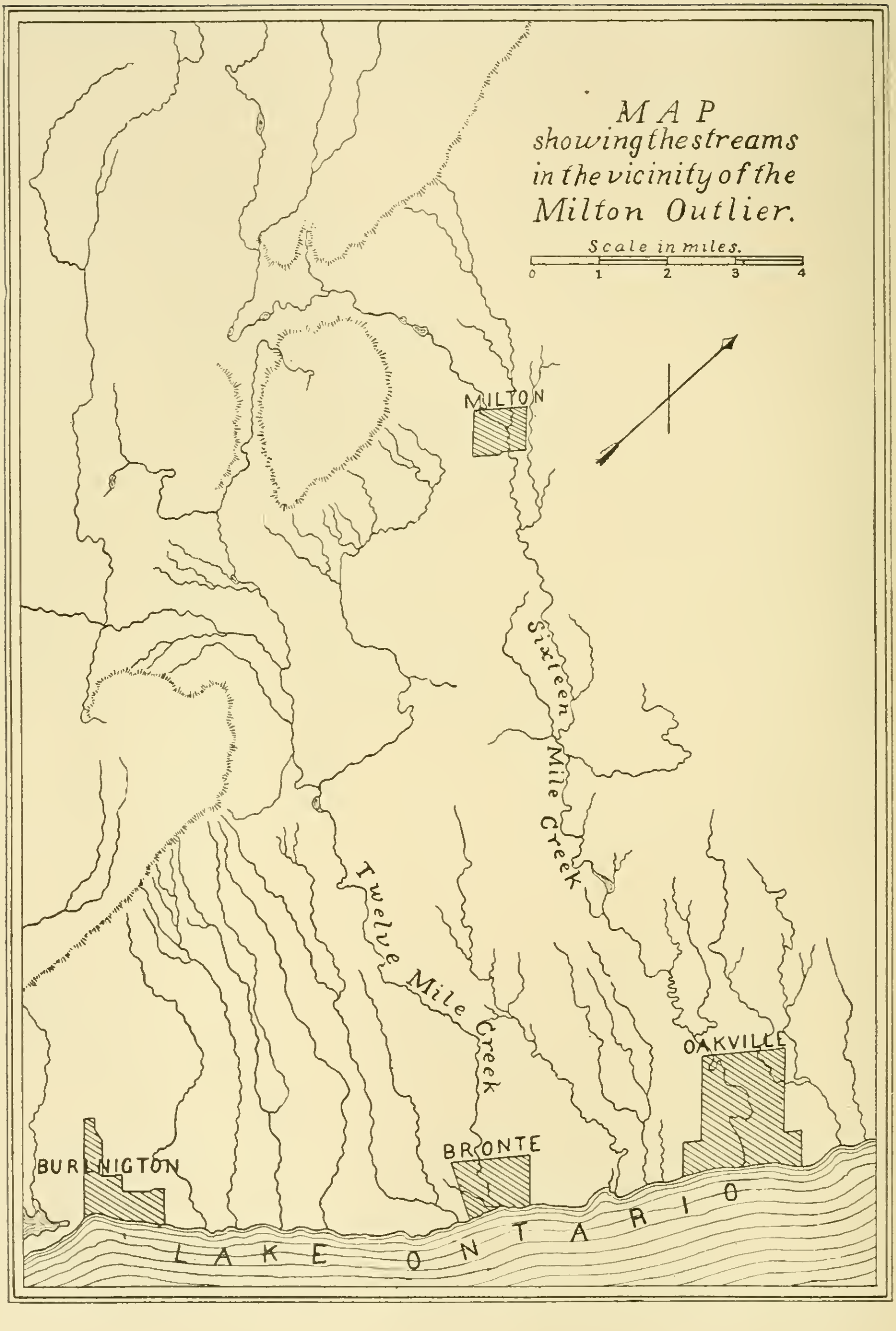
Hence, the systematic form of each, their direction independent of the ice movement, and other features cited, render it very improbable that they are due to glacial erosion. On the contrary, they may all except the Dundas valley, be regarded as due to the development of obsequent drainage, tributary to some master stream or streams running along the inner lowland. Some of them are, in their lower courses, occupied by till, which in some cases is, and in others probably is, that of the lowest till sheet; many of them are graded to a level on the rock floor, which must have been deeply submerged at the time of the deposition of the lowest interglacial beds. In the Dundas valley some stratified deposits are found overlying the till. The similiarity of form and development of the valleys whose relations to the lowest till sheet and to the interglacial beds has been proven, to those in which the relations are unknown, because not worked out, renders it probable that none of them are of interglacial origin. It is possible, though very doubtful, that the upper reaches of some of them may have developed during interglacial time.

Islands and Outliers.—In Lake Ontario towards the eastern end, and extending as far west as Presqu' Isle, are a great many large and small limestone islands and shoals, all lying north of the line between Stony Point and Point Peter. Gull Island, four miles west of Cobourg, is also a limestone island. In the northern part of Lake Huron, between Cape Hurd and Grand Manitoulin Island, are a number of small rock islands. Some of these are of rock fragments at water level, the bed rock not being visible, but the large majority are composed of bed rock in situ. The Manitoulin Islands are rock islands. In Georgian Bay many of the islands are of limestone rock—attention will be specially called to those along the Bruce peninsula (Map III). In Lake Simcoe, along the east side, there are a few islands with limestone bases. Many of these islands are unsubmerged portions of the higher irregularities of the series of escarpments. Some of them lie in front of the main escarpments, as indicated by soundings around them.

On the Central Ontario lowland in Halton county, just west of Milton, is an outlier, capped by Niagara limestone, severed completely


Lions Head
Cape Croker
Colpoys Bay
Owen Sound
WIARTON
OWEN SOUND
MAP
showing the partly submerged valleys and islands in front of the Niagara Cuesta in the vicinity of Cape Croker.
Datum line Georgian Bay level.
Depths and elevations in feet.
Scale in miles.
0 2 4 6 8 10

MAP IV.


MAP
showing the streams
in the vicinity of the
Milton Outlier.
Scale in miles.
0 1 2 3 4
MILTON
Sixteen Mile Creek
Twelve Mile Creek
OAKVILLE
BRONTE
BURLNIGTON
LAKE ONTARIO

from the main escarpment, having a surface area of about four square miles (Map IV). Were the land under water this would form a large island, comparable to some of the islands already noted along the Bruce peninsula. Similar outliers may occur elsewhere along the line of the escarpment. In some other localities in front of the cuesta, outliers, capped with Medina sediments and surrounded by drift-filled valleys, were noted, suggesting a similarity to the Milton outlier, but being further out on the lowland, they had perhaps undergone greater degradation.

In front of the Black River escarpment, notably in Hastings county, are a number of outliers of limestone, with much jointed and fissured upper beds. Some few of the outliers are of sandstone. If the region were partly submerged these also would form islands in front of the escarpment. As already noted, many of these outliers present a steep face, with a talus slope at the base, towards the direction of the ice advance, and a long trail of loose blocks on the lee side.

Some of the islands and outliers were certainly in areas protected from ice erosion. The case of the Manitoulin Islands cannot be considered, as the writer has not sufficient personal knowledge of the facts. In the great majority of cases, however, they do not occupy such protected places, and there is direct evidence that the ice transgressed them. Their relation to the escarpments, and the effects which have been produced by the ice, seem clearly to indicate that they had an existence prior to the advance of the ice sheet. The more salient features were smoothed off, but the essential features are still preserved.

Depth of Excavation.—Another interesting fact is the remarkable uniformity in the depth of excavation of the lowland below the crest of the Niagara cuesta. At Cabot's Head the depth is about 800 feet, at Collingwood 1,100, near Dundas 1,000.* The unsubmerged portion of the Ontario lowland is located on rocks ranging from the Trenton to the Medina; the submerged portion is on Trenton in both cases. The lowland has thus been excavated on rocks of four different horizons, and of very diverse texture.

Lowland Rock-Surface.—An almost continuous transverse section of the rock surface of the lowland is shown along the north shore of Lake Ontario, parallel to the Dundas valley, from Hamilton to Lorne Park (twenty-five miles). Between here and the river Rouge (thirty miles)

*743 measured, 1,000 calculated, Spencer, '81, 323.

there are many exposures in the valleys of creeks and rivers, and in a few places along the shore. It is thus known that there is no extensive discordant deepening for forty miles east of Burlington Heights. The lowest part of this section, with reference to the present lake level, is situated on Lorraine shales and sandstones. The surface is lightly rolling, but the average elevation toward the escarpment is about one foot per mile. At right angles to the lake shore it varies to about fifteen feet per mile.

Between the Rouge and Whitby (thirteen miles), there is no known rock exposure. At Whitby and near Bowmanville the Utica shales come to the surface; between these two points there is possibly a valley twelve miles in breadth, but probably of no great depth. For twenty miles east of Bowmanville, to Gull Island (three miles east of Port Hope) the rock, Trenton limestone, is again concealed. From Gull Island to Presqu' Isle (twenty miles), there are a number of exposures of Trenton limestone. East of Presqu' Isle the rock is continuous to below Kingston.

Between the Rouge and Presqu' Isle the upper edge of the lowest till sheet seldom sinks below the water line. Were there any very deep or cañon-like depression of the rock surface the till might reasonably be expected to give some indication of the existence of such depression, for in every case within the area, where such depressions are known to occur, the till sheets above would give ample evidence by their accordant depressions.

Along the Georgian Bay unsubmerged portions of the old valleys are in some cases over 1,000 feet below the escarpment, and are graded with reference to a level still lower. So far as is at present known there is no evidence of discordant deepening due to the movement of the ice along the front of the escarpment in a direction different from that of the general movement; if such deepening has taken place it is not located on the soft Medina strata, but on the Lorraine, which are known to form escarpments, or upon the Trenton. In no case, so far as the writer is aware, has drift from a higher geological horizon been found overlying a lower horizon, well out on the lowland, a result which must obtain if there has been significant lateral motion of the ice from the Georgian Bay region.

Summary.—The work of the ice sheet in Central Ontario seems to have been that of smoothing off pinnacles, small spurs, and other outlying features of the limestone areas. Only the larger of these topographic forms were able to resist the ice, and these, more or less modified, have remained to form islands or outliers in front of the different escarpments, or the spurs of the intervalley ridges along the valley sides. The essential features of the topography are not destroyed, though they are more or less completely obscured and obstructed by drift.

The relation of the area to the fronts of the ice sheets which crossed it is not yet determined. The results of the writer's studies at present suggest that the great moraine of Central Ontario is largely an interlobate moraine between an ice lobe coming from the east of north, and a lobe coming from the east ; and that the lateral spurs, on the north and on the south sides of the great moraine, represent the positions successively occupied by the retreating ice front.* The area seems to have been almost always one receiving deposits rather than one from which the soil and rock was being removed.

The streams which produced the pre-glacial valleys throughout the Central Ontario lowland, and the obsequent streams of the Niagara cuesta must have been tributary to some trunk stream, or perhaps to two such master subsequents. The location of these trunk streams would normally be along the lines of deepest cutting. Their direction of flow cannot be determined at present, though that of the tributaries is known from the forms of the valleys. Those on the Black River cuesta flowed southwest, those from the Niagara cuesta northeast, east, and southeast. Obviously the trunk stream, though flowing parallel to the escarpment, must have had some outlet from the region. Determining the location of this valley has been one of the chief difficulties to be met by the river-erosion hypothesis for the origin of the basin of Lake Ontario. The attitude with respect to the present St. Lawrence valley, and certain other features of the rock valleys in the vicinity of Kingston, and the immature character of the present St. Lawrence channel render it extremely improbable that the waste from the lowland was ever carried out through this channel. If the drainage of the Ontario lowland was that of a normally developed river lowland there is but one known outlet which is at all suitable, that by the Dundas valley.† The course of the valley from the vicinity of Copetown westward is highly problematic. Spencer considers that it was towards the south, while Grabau (**1901**) has recently advocated an extension towards the west, in continuation of an initial consequent direction. The direction of flow of the streams that occupied this valley has not

*See Chamberlin, '**95**a, p. 768.

†This suggestion had occurred to the writer before he was aware of Dr. Grabau's opinions, referred to below.

been definitely determined. A river flowing westerly through this outlet would be a normal consequent stream, and tributary streams from both sides would occupy the position of normally developed subsequents. The attitude of the broadly open valleys along the Georgian Bay suggest that there may have been a second master stream with an outlet southwestward from the bay. At present our knowledge is so imperfect that the direction of flow of these master streams, and their relations to these different valleys, which may be members of a normally developed system, have not been determined.

The probability that there were streams on the Central Ontario lowland, to which the streams in the preglacial valleys, already described, were tributary, makes it equally probable that similar features were developed to the southeast along the basin of the present lake. At present we know neither the depth to the rock floor of the basin, nor the amount of drift filling. The relation of the basin to the ice lobes is also unknown. Hence differential deepening, which has not operated on the unsubmerged lowland, may perhaps have been in effective operation in the portion of the basin east of the Niagara river, and west of Stony Point.

PLEISTOCENE HISTORY—*A Summary.*—The Pleistocene deposits of Central Ontario present a complex which has not yet been studied in sufficient detail to warrant more than a brief reference to certain salient features. The best known locality is that in the vicinity of Toronto, where the order of succession of the deposits has been established. The probable relations of these deposits to similar beds elsewhere in the area have already been noted. Mention has also been made of certain sands and gravels which overlie the third till sheet in some parts of the area. The fossils of the lowest group of interglacial beds at Toronto indicate that the climate of that part of the region was, for a time, warm and temperate, perhaps like that of Ohio. During this period the lake was connected with the Mississippi drainage, a connection which may have been an inheritance from the cycle preceding the first ice advance. Whether the ice sheet at this time had withdrawn wholly from the region, or only part way, must at present be a matter of conjecture. The fossils of the upper beds of the first interglacial deposits indicate climatic conditions approaching those of the lower Gulf of St. Lawrence and the Labrador coast at the present day. The close of the interglacial period was followed by an interval during which there was a considerable amount of erosion, just how extensive is not determined. The interglacial beds of the latter epochs have, as yet, been little investigated.

PLATE VI.—PLEISTOCENE LAKE BENCHES.

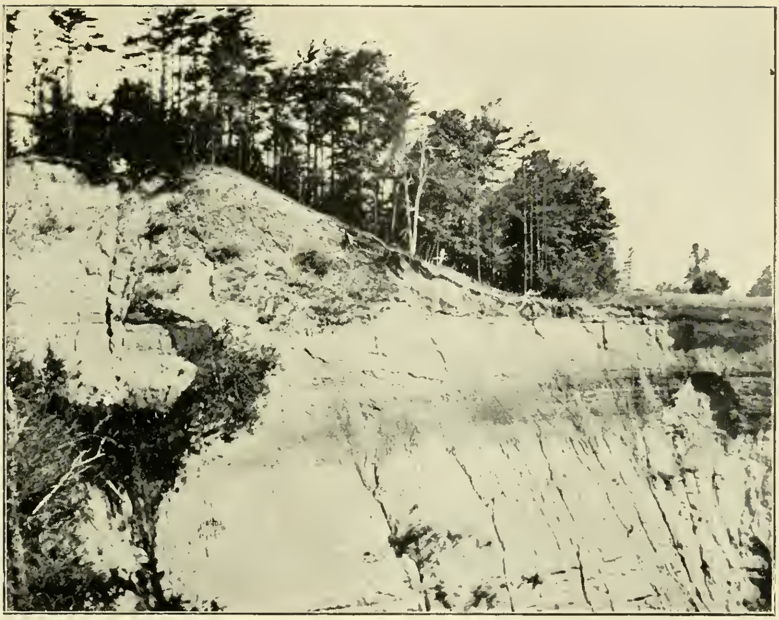


FIGURE 1.—Transverse section of the Iroquois bench and sea-cliff, Scarboro' Bluffs.

FIGURE 2.—Iroquois bench and sea-cliff, and light morainic topography of the third till sheet, Scarboro' Bluffs.

PLATE VII.—PLEISTOCENE LAKE BENCHES.

FIGURE 1.—Iroquois bench and sea-cliff, Scarboro' Bluffs.

FIGURE 2.—Boulder pavement in front of a sea-cliff, near Lake Simcoe and south of Orillia.

While the ice sheet was retreating across Ontario, a series of lakes were formed between its front and the highlands to the south and west. In the latter stages of the ice retreat, portions of the present land area of Central Ontario were beneath the waters of these lakes. The land was being gradually elevated at the northeastern end, so that at present the old shores are not parallel with the surfaces of the existing lakes. The deposits of the different periods of ice transgression and retreat have been so little studied, and so little differentiation seems to have been made between the deposits of sands and gravels of these periods, and those formed by their re-arrangement during the periods of the great Pleistocene lakes, that at present there is much confusion with regard to the history of the area during the Great Lakes epoch. (Plates VI and VII.)

RECENT HISTORY—*A Summary*.—Since the withdrawal of the Pleistocene lakes the amount of erosion has been small. The courses of the present streams are in part determined by the valleys of the preglacial rivers, in part by the position assumed by the drift deposits with respect to the retreating ice sheets, and in part to the controls exercised by the Pleistocene-lake beach-deposits. There is at least one lake (Scugog) whose drainage seems to have been affected by the differential uplift indicated by the present attitude of the old lake beaches.*

Some of the streams have cut through the glacial deposits into the bed rock. Streams entering Lake Ontario west of Toronto, or flowing into the Georgian Bay, have cut deep steep-sided ravines and valleys through drift and shale. Some few, in the vicinity of Oakville, have cut deep straight-sided, flat-bottomed valleys through about forty feet of drift and eighty-five feet of shale.† The present streams meander in courses largely independent of their valley sides, here truncating an old spur, there widening the former meander belt. Sometimes there are two or three back meanders between adjacent spurs of the old valley. In the upper courses, where the stream is still working upon glacial debris, these misfit meanders are especially common. In the great majority of cases there seems to be but one terrace below the general level of the area adjacent to the valley.

North and east of Toronto, the Trent, the Moira, and a few smaller streams, have in part cut new channels in Trenton limestones. The channels, which average perhaps twenty-five feet in depth, are straight-

*This may be true of Pigeon and Chemong lakes also.

†In one case 400 yards in width.

sided and flat-bottomed. In almost all of these the river breaks into rapids, and occasionally plunges over a low fall (Plate VIII). In parts of the lower course of the Trent there are two rock terraces, one a small rock-cut bench, the other due to the removal of the drift debris from the old rock surface. There is reason to think that in parts of the course there are remnants of yet higher terraces upon the drift, but they are not conspicuous topographic features (Plate III, fig. 2, p. 171).

The relations of all of these terraces to the Pleistocene lake levels and to the former water supply are interesting problems which have not been considered. The present valleys are inappropriate in size and form to the present streams in flood.

Parts of the present valleys of these streams and their tributaries, and the valleys of all streams east of the Moira, are rock-valleys, not of recent origin, and have already been described under the caption "Eastern Rock-Valleys."

Along Lake Ontario the waves have cut benches and sea cliffs in the drift deposits. The longshore action is distributing the material, thus derived, east and west from the vicinity of Whitby, forming bars, spits and hooks. Towards the west the most important of these are Toronto Island and Burlington Beach. Towards the east, from Presqu' Isle neck to Point Peter, there are a great many bars blocking the ends of partly submerged rock valleys, and forming large and small lakes. Back from some of these bars, small sand-dune belts have formed.

PLATE VIII.—The young channel of the Trent river below Fenelon Falls.

—Photo. by J. H. Stanton, Fenelon Falls.

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OBSERVATIONS ON BLOOD PRESSURE.

WITH SPECIAL REFERENCE TO CHLOROFORM.

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(Read 27th April, 1901).

INTRODUCTION.

THIS paper contains the results of work which I have conducted during the past three years in the Physiological Department of Toronto University. A grant of money was made by the Scientific Grants Committee of the British Medical Association towards defraying the expenses, and is here gratefully acknowledged.

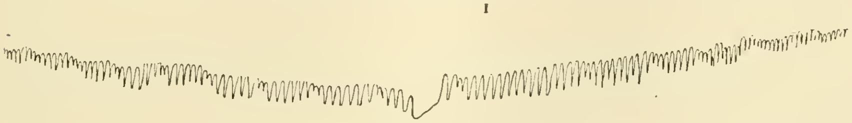
The work has been of a somewhat intermittent character, owing chiefly to the difficulty in obtaining a steady supply of animals. The kymograph used was a Ludwig one, with a glass pen writing in ink upon white paper. The tracings thus obtained were very long, as often experiments extended over several hours, and only short pieces of them are able to be used here to bring out the points mentioned in the text.

I have as far as possible avoided theorizing, being rather content to state the results which were actually obtained under given conditions ; a certain amount of speculation is occasionally inevitable however.

I am greatly indebted to Professor A. B. Macallum for much valuable advice, and to Mr. Scott, D.Sc., for constant assistance in the carrying out of the experiments.

Before actually passing on to discuss alterations in blood pressure produced by definite causes, one should mention that occasionally strange falls in the blood pressure of dogs occur without any apparent cause. If these were not noted they might be wrongly interpreted.

I



TRACING I.—9/38.—Dog under Morphia. No Chloroform for several minutes. This fall in blood pressure occurred without apparent cause and gradually disappeared.

Tracing 1 shows one of these vagaries. The animal, a mongrel spaniel, weighing about 40 lbs., had been given three-quarters of a grain of morphia an hour before the experiment began. It then was chloroformed. After it was completely insensible and for several minutes had not been taking any more of the anæsthetic, this fall in pressure occurred. It looked somewhat like that produced by irritation of the vagus nerve, and it was suggested that the canula in the left carotid might be producing this, but nothing was altered and yet the pressure recovered of itself and the fall did not recur during the two-hour experiment.

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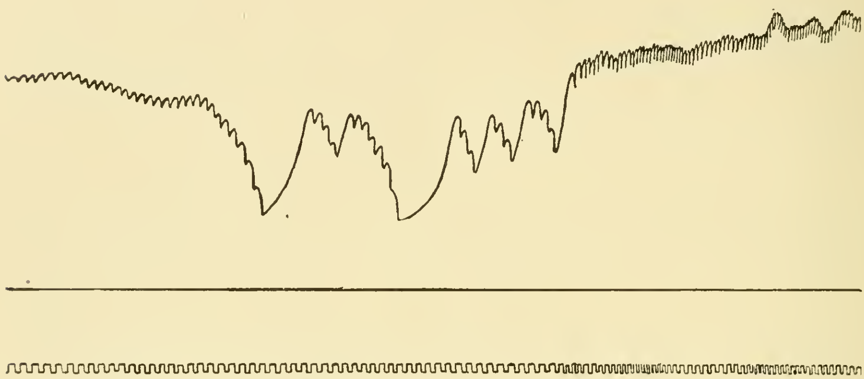


TRACING II.—3/7.—Dog under Morphia. No Chloroform for several minutes. Shows marked fall in pressure with slowing of pulse. Animal horizontal. Cause of fall not apparent and it was spontaneously recovered from.

Tracing 2 shows another fall occurring under similar circumstances to the last. The animal had not had any chloroform for several minutes. In this case the fall is even more marked, and looks extremely like that produced by irritation of the vagus, but it completely disappeared without any of the factors having been altered, and did not recur.

Such falls as these, when they happen to occur during the actual administration of chloroform, may be the unexpected ones described by the Glasgow Commission on Chloroform, and which Lieut.-Col. Lawrie ascribed to asphyxia ; we will refer to this point later on.

It was frequently noted how much different dogs varied as regards the amount of their blood pressure and pulse rate. Often poorly-nourished and small varieties of dogs had high pressures, whilst large, well-nourished animals had the opposite.

The Normal Effects of Gravity.

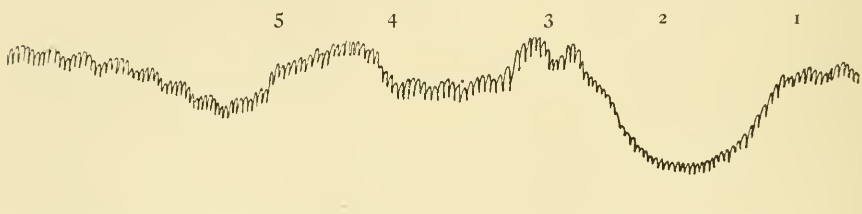
It has long been known clinically that the circulation of the blood is affected by the posture of the body, and that in weakened states of the circulation the blood and other fluids, obeying ordinary hydrostatic laws, tend to accumulate in the most dependent parts of the body. Further, it had been noted that if the posture of the body be *suddenly* altered, *e.g.*, if a person be raised from the horizontal to the erect posture, the inertia of the blood tends to make it lag, and as a result the upper pole of the body, including the brain, temporarily becomes more or less bloodless, and in consequence the individual may actually faint. This method of inducing insensibility was actually employed by a Parisian surgeon, who then operated upon his patient, rendered thus insensible to pain. George Hayem[1] writes as follows: " Phlebotomists had known for a long time the influence of position on the production of syncope when Piorry instituted his experiments on the subject. He bled some dogs upright, and they fell at the end of a certain time into a sort of state of dissolution with suspension of respiration. He stopped the hæmorrhage then and placed the animal head downwards, and immediately it breathed again. Often the same experiment could be repeated many times on the same dog. I repeated the experiments in 1880 and found them to be perfectly correct." It has constantly been observed, moreover, that some individuals are much more susceptible to such changes of posture than others, and that almost any person if temporarily weakened in any manner, will tend to suffer from dizziness or even faintness on suddenly assuming the erect posture.

The effects of different postures and of sudden and gradual alterations of these, have of late years been studied more accurately in animals by means of tracings of the blood pressure. The very beautiful tracings put on record by the 2nd Hyderabad Commission on Chloroform are examples of these observations, and more recently Mr. Leonard Hill has added some of the same nature. My experiments were done on dogs, with the exception of a few upon cats, and they confirm largely what has already been observed and recorded. In each case the animal was secured in a trough which was so constructed that it could be swung into any angle with the horizon, with the canula in the artery always remaining in the axis of rotation. The canula was further carefully kept at the same level as the manometer. Unless for some special reason the canula was always placed in the proximal end of the left

1 "Death from Hæmorrhage," Archive de Physiologie, 1888, p. 102.

carotid artery. The animals were all under the influence of some anæsthetic, the nature of which is mentioned when necessary; but as the effects of posture were more or less the same in all cases, it may be inferred that they were not due to the anæsthetic, except as regards degree. As a rule some morphia was first given hypodermically, and then very little chloroform sufficed to keep them quiet.

Tracing 3 is from a dog under chloroform. Where the tracing begins he is horizontal, and the canula is in the carotid. At 1 the hind feet were suddenly lowered. and the pressure is seen to markedly fall. When placed horizontal again at 2 the pressure rapidly rose and went above the normal for a short time, 3, and then resumed the normal line. This is a very constant feature in such tracings, and is probably due partly to inertia, and partly to the compensation which has been taking place



TRACING III.—1/3.—Normal effects of gravity on carotid blood pressure. Dog under Chloroform. 1 Animal placed vertical (feet downwards). 2 Horizontal again. 3 Compensation continuing. 4 Head downwards. 5 Horizontal again.

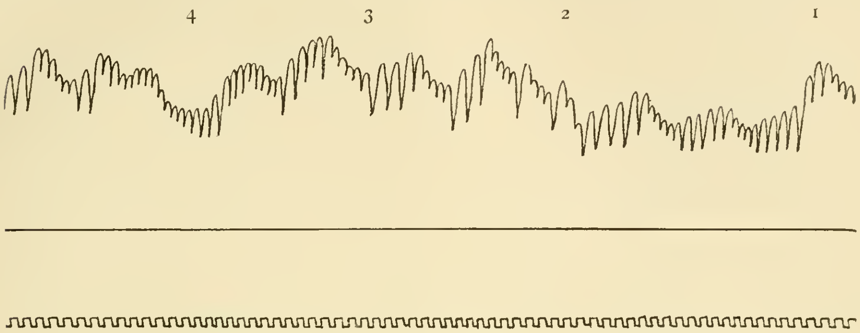
against the effect of the vertical position still continuing after the animal is again horizontal. This compensation, as we shall see, is partly affected by increase in the rate of the heart-beat and partly by constriction of the arteries, while contraction of the abdominal wall is also of service here.[1] At 4 the animal was suddenly placed in the vertical feet-up posture, and at once the carotid pressure rose. This rise was not so great as the fall which occurred in the feet-down posture, and a rule may be deduced that *the lowering of a pole of the body does not raise the arterial blood pressure in it so much as raising that pole lowers it.*

When placed horizontal at 5 the pressure fell below the normal line for the same reasons that it rose above that line at 3.

Some dogs are not nearly so susceptible to the effects of gravity as

1 "Influence of Gravity on the Circulation." Hill and Barnard. Journal of Physiology, Vol. XXI., 1897.

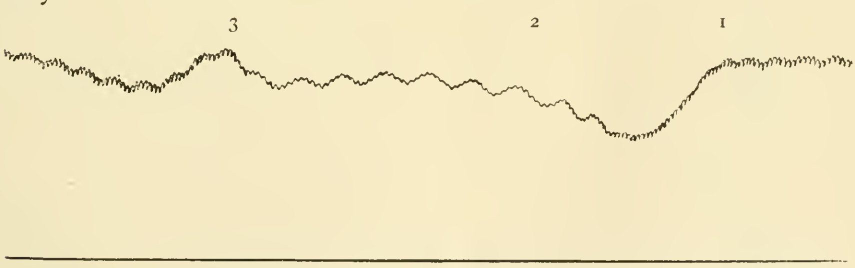
others. Thus, Tracing 4 is from a dog under morphia and chloroform just as in the previous experiment, and yet the change in pressure in the different postures is very slight. I found that cats are especially



TRACING IV.—9/22.—Dog under Morphia and Chloroform. 1 Vertically feet downwards, 2 Horizontal. 3 Head downwards. 4 Horizontal.

immune to the effects of change of posture; and Mr. Leonard Hill records[1] that some animals, *e.g.*, certain species of monkeys, actually *over*-compensate, and thus have a higher carotid blood pressure in the feet-down than in the normal posture.

Most animals compensate somewhat after a short time for the vertical feet-down position. As one might expect, animals that habitually or frequently assume the vertical posture are better able to compensate against any fall in blood pressure in this posture than are animals which do not naturally become vertical. Thus apes and domestic fowls compensate well, while snakes and hutch rabbits compensate badly.[2]



TRACING V.—9/19.—1 Vertically feet downwards. 2 Compensation occurring for vertical posture. 3 Horizontal again.

Tracing 5 shows such compensation. At 1 the animal was placed vertical, and at 2, though still in the vertical posture, the blood pressure

1 "The Cerebral Circulation," page 88.

2 Leonard Hill, "Further Observations on the Influence of Gravity on the Circulation," Supplement to the Journal of Physiology, Vol. 23, Feb. 27th, 1899.

TRACING VI.—9/28.—Canula in carotid. 1 Feet down. 2 Horizontal. 3 Head down. Compensation for this posture shown. 4 Horizontal.

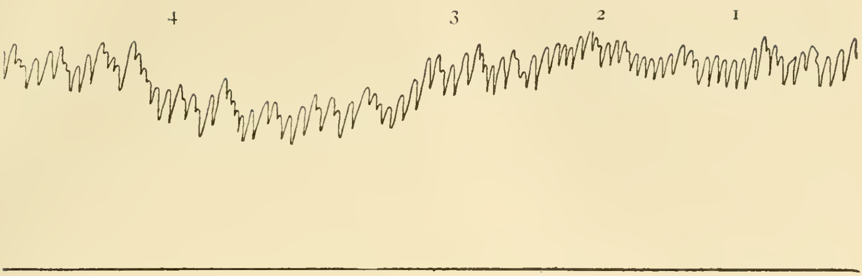
is seen gradually to right itself. At 3, on the animal being placed horizontal, the pressure went for a moment above the normal line, the compensation still continuing.

But if the vertical posture be maintained for a long time this compensation will fail, as shown by Mr. Hill on certain animals[1], and as often shown clinically when people faint from long standing. The swelling of horses' legs from long standing is another familiar illustration of the same thing.

Tracing 6 shows compensation for the feet-up posture. At 3 the feet were raised and the pressure in the carotid temporarily rose, but although the animal was kept in this position it soon fell to the normal line, and when the animal was placed horizontal at 4, the compensation continuing for a while, the pressure actually went below the normal.

When the canula is placed in the proximal end of another artery, *e.g.*, the femoral, as in Tracing 7, the same phenomena are observed. Then, of course, the vertical feet-up posture causes a fall in pressure, and the vertical feet-down a rise. This tracing illustrates well again the rule that the rise produced in the pressure in the vessels of a pole of the body by lowering that pole is not so great as the fall produced by raising it.

1 Ibid.



TRACING VII.—9/26.—Canula in horizontal end of femoral artery. 1 Feet down. 2 Horizontal. 3 Feet up. 4 Horizontal.

The effects of posture are less marked in the distal end of a divided artery than in the proximal. Tracing 8 is taken from the distal end of the femoral artery. In it the respiratory waves appear, and the whole pressure is considerably lower than in the proximal end of the same

TRACING VIII.—9/22.—Canula in distal end of femoral artery. 1 Horizontal. 2 Head downwards. 3 Horizontal.

artery. When the hind feet are lowered the pressure is scarcely altered (before tracing begins), but when they are raised the pressure slowly falls somewhat. This is another illustration of the rule above mentioned.

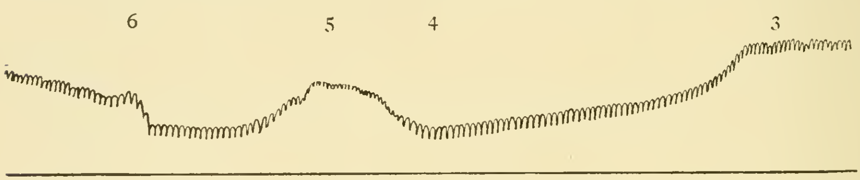
TRACING IX.—1/5.—Canula in proximal end of splenic artery. 12 Vertically feet down. 13 Horizontal. 14 Feet up. 15 Horizontal.

Tracings taken from the proximal end of the splenic artery show a marked fall in the feet-down position, and a slight fall in the feet-up. Thus the pressure falls in both vertical postures, but especially in the feet-down one. Tracing 9 is taken from the proximal end of the splenic artery. At 12 the feet were lowered, and after a short hesitation the pressure fell markedly. At 13 the animal was replaced in the horizontal position. At 14 it was swung into the vertical feet-up position, and a slight fall occurred.

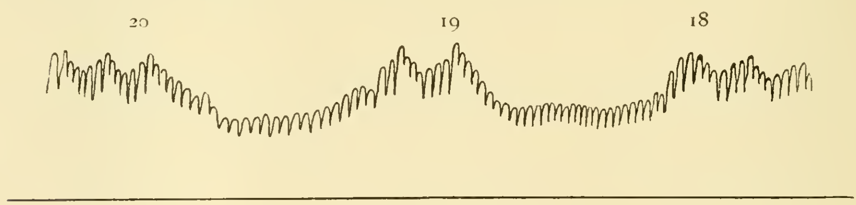
So much for the changes in pressure uninfluenced as far as possible by anything except posture. I next proceed to the effects of certain factors in increasing and decreasing the blood pressure, and its susceptibility to gravity and inertia.

ABDOMINAL PRESSURE.

Tracings 10 and 11 show the effect of firm abdominal pressure in raising the blood pressure while this is low from the animal being in the vertical posture. In 11 the vertical position assumed at 18 produced a slight fall, and abdominal pressure applied at 19 raised the tracing to



TRACING X.—3/10.—3 Vertical. Pressure continued to fall. 4 Abdominal pressure. 5 A pressure removed. 6 Horizontal.



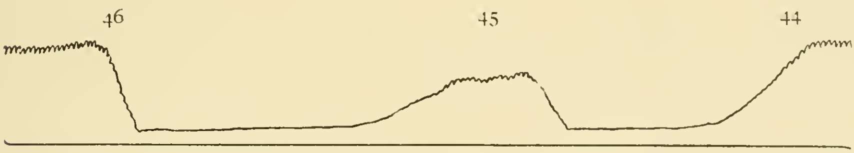
TRACING XI.—1/2.—18 Vertical. 19 Abdominal pressure. 20 Horizontal.

even above the normal line. In the former tracing the pulse was hastened by the abdominal pressure, in the latter it remained unaltered.

Abdominal pressure, however, in order to be effectual, must be of an exceedingly firm nature, and probably pressure upon the aorta itself has something to do with the result. It has been shown indeed that when the aorta is compressed by itself a marked rise in the general blood pressure occurs[1]. Far more abdominal pressure is necessary than would be required merely to empty the abdominal veins—this being the usual explanation of how the resulting rise in blood pressure is brought about.

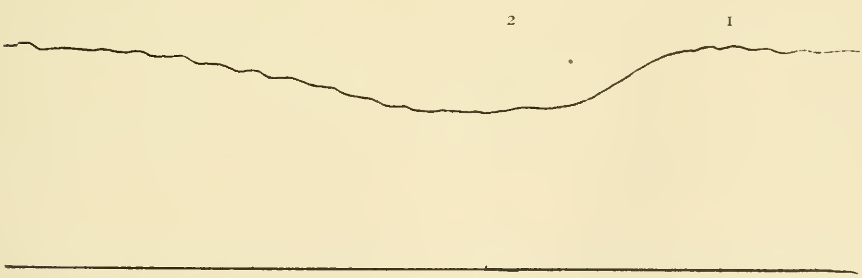
1 J. A. McWilliams, British Medical Journal, Vol. II., 1890, p. 835.

Hill and Barnard give a tracing[1] showing the effect of abdominal pressure. In it the pressure actually goes above the normal line, as it also did in tracing 11 just given, and in order to get such a rise a large amount of abdominal pressure must have been applied, and probably the aorta was more or less completely obstructed by this. I tried the tightening of an abdominal bandage as described by Mr. Leonard Hill,[2] but got almost negative results. Consequently I believe that slight supporting of the abdominal walls by bandages can have little or no effect upon the general blood pressure, and the comfort given by such to a certain class of patients cannot be attributed to any appreciable rise of blood pressure. One point, however, must be here borne in mind, and that is that while in healthy animals the abdominal wall is normal, in certain individuals it is abnormally flaccid.



TRACING XII.—1/3.—Spinal cord previously divided at level of last dorsal vertebra. 44 Vertical. 45 Abdominal pressure. 46 Horizontal.

Tracing 12 shows the effect of abdominal pressure on a vertical animal in which the blood pressure was standing almost at zero on account of previous division of the spinal cord.



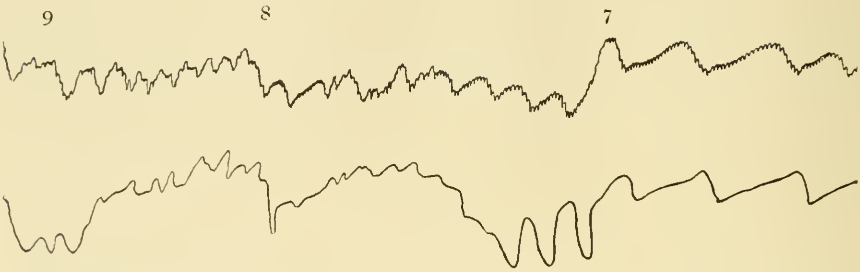
TRACING XIII.—9/20.—Canula in distal end of fermoral artery. 1 Abdominal pressure. 2 Pressure removed.

Tracing 13 shows the effect of abdominal pressure on the blood pressure in the distal end of the femoral artery. The animal being horizontal, as seen the pressure slowly fell, which goes to prove that, as

1 Tracing 86, Journal of Physiology, Vol. XXI., 1897.

2 "The Cerebral Circulation," p. 100.

previously stated, the aorta must be compressed during firm abdominal pressure.

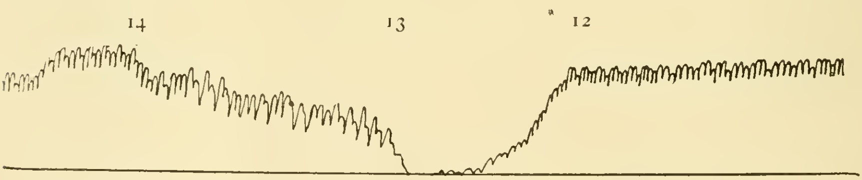


TRACING XIV.—1/3.—Dog under Atropin. 7 Vertical. 8 Struggling. 9 Horizontal. Lower line is respiratory tracing.

Tracing 14 shows the effect of posture on an animal in which the vagi had been paralysed by atropine. At 7 it was placed vertical, with very little fall in pressure. The effects are practically the same as when the vagi are intact.

Division of the Spinal Cord at various levels.

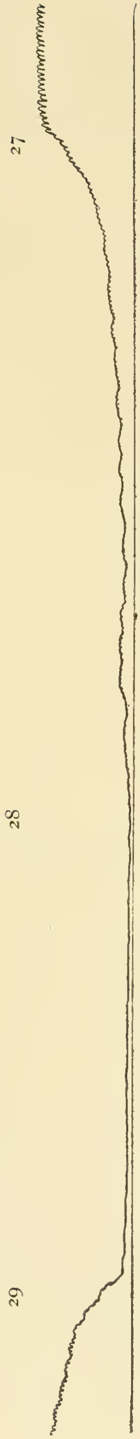
This always produces a great lessening in the compensation for the feet-down position.



TRACING XV.—3/10.—Spinal cord previously divided opposite 8th dorsal vertebra. 12 Vertical. 13 Horizontal. 14 Head down.

Tracing 15 illustrates this point. The cord had been divided opposite the eighth dorsal vertebra, and the pressure in the horizontal position fell somewhat. When the animal was placed in the vertical feet-down posture at 12, the pressure fell rapidly and threatened to actually become negative. The pulse did not hasten, in fact, slowed somewhat. The pressure rose about the ordinary amount in the feet-up posture, as shown at 14.

Tracing 16 shows the carotid blood pressure occurring in a dog in which the spinal cord had been divided near the eleventh dorsal vertebra. In this case the pulse increased greatly in rapidity as the pressure fell on the animal being placed vertical at 27. The animal was kept in the feet-down position for some minutes without any appearance of compensation occurring, and was placed horizontal again at 29. Now the vaso-motor fibres for the splanchnic nerves leave the cord above the level of the eighth dorsal nerve; the section in this experiment took place well below this, and thus the vessels of the splanchnic area were not paralysed by the operation, yet a very marked fall occurred. This would point to the fact that the vessels of the lower part of the body are very largely concerned in the keeping up of the normal blood pressure in the feet-down position, because when they are paralysed the pressure markedly falls.

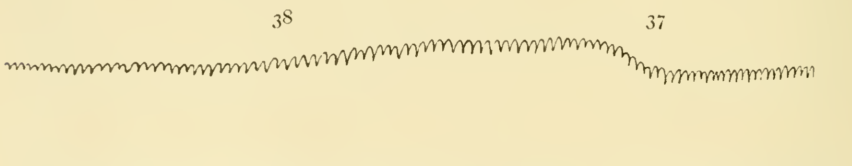


TRACING XVI.—7/25.—Spinal cord previously divided opposite 11th dorsal vertebra. 27 Vertical. 29 Horizontal again. No compensation occurred.

If the distal end of the divided spinal cord be stimulated by the Faradic current, the pressure in the carotid artery is decidedly raised, as shown in Tracing 17. Here the animal, in which the spinal cord had been previously divided in the lower dorsal region was placed feet-down at 43, with the result that the pressure fell. At 44 the distal end of the

TRACING XVII.—1/2.—Spinal cord previously divided in lower dorsal region. 43 Vertical. 44 Faradic stimulation of distal end of cord. 45 Horizontal. 46 Distal end again stimulated—no effect.

divided cord was stimulated as mentioned, and a considerable rise in pressure took place. At 45 the animal was replaced in the horizontal position. This experiment again shows how dependent the general blood pressure of the body is upon the vaso-motor tone of the vessels in its lower part. When the animal was placed horizontal again,



TRACING XVIII.—8/22.—Spinal cord previously divided opposite last dorsal vertebra. Animal horizontal. 37 Distal end of cord stimulated by Faradic current. 38 Stimulus removed.

similar stimulation of the distal end produced no effect. Tracing 18 shows only a slight rise on stimulation of the distal end of the cord while the animal was horizontal. This is a result which one would naturally expect.

The vaso-motor influence of the lower part of the body is supposed to be very limited and chiefly confined to the skin.[1] The experiments just related, however, would suggest that the vascular tone of the lower part of the body is of considerable importance in the regulation of the general arterial blood pressure. Without doubt the splanchnic area is the one chiefly concerned in the regulation of the blood pressure, but it is here suggested that the tone of the lower limbs is of more importance in this regard than is generally recognized.

1 Foster's Physiology, Part I.

THE EFFECT OF CERTAIN DRUGS ON THE BLOOD PRESSURE.

Morphia.—Morphia was used hypodermically upon about forty-five dogs. It was followed in every case by salivation, in the majority of instances by vomiting and in a few by purging. The action of this drug on dogs appears to be very much like that of Apomorphine on man in these respects. Morphia produced distinct slowing of the pulse, and made the animal go under chloroform more easily and stay unconscious longer, and this entirely coincides with what many clinicians have observed.

Chloroform.—This anæsthetic was always administered on a towel, and no attempt was made to measure the dose. In none of the dogs did vomiting occur after the administration of chloroform had been commenced even when they had had morphia previously, and I find no mention of this complication occurring during the anæsthesia of these animals in any literature.

In all of fifty-two dogs killed by chloroform *the respiration distinctly stopped before the heart.* The period between the stoppage of the respiration and the cessation of the heart's action varied from a few seconds to several minutes. As a rule, the more concentrated the chloroform vapour was, the shorter this period became. In nearly all cases the respiration stopped before the pulse tracing had disappeared, but on several occasions, when it seemed as if the heart had ceased, auscultation showed that it was still beating. After the respiration had stopped the first sound of the heart would get weaker and weaker and at last cease, while the second sound would remain loud and clear for some time and then gradually also cease. The question as to whether the respiration or the heart stops first in chloroform poisoning is one about which a great deal of controversy has raged, and the point is not yet unanimously settled. The Hyderabad Commission held that the respiration always stopped first, and that there was no such thing as chloroform syncope.[1] Dr. A. R. Cushny performed many careful experiments with different dilutions of chloroform to see the effect on the heart. If the vapour was very concentrated the heart was affected, but he says, "Although I cannot agree with the Hyderabad Commission that the heart always continues to beat after the respiration ceases, yet the difficulty of maintaining the concentration necessary to paralyze the heart simultaneously with the respiration is extremely great, and I

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1 Lieut.-Col. Lawrie, Lancet, March 14th, 1891.

should think in ordinary chloroform administration such a simultaneous paralysis can never occur.[1] Lieut.-Col. Lawrie stated in Appendix No. 3, of the Report of the Hyderabad Commission[2] that chloroform has no direct action upon the heart, and this is quoted again in a letter upon the Report of the Anæsthetic Commission in the British Medical Journal of January 19th, 1901. I have no doubt, however, but that chloroform does poison the heart muscle in the same way as it must more or less poison every organ in the body. As stated, in all of my fifty-two experiments in this regard the respiration stopped before the heart. If the amount of chloroform given be very great, however, artificial respiration, even if immediately commenced, will not save the animal—showing that the heart as well as the respiration is poisoned. Dr. H. C. Wood in an address delivered before the International Medical Congress in Berlin in 1890,[3] says, "We definitely proved that in the dog chloroform has a distinct direct paralysing influence on both respiration and circulation; that the respiration may cease before the heart, or the two functions may be simultaneously abolished; but that in some cases the heart is arrested before the respiration. We have several times seen the respiration continue as long as one or even two minutes after the blood pressure had fallen to zero, and the blood had completely disappeared from the carotid artery." This might well be and yet the heart might have been found to be still beating if auscultation had been practised. In trying to explain the results of the Hyderabad Commission he says further on, "It may be that the heat or other climatic conditions surrounding the pariah dog make his heart less sensitive to the action of chloroform than the hearts of dogs in northern climates." J. Harris[4] in testing an apparatus for producing painless death of lower animals by chloroform, which had been taken out from England to India, found that it would not work, the reason being that the high temperature prevented the concentration of the chloroform vapour. By placing ice in the chambers animals were readily killed. I can quite understand this, and have frequently noticed during the Indian hot weather that it was more difficult than usual to get patients "under," especially if a punkah were swinging near by. On the other hand it is hard to explain the following statement, "It is stated that iced chloroform has been used in 14,000 cases in Würzburg, Bavaria, without any ill results. Rapidity of administration, comparative freedom from danger and absence of nausea

1 Lancet, March 14th, 1891.

2 Lancet, January, 1893.

3 British Medical Journal, August 16th, 1890.

4 Indian Medical Record, May 19th, 1899.

are the advantages claimed for this preparation[1]." My experiments were done through the several Canadian seasons upon many varieties of dogs, and yet I never found the heart stop before the respiration, although, as stated, on several occasions the pulse tracing did so. Dr. B. W. Richardson in my opinion stated the case aright when he wrote that "death in man during chloroform anesthesia is not generally due to respiratory failure, but that in animals the physiological death from chloroform is a respiratory failure."[2] He says that Snow also thought so.

In my experiments the animals were killed with chloroform in the horizontal and vertical postures, after poisoning with various drugs, cutting of one or both vagi nerves, division of the spinal cord at various levels, opening of the abdomen, bleeding, etc., and yet, as stated, the respiration always stopped before the heart.

Post mortem examinations were made on most of the animals so killed, but the condition of the heart and great vessels then does not seem to be any indication of what it was just after death, as artificial respiration, abdominal pressure and other restoratives were all tried, and these must have altered the conditions of things. Such remarks may also apply to the results of post mortems on persons who have so died. With this proviso, one may say that the right side of the heart was always engorged with blood, and that the left was generally more or less empty, but occasionally also was full. In one case after death from chloroform, followed by artificial respiration, the animal's body was frozen, and transverse sections of the chest made. Here the right side of the heart was found to be engorged and the left was partially filled with blood.[3]

The most dangerous time for a dog is while he is going under first. Then, if he be struggling, the danger is great. In most cases, as already stated, when the breathing had stopped under these circumstances, artificial respiration would restore it and save life, but occasionally it failed to do so.

When chloroform is given slowly and sufficiently diluted then the animal goes under completely without necessarily much, if any, fall in blood pressure. This is in agreement with Shore and Gaskell's results.[4] The least struggle, however, even if only of the nature of increased respiration, temporarily sends up the blood pressure more or

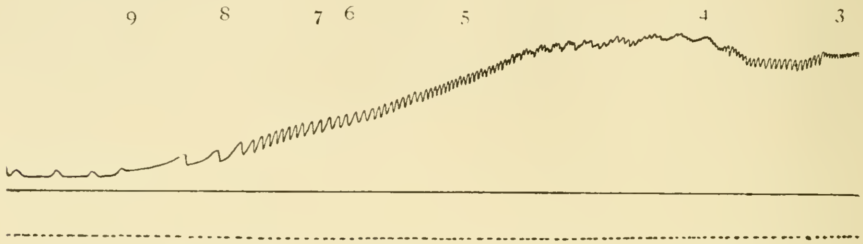
1 Philadelphia Medical Journal, 1900, Vol. II., p. 1,113.

2 Asclepiad, 1890.

3 "Death from Chloroform," by the author, Canadian Practitioner and Review, Feb., 1898.

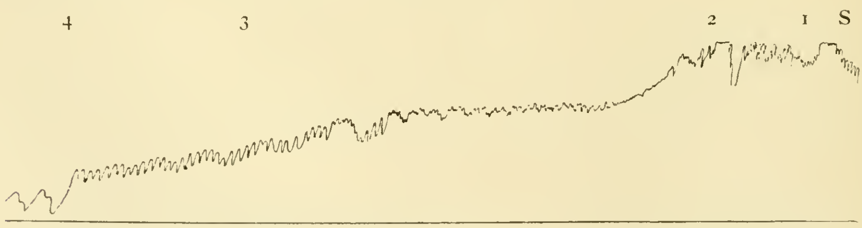
4 British Medical Journal, 1891, Vol. II., p. 1089.

less, and as surely lowers it immediately afterwards. This after lowering may be partly of the nature of compensation, but it is most probably due, in part at least, to the struggling causing a greater inhalation of the drug, and hence poisoning by it.



TRACING XIX.—9/31.—Chloroform poisoning. Animal Horizontal. 3 Chloroform pushed. 4 Struggling. 5 Pressure falling and pulse slowing. 6 Chloroform removed. 7 Respiration ceased. 8 Pulse very slow. 9 Artificial respirations.

Tracing 19 shows a very typical tracing of chloroform *poisoning*. The animal was horizontal when the chloroform was pushed at 3. He had not had any morphia. A slight temporary fall, with slowing of the pulse occurred, which is very frequently the case, followed by struggling at 4, with a consequent rise and hastening of the pulse. Then about forty seconds after the commencement of the chloroform, a rapid fall with slowing of the pulse, set in. The chloroform was removed at 6, but several seconds later, the respiration ceased, and the pressure fell more, with great slowing of the pulse. Artificial respiration was commenced twenty seconds after the respiration failed, and the animal recovered. Even though the chloroform towel be removed as soon as ever the struggling sets in, the temporary rise followed by a fall occurs, sufficient chloroform vapour being present in the air passages to produce this train of events. Conclusion 19 of the Anæsthetic Committee of the British Medical Association is that "struggling must be regarded as a source of very grave danger under chloroform," and Lieut.-Col. Lawrie's rule, as given in his recent book on chloroform (p. 111) is "never to give chloroform while there is struggling or irregular breathing." With this clinical rule my experiments on dogs entirely agree.



TRACING XX.—3/11.—Chloroform poisoning. S Struggling. 1 Chloroform pushed. 2 Pressure began to fall quickly. 3 Chloroform removed as pulse slowing. 4 Respiration stopped.

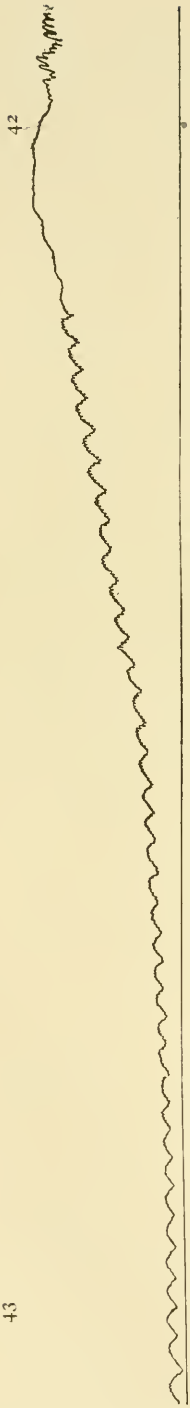
In Tracing 20 another illustration of the same thing occurs. At S, the animal, which was not at all under the influence of an anæsthetic, began to struggle violently, and the result was a rise in pressure, with quickening of the pulse. At 1 chloroform was started and pushed. The pressure began to fall quickly at 2 with great hastening of the pulse. At 3 chloroform was stopped, as the slowing of the pulse heralded threatened respiratory failure, but at 4 the respiration did cease, with the usual great slowing of pulse and fall of blood pressure. The animal eventually recovered under artificial respiration. This extreme and sudden fall of the blood pressure occurring synchronously with the stoppage of the respiration was noted by the first Hyderabad Commission, who stated the case as follows. "Although a gradual loss of tension in the arteries took place after the first stage, the decrease of tension was more abrupt when the respiration became affected. The stoppage of the respiration was always succeeded by a sudden increase in the relaxation of the coats of the femoral artery and a fall in tension." It is difficult, however, to understand how they could have concluded that, "It was further observed that struggling demanded that the chloroform be pushed and not withheld." No advice could well have been more dangerous than this, and fortunately the second Hyderabad Commission put it right. The chart just given shows that struggling raises the pressure, and that if chloroform be commenced *during* the struggling, then the pressure falls almost at once, in fact, it seems from what we have observed that the preliminary rise in blood pressure in chloroform anæsthesia is not due to any vaso-motor stimulation, but rather to the almost constant slight struggling—it may be only increased breathing—which occurs then. Dr. J. A. McWilliam[1] found that "with moderate respiration the results of a certain dose of chloroform were very slight, whereas the same dose during exaggerated respiration caused great depression and extensive fall of blood pressure."

While all admit that a considerable dose of chloroform produces a great fall in blood pressure, it is not settled how it does so. Many believe that it is due to the paralysing action of the drug on the vaso-motor centre,[2] while on the other hand, Shore and Gaskell's cross circulation experiments seem to prove that it is due to the direct action of chloroform on the heart. These cross circulation experiments, however, involve a very difficult technique, and when repeated by others have not always given the same results.[3]

1 British Medical Journal, Oct. 1890.

2 Journal of Physiology, Vol. XXI., Nos. 4 and 5, 1897.

3 "Cross Circulation Experiments," Lieut.-Col. Lawrie, Lancet, 1898, Vol. II, p. 24.



TRACING XXI.—14/39.—Dog horizontal. 42 Right carotid divided and with it the vagus. 43 Respiration still very ample although pressure almost zero.

There can be no doubt but that chloroform can weaken and even paralyse the heart, and that chloroform can weaken and even paralyze the vaso-motor centre, but the question at issue is, which of these two actions is it that occurs first. In my opinion it would be exceedingly unlikely if either one should occur alone, and the probabilities are that the fall in pressure is due to both factors occurring in different degrees in different animals. In one case the vaso-motor centre might prove the more susceptible to the poison, while in another the heart might first show its effects. In both cases a fall in pressure would occur.

The importance of the fall is very differently guaged by different observers. As already stated, all have noticed it. The Hyderabad Commission actually considered it a safeguard, arguing that where the pressure was low less chloroform would be carried to the centres, and therefore these were not so likely to be poisoned; and J. A. McWilliam argued in the same manner.[1] On the other hand Mr. Leonard Hill thinks that this fall in pressure, due in his opinion to paralysis of the vaso-motor centre by chloroform, is actually the cause of death in such cases, and he believes that the respiratory failure which occurs so constantly in dogs poisoned by chloroform is due to the anæmia of the respiratory centre resulting from a fall in pressure. Without venturing to express an opinion on so important a point one may say that a mere fall of blood pressure with assumed anæmia of the respiratory centre does not soon cause stoppage of the respiration but rather stimulates it, as shown in Tracing 21, where the animal was bled to death and the respiration continued even after the blood had ceased to flow from the severed vessel. H. C. Woods and W. S. Carter[2] have shown that even great

1 British Medical Journal, 1890, Vol. II. p.834.

2 Journal of Experimental Medicine, Vol. II, p. 139.

anæmia of the brain produced by tying both carotids and both vertebral arteries produces little if any change as a rule in the respiration. From this fact they were led to the conclusion that, "The respiratory centres are remarkably insensitive to a lowering of their blood supply." Further, I have frequently observed that the respiration stopped while the pressure was still comparatively high, and only after the cessation of the breathing did the pressure somewhat suddenly fall to zero. Tracing 20 illustrates this point. One might say in fact that the pressure only falls very low after the respiration fails and that probably the stoppage of the respiration due to poisoning of the respiratory centre is the cause of this sudden extra fall in the already slowly falling pressure. It seems that as the factors which maintain the blood pressure become weakened by chloroform the respiratory pump becomes more necessary to the upkeep of this pressure than it usually is, and hence when it stops the pressure drops at once. Possibly, however, an anæmic respiratory centre is more susceptible to the toxic effects of chloroform than one not so anæmic, and this special susceptibility Mr. Hill believes he has noticed along with others.[1]

When the blood pressure falls greatly from chloroform and remains low, life must be endangered, but in my experience animals do not die more easily from chloroform administered in the vertical than in the horizontal position, and it is decidedly harder to kill a dog with chloroform when the pressure is very low from hemorrhage than when this is not the case. The second Hyderabad Commission noted this point thus, "In Experiment III, the splanchnics were divided, a proceeding which, as is often said, bleeds the animal into its own vessels. The pressure was after this exceedingly low, but chloroform was given and various other actions taken, and then chloroform had to be pushed on a saturated sponge enclosed in a cap for eleven minutes before the respiration ceased."[2] Again J. A. McWilliams writes as follows,[3] "The fall of blood pressure is in a certain sense protective. It retards the continued access of the anæsthetic into the vital organs. I have frequently been struck with the good resisting power shown to the influence of both chloroform and ether in animals in which a very low pressure was present due to other causes than anæsthetics, *e.g.*, vaso-motor paralysis." He rightly adds that, "On the other hand the fall of blood pressure may become excessive and prove a source of great danger." If then it is harder to kill a dog by chloroform

1 "Causation of Chloroform Syncope," by L. Hill, British Medical Journal, April 17th, 1897.

2 Second Hyderabad Commission Report.

3 British Medical Journal, October, 1890.

when the pressure is low than when it is not so, why should it be that all anæsthetists are agreed upon the danger of giving anæsthetics when the patient is sitting up? The answer I believe lies in the fact that most deaths which occur in practice in the administration of chloroform are *not* cases of poisoning from the drug but *are due to syncope resulting either from the pain of an operation commenced too soon or from fear.* A close perusal of the Clinical Report of the Lancet leads one to the same conclusion. Very many of the deaths which have occurred during the administration of chloroform were not cases of poisoning at all, as the dose was too small. The following cases summarised from this report are probably examples of these.

Series A, Case 3.—Fistula in ano, dose ½ dram on towel, anæsthesia incomplete, felt pain; in one minute pulse failed.

Case 28.—Extraction of tooth, sitting posture, 25 drops on sponge, only 4 or 5 respirations, operation not begun, on being asked a question answered in thick and trembling voice and stretched out her arms, face became bluish, eyes haggard, head and arms fell, she was dead.

Case 73.—For delirium tremens following a fracture, ½ dram on lint, after 2 or 3 inspirations the man writhed and fell back dead, not under influence.

Series B, Case 56.—For removal of finger, 30 drops on lint, syncope.

Case 105.—Castration, 15 to 20 drops on lint, pulse ceased.

Case 285.—Reduction of dislocation, ½ dram, pupils dilated and heart's action failed.

Case 426.—Dressing sprain of ankle, a few drops, syncope.

Sir J. Y. Simpson well remarked in this connection[1] that, "All the patients that die under the hand of the operator when chloroform is used do not necessarily die from the effects of the chloroform upon the constitution. In several of the recorded cases the dose given was too small to have had any such fatal effect. Before the time that anæsthetics came to be used deaths on the operating table often occurred. Such cases have been recorded by Brodie, Cooper, Home, Travers, etc., etc., but they excited no marked share of professional

1 Works of Sir J. Y. Simpson, p. 148.

attention, as they were generally supposed to be accidents against which no action could be of any use. Of late years and since chloroform has been employed they have usually been directly and at once ascribed to the deleterious action of chloroform." He then gives details of a number of cases of fatal syncope immediately before or during operations in which chloroform had not been used. Here is one example, "A few days after the discovery of chloroform, a case of hernia which had been strangulated for a few hours was brought into the Infirmary, and Professor Miller thought it a case demanding operative interference and one in which chloroform should be tried; but I could not be found in time to give it, and the patient was operated on without an anæsthetic. Professor Miller had only proceeded the length of dividing the skin when the patient fainted and died with the operation unfinished. If the chloroform had happened to be used and this fatal syncope had occurred while the patient was under its action the whole career of the new anæsthetic would have been at once arrested. Such cases teach us at least that caution is required in our reasoning and inferences, seeing death may occur and has occurred in operations without chloroform, and with phenomena quite similar to those ascribed to the action of chloroform." The distinction between deaths from chloroform and deaths simply occurring during the administration of chloroform is even more important to-day than it was in Simpson's time. Nowadays so many patients have a dread of chloroform that one would expect cases of syncope to occur occasionally when patients are going under the anæsthetic and are still conscious and afraid. R. Ballard[1] discusses this point well and argues that children and dogs are less apt to be afraid, and hence are less likely to suffer from syncope; and the British Medical Journal in an annotation suggests[2] that the reason why parturient women are less apt to suffer from chloroform is that they do not dread it. Snow[3] mentioned several cases in which, although chloroform was administered, death was attributed by him to fright. In all only a small quantity of chloroform had been given and that freely diluted, and in every case great fear and apprehension were noted before the administration. The fact that many deaths which occur during the use of chloroform are not due to the use of the anæsthetic, is further brought out by the recent Report of the Anæsthetic Committee of the British Medical Association. Out of eighteen cases of death "under chloroform" they found that only three were due entirely to the drug, four were chiefly due to it, and the remaining

1 Lancet, 1898, Vol. I, p. 1,253.

2 British Medical Journal, 1900, Vol., p. 35.

3 Treatise on Anæsthetics, 1858.

eleven—more than 50 per cent—were doubtful. Their 17th Conclusion is that, "Imperfect anæsthesia is the cause of a large number of cases of danger under chloroform."

We have seen that workers differ greatly as to the danger of great falls in blood pressure due to chloroform administration. It seems clear, however, that such falls are at any rate indications that the patient is very deeply under, and if only one could clinically recognize such a fall with ease it would be a valuable danger signal. Duplay and Hallion[1] hope before long to describe a method of ascertaining the blood pressure during surgical operations under anæsthetics; and if they or any one else should succeed, a valuable result will have been attained.

Occasionally during the administration of chloroform, *sudden* falls of blood pressure may occur, along with marked slowing of the pulse, evidently of the nature of vagus inhibition of the heart. Such falls were noted by the Glasgow Commission, and were considered to be a cause of sudden death under chloroform.[2] The members of this Commission, in discussing the points of agreement between them and the Hyderabad Commission, say that "Both observed peculiarly sudden and unexpected falls of pressure and slowing of the heart. The Commissions differed as to the origin of these. The Hyderabad Commission attribute it to asphyxia; the Glasgow Commission say not asphyxia, whatever may be the cause."[3] These falls closely resemble those which we gave early in this work as occurring occasionally in dogs apparently without cause; no chloroform being administered at the time. When they occur during the administration of chloroform the Hyderabad Commission, as stated, consider them to be asphyxial and of no danger, in fact, the opposite, as tending to prevent the further absorption of the poison. Lieut.-Col. Lawrie[4] dogmatically states that, "The special effects which the Glasgow Commission attributed to chloroform were produced by accidental asphyxia . . . the slowing of the pulse and circulation through stimulation of the vagus is a safeguard in chloroform poisoning."

Tracing 22 shows such a fall. Chloroform was started at 7, the dog being already slightly under. He struggled a little. The sudden fall occurred ten seconds later. The chloroform was not removed, and yet

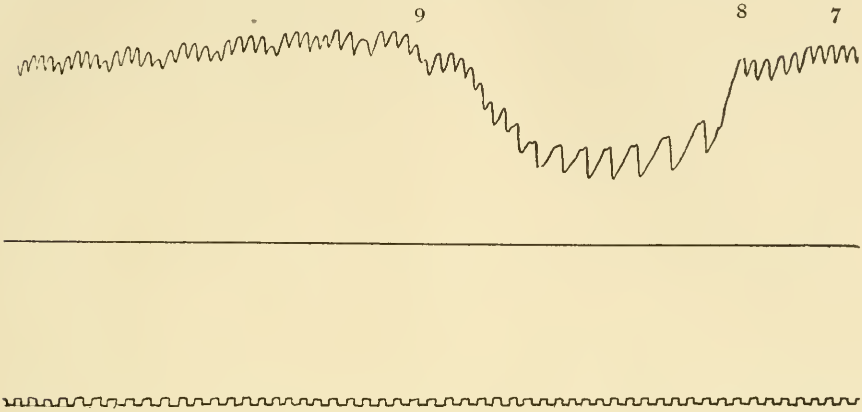
1 Archives Generales de Medicine, Aug. 1900.

2 Journal of Anatomy and Physiology, Vol. XIII., p. 395.

3 "Remarks on the 2nd Hyderabad Commission," by Drs. McKendrick, Coats and Newman, British Medical Journal, June 14th, 1900.

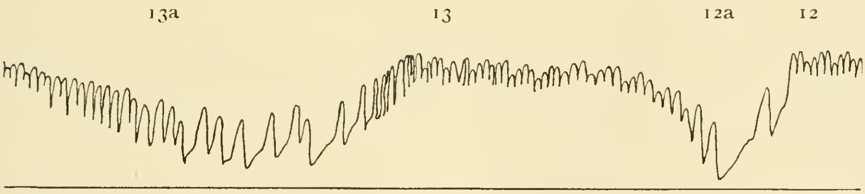
4 Lancet, June 21st, 1900.

twenty seconds later the fall had been partly recovered from and the pulse had resumed its former rate. A strong resemblance will be seen between this tracing and one produced by either stimulation of the



TRACING XXII.—9/23.—No Morphia. 7 Chloroform pushed, dog being horizontal. 8 Sudden fall in pressure with slowing of pulse. Chloroform continued and yet pressure normal again at 9.

distal or the proximal end of the vagus, as shown in Tracing 23, and it seems that all are agreed that such falls are of the nature of vagus inhibition. Whether the vagus centres are directly stimulated by the chloroform, or are more or less reflexly affected through afferent nerves, or whether the stimulation is of the nature of asphyxia, it is hard to say. From the fact that the fall disappears even if the chloroform be continued, I would be inclined to agree with the Hyderabad Commission



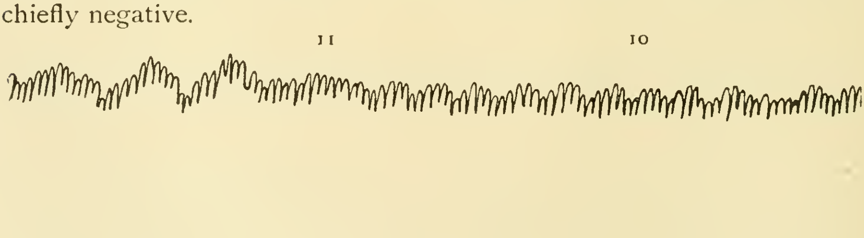
TRACING XXIII.—3/11 Dog horizontal. Left vagus previously divided. 12 Distal end of cut vagus stimulated by Faradic current. 13 Proximal end stimulated.

that such is not a source of danger; and as regards the nature of the vagus inhibition, from the same fact, I would consider that it is caused by strong vapour irritating the sensory branches of the vagus, and as the sensory nerves become numbed the reflex disappears. It is not a constant phenomenon, however, even when very concentrated vapour is used. Another case from similar stimulation might show reflex laryngeal spasm. The fact that these occasional, probably safe, falls in pressure do occur in dogs is not sufficient ground in my opinion for changing the view that usually a markedly slow pulse is an urgent indication for the immediate removal of the anæsthetic.

As a rule, unless an animal has just been struggling violently, the pulse becomes considerably slowed when he is going under chloroform and becomes fast when he is coming out. The exception mentioned is struggling, and this, as has been already shown, may so hasten the pulse that it only slows when, or just before, the respiration stops. The cause of this slowing is doubtful, some attributing it to stimulation of the vagus by the chloroform vapour, some to the direct poisoning of the heart muscle, and others again to asphyxia. Whatever the cause of this gradual slowing may be, it is a valuable sign clinically. The fall of pressure is evidently not due alone to it, as shown by the fact that such falls occur when the vagi are divided, when atropine is given (vide tracing 47), or while the pulse is still fast after struggling (vide tracing 20). Nevertheless the fall in pressure and the slowing of the pulse as a rule go hand in hand in deep chloroform narcosis.

As regards the effect of posture during chloroform narcosis, generally speaking the animal is rendered less resistant to the effects of gravity than is one not so poisoned, and hence the vertical feet-down position produces a greater fall than it would otherwise do. If the animal be first placed feet-down and then chloroform be pushed, as might be inferred, the pressure falls more freely than when the drug is given in the horizontal position. This is in accordance with Mr. Leonard Hill's observations, when he considers chloroform to be the most powerful agent known for abolishing the mechanisms which compensate for the influence of gravity.[1]

The effects of various operations on the blood pressure while the animal was under chloroform were tried, and as a rule they were chiefly negative.



TRACING XXIV.—3/8.—Dog under Chloroform and horizontal. 10 Abdomen opened freely. 11 Splenic artery tied.

Tracing 24 is taken from a dog completely anæsthetized with chloroform and lying horizontal. At 10 the abdomen was freely opened

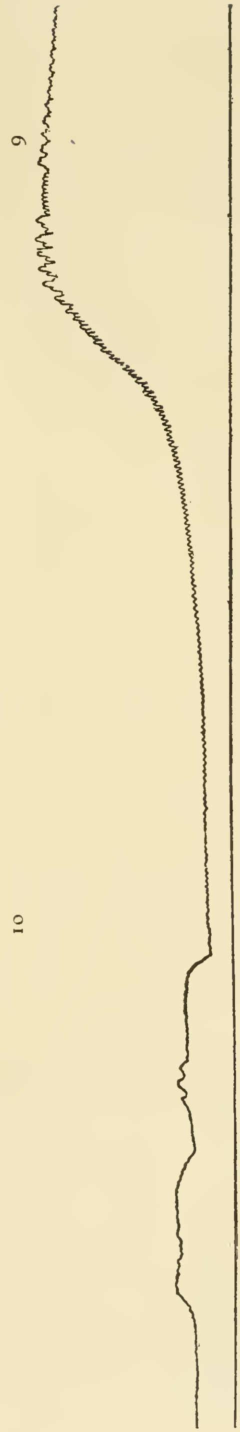
1 Journal of Physiology, Vol. XXI, Nos. 4 and 5, 1897.

and the splenic artery was tied at 11, and yet no fall in pressure occurred. A slight rise in fact occurred at 11, which might have been due to the tying of the artery. Thus no sign of shock appeared, and this is in accordance with the results obtained by the Hyderabad Commission, who were unable to produce shock in dogs under chloroform by any operation they tried.

COMPLICATIONS ARISING DURING THE ADMINISTRATION OF CHLOROFORM.

Various complications may occur at any time during the administration of chloroform, which produce more or less effect on the blood pressure and are as well often dangerous. Vomiting never seems to take place in dogs under chloroform, as already noted. The effects of struggling have been already discussed.

The inhalation of fluid when occurring during anæsthesia is sometimes a source of great danger. Tracing 25 is taken from a dog under chloroform. When this tracing begins he is already in the feet-down position. At 9 two hundred ccs. of an aqueous solution (of chloretone) were poured down his throat. He swallowed distinctly several times, but did not seem to breathe again. The pressure after a slight transitory rise fell rapidly, but the pulse remained fast. At 10 he was placed horizontal, and some rise of pressure occurred. Artificial respiration was tried, but did not seem to work well—no sign of life appeared and he was evidently dead. On opening the thorax the right side of the heart was found to be enormously distended with blood, as were also the veins entering it. The left side was nearly empty. The lungs contained



TRACING XXV.—7/18.—Dog under Chloroform and vertical. 9 200 c.c. of fluid poured into fauces. 10 Horizontal.

frothy blood, and evidently the animal had died of drowning. This chart shows that in acute asphyxia, as stated by the Hyderabad Commission, the pressure may fall rapidly. The pulse, however, did not slow as the Commission found it to do, and the chart bears no resemblance to one in which death occurs from chloroform poisoning (unless indeed the animal be already under atropine.)

In Tracing 26 a dog under chloroform had already had some solution (of chloretone) poured into the stomach by means of a tube. While still vertical the remains of the solution, about 1 oz., were poured into the fauces, the tube having been removed. At once at 7 the

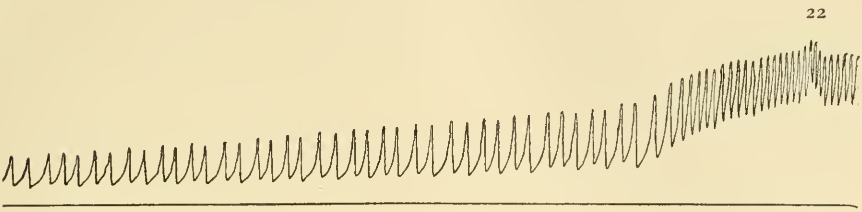
TRACING XXVI.—3/8.—Dog under Chloroform and vertical. 7 An ounce of fluid poured into fauces-gasped. 8 Some of solution inhaled. 9 Horizontal. Animal recovered.

pressure fell, then rose for a few seconds, and then began to fall steadily with slight hastening of the pulse. The animal was placed horizontal and began to breathe again, and finally recovered. This chart shows the danger of even small quantities of fluid accumulating in the fauces while the laryngeal reflex is done away with by an anæsthetic. Even tracheotomy might not save the patient, as the fluid is quickly drawn into the lungs themselves, as shown by the post mortem examination of the dog from which Tracing 25 was taken.

These charts agree then with Lieut.-Col. Lawrie's contention that in asphyxia the pressure falls. The fall is only marked in *obstructive* asphyxia, however. When asphyxia is brought about by free opening of the pleural cavities producing pneumo-thorax, so that although the animal is breathing hard no new air enters the collapsed lungs, then after many violent acts of inspiration the respiration ceases and then only the pressure falls and the pulse becomes markedly slowed. Kcnow and Shenbeck[1] showed that in animals whose respiration was paralyzed by Curara asphyxia produced a rise in pressure, then a gradual fall, then a strong increase and finally a fall to death.

1 "On Blood Pressure in Asphyxia," Skandin Arch. f. Physiologie, I, 603-641. Tap. 5, 6.

Tracing 27. In this animal double pneumo-thorax had been produced in the manner described with very free openings through the parietes. The whole tracing is too long to reproduce here, but the

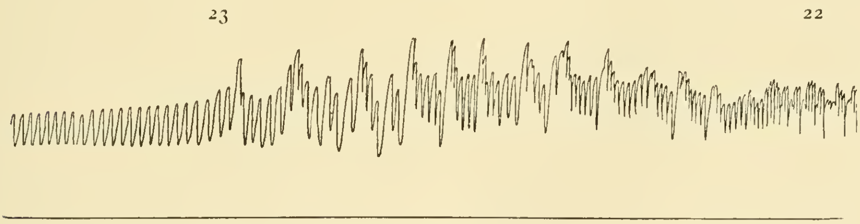


TRACING XXVII.—3/11.—Double pneumothorax previously produced with very free openings through parietes. Pressure well maintained until respiration stopped at 22. Pressure then fell with slow pulse—asphyxia.

pressure was well maintained until respiration stopped at 22. Then it fell and the pulse became very slow and ceased a couple of minutes later.

A momentary rise will be noticed at 22 just after the respiration ceased.

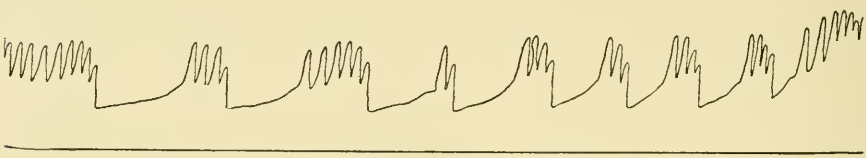
In Tracing 28, on the other hand in which both pleuræ had been freely opened, a marked respiratory wave appeared with some rise in pressure when the trachea was clamped at 22. At 23 the attempts at



TRACING XXVIII.—3/11.—Double pneumothorax previously produced. 22 Trachea clamped. 23 Efforts at breathing ceased and then arterial pressure fell.

respiration stopped, and then the pressure fell slowly with marked slowing of the pulse. About three minutes later the pulse became fast and the pressure rose a few mms., giving a good example of delirium cordis.

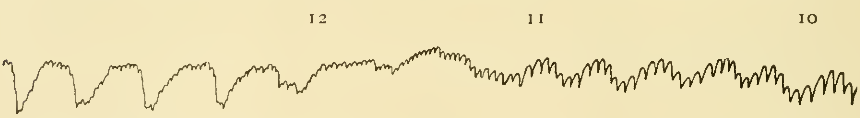
Thus it may be seen that very different tracings are got in different conditions of asphyxia, and that no one description will suffice for every form. This probably is the reason why Lieut.-Col. Lawrie and his critics got so far apart in their statements, some asserting that the pressure fell in asphyxia, and others that it rose, *e.g.*, Dr. J. H. Potter wrote:[1] "We have in asphyxia increased blood pressure with lividity; in fatal chloroform narcosis we have just the opposite, viz., diminished blood pressure with pallor." They were both right probably, for, as shown, the question of whether the pressure rises or falls in asphyxia entirely depends on how the asphyxia is produced. It seems that as long as efforts at respiration are made as in 28 the pressure is maintained but as soon as such efforts cease then the pressure falls. In chloroform poisoning the respiratory centre is paralyzed and there are no efforts at respiration and therefore the pressure may at once fall.



TRACING XXIX.—9/31.—Animal inhaling strong Chloroform vapour. Inspiratory stridor.

Inspiratory Stridor.—Tracing 29 shows the effect upon the blood pressure of inspiratory stridor produced by the inhalation of strong chloroform vapour. A fall occurred during each inspiration.

In Tracing 30 the left vagus had been already cut. At 10 the right vagus was being handled, which produced some fall of pressure. At 11 this nerve was divided and immediately the pulse increased in rate, the pressure rose and the respiratory curve became more marked. Each dip in the tracing was accompanied by an inspiratory stridor,

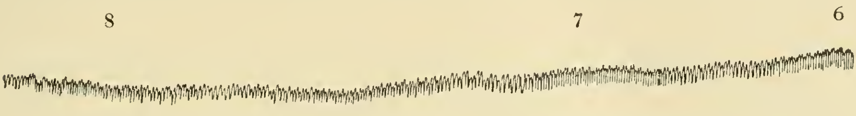


TRACING XXX.—9/31.—Left vagus already divided. 11 Right vagus cut. 12 Inspiratory stridor set in evidently of a paralytic nature.

evidently caused by the flapping together of the vocal cords, the muscles of which had been paralyzed by division of the vagi. This result, however, is not a constant one on division of the vagi. Thus, either spasm of the laryngeal muscles as shown in Tracing 29, or paralysis, as shown in Tracing 41, may produce inspiratory stridor, and this stridor is shown to have a marked effect on the pressure. The stridor due to spasm disappears as the animal becomes more deeply anæsthetized; that due to paralysis does not so go away.

1 The Lancet, Vol. I, 1890.

A Rigid Condition of the Body may occur while the animal is deeply under chloroform and may disappear as he comes out. Tracing 31 is taken from a case of this kind. The animal was completely anæsthetized and still taking chloroform at 6, but the jaws were rigidly closed. At 7 he was still very rigid and the jaws could not be forced apart. Chloroform was stopped at 8, and soon the spasm lessened and the jaws relaxed, and without giving any more chloroform a stomach



TRACING XXXI. – 1/4.—Animal completely under Chloroform. 7 Rigid condition of body. 8 Chloroform stopped and rigidity soon ceased.

tube was passed with ease. I have seen this rigidity occur clinically occasionally under ether to a marked degree, and to a less extent under chloroform. G. O. C. Mackness[1] mentions a case where "suddenly clonic spasms of the face and limbs came on" under chloroform where the patient recovered; while E. W. Dickson[2] gives an instance where such ended fatally. Fourteen cases are mentioned by the Anæsthetic Committee of the British Medical Association in which fits or epileptic convulsions were noticed.[3]

METHODS OF RESUSCITATION IN CHLOROFORM POISONING.

Of all methods tried artificial respiration was found to be by far the most certain method of restoring animals in which the respiration had stopped as a result of chloroform poisoning. In the majority of cases it was successful, though occasionally, even when started as soon as the natural respiration had ceased, it failed to save life. In these latter cases probably the heart and vaso-motor centre were paralyzed almost synchronously with the respiration. The method followed was rhythmical compression of the chest, and the effect on the blood pressure consists in a rise during each expiration. In using the method the air passages must be kept free by keeping the tongue firmly pulled out and if necessary introducing the tip of the index finger into the rima glottidis, by which means the vocal cords are kept apart. The Lancet Clinical Report mentions a case "where mechanical stimulation of the larynx by pressing the finger down to the rima glottidis was said to

1 British Medical Journal, Dec. 7th, 1895.

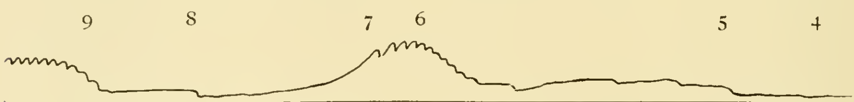
2 British Medical Journal, Oct. 19th, 1895.

3 Report of the Anæsthetic Committee, British Medical Journal, Feb. 23rd, 1891.

have been successful in restoring life," and apart from this probable reflex action, the pressure of the finger assures the operator that the cords are swung apart.

One soon learns to tell whether artificial respiration will be successful or not in a given case by the "feeling" of the chest. If this feel elastic, and it be easy to make the air pass in and out, all is well; but if this be not the case, even although the air passages be free, it is bad. Why this should be I am not quite certain, but certainly artificial respiration on a dead dog will not produce the amount of respiratory tide in such that it will do in a living animal. It must be remembered that when one compresses the chest the air is not thus *directly* pressed out, but the compression lessens the cubic capacity of the thorax and then the elasticity of the lungs drives the air out. If this elasticity be lessened then compression of the chest will not so readily cause the air to pass out. This is observed in cases of Emphysema, where the elasticity of the lungs is more or less lost[1] and expiration in consequence becomes difficult.

As the animal recovers under the influence of artificial respiration it becomes progressively easier to make a good flow of air in and out of the chest, until at last this occurs spontaneously, and when once this stage has been reached I have not seen the respiration fail again. The Hyderabad Commission report such cases, however.



TRACING XXXII.—9 40—Dog poisoned with Chloroform. 4 Respiration ceased but heart could be heard on auscultation. 5 Artificial respiration. 6 Pulse appeared and pressure rose. 7 Artificial respiration stopped at 6 and pulse here failed and pressure fell again. 8 Artificial respiration again. 9 Pulse started again and soon after natural respiration commenced and animal recovered.

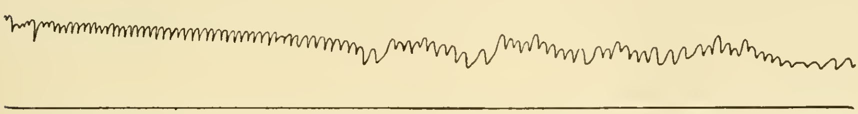
Tracing 32 illustrates the beneficial effect of artificial respiration on the pulse and blood pressure. At 4 the animal was very deeply poisoned with chloroform, the pressure was almost zero, and the respiration had stopped, and the pulse was absent from the chart though the heart could be heard to be beating. The chloroform had been removed. At 5 artificial respiration was started, and the waves produced by it are visible on the tracing. At 6 the pressure suddenly rose, and with it the pulse appeared. At 7 artificial respiration was stopped, and at once the pressure fell again and the pulse disappeared. At 8 artificial respiration was resumed, and at 9 the pressure rose again and the pulse reappeared.

1 Diseases of the Lungs. Sir Douglas Powell.

Artificial respiration was stopped and the pressure fell slightly, but natural respiration soon set in, and after that the animal gradually recovered, the pulse as usual becoming very fast during recovery. This tracing shows then the good effect of artificial respiration on the heart. The method might almost as properly be called "artificial circulation" as "artificial respiration;" which name would constantly remind one that he was directly acting on the heart while performing the movements of the method.

The effect of Tracheotomy on the blood pressure is interesting. Besides being useful in cases where some obstruction in the air passages exists above the level of the wound thus made, the operation appears to

17



TRACING XXXIII.—9/31.—Dog poisoned by Chloroform. Respiration stops. Artificial respiration tried without avail. 17 Trachea opened and respiration commenced at once and remained.

stimulate the respiration reflexly. In Tracing 33 the respiration had ceased as a result of chloroform poisoning, while the pulse continued. Artificial respiration had been performed, but no attempt at natural respiration appeared. The tongue had been drawn out forcibly and repeatedly (Laborde's method), and the air passages were clear. Tracheotomy was performed at 17, and at once the animal commenced to breathe and soon recovered.

Tracing 34 shows the same phenomena even better. Natural respiration had stopped here for several minutes. The puncturing of the trachea at 27 at once was succeeded by a gasp and rise in blood

28 27

TRACING XXXIV.—9/28.—Dog poisoned with Chloroform. Natural respiration stops for ten minutes; Artificial respiration being continued. 27 Trachea opened and artificial respiration continued as before. 28 Natural respiration occurred and animal recovered.

pressure as shown in the tracing, and the animal recovered. The air passages were clear. The effect on the respiration here is evidently of the nature of reflex stimulation. The same thing is seen often when the surgeon is performing tracheotomy for any condition. As the trachea is punctured a violent gasp occurs. In desperate cases of chloroform poisoning it might be justifiable to thus operate in the hope of producing this reflex effect.

Forcible Pulling Forward of the Tongue seems, as has long been noted clinically, to stimulate respiration, and in several of the dogs it seemed to be the last straw starting respiration. Professor Syme in a clinical lecture delivered in 1884[1] said, "Attention to the tongue is another point we found of great consequence. When respiration becomes difficult or ceases we open the mouth, seize the tip of the tongue and pull it well forward, and there can be little doubt that death would have occurred in some cases if it had not been for the use of this expedient." It has been recently demonstrated that pulling on the tongue does not open the glottis, and it would seem more probable that it reflexly stimulates the respiration. The chloroformist who has been trained in Edinburgh always has a pair of fenestrated artery-forceps handy for this purpose.

The preceding notes and tracings emphasize the following points as regards the effects of chloroform.

First, that any struggling during its administration greatly hastens the toxic effects, and that hence the drug should be removed while such lasts, and then should be given more gradually when the patient is quiet again. One frequently sees clinically the chloroform pushed at such a juncture, especially if the struggling has been started by the surgeon commencing the operation; but struggling without any such cause generally indicates that the vapour is too concentrated, and struggling due to this is doubly dangerous.

Second, a fall in blood pressure is hard to detect accurately clinically, but it is usually accompanied by slowing of the pulse, and such is a danger signal, and the chloroform should be at once removed. This slowing may occasionally be of a transient nature and due to stimulation of the vagi by concentrated vapour; or it may be the more serious slowing in the wake of which lies respiratory failure.

Third, if respiratory failure should occur—and it is much more likely to do so during the preliminary administration than later on—then artificial respiration is by far the most valuable method of restoring it. Artificial respiration not only keeps up the respiratory tide but also, as shown in Tracing 45, directly stimulates the circulation and raises the blood pressure, in fact the circulation may be feebly carried on for a

1 Lancet, June 21st, 1885.

short time by respiration alone. Thus, whether one believe that chloroform kills by respiratory paralysis or by heart failure, or by vaso-motor paralysis, artificial respiration should be resorted to at once, and should rank, in my opinion, above all other remedies which may afterwards or along with it be attempted.

Fourth, forcible pulling out of the tongue, the placing of the finger in the rima glottidis, and the performance of tracheotomy all seem to stimulate respiration.

Fifth, the horizontal position is advisable when chloroform is being administered, not because respiratory failure is less likely to occur in that position, but because the risk of syncope is thus greatly lessened. Syncope is in my opinion the cause of death in most cases reported, and in many at least is not due to chloroform poisoning at all, but to the shock of pain or emotion before the patient is fully under or when he is coming out.

The actions of three drugs were investigated in regard to choroform poisoning. These were, Nitrite of Amyl, Hydrocyanic Acid, and Atropine.

Nitrite of Amyl.—This drug was given in several cases where the respiration had ceased as a result of chloroform poisoning, but no beneficial results were obtained. In each case one or more capsules containing the drug were crushed in the fauces while artificial respiration was being maintained.

Hydrocyanic Acid.—On the first of January, 1898, an article appeared in the *Lancet* by Professor Hobday, of the Royal Veterinary College of London. In it he suggested the use of hydrocyanic acid as an antidote in cases of chloroform poisoning. The dose recommended was 1 minim of Scheele's acid by the mouth for every seven or eight pounds of body weight. He stated that he had found it invariably useful in animals in which, as a sequence to chloroform poisoning, the respiration had stopped. He had already published[1] a list of forty-three observations on various animals, including dogs, cats, horses, sheep and calves, showing the results obtained by this method of resuscitation. In the last paper he refers to a series of fifteen additional consecutive cases in which H.C.N. had been successfully used in dogs after the respiration had actually ceased. Dr. A. Wilson, of Manchester, in the next number of the *Lancet*[2] opposed this view very strongly, arguing on theoretical grounds that as H.C.N. is the most powerful of all respira-

1 Journal of Comparative Pathology and Therapeutics, June 1896.

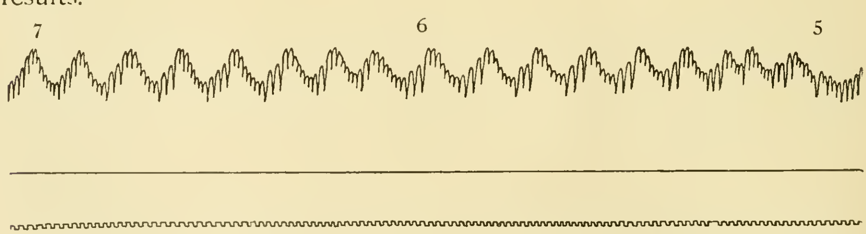
2 Lancet, Jan 8th, 1898.

tory, poisons, hence it could not be useful when the respiration was already paralyzed by chloroform. He concluded by saying that "It would be useful if Professor Hobday could give an account of the effects of the dose he recommends on a healthy animal." The argument that because H.C.N. is the most powerful of all respiratory poisons in fatal doses, therefore it could not do good in any dose, was so obviously open to criticism that I determined to take advantage of Dr. Wilson's suggestion to Professor Hobday and to try the effect of the drug in non-lethal doses on a healthy animal.

Ex. I.—A healthy mongrel collie dog, weighing thirty-three pounds, was given hypodermically in the back 9 3-7 minims of Acid Hydrocyan. Dil. (equal to 4 5-7 minims of Scheele's Acid, *i.e.*, 1 minim of Scheele's Acid to every seven pounds of body weight) at 2.48 p.m. The room was at 58° F. Notes were made every thirty seconds of his condition. They may be summarized by saying that in two minutes the animal was breathing hard and rapidly, with mouth open as if after exertion. This continued for five minutes and then he vomited. The breathing gradually became normal again. He vomited once more at 3.1 p.m. and then all symptoms disappeared and he remained well.

The effect then of a dose of 1 minim of Scheele's acid to every seven pounds of body weight is, very briefly, first, great stimulation of the respiration; second, vomiting; third, recovery.

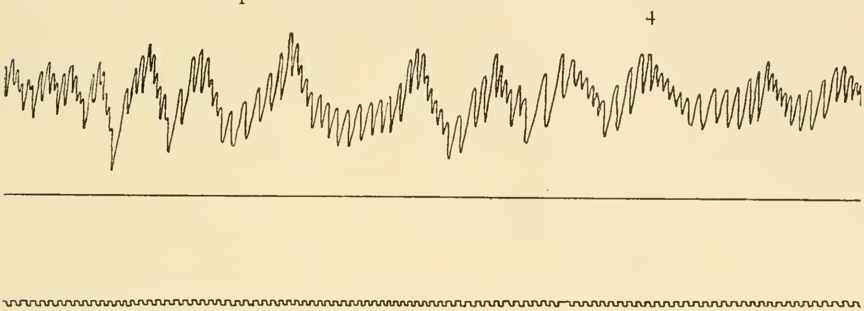
In order to note the effect of the drug on the blood pressure, pulse and respiration, several dogs were used. They were previously given some morphia hypodermically and afterwards just enough chloroform to keep them quiet so that there should be no struggling to mar the results.



TRACING XXXV.—1/4.—Effect of small dose of H.C.N. Dog slightly under Morphia and Chloroform and horizontal. 5 Five minims dilute H.C.N. into fauces. 6 Respiration more ample. 7 Pulse raised from 87-106. Recovery.

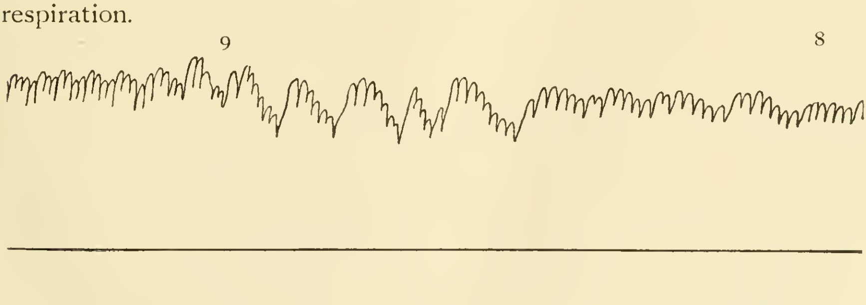
Tracing 35 illustrates the effect of a small dose of the drug. The animal had been given ½ grain of morphia hypodermically half an hour before the experiment began. He weighed about seventeen pounds. At 5 he was lying horizontal and breathing quietly, twelve to the minute, with a pulse of eighty-seven, and was slightly under chloroform.

5 minims of dilute H.C.N. (equal to 2½ minims of Scheele's acid) were injected into the fauces. The respiration almost at once became more ample—although this is not fully brought out in the tracing—but it was not hastened; and the pulse was raised from 87 to 106. No bad effects followed, and 7½ minutes later the animal was reported as quite normal again. Here then a dose of about 1 minim of Scheele's acid to seven pounds of body weight increased the amplitude of the respiration, slightly hastened the pulse, but did not otherwise alter the tracing, and no bad effects were produced.



TRACING XXXVI.—9/29.—Effects of small dose of H.C.N. Dose administered just before tracing begins. 4 Respiration very ample, pulse slightly irregular, pressure unaltered.

Tracing 36 is from a dog of about nine pounds weight which had been given ¼ grain of morphia half an hour before the experiment commenced. One minute before the tracing commences 3 minims of dilute H.C.N. were injected hypodermically. A few seconds later (at 4), the respiration was deep and sighing and somewhat irregular, the pulse was slightly faster, and the pressure remained unaltered. After that the respiration became hastened, but beyond the tracing the animal completely recovered. Thus a slightly larger dose than Professor Hobday recommended, about 1 minim of Scheele's acid to six pounds of body weight, produced no bad effects when given hypodermically. The chief alteration noticed was marked increase in the amplitude of the respiration.



TRACING XXXVII.—3/10.—Effects of H.C.N. Dose administered at 8. 9 Breathing very ample, pressure rather higher.

TRACING XXXVIII.—13/35.—Repeated doses of H.C.N. 17 First dose, respiration excited and somewhat irregular, pulse hastened and pressure slightly raised.

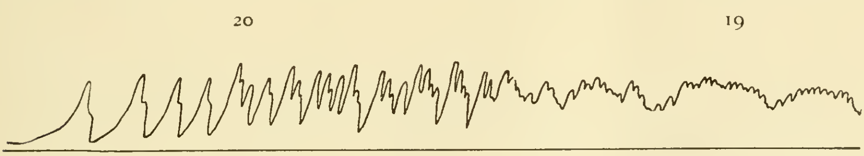
Tracing 37 is from a dog twenty-two pounds in weight which had previously been given ½ grain of morphia hypodermically. Some minutes before this tracing begins he had been given 6 minims of nitrite of amyl, but the effects of this had completely worn off. He was now given three minims of Scheele's acid by the mouth, *i.e.*, about the maximum dose recommended by Professor Hobday. In a few seconds, as shown by the tracing, the respiration became very ample and then hastened, the pulse somewhat increased in speed and the pressure was a little raised. No bad effects ensued.

Next we come to a series of experiments undertaken to find the effects of repeated doses of H.C.N.

Tracings 38, 39 and 40 are from the same dog and show well the effects of repeated doses of the drug. The dog was a fox terrier weighing about twelve pounds. At 17, 3 minims of Scheele's acid were placed on the back of the tongue. Almost immediately the pulse hastened, the respiration became greatly excited, irregular and more ample, and the blood pressure rose somewhat. A couple of minutes later, fifteen seconds before tracing 39 begins, the dose was repeated. The same effects occurred, but soon wore off, and at 18a the tracing looks as it did before the first dose of H.C.N. was administered. Two minutes later the dose was again repeated at 19 (in tracing 40). The respiration once more became excited, but soon grew infrequent, the pulse became much slower and the pressure fell, making the chart look like that from one form of asphyxia, and soon the animal died of respiratory failure. The thorax was quickly opened and the heart was seen to be still beating—and it continued to beat even after it was completely removed from the body; and, when the contractions had ceased, they could for several minutes be started again by simply putting the heart under a stream of cold water. But although the heart was beating when the chest was opened it was practically empty, and a wound made in the left ventricle did not bleed to any extent. In this case death resulted from 9 minims of Scheele's acid given to a twelve pound dog in three doses within four minutes. The result was death from respiratory failure, but before this occurred

TRACING XXXIX.—9/26.—Repeated doses of H.C.N. 18 Second dose given. Respiration excited. 18a Pulse slowing.

there was great and repeated stimulation of the respiratory centre. As with many other drugs, a small dose produces one effect and a larger dose the opposite—stimulation in the one case, paralysis in the other. It is the first stage, that of stimulation only, which is produced by the dose recommended by Professor Hobday. N. Grehaut[1] showed that repeated small doses of H.C.N. produced powerful stimulation of the respiration. After the injection of 5 c.c. of a 1/10,000

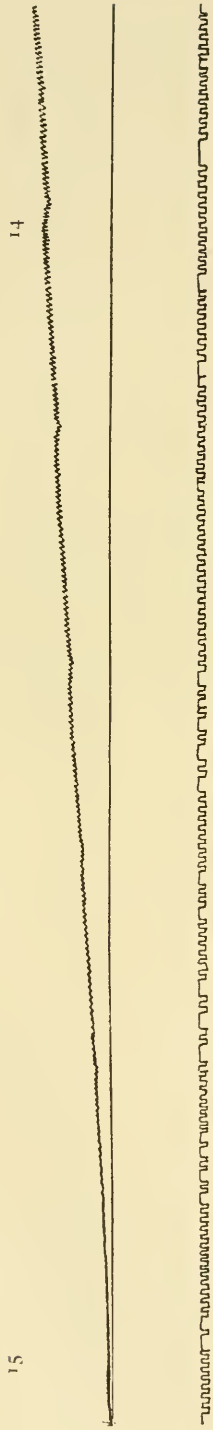


TRACING XL.—9/17.—Repeated dose of H.C.N. 19 Third dose. Respiration excited and then slowed and stopped about 20. Pulse slowed, pressure fell to zero.

solution of pure H.C.N. into the jugular vein of a dog weighing 10 kilo., the respiratory movements immediately became more ample and soon again returned to their ordinary rhythm. He found that 7/1,000 c.c. of pure H.C.N. killed a 9 kilo. dog in seventeen minutes.

Next, single doses just sufficient to produce death were given to several dogs in order to note the sequence of events. An animal weighing about sixteen pounds was given 10 minims of dilute H.C.N. (equal to 5 minims of Scheele's acid) hypodermically. He had already had 3 minims half an hour before. The pulse rate, blood

1 Physiolog. Researches on H.C.N. Archiv. de Physiol, 1890, p. 133.



TRACING XLI.—7/16.—Final stage of fatal H.C.N. poisoning. Last respiration occurred about 14. 15 Heart stops.

pressure and respiration were noted at frequent intervals as given below:

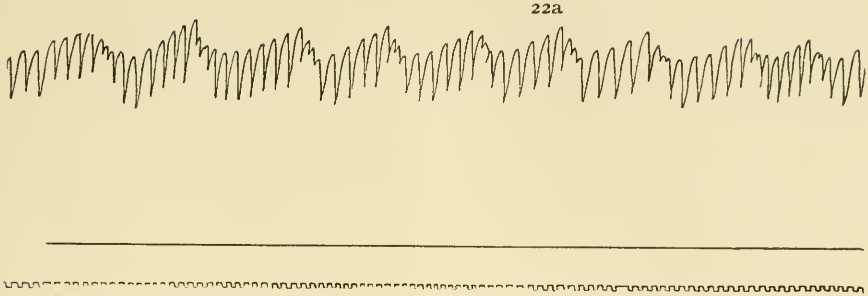
	Pulse.	*Respiration.*	*Blood Pressure.*
Just before administration..	105	24	125 mms.
Two minutes after administration..............	156	39	125 mms.
Two minutes later........	228	29	140 mms.
Two minutes later........	168	24	140 mms.
Two minutes later........	174	27	140 mms.
Four minutes later........	160	18	
Four minutes later........	111	12	
Four minutes later........ (Tracing assuming an asphyxial type with great amplitude of pulse waves.)	44	10	
Four minutes later........	34	5	
Two minutes later........	88	7	Pressure now commencing to fall steadily.
Two minutes later........	118	2	

Tracing 41 shows the last stage of this case. The last respiration occurred at 14, the pressure fell steadily, and the pulse became fast at the last.

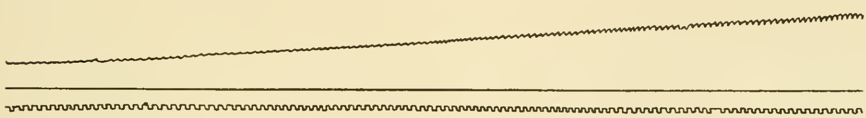
Another dog was treated in the same manner, and the next tracings show the effects at intervals. The animal weighed about ten pounds and was given 10 minims of dilute H.C.N. hypodermically, following on 4 minims a few minutes before. He had had no morphia. Just before the last dose was given the pulse was 82, the respiration 16, and the pressure good. (He had quite recovered from the first small dose.)

	Pulse.	*Respiration.*	*Blood Pressure.*
Two minutes after last administration	160	26	In statu quo.
Two minutes later..	50	7	In statu quo.
Three minutes later (tracing 42)......	42	7	In statu quo.
Three minutes later.	100	Just stopped	Falling steadily.
Six minutes later...	Tracing 43 shows the termination of this experiment. The heart stopped after beating rapidly to the end.		

22a



TRACING XLII.—9/28.—Poisoning by H.C.N. seven minutes after fatal dose was given. P. 42 R. 7 pressure still maintained.



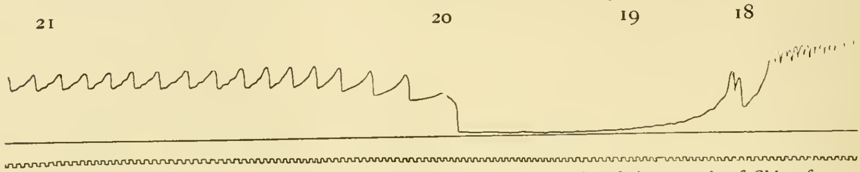
TRACING XLIII.—9/34.—Poisoned by H.C.N. Last stage. Respiration already stopped.

The sequence of events then from a small fatal dose is as follows: First, preliminary stimulation of the respiration and pulse, the former increasing in amplitude as well as in speed; second, slowing of the respiration and pulse, the pressure being maintained; third, stoppage of the respiration, pulse increasing in speed and pressure falling; fourth, pressure falling to zero, pulse remaining fast until death.

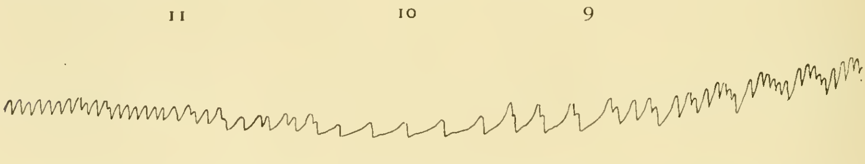
I poisoned four dogs thus which had previously had a hypodermic dose of morphia. In none of them did vomiting occur, nor convulsions. One dog which had had no morphia or chloroform showed a strong tendency to convulsions before death from a dose just sufficient to obtain a fatal result. He vomited frequently. When the dose of the drug is largely in excess of what is necessary to produce death, then the animal almost at once passes into a state of convulsion.

From these experiments it would seem that the dose of Scheele's acid recommended by Professor Hobday, viz., 1 minim of Scheele's acid to seven pounds of body weight, is a safe one in a dog. Such a dose, or a lesser one, produces the stage of stimulation of the pulse and respiration with no alteration in the blood pressure. The effect of the drug soon wears off, and the animal seems to be none the worse.

The next tracings show the effect of using H.C.N. when danger *has occurred* from chloroform poisoning. A thirty-pound dog while vertical was given chloroform until the respiration stopped. In Tracing 44 the respiration stopped at 18. At 19, 4 minims of dilute H.C.N. were administered hypodermically. At 20 he was placed horizontal and respiration of a very fast nature at once commenced. In the tracing the big waves represent the pulse, slow because asphyxial, and the little ones are the respiration. The pulse is sixteen per minute, and the respirations are sixty. Beyond this the pulse hastened, the pressure improved and the animal completely recovered. No artificial respiration was here employed, and it seems probable that this animal was saved by H.C.N. The placing of him horizontal greatly aided the result.



TRACING XLIV.—3/10.—Antidotal use of H.C.N. Animal vertical. 18 As a result of Chloroform pressure fell and respiration stopped here. 19 Animal apparently dead. H.C.N. hypodermically. 20 Horizontal. 21 Respiration 60 per minute, pulse 20 (large waves are pulse and smaller ones respiration). Animal recovered.



TRACING XLV.—3/11.—Antidotal use of H.C.N. Animal being vertical, Chloroform pushed, pressure fell and respiration ceased at 9. 10 H.C.N. given. 11 Pressure rising, respiration rapid (not shown on tracing).

In another case an animal was apparently saved from fatal chloroform poisoning only to die from H.C.N. poisoning. A dose equal to 1 minim of Scheele's acid to three pounds of body weight was used, and hence it was not to be wondered at that he eventually died. Tracing 45 was taken from this case. While the animal was in the vertical posture chloroform was pushed, and at 9 respiration stopped, and the pulse as usual became exceedingly slow—ten per minute—and the pressure fell to almost zero. At 10, twenty-five seconds after the cessation of respiration, 7½ minims of dilute H.C.N. were injected into the fauces. Respiration of a very rapid nature commenced almost at once, although it produced no sign on the tracing, and at 11 the following note was made: "Blood pressure rising; respiration good although not shown on chart." Thus, so far nothing but good had resulted from the H.C.N., and the animal seemed certain to recover. But now symptoms of H.C.N. poisoning set in as follows: 2½ minutes from the time of administration of the H.C.N. natural respiration stopped, and the pulse was thirty-eight. One minute later artificial respiration was commenced. Five minutes later the animal was placed horizontal, with the result that the pressure rose slightly. The finger was introduced into the glottis. With each artifical respiration a spasm of the depressor muscles of the lower jaw was felt. This spasm gradually spread to the muscles of respiration, and eight minutes after the administration of the H.C.N. the spasm evolved itself into natural respiration. This stopped two minutes later, and artificial respiration this time failed to restore it. The animal died 19½ minutes after the administration of the H.C.N. This animal "died cured" so far as the chloroform was concerned.

Bearing in mind the extreme uncertainty of the strength of preparations of H.C.N., I took special care to procure from a reliable source a fresh supply for each of the experiments. Scheele's acid is roughly double the strength of the dilute Hydrocyanic Acid of the British Pharmacopœia. I used the drug both by the mouth and hypodermically, and seemed to get about the same results by either method. My experiments taken in conjunction with the more numerous ones of Professor Hobday would suggest that in *true* cases of chloroform poisoning, when the respiration has stopped or seems likely to do so, it would be well to try the use of a medicinal dose of this powerful drug. It could be given, hypodermically or by the mouth, as an adjunct to artificial respiration and other restoratives. Cyanide of potassium would be the most suitable preparation to keep on hand for such emergencies, being a more staple body than the solution of acid. The B.P. dose of the dilute acid is 2 to 6 minims, and that of the U.S.P. 1 to 15 minims. Professor Hobday recommended a dose of 1 minim of Scheele's acid to seven pounds of body weight. For a man weighing, say 140 pounds, according to this the dose would be 20 minims of Scheele's acid; that is 40 minims of the dilute acid. Although such a dose appears to be safe in animals, I should very much hesitate to recommend it in practice even in an emergency; but the full B.P. dose of 6 minims could be employed with absolute safety, and that of the U.S.P., viz., 15 minims of the dilute acid, might be used if necessary. Forty-nine minims is the smallest fatal dose of which I can find any record.[1]

Atropine.—The action of this drug has been very thoroughly and repeatedly studied of late, and my experiments do little but confirm the results which others have obtained. Atropine has been termed by Binz[2] "the most powerful of all stimulants"; Wood and Cerna[3] have

1 Taylor's Medical Jurisprudence.
2 Lectures on Pharmacology Binz, Vol. I., p. 93.
3 Journal of Physiology, 1892, p. 882.

proved its potent action in stimulating the respiration, increasing "the air movement," as it does in suitable doses, from 100 to 300 per cent. Nevertheless the drug does not seem to have come into general use in practice as a stimulant, although Lauder Brunton and others have urged its value in conditions requiring stimulation. I have discussed this question more fully elsewhere.[1]

I used ten dogs in studying the action of atropine in conjunction with chloroform, and the results may be discussed under two headings:

First, The effects of atropine when administered previously to the giving of the anæsthetic, and

Second, the antidotal action of atropine when given after poisoning from chloroform has occurred.

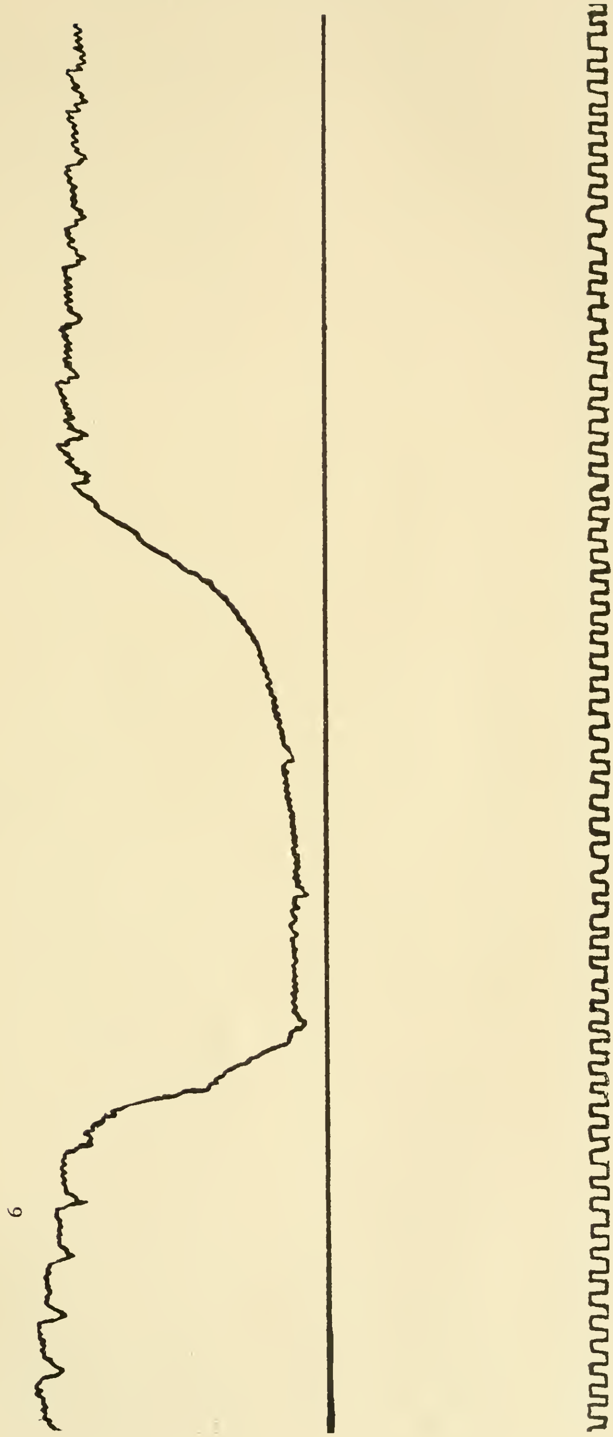
Seven of the dogs were previously given morphia, while three were not so treated.

In the first place it may be noted that the effect of gravity on an animal under the influence of atropine may be quite as marked as without it. In Tracing 46 the dog, while thoroughly under atropine, was placed vertical at 5 and the pressure is seen to fall in a very decided manner. This is scarcely what one would have expected on general principles, and it may be that the heart is already beating up to its maximum and hence can do no more when called upon to compensate for the fall in pressure. In tracing 14, however, gravity produced very little effect under similar conditions.

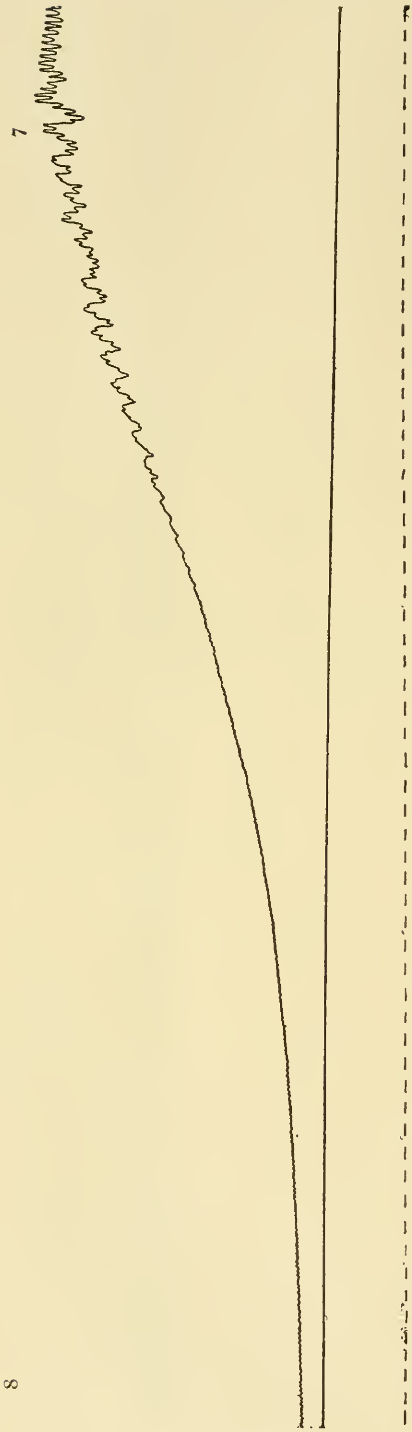
The use of atropine with a view to preventing danger during the administration of chloroform has long been strongly recommended by certain writers, and has been as vigorously opposed by others. Many anæsthetists, especially in Scotland, regularly give a hypodermic injection of it either alone or combined with morphia before commencing chloroform, and in Lyons this method is much used.[2] The Glasgow Commission found as a result of their experiments that atropine lessened the danger of death from chloroform, believing as they did that strong inhibition of the vagus from chloroform was a real danger, which would be impossible when this nerve was paralyzed by atropine. Lieut.-Col. Lawrie, on the other hand, representing the second Hyderabad Commission, opposed the use of the drug, arguing that: "If the Committee regard the effect of atropine as beneficial they

1 "Notes on Atropine," by the author. Montreal Medical Journal, October, 1900.

2 "Les Accidents du Chloroforme et leur Remède." Ann. et Bull. de la Soc. de Méd. Gand. 1889, p. 253.



TRACING XLVI.—9/13.—Dog under atropin. 5 Vertical. 6 Horizontal again.



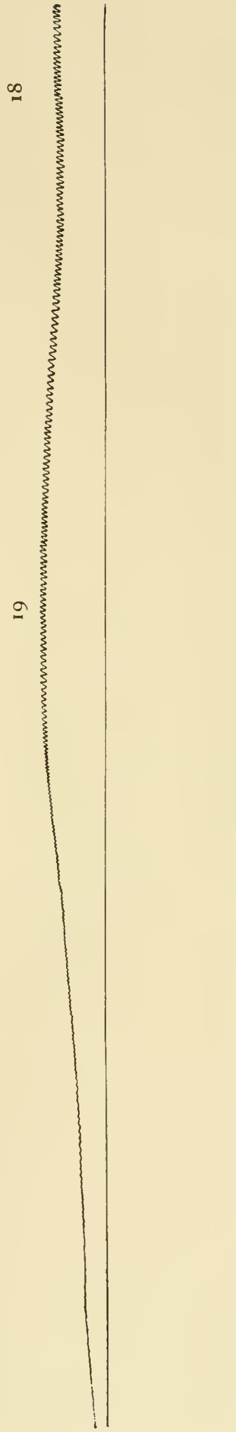
TRACING XLVII.—7/18.—Dog under Atropin. Horizontal. Chloroform pushed at 7 while animal struggling. Pressure fell rapidly, but pulse remained fast. Respiration did not stop and animal recovered. 8 Chloroform removed.

must intend to imply that the inhibitory action of the vagus is a danger in chloroform administration when atropine is not used, or that the normal action of a healthy nerve is a danger to life." This is in my opinion a most fallacious argument. An animal inhaling concentrated chloroform vapour is not in a normal condition; and because in normal life the gentle inhibitory action of the vagus does no harm, it does not at all follow that a great amount of the same inhibition set up by the action of chloroform may not be dangerous. Although I consider that this argument is fallacious I do not go so far as the Glasgow Commission did in believing that vagus inhibition is really a danger in chloroform administration. When such inhibition occurs, if the chloroform merely be continued, the reflex is soon deadened and then the heart is released. In my limited experience a dog under the influence of atropine is decidedly harder to kill with chloroform than one not so conditioned, whatever be the theory as to how the atropine acts. If, however, chloroform be pushed persistently in an atropinized dog the pressure falls steadily, the pulse remains fast, and after some minutes — 5½ minutes in one experiment and 7½ in another —the respiration stops, and two or three minutes later, the pressure being nearly at zero, the pulse ceases.

In Tracing 47, the animal having been previously placed under atropine, chloroform was pushed at 6. No morphia had been given. A good deal of struggling occurred, followed by a rapid fall in blood pressure. Chloroform was removed, and the animal quickly recovered. It is interesting to note that although the pressure fell so low the respiration did not stop as would almost certainly have been the case if atropine had not been given. This tracing shows incidentally that the fall in blood pressure in chloroform poisoning is not dependent upon slowing of the heart's action.

The action of atropine as an antidote to chloroform poisoning does not seem to have attracted much attention. Dr. H. C. Wood[1] found that in a dog in which the respiration had stopped from chloroform poisoning "a hypodermic injection of 10 c.cs. of a two per cent. solution of atropine altered the rate of the pulse but had no apparent effect on the pressure and respiration, and in no wise prevented the final cardiac arrest." 10 c.cs., however, was such an enormous dose, representing as it does about three grains of atropine, that he might well have got such a result when a more moderate quantity might have saved the animal. This quickening of the pulse which Dr. Wood refers to is shown in Tracing 48. In this case the animal had been atropinized and then chloroform had been pushed until the respiration stopped at 18. After that the pressure rose gradually and then as gradually sank again with hastening of the pulse. After the pulse

1 British Medical Journal, Aug. 16th, 1890.



TRACING XLVIII.—7/23.—Dog previously atropinized. Then Chloroformed. Respiration stopped at 18. 19 Shows slight rise in pressure.

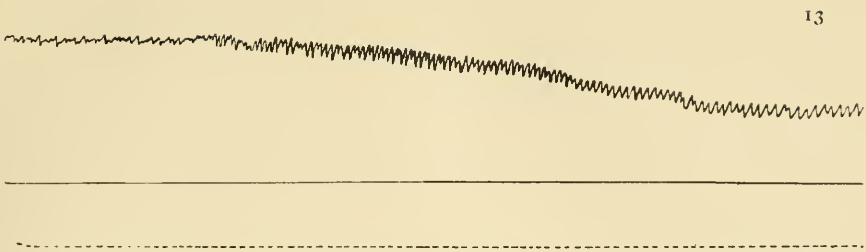
had disappeared the second sound of the heart could be heard on auscultation for about half a minute and then it too disappeared and the animal was dead.

This slight rise after the respiration had stopped occurred in all the dogs poisoned with chloroform following on atropine, and probably points to a circulation made more vigorous by atropine. Dr. Reichert, of Philadelphia, in a recent article,[1] shows that after the respiration has been paralyzed by atropine, if the breathing be maintained artificially, then even as much as six times the lethal dose of the drug may be administered and yet recovery may ensue. He shows, as is of course well known, that atropine kills by paralysis of the respiratory centre; that next the vaso-motor centre is poisoned; and last of all the heart. These results if confirmed emphasize the importance of artificial respiration being continued if necessary for hours in cases of atropine poisoning in man.

In several dogs in which the respiration had stopped as the result of an overdose of chloroform, I found that atropine seemed to have a powerful antidotal action, acting in this respect very much as Hydrocyanic acid does. In a fox terrier dog which was being anæsthetized by chloroform the respiration, subsequent to some struggling, rather suddenly ceased. The canula had not yet been introduced into the carotid. Artificial respiration was used without success. The tongue was forcibly dragged upon and was seen to be deeply cyanosed. The heart could not be heard on auscultation. 1/50 of a grain of atropine was injected under the skin over the precordia, and the swelling thus produced was rubbed until it disappeared. About one minute later the heart was felt to be beating rapidly, artificial respiration was stopped, and in a few minutes natural respiration commenced in a shallow manner and the animal recovered. Exactly the same sequence of events occurred in another dog. Unfortunately in neither of these animals had the canula been adjusted in the carotid, and therefore we are unable to produce any tracings. The former dog had had ¼ grain of morphia hypodermically thirty minutes before the emergency occurred; the latter had not had any.

I reproduce one tracing from a case in which recovery from chloroform poisoning seemed at least to be hastened by the use of atropine. In Tracing 49 the animal was so deeply poisoned by chloroform that the respiration had already stopped. Atropine was injected at 13.

1 Philadelphia Medical Journal, Jan. 19th, 1901.



TRACING XLIX.—9/26.—Dog poisoned with Chloroform. Respiration had already stopped. Atropine injected at 13. No artificial respiration used.

The pulse soon became fast and respiration commenced. No other means of resuscitation were employed.

My experience would lead me to the following conclusions:

First, the previous use of atropine lessens the tendency to death from chloroform poisoning in dogs. Theoretically also one might assume that from its powerful stimulating effect on the circulation it would, especially if combined with morphia, tend to lessen the chance of syncope occurring during, but not necessarily due to, chloroform administration.

Second, that when, during the administration of chloroform, danger has occurred, either in the form of syncope or of respiratory failure, atropine in moderate doses (say $\frac{1}{100}$ grain) would tend to stimulate both the circulation and the respiration, and hence would be a valuable adjunct to other means of saving life in such emergencies.

REV. HENRY SCADDING, D.D.

Since the last issue of THE TRANSACTIONS OF THE CANADIAN INSTITUTE, one of its most honoured members has passed away in the person of the Rev. Dr. Scadding, who died on 6th May last, in Toronto, at the great age of eighty-eight. Born at Dunkeswell, Devonshire, England, on 29th June, 1813, where his father was factor to Major-General Simcoe, he came to Canada when only seven years of age, and his whole subsequent life has been identified with Toronto, except the four years—1833-1837—that he spent at St. John's College, Cambridge. Before proceeding to Cambridge for his university education, he had received his preliminary training at Upper Canada College, of which institution he was the first "head-boy." Receiving his B.A. degree in 1837, he returned to Canada, and became one of the masters in Upper Canada College. He was also the first rector of Holy Trinity Church, Toronto, and laboured for many years in both capacities, till compelled by failing health to relinquish active work. But though in a manner retired from public life, he by no means became an idler. His eighteen years of editorship of *The Canadian Journal*, and his numerous contributions to its pages, are the record of a busy, though tranquil life. In addition to the numerous papers read by him before The Canadian Institute, he published several volumes, chiefly elucidating historical and archæological points relating to Canada, and especially Toronto. For the six years, 1870-1876, he filled the office of president of The Canadian Institute. He was also the first president of The York Pioneers, and was one of the founders of The Ontario Historical Society. He was an M.A. of Cambridge, 1840;

D.D. of Cambridge, 1852; and D.D. of Oxford, 1867. Dr. Scadding was pre-eminently a lover of books, and in his long life accumulated a large and valuable library, in which were many rare and curious books. He was also an enthusiastic numismatist, and until his latest years he preserved a deep interest in his native county, and was on terms of intimacy with various members of the Simcoe family; and one of the objects for which he most earnestly laboured was the erection in Toronto of a statue of the first lieutenant-governor of Upper Canada. It must have been a peculiarly heavy affliction to him that in his later years his sight so failed him that his whole intercourse with his beloved books was maintained through a reader. And yet he did not repine. His was a peculiarly gentle and placid nature. Courteous, kind, modest, unassuming, he shewed himself in his relations with his fellows the genuine, humble, Christian gentleman. Of him it may truly be said that he wore the white flower of a blameless life;

> Cui Pudor, et Justitiæ soror,
> Incorrupta Fides, nudaque Veritas
> Quando ullum inveniet parem?

It will be long before his venerable figure will pass out of the recollection of his friends. It was fitting that the end of such a life should come gently and quietly. There was no disease, no pain; it was only the exhaustion of Nature's powers; and he slept into eternal life as peacefully as he had lived.

AN INVESTIGATION INTO THE EFFECTS OF WATER AND AQUEOUS SOLUTIONS OF SOME OF THE COMMON INORGANIC* SUBSTANCES ON FOLIAGE LEAVES.

BY JAMES B. DANDENO, A.M.

(Assistant in the Botanical Museum of Harvard University.)

(Read 4th May, 1901.)

CONTENTS.

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* The inorganic substances referred to include some of the strong acids, a few alkalies, and some of the more common mineral salts.

INTRODUCTION.

SINCE so many questions, relating more or less directly to the main one, are involved in the progress of the proper discussion of the chief subject, and since the field is so very broad, it became necessary to select from the very large number of experiments involved, those which seemed to tend more directly towards a full development of the subject. As the main question is an investigation into the effect of certain solutions of inorganic salts upon foliage leaves, and, as the intelligent answer to this question depends so largely upon the capability of leaves to absorb water and the more dilute solutions, *e.g.*, rain water, soil water and spring water, it was found necessary to investigate the matter of the absorption of water and aqueous vapour by leaves. This naturally led to a consideration of the atmospheric conditions which might, through time, give rise by adaptation to certain qualities which leaves may have acquired through ages past. Enquiry was made at the Weather Bureau at Washington, to learn to what extent inorganic salts were known to pervade the atmosphere, either in the neighbourhood of the sea or inland, but it was found that, so far as America was concerned, no work of any importance had up to the present been done. The investigations made in Europe were too general in character to apply directly to the subject under discussion. This made it necessary to investigate the matter experimentally, and a series of experiments was performed to ascertain if the salts of the sea did permeate the atmosphere without the aid of spray or winds. These experiments are described in detail further on.

Investigation was also made into the question suggested by a statement of Sachs, that distilled water which remains upon a leaf of a plant becomes alkaline.

Plants adapted to a moist climate were selected and arranged as shown in Fig. 7, the roots being supplied with nothing but distilled sterilized water and air, while the leaves were fed with a nutrient solution furnished by means of an intermittent spray. Composition of the nutrient solution :—H_2O, 1000.0 grams ; KNO_3 1.0 grams ; $MgSO_4$ .5 grams ; $CaSO_4$ .5 grams ; K_3PO_4 .5 grams ; $FeSO_4$ .01 grams. The object of this experiment was to determine whether a plant could *use* a nutrient solution so applied. On account of the long duration of the experiments, the solution had to be changed from time to time.

To learn from another point of view how a nutrient solution affects the early growth of young leaves developing from the bud, a series of experiments was performed with young willow twigs, during the months of March and April, this being the time of the year when the process of the opening of buds seems to depend only upon a favourable temperature. This experiment was designed also to test water absorption by young leaves.

The solutions (other than nutrients) used in the experimental work in connection with this paper were made by dissolving the molecular weight, in grams of the substance, in a liter of water. This method of preparation is similar to that indicated by Pfeffer* (Ewart's Trans. 1900, p. 146), and by Detmer and Moor (p. 326), who designate them *normal solutions*. This method is also that adopted by True (1898, p. 410-411) and (1900, p. 185). These, however, are not *normal solutions* as defined by Mohr† and other analytical chemists. When salts contained water of crystallization, or hygroscopic water, it was found more convenient to determine the specific gravity of the solution and from this calculate its concentration. A convenient apparatus for finding the specific gravity of a solution was arranged ; and as such apparatus may be of some use in a laboratory of plant physiology, a full description with diagram is given (Fig. 1). The stock solution once obtained, there was no difficulty in preparing solutions of less concentration as occasion required. The solutions of hydrochloric and of sulphuric acids were *normal solutions*, as were those of potassium and of sodium hydrate. These were procured ready prepared from the chemical supply house.

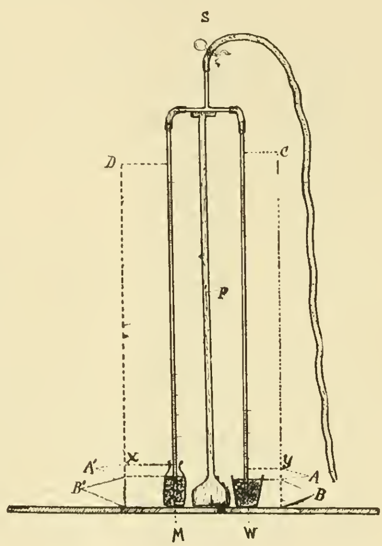

FIG. 1.

Two small glass tubes, held in a vertical position by the support P, are finely graduated and connected by a "T" tube to the leg of which is attached a rubber tube. One of the tubes is inserted in distilled water at the temperature required, the other in the liquid to be measured. The stop-cock S is opened and the ascent of liquid due to capillarity, A and A', measured. Then by sucking on the rubber tube the liquids rise to D, etc. Now the sp.g. of M is $\frac{Cy}{Dx}$.

* See bibliography at the end.

† Titrimethode, p. 56.

Both kinds of solutions, since they differ so little when in dilute form, are designated in this paper, "*m*"; and the dilution is indicated by a number written as the denominator of a fraction.

Since so much seemed to depend upon the question of water-absorption by leaves, a rather full discussion is entered into, more particularly upon the literature pertaining to this subject, in order to establish upon what grounds present views of plant physiologists are based. Some experiments were made to test water-absorption directly, but it was found that more light could be thrown upon the subject from other sources. It was from the indirect side mainly that the question was attacked, as it seemed, in the judgment of the writer, to be more productive of fruitful results; so the question of guttation drops, dew-drops, and calcareous incrustations upon certain plants, was examined in some detail with a view to learn something of their cause, chemical nature and function.

For the examination into the effects of solutions applied to the cut ends of the petioles, leaves were selected which would readily show an acid or an alkaline reaction, and which would live for a considerable time in water without sending out roots. It was not so much to determine how long leaves could endure the solution and live, as to examine into the effects produced by the solutions so applied, and to learn something of the cause of death. The same is true with regard to the application of the solutions to leaf surfaces; and a comparison is made of the effects of some of these upon Spirogyra, with that upon leaves.

On commencing the work of experimentation, and upon examining into the literature pertaining to the subject, it was found that the question first to be answered was in regard to whether living green foliage leaves absorb water through the epidermis, or through the stomata, in some way or other, from the surrounding medium, either as liquid or as vapour. The popular notion is that water is so absorbed, but the view expressed by many of the recent text-books on botany is quite generally opposed to this. Some writers have "proved" experimentally that water could be absorbed, while others apparently equally reliable "proved" the opposite. This condition of affairs rendered it necessary to examine the question in detail by careful and prolonged experiment, and to study with considerable care, the literature which pertains directly and indirectly to this subject. That water-absorption is the very "corner-stone" of the problem is at once apparent, because all the solutions used are aqueous, and many of them are in very dilute form. Plants under experimentation are always regarded as *living* plants—not simply as so much tissue—and from this point of view the phenomena are chiefly considered.

In order to clear the way to the main issue it was found necessary to investigate certain questions which at first sight may appear not directly a part of the subject; but it is only through these subordinate and relative questions that we are able intelligently to explain, with any degree of exactitude, the various elements, as connected with, and forming a part of the whole general subject which is a unit. It is recognized to be of importance that one who undertakes a problem should confine himself strictly to that problem; but it is of greater importance that one should, while confining himself within the limits of his problem, understand and explain not only the details relative to it, but the relationship of that problem, if solved, to scientific knowledge already obtained. In other words he must be able to assimilate his results with the laws of science. This is mentioned in order that it may be fully understood at the outset that the question has branched out into directions not contemplated at the commencement.

The "spotting" of the tobacco leaf is dealt with in some detail because of its economic importance, it being a particular aspect of the question involved under the heading:—"The effects of solutions applied to leaf surfaces."

II.—HISTORICAL RÉSUMÉ.

AS has just been said in the introduction, the question of water-absorption by leaves, being so important a part of the general subject, and having been dealt with by several writers (some as early as the middle of the 17th century), will have a prominent place in this chapter, not only because of its importance of itself, but also because it is the only side of the problem that has been investigated to any considerable extent, though unfortunately with results that seem never to have settled the question. The subject of the absorption of dilute solutions by leaves, being so closely related to that of water-absorption, would be in a similar position to-day, were it not that attention has been turned in this direction by the practice of spraying plants, subject to fungous diseases, with solutions known as fungicides; and of killing of weeds by means of a spray of a poisonous solution.

The first author of any note was Mariotte (1679, p. 133), who showed by experiments that living foliage leaves could absorb water both as rain and as dew. His work was important, and he has been quoted by many, especially by the earlier writers, possibly in part because of his careful and close reasoning coupled with his rather unique experiments. After Mariotte the records of work done in this connection are fragmentary and scattered, and it was not till about fifty years later, when the experiments of Stephen Hales (1726, p. 56) were published, that a new impetus was given to the subject. In 1753 Bonnet (1754, p. 26) made some important investigations relative to water-absorption and came to the same general conclusions as did Mariotte and Hales, namely, that water was absorbed by leaves with evident advantage to plants, and that water-absorption was a normal function of leaves. Senebier in his text (1806, 3, p. 94) refers to the works of Bonnet and Hales, and, after reviewing some of their experiments, concludes that water is aborbed. In the work of Dutrochet (1837, p. 328), one finds the idea that water is absorbed somewhat extended, and as Dutrochet puts it—"Physiologistes ont consideré les feuilles comme des sortes de racine aériennes destinées à puiser dans l'atmosphère l'eau et les autres principes qui contribuent à la nutrition du végétal." This view seems rather too strong on the side of water-absorption when one examines the works of the earlier writers whom he quotes, and upon whose works he very largely bases his statements.

All the works just mentioned are now looked upon as classic, and the names will be remembered as long as plant physiology is deemed a subject worthy of investigation. These works form a sort of epoch, not only in matter of time, but also in views and conclusions; and one is struck by the singular similarity in aims and argument among those authors.

In the botanical works of Treviranus (Vol. I., 1835), we find that the views upon the question are opposed in part to that of the authors just mentioned, especially to that of Bonnet and Hales; and his work introduces a side of the question which has stood its ground up to the present time. That a function of foliage leaves was to absorb water and dew, had up to this time been looked upon as established, not so much because of the works and views of Mariotte, Hales, Bonnet, Senebier and Dutrochet were accepted as proving so much, but rather because the view was according to the popular notion and seemed self-evident. The experiments of Treviranus, and of others of less note, about the same time, raised some startling questions in regard to the then recognized power of leaves to absorb, and it would be well to give the words of Treviranus—"Man muss daher wie ich glaube eine Einsaugung von tropfbarer Flüssigkeit durch Blätter nur da zulassen, wo entweder die Oberhaut fehlt oder, wie bei, unausgebildeten und ueberhaupt bei zarten Blättern sehr dünn ist." . . . . . . "Nur Dunst wird eingesogen."

Two rather important papers by Garreau (1849, 1851) appeared later, the first of which dealt with this question of water-absorption. Garreau goes into the matter thoroughly and examines this question from the standpoint of anatomy, as well as that of physiology, and concludes that water can be absorbed. One ought to draw attention to the fact that *do* absorb and *can* absorb are two different things, a point which will be discussed in the chapter dealing with water-absorption.

Hugo von Mohl (1852) gives but little attention to the subject, but states in a somewhat general way that water-vapour is absorbed. It is important to notice that the view here expressed by von Mohl was a cautious one, and that it was now considered by no means certain that water-absorption was a function of leaves. Then came the work of Duchartre (1861, p.109), whom one might call the founder of the position taken by writers of modern text books in regard to absorption of water by leaves. One finds Duchartre's work almost always referred to, while the works of several others of no less importance seemingly ignored. One reason probably for this is that Duchartre had performed his experiments with growing plants; while most of those holding opposite views had based their conclusions upon experiments with detached leaves and cut shoots. Moreover, Duchartre held that the moistening of a leaf surface by rain or dew only caused a diminished transpiration, which resulted in an increase of turgor; and stated also that as transpiration was a normal function of leaves it was not easy to see how absorption could also be a normal function at the same time.

Important special papers upon the subject then began to appear, notably that of Cailletet (1872, p. 242), who showed by means of a manometer that water was readily absorbed under certain conditions; and Boehm (1877) who placed leaves (not detached) of seedlings under such conditions in which it was possible for them to absorb water to advantage when, under like conditions, the roots were unable to do so. The works of Mer (1878, p. 105) and Boussingault (1878, p. 289) corroborated those of Cailletet and Boehm. The work of Henslow

(1880, p. 313) was exhaustive, and he thought he had settled the question of water-absorption; and, as he says, he thought he had settled it in the affirmative. Lindley in his early work (1866, p. 193) referring to stomata, says :—" It is by means of this apparatus that leaves absorb water and gaseous matter from the atmosphere." In Lindley's "Theory of Horticulture," (1859), the belief is expressed that leaves do absorb fluid from the air; and it is stated that the stomata are well adapted to this purpose. Lindley refers at some length to the work of Knight (1886), in which it is stated that leaves may absorb to the extent that a descent of sap is produced in the alburnum, and also that one leaf may be made to supply its neighbour below it with water. Gregory (1886) proved that leaf hairs of many plants contributed actively to the supply of water in the plant.

Since this time but little work of importance has been done, though the question seems farther from being settled now than it was a hundred years ago. Of the later works on the subject, two of them are deserving of mention,—Burt (1893), and Ganong (1894, p. 136). The former concluded that leaves and cut shoots may absorb water, while the latter concluded that leaves do not function as water absorbers to an extent sufficiently great to be worthy of note.

As to the text books on botany and on plant physiology, other than those mentioned, commencing with Pfeffer, one finds that the positions taken, though varied to some extent, are generally and rather uniformly on the side of non-absorption; at least one might say, judging from the attention paid to the matter, it was considered of little or no physiological importance. It is interesting to notice further that of those works, such as Pfeffer (1881) and Detmer (1883), that have been recently revised, there is little modification of the view rather cautiously expressed in the early editions; and, with a proviso or two, practically the same stand is taken in the latest editions as in the first. This is as might be expected, for no work of any importance had been done in this line in the meantime. Sachs, in his "Plant Physiology" (1887) takes the ground that the question has not been at all satisfactorily settled. In Goodale's Physiology almost no attention is given to the matter. Van Tieghem (1884) states that water vapour is readily absorbed by the plant. Vines (1886) gives some attention to the subject, but leaves one to *infer* rather than to *read* his conclusions. He admits that under peculiar circumstances water and dilute solutions may be absorbed, but holds generally to the idea that this, if it be a function at all, is of but little consequence. In both works of Pfeffer scant attention is given the subject, and it is plain that Pfeffer had little, if any, experimental knowledge concerning it, though he cites some of the more important works relating thereto. In the work of Detmer (1883, p. 112) is discussed to some extent the question, with the conclusion, that if the tissue of the leaves is fully turgescent, then there will be no absorption, but if not turgescent, then they are capable of taking in water which may be in contact with the leaf surface. Sorauer (1895, p. 32) states that it is only in cases of extreme dryness that plants are able to make use of the heavy deposits of dew. The view expressed by Haberlandt (1896) is worthy of special notice because he goes into the matter more fully, and because his views stand out somewhat prominently in contrast to those expressed in almost all the other modern text books. He has no doubt whatever that foliage leaves can, and do, absorb water to the advantage of the plant, and he mentions some plants whose leaves function regularly as water-absorbers. In the work of Detmer and Moor (1898) we have the view rather cautiously expressed :—"The question of water-absorption by leaves is not of great physiological interest,"—and but a very few lines are devoted to the subject. In Macdougall's work (1898) it is stated that leaves do not absorb water. Very little is said concerning the question in Strasburger (1900), excepting that in some peculiar cases, as in that of scaly hairs, water may be absorbed. In a recent text book on Plant Physiology, that of Belzung (1900) the question is discussed with the conclusion that water is absorbed as dew, and may be absorbed under other peculiar circumstances.

From this brief summary it may be seen that the subject has had a rather peculiar history, and one which is not without interest from the standpoint of physiology as well as that of economic botany.

The question of the absorption by leaves of solutions introduces a new element into the discussion ; and until quite recently very few experiments of importance relating to the subject have been performed. Whatever work has been done has been from the side of the injurious effects of solutions causing poisoning of the leaves, and even of the whole plant. Boitard (1829) noted that if mist contained saline matter it was injurious to plants ; but no work relating to the question, so far as can be learned, was done until 1872, when R. Angus Smith (1872) was appointed to look into the effect produced upon vegetation in the neighbourhood of chemical works in England. That the fumes from these chemical works did affect the plants injuriously was readily seen, but whether it acted directly upon the leaves or upon the roots was a disputed question. Smith showed pretty clearly that the leaves were affected directly. Schlössing (1874, p. 1700), and Mayer (1874) showed that solutions of ammonium carbonate were absorbed by leaves ; and Schlössing showed that the ammonia thus absorbed increased the growth of the plant. Boussingault (1878) showed that calcium sulphate, potassium phosphate and potassium nitrate may be absorbed by leaves of plants. There is no mention made by Boussingault as to how these substances affected the tissue of the leaf, or whether they proved of advantage or of disadvantage to the leaf of the plant as a living organ or organism.

Cuboni sprayed leaves with lime, and concluded that it increased the growth, if not by the absorption of the solution of lime, then by a stimulation. In Sachs' Lehrbuch reference is made to the work of Boussingault, and Sachs further states that the absorption may be proved by using a soluble lithium salt, then using the flame test to determine the extent of the absorption. Oliver (1893), after investigating the effects of urban fog upon vegetation, showed that certain acid vapours and other substances affected the leaves, injuring the chlorophyll. About this time investigations were made into the effects of the bordeaux mixture (other than as a fungicide) upon plants. Prominent among these investigators were Zimmerman (1893, p. 307), Rumm (1894, p. 445), Galloway (1895), Frank and Krüger (1894, p. 8), and Aderhold (1893). Aderhold concluded that the increase in growth was due to the lime of the bordeaux mixture being absorbed by the roots. Frank and Krüger held that it acted as a stimulus, as did also Rumm, while Zimmerman opposed this view maintaining that the solutions were absorbed directly by the leaves.

It has recently been observed that the application of ether vapour to foliage leaves stimulates the plant to a more rapid development, and horticulturists have taken advantage of this to force plants for the market (Fisher, 1900, p. 283-284). This shows rather clearly that chemical substances applied to the leaves of plants may be made to promote growth as well as to injure the plants. In the work of Lawes and Gilbert (1883), it is shown that certain substances in the air, especially in the neighbourhood of towns and cities, at times, affect injuriously the foliage of plants. Some investigations with this end in view, were recently carried on by Wieler and Hartleb (1900, p. 188) with a view to learn the effects of HCl upon the assimilation of plants.

The question of the destruction of weeds by poisonous chemical substances, has quite recently become a prominent one with experiment stations, and this question has thrown much light upon the subject directly under discussion ; for it has been shown that certain poisonous solutions may kill some plants and actually promote growth in others. Some experiments have been performed with this end in view,—notably Wales (1900), Foulkes (1897) and Bollery (1899). The object of these investigators was to find a solution which when sprayed on the leaves of a grain crop, would kill the weeds and not injure the grain. The weed most troublesome was *Brassica sinapistrum* which occurs so commonly in grain fields. They found that a 2°/₀ solution of $CuSO_4$ would kill the mustard and, not only not injure the wheat or oats, but actually increase the yield at harvest. Other experiments have been performed within the past year, both at Ottawa, Ont., and Guelph, Ont., as well as in England and France, with similar results to those obtained by the authors just mentioned.

The matter of the destruction of weeds in gravel walks and waste places is not so important, though some work has been attempted with this end in view. Jones and Orton (1899) used several substances such as NaCl, $CuSO_4$, arsenic, kerosene, carbolic acid, etc., and found carbolic acid and sodium arsenate the best. Superphosphates were found by Mazières (1899, p. 851) to be very effective in killing some Cruciferous plants, while ammonium sulphate and other salts were tried with varying results.

It will be seen at a glance that this physiological problem is being pressed forward from its economic, rather than from its scientific side ; and that it is only within the last few years that attention has been drawn to the fact that an increase in growth resulted in some cases from the application to leaf surfaces of substances in solution. There are many experiments, if the records are trustworthy, which prove beyond a doubt that a spray of certain solutions may increase the growth of plants ; but there are several views as to the real cause of the increase—(1) it may act as a stimulus pure and simple—(2) it may act upon the soil and through this upon the roots—(3) it may be absorbed as food by the leaves. The experiments described in this paper aim at answering this question.

III.—ABSORPTION OF WATER BY FOLIAGE LEAVES.

THE experiments relating to this subject were designed to determine whether detached leaves and cut shoots could absorb water. Four series of experiments were arranged. For the first series the leaves were collected on June 6th, at 4 p.m., and allowed to wilt until 9.30 a.m., June 7th, when some of them were placed in distilled water and left immersed for twenty-four hours. The others were similarly immersed six hours after the first, and left for eighteen hours (Fig. 2). The second series of experiments was with cut shoots, and was conducted similarly to the first, excepting that no weighings were made. The third was to test absorption by the petiole. The fourth series was designed to test by measurement the amount of water absorbed by a leaf under conditions such as are shown in Fig. 4.

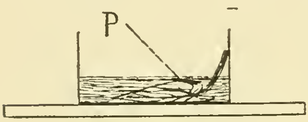

FIG. 2.
Leaf immersed in water, P, a strip of litmus paper.

Immediately after the leaves (series I. and II.) were immersed, a small strip each, of red and of blue litmus paper was laid on each leaf at a point where part of the leaf was close to the surface of the water (Fig. 2). This was to determine if acids or alkaline substances left the leaf tissue to enter the surrounding water, for if such substances did so, it was not unreasonable to suppose that this indicated that the water from the vessel had entered the tissue of the leaf according to the laws of diffusion.

The question of the passage of neutral salts through the epidermal tissue under similar conditions is discussed in Chapters IV. and V.

It is thought that the weighing of the leaves before and after immersion would not of itself decide the question, because it is impossible to know that the leaves are externally in the same condition as when weighed previous to immersion; and, moreover, as the leaves are constantly changing in weight by losing water by evaporation, very accurate quantitative results are not easy to obtain. Since substances are extracted from leaves by the application of distilled water (Chapters IV. and V.), account of this must be taken in the weighings.

In order to bring out more prominently the results it was thought well to use a scale of numbers—12 . . . . 0, the number 10 indicating complete restoration of normal turgidity, and 0 indicating no restoration, and therefore no absorption. The other numbers represent grades of restoration, twelve being greater than normal in the plant, two things being taken into consideration in grading the leaves—(1) the increase or decrease in weight, (2) the visible rigidity.

EXPERIMENTS TO TEST WATER ABSORPTION BY DETACHED LEAVES.

Exper. I.; Time Absorbing 24 Hours.

Leaf.	Reaction.	Turgescence grade by No.	Reaction of water after stirring.	Is water in intercellular spaces?
Napaea	acid	10	acid	no
Rosa acicularis	acid	3	acid	no
Echinops	neut.	12	neut.	no
Polygonum	acid	5	acid	no
Dicentra	neut.	12	acid	no
Clematis	acid very	10*	acid	no
Plantago	alkal.	0	alkal.	no
Silphium	alk., very	10	alk., slightly	some
Inula	neut.	10	sl., acid	no
Potentilla	neut.	7	acid	some
Nasturtium	neut.	10	acid	no†
Thermopsis	neut.	8	acid	no†
Hydrophyllum	neut.	10	acid	no
Geranium	neut.	10	acid	no
Onopordon	neut.	10	acid	no
Aesculus	alkal.	10	neut.	no
Funkia	acid	7	acid	no
Viola	alkal.	6	acid	some
Helianthus	alkal.	9	acid	no
Rudbeckia	alkal.	9	acid	no
Serratula	alkal.	9	acid	no
Tilia	neut.	8	sl., acid	considerable
Poterium	acid	10	acid	no
Silphium	alkal.	10	neut.	no
Xanthorrhiza	alkal.	8	acid	no

Exper. II.; Time Absorbing 18 Hours.

Zizia	acid	9	acid	no
Saxifraga	acid	9	acid	no
Quercus	acid	5	acid	some
Convallaria	acid	9	acid	no
Epidendrum	neut.	10	neut.	no
Acer	neut.	8	neut.	no

* Branch with six leaves more turgid than either of the two separate ones.

† The cut end of the petiole had dipped under the surface of the water.

EXPERIMENT TO TEST WATER ABSORPTION BY DETACHED LEAVES BY USING THE BALANCE TO DETERMINE THE INCREASE IN AMOUNT.

EXPER. IA.; TIME ABSORBING 24 HOURS.

Leaf.	Weight in grams.	
	Weight Wilted.	Weight after Immersion.
Napaea	.8632	.9821
Rosa	.634	.6372
Echinops	.512	.711
Polygonum	.6501	.698
Dicentra	.3213	.4381
Clematis	.6318	.7316
Plantago	1.9316	1.930(loss)
Silphium	3.218	3.9311
Inula	6.8321	7.1214
Potentilla	.3619	.3922
Nasturtium	.9876	1.0830
Thermopsis	.3216	.3321
Hydrophyllum	.5625	.5834
Geranium	1.1230	1.6624
Onopordon	2.1123	2.9106
Aesculus	5.2314	7.1216
Funkia	.3101	.3233
Viola	.7123	.7864
Helianthus	.8916	.9126
Rudbeckia	.7328	.8611
Serratula	.8102	.9386
Tilia	1.6381	2.1111
Poterium	.7216	.8014
Silphium	1.1236	1.2116
Xanthorriza	1.5128	1.5812

EXPER. IIA.; TIME ABSORBING 18 HOURS.

Leaf.	Weight in grams.	
	Weight Wilted.	Weight after Immersion.
Zizia	.5138	.5812
Saxifraga	1.213	1.7126
Quercus	1.3261	1.8120
Convallaria	1.1206	2.101
Epidendrum	.8162	.9613
Acer	1.2136	1.3026

From these experiments there is little room for doubt that the leaves had absorbed water, which resulted in a renewal of turgor, to a greater or less extent, and which produced a substantial increase in weight. Before weighing the leaves—upon taking them from the water,—they were dried between sheets of absorbent paper for a couple of minutes, but, as was said before, it was difficult to know just when the surface water was dried off, and in some cases this was evident in the abnormal increase in weight. Experiments with cut branches were arranged in a similar manner, and the results recorded show a condition similar to that recorded in the table given above, but no weighings were made for two reasons, (1) the branches being large and cumbersome could not be weighed with a fine and delicate balance, and if weighed with an ordinary pulp balance, accurate results could not be expected ; (2) if turgor be restored wholly or in part, this can easily be recognized by the general appearance, especially so in the case of cut shoots.

Branches of the following plants were tested :—Rosa, Polygonum, Clematis, Potentilla, Zizia, Acer, Dicentra, Hydrophyllum, Quercus and Tilia; and as the results were practically the same as those given concerning single leaves, it was deemed unnecessary to give a record of the special observations. In all cases care was taken to have the cut end of the shoot *not* immersed in the water. It is clear that, had the cut end of the shoot touched the surface of the water, or dipped below it, the value of the experiment would have been destroyed.

The water in which these leaves were immersed was found to have acquired some substances from the tissue of the leaves, as shown by the litmus indicator. Since leaves lose substance to the surrounding water, weighing of the leaf alone will not determine increase due to absorption of water. It was partly because of this fact that the weighings were correlated with other phenomena in estimating the grade given in column three. The restoration of turgor in cut shoots was more readily determined because the smaller branches, the petioles, as well as the blades of the leaves, aided in making comparisons. These results as tabulated are corroborated by Cailletet, Boussingault and Henslow, who used slightly different methods.

Duchartre opposed these views by saying that leaves and cut shoots do not function as living plants, and that they may be compared to the detached limb of an animal. It is not difficult to see, that for purposes of comparison, there is little similarity. His chief experiments were performed with plants in flower-pots, and in consequence he had to use a coarse balance for making his weighings.

He gives these records :—

Weight of plant in evening	1730.6	grams.
Weight next morning (six o'clock)	1733.2	"
Weight after wiping leaves	1730.8	"
Weight of plant in evening	1677	"
Weight next morning (six o'clock)	1679.4	"
Weight next morning (nine o'clock)	1677	"

One can see that the weighings could not have been very accurate, and the differences given prove little or nothing either way, as they are scarcely beyond the margin of error in using coarse balances as indicated by those figures. When one examines Duchartre's work, he is more and more struck with surprise at the prominence given to it, in face of the work done by such men as Cailletet, Boehm, Henslow and others. It is not so much an examination into the writings of these men, as it is a study of their experiments, that carries conviction.

In order to supplement the experiments to demonstrate the capability of leaves to absorb water, an apparatus was arranged somewhat after the method of Bonnet (Fig. 3). Leaves with petioles of considerable length were selected and placed with the petioles in the form of a "U" dipping into a vessel of water. The blades and the cut ends were exposed to the air. Leaves placed under these conditions remained green and turgid much longer than leaves exposed wholly to the air. Several kinds of controls were arranged to support this experiment. This indicates that leaves can absorb water through the surface of the petiole.

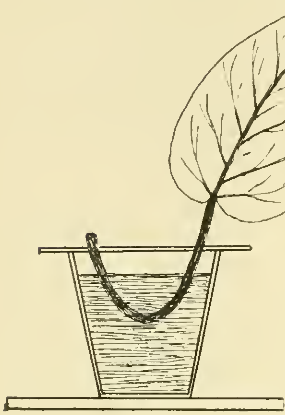

FIG. 3.

Leaf with petiole immersed in water.

A point brought out, during the course of these experiments, was that leaves, *e.g.* Primula, possessing trichomes, seemed to absorb through the petiole more to the advantage of the leaf than did those without trichomes.

In order to enquire into the matter more fully, an apparatus was arranged as shown in Fig. 4, where the capability of a leaf to absorb water in one surface and transmit it to the other is tested. Leaves of

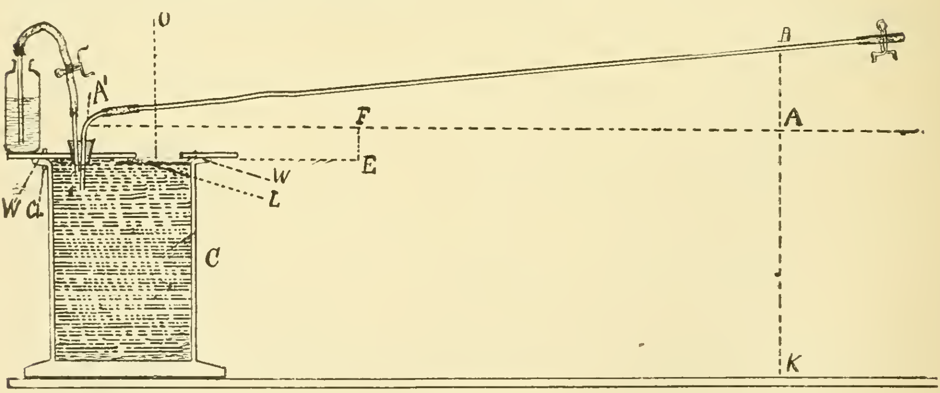

FIG. 4.

Apparatus for testing the power of absorption and transpiration of leaves. C, a tank containing water or a solution. W W, a piece of plate glass ground to fit the edge of the tank C. O, an aluminum gauze. L, a leaf. Cl, a clamp. D, a jar from which the tank can be supplied from time to time. EF, represents the height of liquid due to capillarity. AB represents the amount of pressure up against the leaf L. The margin of the jar is first smeared with grafting wax. As the water evaporates through O the liquid flows towards the left from B, and can be measured readily. When it has reached A' open the clamp on the tube at D and suck the end B until the water comes to the desired point, then close the clamps at B and D; then open clamp at B. BA may be made long or short to increase or decrease the pressure as may be required.

Acer, Ampelopsis, Liriodendron and Tilia were tested, first with the lower surface in contact with the water, and then with the upper surface in contact. It was found that in the case of Tilia it was with the greatest difficulty that leaves free from small holes could be obtained, and that when one was found, apparently without holes, there was observed a very rapid transmission of water through the leaf, and it was necessary to elevate and lower the measuring tube with the greatest care so as to keep the liquid in contact with the leaf and yet to have no pressure against its surface. With the leaf of the Liriodendron the transmission was very slow, and several centimeters pressure did not materially hasten it. During the course of the experiments it was noticed that the humidity of the atmosphere affected the rate of transmission of water. There was a decrease in the rate of transmission of water, associated with a decrease in the humidity of the atmosphere, as indicated by the psychrometer. This was also the case when filter paper was in the position of the leaf, but there was in no instance an increase in volume as resulted with the leaves.

The records given in the following table show the change in volume of liquid in the apparatus to which the leaf was fixed, the temperature of the room, and the relative humidity of the atmosphere. The measurements upon the horizontal tube of the apparatus are made from a fixed point, so that the figures in the column under "distance" at once indicate an increase or decrease in the volume of liquid in the tank.

I.

Date.	Time. (o'clock).	*Distance.	Temperature.	Humidity.
June 21	4.50	772	74	75
" 22	8.45	731	75	93
" 22	1.00	678	73	85
" 22	4.35	703	77	82

II.

Date.	Time. (o'clock).	*Distance.	Temperature.	Humidity.
June 24	1.20	125	73	73
" 25	8.35	150	68	75
" 25	12.20	78	72	80
" 25	12.55	53	72	80
" 25	2.25	6	73	81
" 25	5.00	56	73	85

III.

Date.	Time. (o'clock)	*Distance.	Temperature.	Humidity.
June 27	8.30	197	71	76
" 27	11.45	159	72	79
" 27	2.00	34	76	76
" 27	2.05	68†	76	76
" 27	3.20	19	76	76
" 27	4 20	56	77	80
" 27	5.10	75	75	80
" 28	8.30	141	72	80

IV.

Date.	Time. (o'clock)	*Distance.	Temperature.	Humidity.
June 22	4.35	703	77	82
" 23	8.10	995	71	79
" 23	10.00	1025	74	74
" 23	12.40	1019	74	77

* The larger number denotes DECREASE, and the smaller INCREASE.

† Pressure raised to ten inches for five minutes.

V.					VI.				
Date.	Time. (o'clock).	*Distance.	Temp-erature.	Humi-dity.	Date.	Time. (o'clock).	*Distance.	Temp-erature.	Humi-dity.
June 25	5.00	56	73	85	June 22	8.45	772	75	93
" 26	8.35	212	68	75	" 22	1.00	712	73	85
" 26	1.00	175	70	76	" 22	2.40	691	76	80
" 26	2.20	128	73	77	" 22	4.35	691	77	82
" 26	4.10	41	72	81	" 23	8.10	995	71	79
" 26	5.10	28	72	81					

I. Liriodendron leaf; upper side up; pressure four inches; diag. VII.
II. " " lower " " " " "
III. Ampelopsis " lower " " " " "
IV. " " upper " " " " "
V. Acer " lower " " " " "
VI. Acer " upper " " " " "

SUMMARY OF MEASUREMENTS.

UPPER SIDE OF LEAF UPPERMOST.

Exper.	Day.	Night.
1	28 mm. (increase in tank).	41 mm. (increase in tank).
4	—24 (decrease in tank).	—292 (decrease in tank).
6	81 (increase in tank).	—304 " "

LOWER SIDE OF LEAF UPPERMOST.

2	206 mm. (increase).	—25 mm. (decrease).
3	272 "	—216 "
5	184 "	—286 "

NUMBER OF STOMATA PER SQUARE mm.

Leaf.	Upper Surface.	Lower Surface.	Average.
Acer, (5 and 6)................	0	300–350	325
Ampelopsis, (3 and 4)............	0	80–120	100
Liriodendron, (1 and 2)...........	0	180–220	200

* The larger number denotes DECREASE, and the smaller INCREASE.

These experiments are difficult to conduct, and several had to be discarded owing to defects in the leaves and to the entrance of air when filling the apparatus. It was thought that a living leaf would act, in a measure, just as a paper would in the same position, but it was found that there was no important point in common between them. The loss of water from the jar, over which the paper was placed, was uniform and constant, this being shown by the movement of water along the horizontal tube. The number in the column under "distance" indicates a distance from the fixed point placed out towards the open end of the tube, so that the larger number indicates a diminished volume of liquid in the apparatus, and the smaller number an increased volume. The diameter of the tube was such that 10 cm. in length indicated 1 cub. cm. of water.

From these experiments we may conclude that the leaves used, absorbed water, as vapour from the air, and as liquid from the tank. There was generally a loss of water from the tank during the night and a gain during the day. The increase in amount in the tank during the day was much greater when the lower side of the leaf was exposed to the air.

In regard to changes occurring at night, there seemed to be little difference, whether the leaves had their upper side up, or their lower side up. During the day, however, there was a remarkable difference.

The table showing the relative numbers of stomata is given, though no application is made of it further than to show that the stomata are found upon the lower surface only.

Filter paper, placed in the position of the leaf in the experiment, produced a steady decrease in amount of water in the tank.

The question of water absorption by *attached* leaves is not so easily dealt with. In an experiment with willow twigs it is shown that water, as well as a nutrient solution, may be absorbed by developing leaves. (See exper. Chapter X.).

Some plants, *e.g.*, Ampelopsis, (Fig. 11), have certain peculiarities of leaf structure which seem to indicate an adaptation for absorption. Such are the corrugations over the veins and in the regions of the stomata. It may be that the striations around the bases of the trichomes in Primula (Fig. 5, 1) serve a similar purpose. The tissue next the epidermis over the veins in Ampelopsis is composed of thin-walled parenchyma cells without chlorophyll (Fig. 10, H). Just how far one is justified in reasoning from anatomy to function is not easy to say. These anatomical conditions are not mentioned as proof, but as evidence in favour of absorption.

Stahl has endeavoured to show that corrugations and hairs over the veins, aid in shedding water, but this does not accord with the results of the experiments performed by the writer and recorded in Chapter VII. The hairs along the veins, by capillary action, cause solutions to ascend the petiole and pass out over the veins to such an extent that the leaf becomes coated with salt when the water evaporates. Henslow (1888) and Garreau (1851) held that such corrugations and hairs over the veins aided in absorption, as well as in spreading drops of water over the leaf.

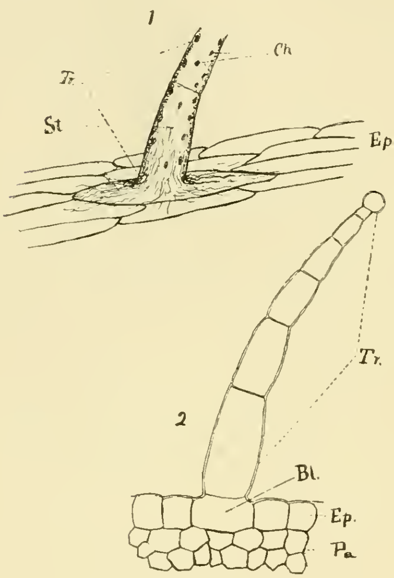

FIG. 5.

(1). A semi-perspective view of a trichome of Primula stellata, partially laid over towards one side showing trichome Tr. in part; Chloroplasts, ch.; striations, St.; surface view of epidermal cells, Ep.

(2). Optical section of a trichome Tr.; part view of cross section of petiole showing basal cell Bl. of trichome, also epidermal layer and parenchyma Pa.

It is shown elsewhere in this paper that solutions are absorbed by leaves even when the plant is in a saturated atmosphere (Chapters VI. and VII.).

The evidences in support of water absorption by leaves upon the plant may be summarized as follows :—*Detached* leaves absorb water, and since they function, in a measure, as when attached to the living plant (Chapter VII.), it may be concluded that *attached* leaves absorb. Dilute solutions are absorbed, and therefore water *may be* absorbed. Certain anatomical structures make it seem probable. Since distilled water will extract inorganic salts from leaves, it follows that water may enter the tissue during the process. The historical evidence is overwhelmingly in favour of absorption. Since substances contained in water of guttation and in dew, are resorbed by leaves under certain conditions, it seems probable that water may be absorbed. This last mentioned point will be discussed in detail in the following chapter.

IV.—INCRUSTATIONS, GUTTATION DROPS, DEW.

Certain plants, belonging to the orders *Saxifragaceæ* and *Plumbaginaceæ*, are frequently, when under natural conditions, found with incrustations upon the leaves, and the chief object of this chapter is to make clear, if possible, the cause and the function of these peculiar deposits of lime and other substances upon foliage leaves.

The chemical composition of this deposit has been examined by several botanists, notably Treviranus, Mohl (1861, p. 227) and Volkens (1884), and they all agree that the inorganic part of the deposit is composed almost wholly of $CaCO_3$. They found also a certain amount of organic matter associated with it, but this they did not analyze in detail.

To see if this deposit could be produced artificially, dew-drops were taken in early morning from time to time, from leaves of the following plants,—Trapæolum, Lilac, grass, Mentha, and Polygonum, and placed upon clean cover-glasses. The cover-glass being now kept free from dust, the dew was allowed to evaporate. When the drop had evaporated, a deposit of a whitish crystalline substance remained, showing clearly that the dew-drop had held in solution some salts. Upon carefully heating the cover, and then examining with a lens or microscope, one could see that a certain amount of charred substance had been produced by the heating. This charring indicates the presence of some organic substance. The inorganic portion is soluble in dilute HCl, with liberation of CO_2, showing that a carbonate is present. The quantities were too small to permit of a further test for the base or bases in solution, but as considerable of this substance is re-dissolved in distilled water, there arises the suggestion that it is not all $CaCO_3$, but very probably largely potassium carbonate with potassium oxalate.

Since the dew is, in part at least, formed by the condensation of aqueous vapour of the atmosphere, the question arises as to the source of the salts which were deposited when the drops were evaporated. Did they come from the tissue of the leaf or were they derived from the atmosphere? To answer this question a series of experiments was conducted to determine whether similar substances could be *extracted* from leaves by the application of distilled water.

Leaves were gathered on December 12th, and after two hours placed in distilled water in positions as shown in Fig. 2. In no case did the cut end of the petiole dip beneath the surface of the water. The leaves had lost almost nothing by transpiration during the time intervening between gathering and placing in the dish of water.

Leaf.	Reaction, 24 hours.	Reaction, 144 hours.
Pelargonium	Alkal	Strongly alkal.
Abutilon	"	Alk.
Rosa	"	Strongly alk.
Eupatorium	"	Acid.
Crassula	Weakly alk	Alk.
Heliotropium	" "	Strongly alk.
Begonia	Neutral	Slightly alk.
Panicum	Alk.	Alk. especially at margin.
Impatiens.	Weakly alk	Acid.
Arabis.	" "	Alk.
Primula st	Neutral	Acid.
Primula ob.	"	Strongly alkal.
Tropæolum	"	On leaf, acid, water alk.

The water was then allowed to evaporate, which it did in a period of about ten days. The dishes were protected from dust by placing a tray loosely over the top.

The water had not penetrated into the intercellular spaces of the leaves to any noticeable extent. The amount of substance given off by the leaves was considerable in every case, and in some cases was quite remarkable. When the water had all evaporated from the dishes, and the leaves were removed, a beautiful white "print" of the leaf was left upon the bottom of the glass dish. This "print" was deeper at the margin of the leaf, and was composed of feathery white crystals. In the case of three or four of the leaves used, the surface in the neighbourhood of the veins was not in actual contact with the bottom of the dish. Here water lay during the experiment, and in this region there was a prominent accumulation of crystals. Where the leaf surface actually touched the glass there was little or no deposit. This would remove all doubt as to the possibility of dust particles or gases from the air having anything to do with the crystalline deposit. The alkaline or the acid quality of the liquid was peculiar, in that the results in twenty-four hours differ from those in six days, as shown in the table. This is probably due to chemical changes peculiar to the material composing the cell sap in each case; and also due, perhaps, to different degrees of diffusibility of the substances entering the surrounding water from the leaf; and it may have been due to bacteria. In all cases the juice compressed from similar leaves, at the time of gathering from the plant, and twenty-four hours afterwards, was decidedly acid. Then it is fair to conclude that the leaf juices, or cell sap, did not pass out by mechanical means, or by mere filtration, for then the surrounding water would be acid. This throws some light on the question as to why Duchartre, De Candolle, Ganong and others reached the conclusion that plants, as growing plants, could not absorb water through the leaves. The fact was, that in the washing, spraying or drenching of the leaves, some of the cell contents had been taken out into the water by osmosis; and so naturally in the resulting weight there would be a slight decrease owing to this loss of substance, though at the same time there may have been water absorbed which the balances could not show,—nay, there *must* have been if the water used for drenching was pure water.

That leaves when immersed in distilled water lose a considerable amount of substance, was proved by De Saussure (1805) who made some analyses to determine, not only the nature of the substance extracted by the water, but also the amount actually taken out. He collected some leaves of Corylus on May 1st and found that they yielded upon analysis 26 per cent. of dissolved salts which were mostly alkaline. Similar leaves, after being submerged for fifteen minutes in distilled water, yielded only 8.2 per cent. of dissolved salts. The phosphates, he found, were not perceptibly affected by the drenching. De Saussure does not give any details of his analysis of the salt taken out of the leaf by the water, beyond that it is a combination of alkaline salts,—that is to say that they are salts of potassium, calcium, and, it may be sodium. The writer has also found that this substance extracted by water is composed of potassium and calcium carbonates and potassium oxalate, with traces of organic substance. Hence it is found that the residues from the evaporation of the dew-drops, and of this liquid in which the leaves were submerged are practically the same.

According to Van Tieghem (1898, p. 313) the liquid found upon plants in early morning contains in solution calcium bicarbonate in considerable quantity. This is the calcium compound absorbed by the roots of plants, according to Roux (1900, p. 331). As it is a very unstable compound, breaking down readily into $CaCO_3$, CO_2 and H_2O, it is only reasonable to suppose that it is largely through this bicarbonate that the carbonate is found upon leaves in the form of incrustations. From this decomposition CO_2 may be furnished to the plant. It may be that the chemical action resulting in the formation of calcium oxalate in the leaves of some plants, and calcium carbonate in others, results also in a liberation of CO_2.

It has been observed by (Senebier t. 3, p. 98) Morozzo, that dew-drops, remaining upon the leaves until late in the morning, have an acid reaction upon test paper, and that this reaction is due to CO_2 contained in the dew-drop. Senebier analyzed dew and found that when treated with lime-water it gave a flocculent precipitate, which when tested with H_2SO_4 produced an effervescence of CO_2. He concluded, however, that the dew-drop is always a deposition from the atmosphere, and that it occurs in drops for the same reason that water spread upon an oiled surface will collect in drops. He collected dew in large quantity and made some analyses which are rather striking. From 3791 kilos of filtered dew, he obtained 2276 grams of a solid as a residue from the evaporation of the water, and, after treating this residue with alcohol, then filtering, he obtained as a solid 603.74 milligrams; on dissolving this in acetic acid, he obtained 421.29 milligrams of an insoluble white substance which he concluded was $CaSO_4$. From these results we may conclude that, from the amount of dew he collected, he obtained 182.45 milligrams of a carbonate, probably $CaCO_3$. He believed the dew to contain an acid carbonate, as did Van Tieghem. He found also that if he first filtered the dew he got less effervescence of CO_2 when treated with H_2SO_4.

In order to test the effects of a dry atmosphere upon leaves holding dew-drops, a number of leaves were taken in early morning and placed immediately in a dry atmosphere which produced a rapid evaporation of the drops. On examination it was found that a slight deposit of a whitish substance was left upon evaporation. This result showed clearly that a saline substance had been dissolved in the dew-drop. At the time of collecting the leaves for this experiment, other plants of the same species were marked for observation later. In the case of these plants no deposit was found. This experiment was repeated six times during the summer with practically the same result. The substance contained in the dew-drops must have been largely, if not wholly, extracted from the leaves of the plants. These experiments indicate also, that under favourable circumstances, leaves resorb the saline substance contained in the dew; and there is a suggestion also that some of the dew-water may be absorbed in the process.

As the dew experiments seemed to indicate that saline substances were extracted from leaves, and also that under certain conditions these salts were resorbed, a series of experiments was arranged to test the matter from another point of view.

Guttation drops were produced upon the following plants:—Tropæolum, Maize, Tomato and Phaseolus. Five leaves of each were then placed in a moist atmosphere, and five of each in an atmosphere whose humidity was very low. When the water of the guttation drops had disappeared it was found, upon close examination, that a whitish deposit lay where the larger drops were upon the leaves in the dry atmosphere. Upon those lying in the moist chamber no deposit was found.

These drops contained, as shown by analysis, potassium carbonate, calcium carbonate and some organic substances. This analysis is corroborated in part by that of Nestler (1899) who states that he found potassium carbonate in drops produced upon leaves of Phaseolus, and on some of the *Malvaceæ*.

In order to determine whether a substance similar to that produced by immersion could be extracted in a shorter time than that employed in the foregoing experiments, a number of tests were made with growing plants. Ten different species were taken and the leaves subjected to a fine spray of distilled water for fifteen minutes. The water was then carefully collected and slowly evaporated down to dryness. In six out of the ten cases, a faint crystalline deposit was found upon evaporation. In one case, that of Nicotiana, a very considerable amount was found. This plant was one of those which showed with the distilled water, a strong alkaline reaction in a short time. When the leaves of the other three plants were tested as in the first series they also produced a deposit after a short time.

In regard to the calcareous incrustations found upon desert plants, Volkens calls attention to the fact that they occur chiefly upon desert plants which grow upon soil which was once the bottom of an inland sea, and which, therefore, contains a considerable amount of lime. It may be said that two things particularly contribute to their formation,—abundance of material, and an atmosphere periodically moist and dry. They are spread over the surfaces of leaves, according to Nestler, and also Noll, by means of hairs and corrugations, leaving no indication as to the place upon the leaf from which they came. The writer has noticed leaves of plants, other than those producing incrustations, having peculiar striations and trichomes which may function, as Nestler suggested, to transport water or solutions over the surface of the leaf (Fig. 10).

The supply of calcareous substance is furnished by the roots of plants in the form of the bicarbonate of calcium (Roux 1900). Volkens (1884) states that this bicarbonate of calcium is the chief saline substance contained in the guttation drop. This is in accordance with analyses made by the writer, and the view held by Van Tieghem. It is well known that one of the commonest substances found in spring water is calcium bicarbonate, and Roux states that the CO_2 given off by the roots aids in its formation. There is every reason to suppose, then, that the plant obtains its calcium chiefly from this substance. It is also well known that $CaCO_3$, in the presence of CO_2 and water, forms the soluble bicarbonate thus :—$CaCO_3 + CO_2 + H_2O = CaH_2(CO_3)_2$; but, as this compound is very unstable, it breaks down again if the conditions be seriously disturbed. These interactions may be, therefore, important in plant economy.

The function of incrustations are (Volkens, 1884) :—(1) to keep up an equilibrium between absorption of the roots and transpiration of the leaves ; (2) in the excretion of useless and harmful products ; (3) to prevent too rapid evaporation in a dry hot climate. Pfeffer (1897) states that the function may be to induce an abundant formation of dew. Since calcium bicarbonate is a very unstable compound, breaking down into $CaCO_3$, CO_2 and H_2O, it may be, that in the formation of the deposit of $CaCO_3$, a source of supply of CO_2 is suggested. The roots take in the bicarbonate and it is found upon leaves of plants in the morning. In the early morning when photosynthesis is becoming active this bicarbonate begins to break down, resulting in a liberation of CO_2 which is then in demand by the plant. From these data the writer assigns another probable function to these incrustations, namely, that of furnishing CO_2 to the plant.

Since analyses show that the deposits from the dew-drop, the guttation drop, and the water of immersion or drenching, are similar to those in the calcareous incrustations, one may infer that the causes of formation are similar. As the process involved in the formation is one of diffusion, the loss or gain to leaves will depend upon relations existing between internal and external conditions.

In summarizing the results of these discussions and experiments, we may say that the residue obtained from the evaporation of dew-drops, guttation drops, and of the water used in drenching leaves, is practically the same. This residue is similar in chemical composition to that of the calcareous incrustations found upon certain Saxifrages and other plants. The relative proportions of the constituents, however, are different. In the calcareous incrustations, the quantity of calcium carbonate is much more pronounced than is the case with dew and guttation water. Under certain conditions guttation water and dew-drops are absorbed by leaves, leaving no deposit of saline matter on the surface of the leaf. When the evaporation is rapid a deposit is found. When the drop contains calcium bicarbonate in solution, carbon dioxide is liberated during the process of evaporation. It may be that in desert countries the calcareous incrustations, in the presence of moisture during the night, serve the purpose of retaining the CO_2 given out in respiration. Owing to lack of decomposition of vegetable matter there is a low percentage of CO_2 in the air in deserts. This might indicate an economy of some importance to the plant.

These results throw some light upon the question of water-absorption, and suggest something in regard to the nature and the cause of such absorption.

V.—DOES DISTILLED WATER BECOME ALKALINE WHEN PLACED UPON LEAVES OF PLANTS?

In Sachs' Pflanzen Physiologie (1882, p. 305) he states that if distilled water be placed upon the leaves of plants for a few minutes, it becomes alkaline, and he refers to a paper of his own, (1862, p. 259), upon "The acid, alkaline and neutral reaction of the cell-sap of plants." The subject is merely referred to in his paper, Sachs himself having made no direct investigation into this particular point. The conclusions he draws are based upon work done by Payen and Gaudichaud. These two men entered into a warm discussion, in which Payen held that an alkaline reaction is produced by the leaf, and Gaudichaud showed that the sap of plants in general is acid and rarely, if ever, alkaline. He argued further, saying that the few particular cases mentioned by Payen were irrelevant ; and as no further reply was given by Payen, the matter stood thus for a considerable time. However, in looking into Payen's work (1848), one finds that he saw far more in the subject than Gaudichaud gave him credit for ; and also that he had the best of the argument as Gaudichaud recognized later on. Payen gave also in connection, some analyses which are interesting. In one of these he found upon evaporating the water taken from Mesembryanthemum crystallinum, crystals of potassium oxalate. A quotation from Gaudichaud, (1848, p. 35), gives his position upon the question. "Toutes les autres plantes que j'ai observées depuis par ce moyen même Urticées se sont montrées acides dans le même éspace de temps. On sait que l'eau de ces sortes de macérations devient promptement alkaline. * * *

"Je coupai deux ou trois branches de cette singulier plante, et les plaçai dans un verre avec un peu d'eau. Le lendemain matin en poursuivant mes recherches la pensée me vint d'essayer aussi de l'eau du verre où avaient séjourné ces branches et je fus très surpris de voir le rouge passer au bleu."

Later on Gaudichaud made other experiments by placing distilled water upon leaves of many plants, and found an alkaline reaction. Two of the plants he mentioned are Trapaeolun majus and Cucurbita. He accounted for this alkalinity by saying that the alkaline substances diffused out more readily than the others. This agrees with De Saussure's results, which Gaudichaud gives, and which are already referred to in the chapter on incrustations. That alkaline substances diffuse out into the surrounding water, seemed to be quite clear to Gaudichaud.

In the paper, just referred to, by Sachs, he shows that there is generally an alkaline reaction in the sap in the conducting vessels (stem and roots) of the wood, while the reaction of the parenchyma is generally acid. He says :—

"Die beschriebenen Fälle ziegen, dass Payen's und Gaudichaud's Ansicht, als ob alkalische Säfte nur in "specifischen" Zellen einiger "exceptionellen" Pflanzen vorkamen, nicht gerechtfertigt ist, dass vielmehr die alkalischen Säfte in einer grossen Zahl unserer gemeinen Culturpflanzen neben sauren Säften vorkommen ; und zwar ziegen die vorstehenden Untersuchungen, dass gerade diejenigen Säfte vorzugsweise alkalisch sind, denen wir eine hohe Wichtigkeit für das Leben der Pflanzen nicht absprechen dürfen, nämlich in den dünnwandigen Zellen, welche bei vollständig ausgebildsten Gefässbündelen krauter Pflanzentheile zwischen dem Baste und den Gefässröhren liegen. Dass gerade diese dünnwandigen Zellen die wesentlichsten Elemente der Gefässbündel darstellen, darf zunächst aus dem Umstande gefolgert werden, das dieselben in den Gefässbündelen lebenskräftiger Theile wie es scheint, niemals fehlen. Es sind offenbar diese dünnwandigen Elemente der Gefässbündel, welche auch bei solchen Familien der Gefässpflanzen schon auftreten, wo eigentliche Gefässe und Bastzellen noch mangelen, und während in den äussersten endigungen der Gefässbündel der Blattnerven höherer Pflanzen der Bast und die Gefässe beinahe oder ganz aufhören, bilden die Leitzellenbündel die äussersten Endigungen."

This gives the location of the substance which produces the acid and the alkaline reactions respectively. This also lays the foundation for a reasonable explanation for the phenomena chiefly under discussion in this chapter.

The nature of the substance diffusing out through the leaf tissue, to cause this alkaline reaction, is discussed in more detail in the foregoing chapter. Several phenomena developed during the course of the following experiment, which are interesting and important. On examining the recorded details of the experiments, one notices first that the time required to cause a sufficient change of colour of the litmus is an appreciable interval, and it varies widely with different plants, as might be expected. The time required to cause a distinctly alkaline reaction of the water applied to the surface, depends upon the permeability of the cell walls, especially upon that of the cutin upon the epidermal layer, upon the diffusibility of the salts extracted by the external water, and directly upon the readiness with which the acid contents of the cells make their way out and neutralize the alkaline substances taken from the tissue by diffusion. The CO_2 in the atmosphere, and in and about the leaf surface, is no unimportant factor in determining the colour of the test paper. As is shown in the preceding chapter the substance which diffuses out is largely K_2CO_3, $CaH_2(CO_3)_2$, and probably some potassium oxalate. Two of these substances, potassium carbonate and potassium oxalate, have a reaction rather strongly alkaline, while the other is slightly acid to litmus test paper. If the last-mentioned salt ($CaH_2(CO_3)_2$) predominate strongly, there will be a weakly acid reaction, as is shown in some plants. This substance, being so very unstable, breaks down (upon evaporation of the solution), into $CaCO_3$, CO_2 and H_2O, leaving as a residue the carbonate of lime. If, however, water is present and more CO_2 available for absorption into the solution, it would become gradually more and more acid, as is shown in the results of the experiment.

One very important difficulty in the way of success in demonstrating this phenomenon, is that reddened litmus paper will often become slightly blue if placed in distilled water which is allowed to evaporate down to dryness. That there might be some slight action between the water and the sodium or the potassium of the glass, is barely possible. Small quantities of ammonia in the air may have some effect. An experiment was performed to test this phenomenon. Two well-cleaned panes of glass, 160 by 210 mm, were placed face to face together, with a few drops of distilled water and a few strips of red litmus paper between. Owing to the adhesion of the water for the glass and the slow evaporation, the water remained there several days, and the colouring of the paper might be seen readily at any time without disturbing the plates. One could hardly say that atmospheric conditions had much to do, under these circumstances, with giving the water between the plates any quality, either acid or alkaline. The strips of paper all turned slightly blue, showing that the water and glass had probably something to do with the change. This change took place long before the water had evaporated, showing that the evaporating down to dryness was no important factor, though, so far as is known to the writer, this is the only reason assigned for the change.

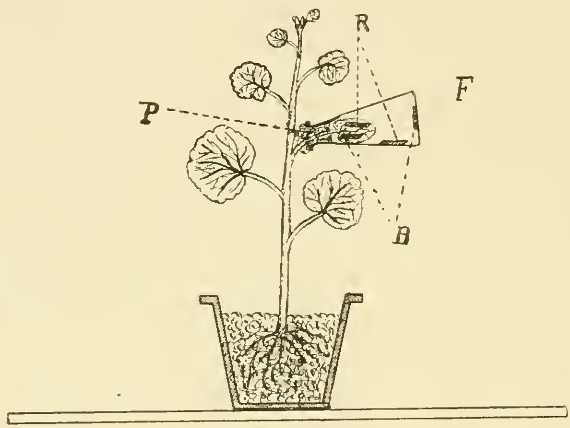

FIG. 6.

F, is a flask enclosing a leaf and kept in position by a support. P, a plug of cotton in the flask. R & B, red and blue litmus paper strips.

Experiment to test the acid or alkaline quality of distilled water which has been allowed to stand upon a living leaf for some time. Plant used, Helianthus. Plant placed under a bell-jar.

I.—*December 15th, 1900.*

3 strips red litmus paper placed on glass slide under jar and moistened, became blue in 24 hours; dry.

2 strips red litmus paper placed on the inside of jar; moist; no change.

3 strips (red) placed upon leaves touching jar; moist; turned blue.

3 strips (red) placed upon leaves not touching jar; dry; no change.

II.—*December 16th.* Observations made in 24 hours; reddened litmus paper used.

4 strips	on glass slide, moist, under jar . . .	*Reaction*,	bluish.
4 "	touching inside surface of jar, moist.	"	3 red; 1 (dry) blue.
4 "	on leaf touching jar, moist	"	bluish.
4 "	in beaker of dist. water under jar. . .	"	red.
4 "	on slide, moist, under beaker not with plant.	"	bluish.
4 "	on inside moist beaker inverted.	"	bluish (dry).
4 "	on under side of leaf, moist.	"	blue.

III.—*December 17th.* Observations made in 24 hours.

2r. 2b., under jar in beaker................	*Reaction*,	2r. and 2 faded.
1r. 1b., under jar on moist slide............	"	1 bluish one blue, (dry)
3b. 4r., touching inside moist jar...........	"	3b. 4r.
2b. 2r., under side of leaf..................	"	2b. 2 bluish.
3r., on upper side of leaf touching jar......	"	3 bluish.
1r., on upper side of leaf not touching jar...	"	1 bluish.
1r. 1b., under beaker in beaker............	"	1r. 1r.
2r., on moist slide under beaker	"	2 bluish, (dry).
1b., under cover glass under beaker........	"	blue.

IV.

	Time.	Reaction.
January 25th.—Pelargonium ; plant in the open air. In all cases the litmus paper was first moistened.		
3r. on upper side of leaf	6 hours.	3 blue.
1r. on upper side of leaf as in (Fig. 6)..........	24 hours.	1 reddish.
February 16th. Plant under jar.		
2r. upper side of leaf........	24 hours.	2 bluish.
2b. upper side of leaf	24 hours.	2 blue.
February 21st. Plant in open.		
4b. on upper side of leaf and left for...........	3 days.	4 blue.
February 21st. Plant under jar.		
3b. on upper side of leaf......................	3 days.	3 blue.
January 25th. Nicotiana ; plant in open.		
3r. and 3b. on upper side of leaf...............	6 hours.	6 blue, very.
February 21st. Plant under jar.		
3r. and 3b. on upper side of leaf...............	6 hours.	6 blue.
February 21st. Chrysanthemum ; plant in open.		
3r. and 3b. on upper side of leaf...............	6 hours.	No change.
2r. and 2b. on under side of leaf...............	6 hours.	No change.
February 21st. Plant under jar.		
3r. on upper side of leaf	24 hours.	No change.
January 25th. Solanum ; plant in open.		
3r. and 3b. on upper side of leaf.	6 hours.	3r. and 3b.
2r. and 2b. on leaf as in (Fig. 6).............. .	24 hours.	4 reddish.
January 25th. Capsicum ; plant in open.		
3r. and 3b. on upper side of leaf.............. .	6 hours.	6 blue.
3r. and 3b. on under side of leaf.......	6 hours.	6 blue.
February 21st. Plant under jar.		
3r. and 3b. on upper side of leaf....	24 hours.	6 blue.
3r. and 3b. on upper side of leaf (Fig. 6)........	6 hours.	3 bluish, 3 blue.
3r. and 3b. on upper side of leaf (Fig. 6).	24 hours.	6 reddish.

From these experiments one can see that there is a certain alkaline reaction of the water which had been left upon the leaf surface for a short time. A very dry atmosphere surrounding the leaves will cause such rapid evaporation of the water that there may be no definite reaction either way on some plants. It is not difficult to see that the time required to extract substances in solution osmotically from the leaf tissue will vary with different plants. Both the substance to be diffused and the septum through which diffusion takes place, have to do with the amount diffused in a given time. This process is in accordance with the general laws of diffusion, the solution within the cells on the one side of the septum and the water on the other. All the experiments described show that a substance is actually extracted from the leaf, the time required being different with different plants.

When the plants were placed under bell-jars to reduce in amount the evaporation from the leaf surface they were under different climatic conditions from the surrounding plants in the open. These differences, however, were not of such a nature as to interfere with the progress of the experiment, as is shown by the numerous controls. During the day time there would be, when photosynthesis is active, a diminished amount of CO_2 in the air in the jar; while during the night when photosynthesis is checked, or stopped altogether, and respiration is still going on, there would be an excess of CO_2 in the air of the jar. The action of an excess of CO_2 would be to render the water drops clinging to the sides of the jar of an acid quality.

When the water had evaporated slowly down to dryness upon a glass surface there was always a *slight* alkaline reaction to the litmus paper. The general results of the experiments with the plant Helianthus (exper. I, II, III), are that distilled water became alkaline in twenty-four hours after being placed upon leaves. The same reaction was found whether the water were placed on the upper or on the lower side of the leaf. As the plant was inside a bell-jar, as in experiment I., it was easy to have a leaf touching the moist inside surface of the jar, with a strip of test paper touching both jar and leaf in the presence of water. The strips placed clinging to the inside surface of the moist jar were in fair comparison with that touching leaf and jar, and in the latter case the portion touching the leaf showed the stronger alkaline reaction. This tends to prove that the alkaline reaction is caused by the leaf, and not wholly or in large part by the glass, as was suggested by the writer to have been possible.

Sachs shows (Bot. Zeit. 1862, p. 257) that the substances contained in the conducting vessels in the stem of the plant, in petioles and in veins of a leaf are alkaline. It is therefore possible that this alkaline substance in solution is largely in the same condition as when found in the xylem of the root. So we have in the leaf an alkaline liquid on its way towards the minute tracheids entering into whatever chemical compounds are natural to the leaf. The alkaline substance which diffuses out through the leaf surface to the distilled water is not in all likelihood *wholly* the same as that coursing upward through the conducting vessels, for there diffuses out through the leaf, potassium oxalate, a compound organic in its nature, and one which therefore has had to do with the plant metabolism. Sachs shows that the liquid in the xylem of the roots is alkaline and that this alkaline quality is maintained throughout its course into the leaf.

To sum up, one may conclude from these experiments and from those recorded in Chapters III. and IV., that a substance is extracted from leaves of plants by the application of distilled water, and that this substance gives generally an alkaline reaction. This alkaline reaction is produced largely by compounds of potassium (potassium carbonate and potassium oxalate). If distilled water extracts salts from leaves it may be that rain-water does, and this will result in a loss of substance to the leaf. If the substance be *injurious* to the plant this process might be called a *process of excretion.* The amount which diffuses out differs with different plants, and the alkaline reaction may be masked by the presence of other substances as shown above. It has been shown that, in the case of the potato plant (1895), distilled water when applied to the leaves, acts as a stimulus to growth, and the suggestion is here offered that the stimulus may be a result of the loss of injurious salts which had accumulated in the leaves, the removal of which substances would benefit the plant.

VI.—THE EFFECTS OF A NUTRIENT SOLUTION AND OF DISTILLED WATER UPON LEAVES OF PLANTS.

The experiments here described in detail were designed to extend at intervals over a period of two years, and to have the plant as nearly as possible under natural conditions. It was expected that these investigations would throw some light upon the much disputed question as to whether leaves can absorb water and solutions to the advantage of the plant. Arrangements were made to have the roots of the plant isolated, so to speak, from the atmospheric conditions surrounding the leaves, and to have the roots supplied with nothing but distilled water and air. It was thought that if no food were supplied to the roots, growth could not continue for any extended length of time. If liquid food be supplied to the leaves and a distinct growth results—that is, a growth more

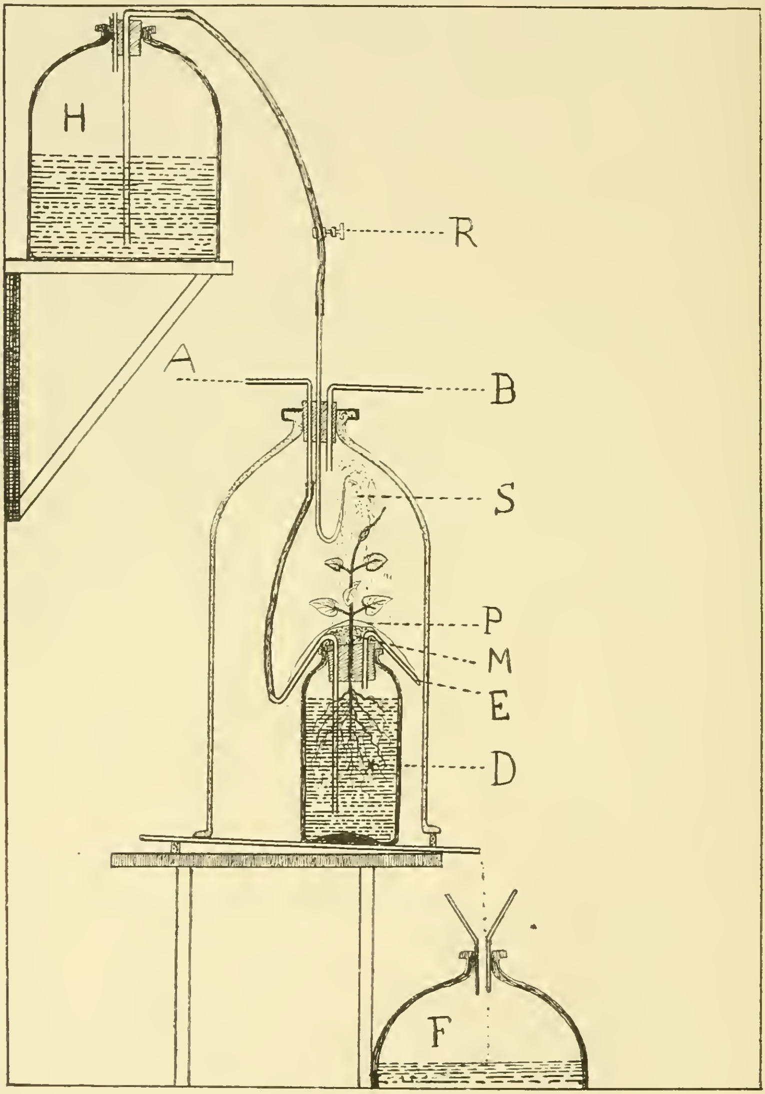

FIG. 7.

H, reservoir for nutrient solution; R, regulating tap; A, tube through which water and air are supplied to the roots; B, ventilator; P, sheet rubber; M, wax; E, exit tube; D, distilled water; S, spray; F, receiver.

pronounced than that in the case of water alone—it is deemed reasonable to conclude that some of the food solution was absorbed to the advantage of the plant. (For illustration of the method used to supply the leaves with food see Fig. 7). It was found expedient to paint the bottles in which the roots were immersed, with black bicycle enamel to protect the roots from the light.

SERIES I.

The first of the following series of experiments was begun on October 13th, 1899, and carried on at the physiological laboratory at the botanic gardens, and is the first of the series to test whether a nutrient solution can be made to support the life of a plant by applying it to the leaves in the form of a spray. The roots were placed in distilled sterilized water and the supply was kept up by means of a system of tubes arranged for the purpose. The corks of the bottles were smeared with a specially prepared soft wax, and above this was placed a piece of sheet rubber, cut so as to go round the stem of the plant in the form of a hollow cone, then cemented in this position so as to shed the liquid used as a spray, and to keep as much as possible of the spray from coming into contact with the wax. The wax served the purpose of doubly securing the liquid at the roots from contamination with the liquid used as a spray. (See Fig. 7).

Plants used—Thunbergia alata.

Plant A.—Roots in distilled water and leaves fed by a spray of nutrient solution (Fig. 7).

Plant B 1.—In distilled water but no spray.

Plant B 2.—Under same condition as B 1.

Plant B 3.—Roots in distilled water, plant under jar and moistened daily.

Plant C.—Control, in flower-pot in soil.

The records of growth in length are for the purpose of inquiring into the manner in which the growth is affected when the plant is placed under these conditions. Measurements are given in millimeters.

PLANT A.

Date.	No. of leaves.	Length of last internodes. Stem.	Branch.	Branch.	Branch.	Branch.	Increase in length.
Oct. 13.	12	..	..	.	..	.	..
17..	24	..	..	..	..	.	..
20..	24	..		..	..	..	..
22..	24	69	19	62	..	.	..
24	26	75	28	9	.	..	37
26..	30	6	6	37	..	..	66
27..	32	6	19	56		..	37
29..	34	6	3	37	..	..	28
31 .	36	6	3	62	..	..	25
Nov. 1..	38	6	3	25	..	..	50
2.	38	6	3	12	..	..	25
5..	40	6	50	25	..	..	69
7..	42	6	28	137	..	..	166
9..	46	6	100	6	..	..	87
11..	49	6	125	6	.	..	69
13 .	51	6	50	3	..	..	25
15.	55	..	12	25	31	25	244
16..	61	..	94	3	3	3	144
17..	61	..	100	19	12	3	69
18	63	..	19	37	19	12	87
19	65	..	50	75	37	25	225
20..	67	..	100	19	78	75	216
22 .	71	.	31	50	37	3	166*
23..	73	..	56	12	62	56	125
25..	81	..	19	3	12	75	150
26.	81	..	31	25	62	75	112
27..	83	.	47	56	81	25	141
28 .	87	..	12	100	12	50	191
29.	89	..	31	19	37	62	125
30	91	..	56	56	62	12	112
Dec. 1..	93	..	6	81	81	31	87
2..	97	..	50	6	6	62	175
3..	101	..	12	31	31	62	112
4..	105	..	50	62	50	75	87
5	107	..	75	100	62	62	75
7 .	111	..	6	50	50	100	172†

PLANT B. 1.

Date.	No. of leaves.	Length of last internode.	Increase in length.	Remarks.
Oct. 27..	8	19	.	
28.	8	25	6	
29..	8	56	31	
30 .	8	19	44	
31..	10	12	62	
Nov. 1..	10	100	137	
2..	10	112	75	Plant wilting.
3..	10	125	0	Turgor slightly restored.
4...	10	125	0	Top dead.
5..	10	125	0	
6.	10	125	0	Dying.
7 .	10	125	0	Dead. (11 *days*).

PLANT B. 2.

Date.	No. of leaves.	Length of last internode.	Increase in length.	Remarks.
Oct. 27..	8	2	..	
28 .	8	6	6	
29	8	19	12	
30	8	37	19	
31..	10	3	16	
Nov. 1..	10	12	41	
2..	10	19	6	Plant wilting.
3..	10	25	6	
4..	10	25	0	Top dying.
5..	10	25	0	
6..	10	25	0	Dying.
7..	10	25	0	Dead. (11 *days*).

*Root is seen at 3rd node on main stem.
†Plant growing vigorously at conclusion of experiment. (55 *days*).

On testing the leaves it was found that starch was present, excepting in the four most recently developed leaves, and in the terminal buds. Another plant (B. 3) was substituted on November 10th.

PLANT B. 3.

Date.	No. of leaves.	Length of last internodes.		Increase.	Remarks.
Nov. 10	4	31	..	.	
11	4	34	..	3	
12	4	37	..	3	
13	6	3	..	6	
14	6	9	.	6	
15	6	22		12	
16	8	3	..	19	
17	8	12	.	9	
18	8	19	..	6	
19	8	44	..	25	
20	8	62	..	19	
22	8	100	..	19	
24	10	6	12	9	
25	10	6	12	0	
26	10	6	12	0	
27	10	6	19	7	
28	10	6	19	0	
29	10	6	22	3	
30	10	6	31	9	
Dec. 1	10	6	37	6	Flower-bud developing.
2	10	6	37	0	" opening.
3	10	6	37	0	" open.
4	10	6	37	0	Flower open.
5	10	6	37	0	" pale.
6	10	6	37	0	Plant dying.
7	10	6	37	0	" dead. (27 *days*).

PLANT C.—(CONTROL, IN FLOWER-POT IN SOIL).

Date.	No. of leaves.	Length of last internode.	Increase in length.	Total increase.	Remarks.
Oct. 17	8	19	0	..	
19	8	19	0	..	
21	8	19	0	..	
23	8	19	0	..	
24	8	6	6	..	
25	10	12	6	..	
26	10	25	12	.	
27	10	37	12	..	
28	10	56	19	..	
29	12	19	22	..	
30	12	25	6	..	
31	12	37	12	..	
Nov. 1	12	56	19	..	
2	12	12	37	..	
3	14	19	6	..	
4	14	44	25	..	

PLANT C.—*Continued.*

Date.	No. of leaves.	Length of last Internode.	Increase in length.	Total increase.	Remarks.
Nov. 5	14	75	31	..	
6	16	12	37	..	
8	16	19	6	..	
10	16	62	44	..	
12	..	.	.	.	Broken off ; bud from 4th node recorded.
13	..	25	..	..	
14	..	3	28	..	
15	..	6	19	..	
16	.	12	19	.	
17	..	37	25	..	
18	..	62	25	..	
19	..	6	19	..	
21	..	12	6	..	
22	..	12	0	.	
24	..	19	3	..	
25	.	31	12	..	
26	..	44	13	..	
27	..	50	6	..	Two buds springing from near the base.
28	..	12	19	..	
29	..	..	..	..	Branches from near the base recorded and only the *total* increase in length of stem given.
30	..	..	.	24	
Dec. 1	..	..	...	38	
2	..	..	..	62	
3	..	..	. .	12	
4	..	..		24	
5	..	..	.	38	
7	..	..	..	53	End of the series. (51 *days*).

It will be noticed that the plant in soil in the flower pot being transplanted from soil, remained almost at a standstill for six or seven days, when growth proceeded more or less regularly. The plants whose roots were in water suffered no such standstill in regard to growth as did the plant in the soil, although all were taken from the same propagating-box.

SERIES II.

This series of experiments was carried through in the physiological laboratory at the botanic gardens during the month of December, 1899. The records given in the table below show the daily increase in length of stem and also the increase in number of leaves. All the plants were subjected to the same conditions of light, and as nearly as possible, to the same conditions of temperature, but owing to the fact that some of the plants were under bell jars, the condition of temperature, as well as of moisture could not be kept exactly the same.

Plant used, Helianthus (Sunflower).

Apparatus was arranged as in Fig. 7 for plants 1 and 2 in the same bell-jar. The spray was a nutrient solution and the roots were in distilled water.

Plants 3 and 4 were arranged as in 1 and 2, excepting that the spray was of distilled water.

Plants 5 and 6 were in nutrient solution, as in the ordinary water culture, but under a jar with ventilation at the top and bottom.

Plants 7 and 8 were under a bell jar with ventilation at the top, and the inside was kept moist by means of flat dishes of water; roots were in distilled water. Numbers 9 and 10 were in soil and were kept under a jar. Numbers 11, 12, 13, 14 were in soil in an ordinary flower-pot in the room.

PLANT.	DATE.										
	Dec. 10.	Dec. 11.	Dec. 17.	Dec. 21.	Dec. 25.	Dec. 28.	Jan. 1.	Total.	Increase.	Av. increase in No. of leaves for the set.	Av. length increase for the set.
PLANT 1.											
No. of leaves	8	8	8	8	8	10	12	12	4	....	....
Length of stem	162	200	200	219	219	237	245	245	83	...	....
PLANT 2.											
No. of leaves	8	10	10	10	12	14	18	18	10	7	....
Length of stem	206	248	248	256	284	284	300	300	94	....	86
PLANT 3.											
No. of leaves	6	8	8	10	12	12	12	12	6	....	....
Length of stem	159	194	225	256	300	300	328	328	169	....	....
PLANT 4.											
No. of leaves	8	8	10	10	10	10	10	10	2	4	....
Length of stem	181	206	256	256	272	272	281	281	100	..	134
PLANT 5.											
No. of leaves	6	8	8	8	8	10	10	10	4	....	....
Length of stem	147	169	182	197	219	244	250	250	103	....	....
PLANT 6.											
No. of leaves	6	6	8	8	8	10	10	10	4	4	....
Length of stem	131	162	162	169	206	209	222	222	91	....	97
PLANT 7.											
No. of leaves	6	6	8	8	8	8	8	8	2	....	...
Length of stem	162	225	256	256	291	300	312	312	150	....	. .
PLANT 8.											
No. of leaves	8	8	8	8	8	8	8	8	0	1	....
Length of stem	150	181	181	181	181	200	203	203	53	....	102

PLANT.	DATE.							Total.	Increase.	Av. increase in No. of leaves for the set.	Av. length increase for the set.
	Dec. 10.	Dec. 11.	Dec. 17.	Dec. 21.	Dec. 25.	Dec. 28.	Jan. 1.				
PLANT 9.											
No. of leaves	8	8	8	10	12	12	14	14	6	....	....
Length of stem	225	262	303	312	387	406	469	469	244	....	....
PLANT 10.											
No. of leaves	8	8	10	12	12	12	16	16	8	7	....
Length of stem	219	281	291	316	337	337	387	387	168	..	206
PLANT 11.											
No. of leaves	8	10	10	10	12	12	14	14	6	....	....
Length of stem	237	284	325	352	381	387	500	500	267	....	....
PLANT 12.											
No. of leaves	6	8	8	10	10	12	12	12	6	...	....
Length of stem	194	253	269	319	363	375	431	431	237	...	....
PLANT 13.											
No. of leaves	8	8	8	10	10	12	12	12	4	.. .	....
Length of stem	219	285	322	381	381	412	462	462	243	....	....
PLANT 14.											
No. of leaves	8	8	8	10	12	12	12	12	4	5	....
Length of stem	137	170	212	269	272	325	365	356	219	....	214

Plants 1-8 were each carefully weighed before being placed in the bottles, with a view to ascertain by weight the increase in growth, if any, but it was found during the course of the experiment that it would be impracticable to employ this method to determine growth throughout a series of experiments continued for so long a time as here contemplated, because some of the plants lost many of their leaves. As soon as a leaf dropped it began to absorb of the solution, as detached leaves do, and consequently quantitative determinations of ash increase might lead to error.

On December 21st plant 1 had developed an aerial root about 16 mm. in length, and on 23rd this had increased in length to 34 mm. On December 21st plant 4 showed three small roots just coming forth from the stem at the second internode, and on 23rd these roots had grown to a length of about 6 mm. On December 25th one of these had grown to a length of 19 mm., but on 28th had withered. The root that developed on No. 1 was dead on December 31st.

On December 30th, observations were made as to the general appearance of the plants, and it was found that the leaves of 1 and 2 showed a tendency to curl, and had the appearance of plants grown in too rich soil. The leaves were much crowded towards the top and they showed a good green colour. Plant 3 seemed to be dying and the uppermost leaves were becoming very pale. It may be that death was being brought about by fungi which attacked a dead leaf that clung to the stem and decomposed there. The stem of plant 4 had become pale and the upper leaves had become a pale yellow and begun to roll in from the tips, not curling at the edges as in 1 and 2. The leaves of plants 5 and 6 curled similarly to 1 and 2, but had a much more stunted appearance. The plants were now removed from the jar and they did better after being removed. These plants do not seem to take kindly to water cultures. Plants 7 and 8, leaves flat and thin, becoming very pale; stems thin and bending. Plants 9 and 10, growing well, and have the appearance of 11, 12, 13, 14.

The difficulty with these plants seemed to be that they could not well endure a moist atmosphere, and they had a tendency to send out aerial roots. Of course, in the case of No's 1 and 2 this would spoil the experiment, as the roots would, or could, then do the absorbing. In nearly every case, however, the atmosphere at some time became too dry for the roots, and so they died down. The spraying, in the case of 1, 2, 3, 4, lasted continuously for about thirteen hours, then a rest was given for at least twelve hours, nearly always much more than twelve hours. The roots were aerated regularly by means of a hand pump through the tube left for this purpose in the bottle. Distilled water was fed regularly to the roots through the tube just mentioned.

Notwithstanding the fact that these plants were not well adapted to this experiment, there are some conclusions of more or less importance to be drawn, and which bear upon the subject under discussion. The average increase in length of stem of 1 and 2 is less than that of 3 and 4, as is also the case with 5 and 6. In both cases the nutrient solution seemed to retard the growth in length of the stem. The average increase in length of stem of 3 and 4 is greater than that of 1 and 2, though the number of leaves is greater in 1 and 2. In the case of 7 and 8 a small increase in length of stem and in number of leaves was found.

The following conclusions, applicable to this plant, may fairly be drawn from the experiment, though due allowance must be made for slight exigencies :—

(1). A spray of water seemed to stimulate growth for a time.

(2). The general effect of placing the plant under a bell jar is to retard growth of stem but promote growth of leaves.

(3). Plants deprived of all food matter, except that contained in the air and in pure water, will grow rather rapidly for a time but will gradually die, the leaves first turning yellow.

(4). Plants grown in a moist atmosphere tend to send out roots at the internodes as well as at the nodes.

(5). A nutrient solution fed in the form of a spray to a plant seems to affect the plant in a way similar to that of a nutrient solution applied to the roots, as in the ordinary water culture, and therefore it may be assumed that some, at least, of the solution had been absorbed* and had been used in the general vital processes of the plant. Though the plant, fed with a solution applied to the *leaves* in the form of a spray, did not show a healthy or vigourous growth, yet the same may be said of the plant whose *roots* were supplied with the solution.

On the completion of the experiments, January 7th, the liquid medium in which the roots dipped was examined, and it was found that in the case of 1 and 2, none of the liquid spray had made its way down into the water about the roots. The liquid, however, showed an acid reaction in all cases. It was found further that those plants growing in distilled water, 7 and 8, had a much more extensive growth of roots than 5 and 6, those in the nutrient solution.

SERIES III.

The following series of experiments was arranged and conducted in the basement of the University Museum where the atmosphere was exceedingly dry at that season of the year, but the temperature was fairly constant, ranging from 60 to 70 degrees F. The plants supplied by a spray were arranged as shown in Fig. 7, while the others were placed under bell jars with ventilators. These were aerated daily, as were also the roots of the plants, and this aeration was accomplished by forcing a stream of air into the liquid surrounding the roots. The capacity of the bell jars was twenty-four liters.

The number of leaves was recorded regularly, and measurements

* Absorption by the leaves is indicated also by the experiments in Chapter X.

were made upon the last three internodes in particular, and recorded, and also upon the whole plant as nearly as possible, so as to give the increase in growth from time to time. The measurements are in all cases given in millimeters.

Plant 1 A.—Thunbergia alata, roots in distilled sterilized water and fed with a spray of nutrient solution.

Plant 1 B.—Fagopyrum esculentum (buckwheat), under the same conditions as 1 A.

Plant 1 C.—Ipomaea purpurea (morning-glory), under same conditions as 1 B., and substituted for it.

Plant 2 A.—Thunbergia alata, roots in distilled water and plant kept under a jar frequently moistened and ventilated, no spraying.

Plant 2 B.—Fagopyrum, under same conditions as 2 A.

Plant 2 C.—Thunbergia, substituted for 2 A. and kept under same conditions.

Plant 3 A.—Thunbergia alata, roots in a nutrient solution, formula given on p. 238 ; atmospheric conditions same as 2 A.

Plant 3 B.—Fagopyrum, same as 3 A.

Plant 4 A.—Thunbergia alata, roots in distilled sterilized water and under the same conditions as 1 A., excepting that the liquid used for spraying was distilled water instead of a nutrient solution.

Plant 4 B.—Fagopyrum, under same conditions as 4 A.

PLANT 1. A.—OBSERVATIONS.

Date, 1900.	No. of leaves.	Length of last internodes.			Total length.	Increase.	Remarks.
		m.m.	m.m.	m.m.	m.m.	m.m.	
Feb. 9......	8	22	31	6	78	....	
11......	8	22	37	19	97	19	
12......	8	22	44	37	122	25	
13......	8	25	44	56	144	22	
15......	10	25	44	87	175	31	
16......	10	44	100	12	200	25	
17	10	44	100	22	209	9	
18......	10	44	100	31	219	10	
19......	12	112	44	3	247	28	
21......	12	119	62	3	283	36	
24......	12	119	125	25	350	67	
25......	12	119	137	50	387	37	
28......	12	119	137	100	437	50	One bud is seen at 5th node.
Mar. 2......	14	137	119	6	467	25	
5......	14	137	125	19	519	47	
8......	14	137	125	22	522	3	
12......	14	137	125	25	525	3	Bud noted on 28th is a flower bud. There is a second flower-bud.

Stem referred to above, died down, and a branch is recorded below.

Mar. 16	4	31	31	3	66	....	
19......	4	31	37	6	75	9	
21.....	4	31	37	12	81	6	
24......	4	31	44	37	112	31	Second flower-bud broken accidentally.
26	6	44	56	3	134	22	
31......	6	44	62	19	156	22	
April 4......	6	44	62	28	166	10	
8......	6	44	62	28	166	0	Flower-bud developing naturally.

Another branch developed which is recorded below; the one recorded above had ceased to grow. The flower mentioned on February 28th dropped off.

April 23......	6	31	44	6	81	....
28......	8	47	44	6	109	28
May 5......	8	47	44	6	109	0
7.....	8	47	44	6	109	0
11......	8	50	47	6	116	7
15	8	50	47	6	116	0

Another branch developed.

May 18......	2	....	19	3	22	....	
20	4	....	19	9	28	5	
23......	4	19	16	3	37	9	
25......	4	19	22	3	44	6	
27	4	19	25	3	47	3	
June 6......	4	19	28	6	53	6	End of experiment; plant living. (118 *days*).

PLANT 1 B.—OBSERVATIONS.

Date, 1900.	No. of leaves.	Length of last internodes.			Total length.	Increase.	Remarks.
		m.m.	m.m.	m.m.	m.m.	m.m.	
Feb. 9	3	....	100	12	112	....	
11	3	....	100	16	116	4	
12	3	....	100	16	116	0	
13	4	....	100	25	125	9	
15	4	....	100	28	128	3	
16	4	100	31	31	134	6	
17	4	100	37	3	141	7	
18	4	100	44	3	147	6	
19	4	100	44	6	150	3	
21	4	106	44	9	159	9	
24	4	106	44	9	159	0	
25	4	106	44	9	159	0	Chlorotic.
27	4	106	44	9	159	0	Very chlorotic.
28	....	....	....	....	....	....	Dead ; killed apparently by fungi. (19 *days*).

PLANT 1 C. SUBSTITUTED.

Date, 1900.	No. of leaves.	m.m.	m.m.	m.m.	Total length.	Increase.	Remarks.
Mar. 8	4	19	75	37	144	....	
12	4	50	37	31	206	62	
16	5	50	37	25	275	19	
19	5	50	37	37	237	12	
21	5	50	50	44	256	19	
24	5	50	50	44	256	0	Dying down from top and attacked by fungi.
26	5	50	50	44	256	0	
31	....	....	....	....	....	....	Plant taken from the jar, carefully cleaned and replaced.
April 4	....	....	....	....	....	....	PLANT 1 C. living but attacked by aphides.
8	....	....	....	....	....	....	" beginning to thrive again.
23	....	....	....	....	....	....	" thriving vigourously.
28	....	....	....	....	....	....	" thriving well and a small branch growing from near base.
May 7	....	22	19	3	66	....	
11	....	129	3	109	43	....	
15	....	19	37	19	175	66	Growing very rapidly.
18	....	37	50	19	178	3	
20	....	37	62	37	209	31	
23	....	75	62	19	278	69	
25	....	69	22	3	291	4	
June 6	....	75	31	19	322	31	Plant healthy. (90 *days*).

PLANT 2 A.—OBSERVATIONS.

Date, 1900.	No. of leaves.	m.m.	m.m.	m.m.	Total length.	Increase.	Remarks.
Feb. 9	4	0	22	22	44	....	
11	4	22	22	6	50	6	
12	4	22	25	6	53	3	
13	6	22	25	12	59	6	
15	6	25	25	3	75	16	
19	6	25	25	3	75	16	Leaves losing colour.
24	6	25	25	3	75	16	Leaves dropping.
Mar. 2	6	25	25	3	75	16	
5	6	25	25	3	75	16	Plant dead. (24 *days*).

PLANT 2 A.—*Continued.*

New plant substituted—same species.

Date, 1900.	No. of leaves.	Length of last internodes.			Total length.	Increase.	Remarks.
		m.m.	m.m.	m.m.	m.m.	m.m.	
Mar. 8	5	12	31	12	67	....	
12......	6	12	31	31	87	25	
16......	5	37	31	19	112	25	
19......	5	37	37	19	119	7	
21	6	37	37	19	119	0	
24......	6	37	37	19	119	0	Two lateral branches starting.
26......	6	37	37	19	119	0	Plant dying from top.
31......	7	37	37	19	119	0	Leaves turning yellow.
April 4......	7	37	37	19	119	0	Leaves dropping.
8......	4	37	37	19	119	0	
23......	4	37	37	19	119	0	Plant dead. (46 *days*).

PLANT 2 B.—OBSERVATIONS.

Feb. 9......	3	0	100	12	112	....	
11......	4	0	100	12	112	0	
12......	4	25	100	16	116	4	
13......	4	100	34	6	125	9	
15......	4	100	34	6	141	16	
16......	4	100	37	12	159	9	
17......	4	100	37	16	153	3	
18......	4	44	19	3	166	13	
19......	4	44	19	3	169	3	
21......	4	44	22	16	184	15	
24......	5	34	25	12	212	38	
25......	6	34	34	19	225	13	
28......	7	34	34	22	228	3	
Mar. 2......	6	37	37	25	231	3	
5......	7	37	31	6	244	13	
12......	7	37	37	9	256	12	
16......	8	37	37	19	266	10	
19......	8	37	19	3	269	3	
21......	8	37	19	3	269	0	
26......	8	37	19	3	269	0	Losing colour and dying from top.
31......	8	37	19	3	269	0	Apparently dying.
April 4......	8	37	19	3	269	0	Losing turgor and leaves dropping.
8......	8	37	19	3	269	0	Dying.
23......	8	37	19	3	269	0	Dead. (73 *days*).

PLANT 3 A.

Date, 1900.	No. of leaves.	Length of last internodes. m.m.	m.m.	m.m.	Total length. m.m.	Increase. m.m.	Remarks.
Feb. 9	6	....	19	6	25	....	
11	6	....	19	12	31	6	
12	6	....	19	12	35	4	
13	6	....	19	12	35	0	
15	6	...	19	12	35	0	
16	6	....	19	12	35	0	
17	4	...	19	12	35	0	
18	4	19	12	3	34	3	
19	4	19	12	3	34	3	
24	4	19	12	3	44	9	
25	4	19	12	3	44	9	
28	4	19	12	3	44	0	
Mar. 2	4	19	12	3	44	0	
5	4	19	12	3	44	0	
12	6	12	16	3	50	3	
16	6	16	37	9	69	16	
21	5	12	37	16	75	6	
26	4	12	37	16	75	0	
31	4	12	37	16	75	0	Top dead; a branch springing from base.
April 4	4	12	37	16	75	0	
8	4	12	37	16	75	0	Growth confined to branch.
BRANCH.							
Apr. 28	4	....	19	12	....	....	Plant not dead but seems stunted, and growth so slow that no further observations are taken. (68 *days*).

PLANT 3 B.

Date, 1900.	No. of leaves.	m.m.	m.m.	m.m.	Total length. m.m.	Increase. m.m.	Remarks.
Feb. 9	3	0	62	6	69	....	
11	4	0	62	12	75	6	
12	4	0	75	19	94	19	
13	4	0	87	22	109	15	
15	4	0	87	25	112	3	
16	4	0	87	37	125	13	
17	4	87	37	3	128	3	
18	4	87	44	9	141	13	
19	4	87	44	12	144	3	
21	4	44	25	3	159	15	
24	4	44	50	3	184	25	
25	4	44	50	19	200	16	
28	5	44	50	25	206	6	
Mar. 2	5	44	50	37	219	13	
5	5	50	44	3	228	9	
12	5	50	50	37	269	41	
16	5	50	50	37	269	0	and lateral branch.. 12
21	5	50	50	37	269	0	" " 16
26	4	50	50	37	269	0	" " 19
31	4	50	50	37	269	0	" " 25
April 4	5	....	....	....	....	..	Growing very slowly; flowers all dropping.
28	5	....	....	...	....	....	Growing very slowly; seems to have spent its energy in flowering. (78 *days*).

PLANT 4 A.

Date, 1900.	No. of leaves.	Length of last internodes. m.m.	m.m.	m.m.	Total length. m.m.	Increase. m.m.	Remarks.
Feb. 9......	7	22	22	3	47	....	
11......	7	22	37	3	62	15	
12......	7	22	44	6	72	10	
13......	7	25	50	16	91	19	
15......	6	25	50	37	112	21	
16......	6	25	50	50	125	13	
17.... ..	6	25	50	53	128	3	
18.....	8	56	75	3	159	31	
19......	8	56	81	3	166	7	
24......	5	56	87	9	178	6	
25......	5	56	87	16	184	6	
28	4	56	87	19	187	3	
Mar. 2......	4	56	87	25	206	19	
5......	4	56	87	25	206	0	Leaves turning white.
10......	4	56	87	31	212	6	
12....	2	56	87	31	212	0	Almost dead, excepting a lateral branch near the base.

BRANCH.

Date, 1900.	No. of leaves.	m.m.	m.m.	m.m.	Total length. m.m.	Increase. m.m.	Remarks.
Mar. 16......	2	....	....	19	....	....	
19......	2	....	....	22	....	....	
21......	2	....	....	22	....	....	
26......	2	....	....	22	....	....	
31......	2	....	.	22	....	....	
April 4	0	....	....	....	.....	....	Dead. (54 *days*).

PLANT 4 B.

Date, 1900.	No. of leaves.	m.m.	m.m.	m.m.	Total length. m.m.	Increase. m.m.	Remarks.
Feb. 9......	3	....	87	12	100	....	
11......	4	....	87	37	125	25	
12......	4	87	44	12	144	19	
13......	4	87	50	16	153	9	
15......	4	87	50	19	156	3	
16......	4	87	50	22	159	3	Commencing to flower.
17......	5	25	12	6	200	41	
18......	6	25	22	6	209	9	
19......	6	25	22	6	212	3	
21......	6	25	22	6	212	0	
24	6	25	22	6	212	0	
25.	6	25	22	6	212	0	
Mar. 2......	6	25	22	6	212	0	Lateral bud developing.. 19
5......	5	25	22	6	212	0	" " 19
10......	4	25	22	6	212	0	" " 19
12.....	4	25	22	6	212	0	
16......	4	25	22	6	212	0	Dying down.
19......	4	25	22	6	212	0	Branch dies.
21......	4	25	22	6	212	0	
26	3	25	22	6	212	0	
31......	2	25	22	6	212	0	
April 4.. ..	2	25	22	6	212	0	Dead. (54 *days*).

SERIES IV.

The following series of experiments was carried on in the basement of the University Museum, and was set up on December 21st, after the manner shown in Fig. 7.

All the plants used were of the species Thunbergia alata.

Plant A. In a spray of nutrient solution, roots in distilled sterilized water, Fig. 7.

Plant B. Same as A.

Plant C. Plant under bell-jar, roots in distilled sterilized water, no spray.

Plant F. Same as C.

Plant E. Plant under bell-jar, roots in a nutrient solution half the concentration of that given on page 238, no spray.

Plant D. Same as E.

Plant G. Plant with roots in distilled sterilized water, spray of distilled water, Fig. 7.

Plant H. Same as G.

A.

Date.	No. of leaves.	Length of internodes.						Remarks.
Dec. 21......	4	25	7	....	....	....	....	Poorest plant and not very green.
24	6	30	9	....	....	...	...	
27......	6	30	12	5	....	....	....	One cotyledon yellow.
31......	4	30	12	8	....	....	....	Cotyledons dropped.
Jan. 3......	6	30	16	15	....	...	..	
7.....	6	30	16	35	4	...	....	
10	6	30	15	40	12	....	....	
14......	8	30	15	40	25	2	..	
18......	8	....	....	...	40	10	..	
24.....	8	....	....	....	65	220	....	
29......	7	....	....	....	68	20	....	One leaf dropped.

A. 1.

Date.	No. of leaves.	Length of internodes.						Remarks.
Feb. 1......	....	...	....	....	....	...	.. ..	Plant removed; broken by accident; strong one substituted.
2......	11	65	3	....	....	....	....	
5......	11	75	10	....	..	...	....	
11......	13	75	40	3	....	.. .	....	
25......	17	75	70	90	20	..	....	Two leaves dropped.
Mar. 10	10	75	70	90	95	80	..	Three well marked flower buds.
18......	8	75	70	90	95	85	....	Flower opening.

A.—41 *days*; A. 1.—46 *days*, *plus*, very healthy at the close of the experiment.

B.

Date.	No. of leaves.	Length of internodes.						Remarks.
Dec. 21......	6	25	6	....	....	....	....	Plant small and not healthy looking.
24......	6	25	9	....	....	....	....	
27......	6	25	9	2	....	....	....	
31......	4	28	9	3	....	....	....	Cotyledons dropped.
Jan. 3......	4	28	9	5	....	....	....	
7......	6	28	9	7	....	....	..	
10......	5	28	9	7	..	....	....	One leaf dropped.
14......	....	....	....	....	....	...	....	Died down seemingly.
15......	....	....	....	....	....	....	....	Revived.
18......	....	....	...	. .	....	..	....	Bent over and apparently dying.
19......	Taken out and replaced by another which had been kept in soil in case of accident. (29 *days*).							
19......	6	10	25	3	...	....	.. .	
24	6	10	25	10	. .	...	....	
29.... .	8	10	25	30	5	....	..	
Feb. 5	8	10	25	45	12	....	....	
11......	6	....	....	45	15	....	....	Two leaves dropped.
25......	4	....	.. .	...	50	3	....	Four leaves dropped.
Mar. 10	2	....	....	....	....	....	....	Broken down by accident.
18......	....	....	....	....	..	....	....	Dead. (58 *days*).

F.

Date.	No. of leaves.	Length of internodes.						Remarks.
Dec. 21......	4	20	4	...	...	....	..	Medium in quality.
24	4	21	4	....	...	...	..	
27......	6	21	3	....	....	....	....	
31......	6	21	30	8	....	..	....	
Jan. 3.... .	6	21	32	18	....	....	...	
7......	7	21	35	30	....	..	....	One cotyledon dropped.
10......	6	21	35	50	12	...	....	Two cotyledons dropped.
14......	6	21	35	55	35	....	....	Lower leaves chlorotic.
18......	6	....	....	....	....	....	....	Lower leaves gone.
24.... .	6	....	....	60	60	....	....	
28......	....	....	....	....	....	....	....	Broken off at lowest internode.
	BRANCH RECORDED.							
Feb. 5......	5	12	....	....	....	....	....	One leaf dropped.
11......	3	12	2	....	.. .	....	...	Leaves turning yellow.
25......	4	12	8	. ..	...	....	....	
Mar. 10......	2	12	10	....	..	...	...	Leaves yellow.
18......	2	12	12	....	....	....	....	Leaves yellow. (87 *days, plus*).

C.

Date.	No. of leaves.	Length of internodes.						Remarks.
Dec. 21......	4	28	15	....	....	....	....	
24......	6	30	18	....	....	....	....	
27......	6	30	28	10	....	....	....	Two cotyledons dropped.
31......	4	30	28	30	....	....	....	
Jan. 3......	6	30	28	55	12	....	....	
7......	6	30	28	60	25	....	....	
10......	6	30	28	60	70	....	....	
14......	7	30	28	60	100	20	....	Two lower leaves chlorotic.
18......	6	30	28	65	110	45	....	
24	....	....	....	....	....	50	....	
29	....	....	....	....	....	60	....	
	BRANCH RECORDED.							
Feb. 5......	4	2	..	...	....	....	....	Leaves turning yellow.
11......	3	....	....	....	....	....	....	
25......	4	5	8	...	....	....	....	Chlorotic.
Mar. 4.....	2	5	8	....	....	....	....	"
18......	4	5	8	8	....	....	...	" (87 *days*, *plus*).

E.

Dec. 21......	4	40	5	....	....	....	....	
24......	4	40	12	....	....	....	....	
27......	6	40	30	3	....	....	....	
31......	....	....	....	..	....	....	....	Plant wilted down.
	SUBSTITUTE.							
Feb. 1......	8	100	3	....	....	....	..	
5	8	110	15	....	....	....	....	
11	8	110	45	..	....	....	....	
25......	12	120	55	...	..	....	....	
	BRANCH RECORDED.							
11......	12	8	...	...	....	....	.....	
25......	15	25	20	....	....	.. .	..	
Mar. 10......	15	30	40	110	120	10	...	
18......	15	30	50	125	175	145	15	

E.—41 days ; substitute (46 *days*, *plus*).

D.

Date.	No. of leaves.	Length of internodes.						Remarks.
Dec. 21	6	40	18	...	...	...	...	
24	6	40	22	...	...	...	...	
27	6	40	25	8	...	...	...	
31	5	40	25	25	...	...	...	One cotyledon dropped.
Jan. 3	6	40	25	35	5	...	...	
7	6	40	25	40	8	...	...	
10	6	40	25	40	10	...	...	
14	4	40	25	40	12	...	...	Lower leaves wilting.
23	4	...	...	...	12	...	...	
24	4	...	...	...	15	...	...	
29	6	...	...	...	25	...	...	
Feb. 5	6	...	...	...	25	...	...	
11	8	...	...	...	28	...	...	
		BRANCH RECORDED.						
11	8	3	...	...	...	...	...	
25	10	3	15	...	...	...	...	
	3	3	...	...	...	...	...	
Mar. 10	10	3	15	20	...	...	...	
		3	15	...	...	...	...	
18	12	3	15	20	30	...	...	
		3	25	...	...	...	...	(87 *days*).

G.

Dec. 21	6	20	6	...	...	...	...	Best plant in the series.
24	6	20	13	5	...	...	.	
27	6	20	18	8	...	...	...	
31	6	20	18	20	...	...	...	Two cotyledons dropped.
Jan. 3	6	20	18	40	3	...	...	
7	6	20	18	50	25	...	...	
10	8	20	18	50	50	5	...	
14	8	20	18	50	60	25	...	
18	10	...	...	...	...	80	10	
24	10	...	...	...	...	100	20	
29	8	...	...	...	...	100	25	Branch at first node.
Feb. 11	9	12	3	...	...	...	..	
25	4	10	15	3	...	...	...	
Mar. 20	4	10	15	30	...	...	...	Slightly chlorotic.
18	2	10	15	30	...	...	...	Chlorotic. (87 *days*).

H.

Date.	No. of leaves.	Length of internodes.						Remarks.
Dec. 21......	4	18	6	....	....	....	....	
24......	6	18	15	....	....	....	....	
27	6	18	15	5	....	....	..	
31	4	18	18	10	....	....	....	Cotyledons dropped.
Jan. 3......	6	18	18	25	....	....	....	
7......	6	18	18	45	5	....	...	
10	6	....	....	50	10	....	....	
14......	6	....	....	50	25	....	....	
18.. ...	7	....	....	....	50	10	....	
24.....	7	....	....	....	55	18	....	
29......	7	....	....	....	55	20	....	Branch at first node.
	BRANCH RECORDED.							
Feb. 5......	8	30	5	....	....	....	....	
11......	4	35	10	....	....	....	...	
25......	4	35	15	....	....	....	....	
Mar. 10	3	35	20	....	....	....	....	
18......	....	....	....	....	....	....	...	Dead. (85 *days*).

From these experiments certain conclusions may be drawn, an investigation into which would throw some light, not only upon the absorption of non-poisonous dilute solutions, but also upon the question of water-absorption. The plants best suited to this foliage culture are those whose leaves are adapted to moist conditions, and those whose roots are fibrous and numerous. Those having tap-roots will draw upon the food stored in the tap-root, defeating, to some extent, the results of the experiments. If plants can utilize food solutions applied to their leaf surfaces, it may be that rain-water, after a period of dry weather more or less prolonged, falling upon leaves, aids directly in nourishing the plants. Galloway and Woods have shown that lime-water used as a spray acts as a food, or at least produces a growth distinctly above the normal. It is now known that the bordeaux mixture causes an increase in growth by supplying food, or by action in the nature of a stimulus.

The nutrient solution applied to the leaves produced a substantial increase in growth, indicating that the solution was absorbed, or that it acted as a stimulus, or both. The conclusions in regard to growth are based upon :—(1) large increase in number of leaves and in total leaf area, (2) increase in length of stem, (3) the production of flowers, coupled with a general appearance which can scarcely be described in words and figures. In the case of the plant Helianthus, the solution used seemed to be too strong when applied to the roots as well as when applied to the leaves. The effects, though in a measure injurious, were similar, making it seem probable that the solution was absorbed.

The effect of water used as a spray was to stimulate growth for a time, then to produce a chlorotic unhealthy appearance. In the case of the buckwheat plant, the characteristic reddish colour of the stem was maintained where the nutrient solution was applied to the leaves, as well as when applied to the roots. Where the spray was of water the stems became pale. These experiments being carried on through such an extended period of time give strength to the conclusions reached. It will be noticed in a few cases that accidents happened to the plants, owing to frequent manipulation of the apparatus necessary to carry on the work. This was unavoidable.

To sum up we may say, that a nutrient solution when applied to the leaves affected the plants as did the solution when applied to the roots, that the nutrient solution produced a substantial growth, and that water used as a spray stimulated growth for a time.

In order to ascertain more fully the effect of the nutrient solution applied to the leaves in this way, it was thought that an estimation of the ash content would throw some light upon the matter.

The following experiment was designed to determine the effect upon the content of ash by feeding a plant with a nutrient solution applied to the leaves in the form of a spray (Fig. 7). Eight plants of the same species (Justicia speciosa) were selected so as to have them as nearly as possible uniform in size and quality. They were divided into two groups of four each, the division being made so as to have by estimation the same amount of ash in each group. This was of course only an approximation, but it was made in such a way as to leave error, if error there was, on the safe side. The selection of plants was not made by the writer.

Group I.—M, N, O, P; Group II.—A, B, G, H.

The plants of Group I. were at once dried and then analyzed to ascertain the content of ash. Those of Group II. were fed, as described above, for seventeen days, when they also were dried, weighed and analyzed in a similar way. The plants in both cases were dried first in air, and then for two days in a desiccator for dry weight calculations.

The following tables show the results of the experiment; weight given in grams, length of stem in centimeters :—

I.

Plant.	Leaves.	Length of stem.	Weight dry.	Weight of ash.	Per cent. of ash to dry weight.
M..................	8	14	.2631	.0045	16.91
N......	8	15	.3900	.0610	15.64
O.....................	6	12	.1719	.0281	16.34
P.................. ..	8	11	.3930	.0653	16.61
	30	52	1.2180	.1589	

Average per cent. of ash to dry weight 16.37.

II. a.

A.....................	6	10	....	....	...
B........	6	12	....	. ..	....
G................	8	14	.. .	. ..	.. .
H	8	11	...	....	....
	28	47	....	....	....

II. b.

A.........	8	11	.3281	.0592	18.04
B..........	8	14	.3667	.0663	18.08
G....	12	22	.5722	.1005	17.56
H	10	14	.3575	.0640	17.90
	38	61	1.6243	.2900	....

Average per cent. of ash to dry weight, 17.89.

Gain per cent. in	number of leaves	35.7
" "	length of stem.................. ...	29.7
" "	dry weight...............	33.37
" "	ash............	82.5

The plants fed with the nutrient solution *contained 1.52 per cent. more ash*, in proportion to dry weight, than did those which were not fed.

The important point brought out in this experiment is, that each plant of Group II. (those fed with a nutrient solution in the form of a spray), contained a higher per cent. of ash in proportion to dry weight than did those of Group I. This is the more striking because it does not depend upon approximations, as do the comparisons in weight with Group I. Since it is impossible to calculate the amount of ash in a living plant in order to ascertain if that plant makes any increase, it is necessary in all such investigations to base results upon estimates with similar plants grown under similar conditions.

The results of this experiment support the conclusions arrived at in regard to the effect of nutrient solutions applied to leaves in the form of a spray. There is not only a visible increase in number of leaves and in length of stem, but also a substantial increase in dry weight and in weight of ash. Since the actual content of ash in proportion to dry weight is increased, there can remain no doubt that the leaves had absorbed some of the substance applied in the form of a spray.

VII.—THE EFFECTS OF STRONG SOLUTIONS APPLIED TO THE CUT ENDS OF THE PETIOLES OF FOLIAGE LEAVES.

It was necessary first to determine whether solutions when applied to the cut ends of the petioles of leaves ascended through the blades. It was found by chemical analysis that solutions did ascend through the blade of the leaf even to the margin. This having been proved, it was then possible to investigate the effects of solutions entering leaves in this way. The leaves were placed in solutions as shown in Fig. 8, and the records of the experiments are self-explanatory.

For the first series of experiments leaves of the following plants were chosen :—Malva, Primula, Nicotiana, Ranunculus and Dicentra.

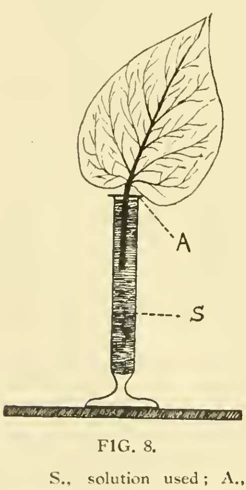

FIG. 8.

S., solution used ; A., cardboard cover to prevent too rapid evaporation.

In twenty-four hours after setting up the experiment, solutions (HCl) and (H_2SO_4) produced a decolorization of the leaf tissue from the base of the leaves outwards in all the leaves, especially so in the leaves of Primula and Nicotiana. This decolorization, in some cases was slightly more extensive in the region of the large veins, but had when small the shape of a semicircle whose centre was at the junction of the petiole and the blade of the leaf. Solution (NH_4OH) had caused a blackening of the tissue in the part of the blade nearest the petiole ; and had produced a deepening of the green colour out towards the margin. Solution ($NaHCO_3$) had caused a "frozen" appearance between the main veins, extending from the margin inwards, especially so in the case of Nicotiana.

On October 15th, seventy-two hours after the leaves had been placed in the solutions, observations were made upon all the leaves and records taken.

$FeSO_4$ *m/16, veins blackening generally but in the case of the Dicentra there was a blackening up the centre.

$ZnSO_4$ m/4, Malva dry, Dicentra whitish up the centre, and the others limp with a freckled appearance.

$HgCl_2$ m/16, leaves all dead ; salt ascending except in Dicentra, brown from the base outwards along the chief veins.

$CuSO_4$ m/16, all dead, with the apparent exception of Dicentra, which seems to be living ; Ranunculus is darkened all over ; Malva dried with a deep green margin.

$Ba(NO_3)_2$ m/4, Malva crisp from the margin inwards, especially between the chief veins ; Dicentra dry along margin.

KCl m/2, Malva dried and salt had crept well out along the veins, Dicentra wilting at the margin.

Na_2CO_3 m/4, Malva dried and discoloured outward from a yellowish green to dark ; Ranunculus darkened on the veins, with the dark colour spreading ; Dicentra dry at the margin.

HCl m/4, all brownish from the base outward, the region next the margin is green and apparently living.

H_2SO_4 m/4, similar effect to that produced by HCl, but rather more extensive decolorization, especially so in Ranunculus.

KOH m/4, dark from the base outward with all the leaves resembling the effect produced by Na_2CO_3.

NaOH m/4, same effect as KOH.

NH_4NO_3, m/2, all slightly wilted, and some have a frozen appearance near the margin.

$NaHCO_3$ m/4, all wilted dry and Ranunculus darkened.

NH_4OH 5%, all wilted and dead ; Malva very dry.

* "m" is a solution made by dissolving the mol. wt. in grams of the substance in a liter of water, thus $FeSO_4$ m/16 means 152 grams in 16 liters of water.

Glycerine m/2, all fresh, Nicotiana spotted.

Gr. sugar m/2, all fresh.

Ca. sugar m/2, all fresh.

$MgCl_2$ m/2, similar to KCl.

NaCl m/2, similar to that of KCl.

It was found that in ten to twelve hours after taking the leaves from the solutions some of them underwent other changes worthy of note. The part of the blade of the leaf turned yellowish-brown by the acids had changed to a bluish colour, showing that very probably the tissue was undergoing further changes, resulting in a product having an alkaline reaction.

Judging from their effects. these substances may be classified as follows :—

Acids	HCl, H_2SO_4.
Alkalies	KOH, NaOH.
Decomposable alkalies........	Na_2CO_3, NH_4OH.
Poisons........................	$CuSO_4$, $HgCl_2$, $Pb\overline{A}_2$, $FeSO_4$, $ZnSO_4$.
Osmotically active substances..	$MgCl_2$, KCl, NH_4NO_3, etc.

In a second series of experiments the arrangement was as shown in Fig. 8, and continued for three days, when the leaves were taken from the solutions, and observations made upon their conditions then and afterwards. The acids, in a measure, acted as did the alkalies, showing that these substances alike penetrate readily the walls of the cells in all directions, and do not follow the veins. Acids and alkalies to some extent dissolve the cellulose, and so make a way for themselves readily in all directions.

LEAF OF PRIMULA STELLATA, PLACED AS IN FIG. 8.

I.

Strength of Solution, m/4; set up November 9th.

Solution.	Died in.	Remarks.
NaCl...........	16 days	Salt ascended.
Ba $(NO_3)_2$......	1 day	
$KClO_3$	2 days	
KCl............	18 days	Salt ascended.
$MgCl_2$..........	8 days	
KNO_3..........	10 days	Salt ascended.
Na_2CO_3.....	4 days	Like that of NaOH.

I.—*Continued.*

Solution.	Died in.	Remarks.
$ZnSO_4$	2 days	Depressed spots.
KI	3 days	
KBr	7 days	
HCl	3 days	Petiole and base of leaf very red.
H_2SO_4	2 days	Like that of HCl.
NaOH	3 days	Petiole and base blue.
KOH	2 days	Stronger than that of NaOH.
NH_4OH 2.5%	2 hrs.	Killed by gas, not by solution.
$CaCl_2$	4 days	
c. sugar	5 days	
g. sugar	4 days	Sugar decomposed and leaf killed probably by an [acid.
Glycer.	4 days	Became freckled.
$NaHCO_3$	4 days	Salt ascended.
NH_4NO_3	6 days	Bluish, due to a decomposition NH_4NO_3.
K_3PO_4	1 day	Seem to be very poisonous.

II.

Strength of Solution, m/8; November 21st.

NaCl	11 days	
$Ba(NO_3)_2$	2 days	Crisp.
$KClO_3$	3 days	Veins lighter.
$CuSO_4$	1 day	Limp.
KCl	15 days	Salt ascending.
$MgCl_2$	9 days	Base blue.
KNO_3	15 days	Water in intercellular spaces as if frozen.
Na_2CO_3	6 days	Base blue.
$ZnSO_4$	2 days	Frozen appearance.
KI	2 days	Acted between the veins first.
KBr	9 days	Freckled.
$FeCl_3$	2 days	Veins not blackened.
$Pb\bar{A}_2$	2 days	
$HgCl_2$	2 days	
HCl	3 days	Base and petiole red.
H_2SO_4	3 days	Same as for H_2SO_4.
NaOH	6 days	Base blue.
KOH	5 days	Similar to NaOH.
NH_4OH	1 day	(1.75% strength), base blackened.
$CaCl_2$	7 days	
$FeSO_4$	2 days	Veins blackened.
g. sugar	6 days	
$NaHCO_3$	5 days	Salt ascending.
NH_4NO_3	7 days	
K_3PO_4	1 day	
c. sugar	14 days	
Glycer.	16 days	

III.

Strength of Solution, m/16; December 1st.

Solution.	Died in.	Remarks.
$NaCl$	27 days	Salt ascended, turned yellow.
$Ba(NO_3)_2$	2 days	Wilted first between the veins.
$KClO_3$	8 days	Wilted from the margin.
$CuSO_4$	1 day	Limp.
KCl	18 days	Margin bluish.
$MgCl_2$	8 days	
KNO_3	36 days	Salt ascended.
Na_2CO_3	5 days	Salt ascended, base blue.
$ZnSO_4$	4 days	Bluish between veins, light green above.
KI	6 days	Margin crisp.
KBr	7 days	Margin crisp.
$FeCl_3$	5 days	Limp, veins not blackened.
$Pb\bar{A}_2$	1 day	Bluish.
$HgCl_2$	1 day	Died from the base outward.
HCl	2 days	Petiole and base red.
H_2SO_4	2 days	Same as HCl.
$NaOH$	10 days	Base blue.
KOH	8 days	Same as NaOH.
NH_4OH	7 days	(.625%) base blackened.
$CaCl_2$	35 days	Spotted bluish at margin.
$FeSO_4$	2 days	Veins blackened
c. sugar	14 days	Turned yellow.
g. sugar	8 days	Killed by an organic acid (probably $C_3H_6O_3$.
glycer	14 days	Did not turn yellow.
$NaHCO_3$	10 days	Salt ascended.
NH_4NO_3	14 days	Did not turn yellow.
K_3PO_4	2 days	Limp.

IV.

Strength of Solution, m/32; December 12th.

$NaCl$	41 days plus	Fairly fresh.
$Ba(NO_3)_2$	2 days	Blue.
$KClO_3$	16 days	Glazed looking, veins light green.
$CuSO_4$	1 day	Limp.
KCl	33 days	
$MgCl_2$	16 days	Turned yellow.
KNO_3	36 days	Wilted.
Na_2CO_3	9 days	Salt ascended, bluish spots.
$ZnSO_4$	5 days	Blue spots on back.
KI	8 days	Crisp margin.
KBr	41 days	Salt ascended.
$FeCl_3$	8 days	Wilted from margin.
$Pb\bar{A}_2$	2 days	Veins dark towards the margin, blue.
$HgCl_2$	4 days	Yellow up the veins.
HCl	3 days	Petiole red.
H_2SO_4	3 days	
$NaOH$	18 days	Salt ascended.
KOH	11 days	Salt ascending.
NH_4OH	1 day	(.3125%) base blackened.

IV.—*Continued.*

Solution.	Died in.	Remarks.
$CaCl_2$	41 days plus	
$FeSO_4$	2 days	Veins darkened, blade blue.
c. sugar..........	14 days	Turned yellow.
g. sugar..........	15 days	Solution acid.
glycer..........	24 days	
$NaHCO_3$..........	17 days	Salt ascended.
NH_4NO_3	14 days	Wilted from margin.
K_3PO_4..........	2 days	Died from margin.

V.

Strength of Solution, m/64; January 5th.

NaCl..........	54 days plus	
$Ba(NO_3)_2$	3 days	Bluish.
$KClO_3$	4 days	
$CuSO_4$..........	1 day	Very limp.
KCl	27 days	Spotted yellow, salt ascended.
$MgCl_2$..........	14 days	Turned yellow.
KNO_3	30 days	Turned yellow.
Na_2CO_3..........	36 days	Limp.
$ZnSO_4$	3 days	Freckled on upper side, spotted blue on back.
KI..........	5 days	Base bluish.
$FeCl_3$	4 days	Veins not blackened.
$Pb\overline{A}_2$	2 days	Blade stiff, not limp, back blue.
$HgCl_2$..........	5 days	Petiole blue.
HCl	5 days	Petiole red.
H_2SO_4	4 days	Petiole and base red.
NaOH	14 days	Very limp second day, turgor restored on third day.
KOH..........	11 days	Very limp second day, turgor restored on third day.
NH_4OH	1 day	(.3125%) base blackened.
$CaCl_2$	54 days	
$FeSO_4$	5 days	Veins blackened.
c. sugar..........	12 days	Wilted, not yellow.
g. sugar..........	10 days	Solution reddened.
glycer..........	54 days plus	
$NaHCO_3$.	14 days	Gradually wilted down.
NH_4NO_2	17 days	
K_3PO_4..........	6 days	Died from margin.
KBr..........	47 days	Turned yellow.

VI.

Strength of Solution, m/128; January 22nd.

NaCl	44 days plus	Salt ascending.
$Ba(NO_3)_2$	5 days	Spotted.
$KClO_3$	4 days	Crisp margin.
$CuSO_4$..........	1 day	Limp.
KCl	44 days plus	Salt ascending.
$MgCl_2$	30 days	Turned yellow as it wilted.
KNO_3	44 days plus	
Na_2CO_3	30 days	Crisp margin, turned yellow as it died.
$ZnSO_4$	13 days	Back of leaf blue.
KI	14 days	Margin blue.

VI.—*Continued.*

Solution.	Died in.	Remarks.
KBr	44 days plus	Salt ascending.
$FeCl_3$	4 days	Margin, blue, veins not blackened.
$Pb\bar{A}_2$	14 days	Petiole blue.
$HgCl_2$	4 days	Base most affected, petiole blue.
HCl	4 days	Petiole red.
H_2SO_4	4 days	Petiole red.
NaOH	44 days plus	Salt ascending.
KOH	29 days	Salt ascending.
NH_4OH	34 days	Much wilted second day, but recovered afterwards [(.15625%).
$CaCl_2$	44 days	
$FeSO_4$	4 days	Veins blackened.
c. sugar	13 days	Petiole decomposing.
g. sugar	12 days	Petiole decomposing.
glycer	44 days plus	
$NaHCO_3$	34 days	Became yellow from margin.
NH_4NO_3	32 days	Yellow margin.
K_3PO_4	5 days	

Summary of records of experiments I., II., III., IV., V., VI. The figures in the vertical columns denote the time required (in days) to kill the leaf. The letter "p" after a number indicates that the leaf was living and fresh at the termination of the experiment.

Solution.	m/4	m/8	m/16	m/32	m/64	m/128
NaCl	16	11	27	41p	64p	44p
KCl	18	15	18	33	27	44p
KNO_3	10	15	36	36	30	44p
$CaCl_2$	4	7	35	41p	54p	44p
$MgCl_2$	8	9	8	16	14	30
Na_2CO_3	4	6	5	9	36	30
KBr	7	9	7	41	47	44p
$KClO_3$	2	3	8	16	4	4
KI	3	2	6	8	5	14
NaOH	3	6	10	18	14	44p
KOH	2	5	8	11	11	29
glycerin	4	16	14	24	54p	44p
$FeCl_3$	—	2	5	8	4	4
$FeSO_4$	—	2	2	2	5	4
K_3PO_4	1	1	2	2	6	5
$CuSO_4$	—	1	1	1	1	1
$NaHCO_3$	4	5	10	17	14	34
NH_4NO_3	6	7	14	14	17	32
$Ba(NO_3)_2$	1	2	2	2	3	5
$Pb\bar{A}_2$	—	2	1	2	2	14
$HgCl_2$	—	2	1	4	5	4
HCl	3	3	2	3	5	4
H_2SO_4	2	3	2	3	4	4
c. sugar	5	14	14	14	12	13
g. sugar	4	7	8	15	10	12
$ZnSO_4$	2	2	4	5	3	13

Experiments with solutions applied to the cut ends of the petioles of leaves (Fig. 8). May and June.

I.—LEAF OF PRIMULA OBCONICA.

Solution.	Strength.	Time to kill.	Remarks.
*1	m/4	5 days	Wilted from margin.
2	"	10 days plus	
3	"	9 days	Wilting.
4	"	9 days	Wilting.
5	"	6 days	Large veins turning white.
6	"	5 days	Veins blackening from base.
7	m/400	8 days	Spotted.
8	m/4	2 days	Very crisp.
9	m/400	10 days plus	Very healthy looking.
10	—	10 days plus	
11	—	10 days plus	

II.—LEAF OF CUCURBITA.

1	m/4	10 days	Margin became crisp.
2	"	9 days	
3	"	7 days	Trichomes much swollen.
4	"	6 days	Covered with crystals.
5	"	9 days	Turned dark brown.
6	m/50	3 days	Veins black.
7	m/400	6 days	Veins turned yellow.
8	m/4	2 days	Coated over with crystals.
9	m/400	10 days plus	Very green but glossy.
10	—	10 days plus	Turning yellow at margin.
11	—	10 days plus	Turning yellow at margin.

III.—LEAF OF TRAPAEOLUM.

1	m/4	1 day	Soft.
2	"	1 day	Dry.
3	"	1 day	Dry.
4	"	1 day	Soft.
5	"	1 day	Soft.
6	m/50	1 day	Dry.
7	m/400	1 day	Dry.
8	m/4	2 days	
9	m/400	10 days plus	Very fresh looking.
10	—	9 days	Wilted from margin.
11	—	2 days	Crisp.

*1. NH_4NO_3; 2. KCl; 3. $MgCl_2$; 4. KNO_3; 5. $NaHCO_3$; 6. $FeSO_4$; 7. $HgCl_2$; 8. $Ba(NO_3)_2$ 9. $Pb\bar{A}_2$; 10. H_2O dist.; 11. H_2O tap.

IV.—LEAF OF LUPINE.

Solution.	Strength.	Time to kill.	Remarks.
1	m/4	5 days	Had a tendency to curl.
2	"	4 days	Same as 1.
3	"	5 days	
4	"	5 days	Tendency to curl.
5	"	5 days	Much curled.
6	m/50	2 days	Veins black.
7	m/400	1 day	Veins yellowish.
8	m/4	2 days	Salt on surface.
9	m/400	10 days plus	Very fresh looking.
10	—	8 days	Dried spotted yellow.
11	—	7 days	Spotted.

V.—LEAF OF PRIMULA OBCONICA.

1	m/8	10 days	Spotted and "frozen" appearance.
2	m/8	10 days plus	Wilting at the margin.
3	m/8	10 days plus	Spotting.
4	m/8	10 days plus	Quite fresh.
5	m/8	7 days	Spotted, then all yellow.
6	m/100	3 days	Veins blackened.
7	m/400	5 days	Sick looking in two days.
8	m/8	3 days	Wilting second day.
9	m/400	10 days plus	Very fresh.
10	—	10 days plus	Fresh.
11	—	10 days plus	Fresh.

VI.—LEAF OF CUCURBITA.

1	m/8	3 days	"Frozen" appearance.
2	m/8	3 days	Crisp at margin.
3	m/8	2 days	Spotting.
4	m/8	3 days	Salt ascending.
5	m/8	3 days	Freckled appearance.
6	m/100	2 days	Veins blackening.
7	m/400	2 days	Veins becoming yellow.
8	m/8	1 day	Salt ascending.
9	m/400	10 days plus	Very fresh.
10	—	6 days	Yellow at margin.
11	—	5 days	Yellow at margin.

VII.—LEAF OF CUCURBITA.

*191	m/8	3 days plus	Salt on the upper surface.
74	m/8	3 days plus	Salt upon the upper surface.
19	m/8	2 days	Wilted second day.
73	m/8	2 days	Died in spots.
72	m/8	3 days plus	Quite fresh.
100	m/8	3 days plus	Fresh, salt not all dissolved.
76	m/256	1 day	Veins brown.

* 191. Neut. sol.; 74. K_3PO_4; 19. $KClO_3$; 73. $ZnSO_4$; 72. Na_2HPO_4; 100. $CaSO_4$; 76. $CuSO_4$ 160. K_2O; 70. KI; 71. Na_2CO_3; 111. KBr; 75. Na $\bar{A}$; 10. H_2O dist.; 11. H_2O tap.

VII.—LEAF OF CUCURBITA.—*Continued.*

Solution.	Strength.	Time to kill.	Remarks.
160	m/8	2 days	Poisoned look, salt ascended.
70	m/8	2 days	Died in spots.
71	m/8	2 days	Very limp.
111	m/8	3 days plus	Very fresh.
75	m/8	3 days plus	Salt ascending on upper side.
10	—	3 days plus	Fresh.
11	—	3 days plus	Fresh.

VIII.—LEAF OF LUPINE.

191	m/8	3 days plus	Fresh.
74	m/8	2 days	Dead from the margin.
19	m/8	2 days	Light green, veins yellow.
73	m/8	3 days	Wilted second day.
72	m/8	3 days	Light green.
100	m/8	3 days plus	Salt not all dissolved.
76	m/256	1 day	Lighter green.
160	m/8	3 days plus	Almost dead.
70	m/8	2 days	Veins green.
71	m/8	3 days	Brown at margin.
111	m/8	3 days	Fresh.
75	m/8	3 days	Veins green.
10	—	3 days plus	Fresh.
11	—	3 days plus	Fresh.

IX.—LEAF OF PRIMULA OBCONICA.

191	m/8	3 days plus	Fresh.
74	m/8	3 days plus	Fresh.
73	m/8	2 days	Spotted.
76	m/256	1 day	Very limp.
160	m/8	2 days	Poisoned looking.
70	m/8	3 days	Dead in spots.
	All others	3 days plus)	

VII. *a.—Leaf of Acer.*

1	m/4	3 days	Colour lighter green, wilting from margin.
2	m/4	3 days plus	Almost dead, wilting from margin.
3	m/4	3 days plus	Almost dead, tendency to spot.
4	m/4	3 days	Freckled between veins and with yellowish spots.
5	m/4	3 days	Veins darkened, dried from the margin, black near the petiole.
6	m/50	2 days	Wilting in twenty-four hours, petiole and veins blackened, blade deeper green.
7	m/400	4 days plus	Slightly wilting fourth day, petiole dying.
8	m/4	1 day	Frozen appearance between veins.
9	m/400	5 days plus	This leaf remained fresh much longer than any of the others.
*10	—	5 days	Wilted.
*11	—	5 days	Wilted.

* Numbers ten and eleven died in five days while number nine remained fresh until May 28th, eleven days longer.

VIII. *b.—Leaf of Ampelopsis.*

Solution.	Strength.	Time to kill.	Remarks.
*1	m/4	4 days	Veins darkening, brown in streaks.
2	m/4	4 days plus	Dead and crisp at the margin.
3	m/4	4 days	Blade spotted.
4	m/4	3 days	Blade spotting, wilting and limp second day, drying up fourth day.
5	m/4	4 days	Base brown, margin green, petiole and veins darkening.
6	m/50	2 days	Margin black.
7	m/400	4 days plus	Blade wilting some, petiole dying, margin black, veins dying near base of leaf.
8	m/4	1 day	Frosen appearance between veins, veins decolourized.
9	m/400	4 days plus	Slightly wilted.
10	—	4 days	Wilting on second day.
11	—	5 days plus	Fresh.

* In the cases in which the leaf died it became blackened after death.

Certain solutions seemed to produce, after a few days, a translucent appearance in the region between the main veins. It appeared as though the intercellular spaces in this part of the leaf were injected with water. On examination it was found that the cells in this region were plasmolyzed. Then, from these considerations, namely that the cells were plasmolyzed, that water appeared to be in the intercellular spaces, and that solutions of considerable concentration were known to ascend through the blade, it may be concluded that the cells in this region were killed by an osmotic action, causing a loss of water to the cell.

It was noticed that normal solutions of H_2SO_4 and of HCl had practically the same toxic power, as might be expected, since they are chemically equivalent and contain the same amount of replaceable atoms of hydrogen. This result does not accord with the results obtained by Kahlenberg and True (1896, p. 92), who showed that 1/6400 gram-equivalent solution of H_2SO_4 was as Toxic to Lupine radicles as 1/3200 gram-equivalent solution of HCl. Judging from their results and conclusions, one might infer that they regarded a gram-equivalent per liter solution of H_2SO_4 to contain *twice* as many ions as a gram-equivalent solution of HCl which, however, is not the case according to Mohr,* Talbot† and others.

From the experiments here recorded it may be concluded that certain salts kill the leaf by osmotic action, while others produce death by chemical action. The latter may be classed as poisons and the

* Titrimethode, p, 56.

† Quantitative chem. anal., 1900. p. 65.

former as non-poisons. Among the poisons, $CuSO_4$ was the most deadly. With dilute solutions it always rendered the leaf quickly limp, commencing at the margins. With strong solutions there seemed to be a complete "paralysis," the whole leaf becoming wilted in a very short time. With $HgCl_2$ the action took place from the base outwards, showing that the solution made its way out laterally from the vein only with great difficulty, while the $CuSO_4$ seemed to penetrate the whole leaf rapidly, causing a wilted condition but no discolouring.

The experiments with the sugars are of little importance, because of the fact that a fermentation took place quickly, and the solution became a solution of an organic acid, resulting from a decomposition of the sugar. The grape sugar, as might be expected, was much more susceptible to fermentation than was the cane sugar.

Certain of the salts, notably $ZnSO_4$, produced a depression of the surface in spots, due to a decrease in the turgor at that point, generally without that "water-logged" appearance so common in cases where a leaf is being killed by osmotic action.

The cause of the "water-logged" appearance seen in the case of certain non-poisonous, but strongly osmotic substances, is due to the fact that these salts in solution upon reaching the thin walled parenchyma cells in the leaf, draw water osmotically from the cells into the intercellular spaces, at first in the region between the main veins. In some cases this is noticed at the margin of the leaf, but the drying action of the air upon the water drawn into the intercellula spaces, causes the leaf to become crisp and dry along its margin.

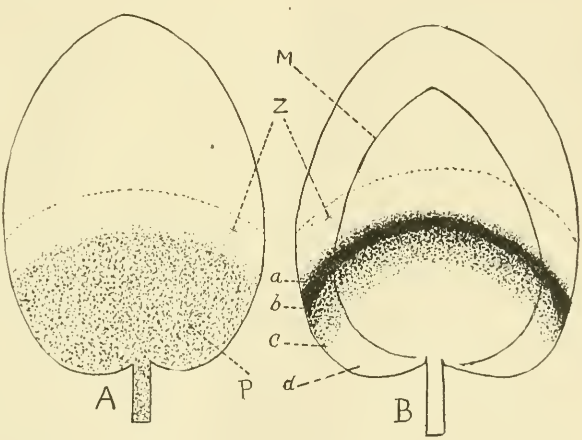

FIG. 9.

A, filter paper cut in the form of a leaf, it is saturated with ferric chloride, and then placed in a solution of potassium ferrocyanide; P, a precipitate; Z, a zone of water; B, saturated with potassium sulphocyanide, and then placed in a solution of ferric chloride; a, dilute solution; b, strong solution; c, partially soluble in excess; d, wholly soluble in excess; M, margin of area if a living leaf be used.

The salts ascend readily into the leaf and penetrate the whole blade, as is proved by testing the leaf for the salt taken up by the petiole. The cells of the leaves having the "water-logged" appearance are plasmolyzed, showing that water has been extracted osmotically by the salt. This phenomenon is mentioned by Vines (p. 40).

Certain solutions will, therefore, if applied to the cut ends of the petioles, kill the leaves by extracting water from the cells. This effect is not so rapid as might be expected, because the solution which ascends the petiole and enters the leaf, is not of the same concentration as that supplied in the test-tube. This fact is not as yet generally known to botanists, because in the experiment to demonstrate the rate of ascent of sap in plants, a solution of a lithium salt is taken, and the distance of ascent is determined at any given time by the height of the lithium salt. It is known that the experiment with eosin is not accurate, for the reason that the water ascends faster than the eosin. What is true of eosin is true of lithium, and probably of any other solution, as may be demonstrated as follows :—Saturate a piece of filter paper with a solution of potass-sulpho-cyanide, allow it to dry, then place one end of the paper in a solution of ferric chloride. As the iron solution ascends, the height of the solution is indicated by the deep brown-red colour, formed where the salts meet, in consequence of the chemical action between them. A zone of water may be seen to advance ahead of the substance in solution by the translucent effect which it produces upon the paper. The difference between the height of solution and the height of this zone of water at any given time is considerable, as is the case with eosin and other coloured solutions. In the case of lithium it can not be seen because the solution is colourless, but it acts as other salts do, as has been shown by experiment (Fig. 9).

In Detmer and Moor (p. 233) it is stated that the lithium ascends as *high* as the water does in which it is dissolved. This is not opposed to the ground taken by the writer, namely, that lithium in solution does not ascend as rapidly as the water. This has been proved by the writer by cutting the strip of absorbent paper *just below* the point reached by the water *while the water is ascending* and *before* it has reached its maximum height. This portion of absorbent paper contains no lithium. Sachs' view was that a decomposition of the lithium must result if the water ascended higher than the lithium salt, for he says (p. 236) :—"The lithium solution possesses, as I convinced myself with the aid of the paper strips previously mentioned, the advantageous property of ascending without being *decomposed*."

The present view of the lithium test for the rate of ascent of water is based chiefly upon work done in 1877 by Sachs' (1878, p. 165). He shows that in all coloured solutions used, water ascends faster than the substance in solution ; he cites sixteen solutions in which it is said the water is known to rise more rapidly than does the substance in solution, and he states that three substances, colourless in solution, ascend *as high as the water*. Lithium nitrate is one of the latter. It should be stated that Sachs' experiments were chiefly for the purpose of determining whether solutions ascended on the surface of the cell walls (by capillarity), or in the substance of the wall (by imbibition). Lithium, he said, ascended by capillarity and reached a point as *high* as the water and so there was no decomposition. Sachs' results, therefore, are not contradictory to those of the writer.

It is shown by using such solutions as potassium ferro-cyanide and ferric chloride, or mercuric chloride and potassium iodide, or any pair of solutions which produce a precipitate soluble in excess of either solution, that the solution loses in concentration as it ascends.

The solutions used to demonstrate the fact that water ascends *more rapidly* than the substance in solution were :—

Potassium ferro-cyanide and ferrous sulphate.

Potassium sulpho-cyanide and ferric chloride.

Potassium iodide and mercuric chloride.

Potassium iodide and lead acetate.

Copper sulphate and potassium ferro-cyanide.

Silver nitrate and hydrogen sulphide solution.

Silver nitrate and potassium iodide.

Salicylate of soda and ferric chloride.

To test the rate of ascent of water by the rate of ascent of lithium in solution will lead to error, as has been proved by actual experiment, and as is inferred from experiments with other solutions which produce a precipitate (see Fig. 9).

A leaf whose petiole is in a solution does not absorb the solution in quite the same way as does a filter paper. The solution in the case of living leaves extends over the leaf so that the area affected is symmetrical to the whole leaf (Fig. 9 B, and Photo. 1-6). With the filter paper the solution extends equally in all directions without any regard to the outline, commencing at what represents the base of the leaf (Fig. 9).

Where the solution was not of such a nature as that of strong acids or alkalies, but where it followed the veins, it was found that the distances from the base of the leaf to the extreme point of discolourization along each vein was proportional to the total length of the vein, causing the solution to reach the margin of the leaf at all points about the same time (Photos. 1, 2, 3).

Experiments to test the effects of water upon certain leaves when applied to the cut ends of the petioles. Primula obconica.

Leaves A, B, C, D, E with petioles in distilled water (Fig. 8).

Leaves F, G, H, I, J with petioles in tap water.

Weight.		Difference.		Conditions.
October 5th.	November 12th.			
A ... 1.5625	1.48	.0825	Loss	Fresh.
B ... .9375	.976	.0385	*Gain*	Fresh.
C.... 1.30	1.314	.014	*Gain*	Fresh.
D ... 1.30	.875	.425	Loss	Yellow and wilted.
E ... 1.75	1.245	.170	*Gain*	Fresh.
F ... 1.625	1.097	.0755	Loss	Yellow and wilted.
G.... .975	.705	.270	Loss	Yellow and wilted.
H ... .9375	.916	.0385	*Gain*	Fresh.
I.... .9125	.766	.1465	Loss	Wilting.
J.... .925	.979	.054	*Gain*	Fresh.
November 12th.	December 3rd.			
A ... 1.48	1.4245	.0555	Loss	Fresh.
B ... .976	.9109	.0651	Loss	Fresh.
C.... 1.314	1.2829	.0311	Loss	Fresh.
D	....	....	....	Not weighed; as the leaf was dead.
E . 1.245	1.1935	.0515	Loss	Fresh.
F ... 1.097	.933	.164	Loss	Wilted.
G	....	....	....	Not weighed; leaf dead.
H ... .916	.9845	.0685	*Gain*	Fresh.
I.... .766	.7209	.0451	Loss	Fresh.
J.... .979	.9569	.0221	Loss	Fresh.

Examined on December 15th for starch.

A.—Plenty of starch all through the blade, especially between veins.

B.—Starch in veins from base outwards.

C.—Starch in veins; none at margin; leaf commencing to wilt.

D.—No starch.

E.—Considerable starch in mid-veins; some elsewhere.

F.—Abundant starch.

G.—No starch; leaf was yellow and dead.

H.—Starch abundant in distal part of blade; lobes of base yellow.

I.—Considerable starch.

J.—Slightest trace of starch near one part of margin.

During this experiment and from others used as controls in the solution experiments, a few very remarkable phenomena developed. It would appear as though these leaves had gone on living and performing all the functions natural to them, although detached from the plant, and some of them had actually increased in weight. Very many leaves under such conditions develop roots from the petiole, but it was not so with these. In only one case did a leaf of Primula develop roots, and this one was standing in water along with a leaf petiole of Impatiens which developed an extensive root system. When the leaves were gathered for the above experiment, they were immediately placed in paraffin paper to render the loss by evaporation as small as possible during the interval between gathering and placing in water.

Those leaves which, during the experiments, had begun to turn yellow had lost considerably in weight and showed little or no starch. One leaf (not referred to in the above table) which had been gathered on May 31st, remained apparently alive and fresh till October. On October 15th it was becoming yellow, but showed on examination considerable starch in its mid-veins. It had developed no roots. One fair conclusion from these experiments is that these detached leaves function* as if upon the plant, though they have no roots to supply food. The petioles of these leaves were always surrounded with water for from one to two inches above the surface of the liquid in the vessel, the trichomes raising the water by capillary action.

The leaf of Primula stellata lends itself readily to experiments such as those described in this chapter, because it will live on for months without sending out roots, if the petiole be kept in water; and the lower surface being reddish, acid and alkaline reactions are clearly marked.

These results may be summarized as follows:—

Some solutions kill the cell by extracting water osmotically from it, thus hastening its drying out. Other solutions affect the cell contents without causing plasmolysis.

Ferrous sulphate produces a blue colour which is shown on the cell wall. This colour appears black when in the veins of leaves. This blue colour is not produced by *ferric* iron, showing that it is not due to the presence of tannin.

The rate of ascent of solution through the leaf blade varies as the length of vein.

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* They produce starch, retain their turgor, and keep the chloroplasts in a normal condition.

Solutions do not ascend through the blade of the leaf as they do through filter paper (Photos. 1-6, and Fig. 9).

The lithium test for the rate of ascent of water is not accurate for filter paper, and may not be therefore, for plants.

Detached leaves may carry on some of their normal functions for a long period of time.

VIII.—On the Effect of a Solution Applied to the Leaf Surface.

This introduces one of the most important phases of the subject, and one towards which the attention of the fruit-grower and the farmer has been directed for the last decade or more. Since the protecting of plants from the ravages of fungi and of insects has been attempted by means of spraying with liquids of a more or less poisonous character, the attention of many, especially of those in experiment stations, has been turned towards the increase or decrease in crop, owing to the application of the poison, in solution, to the leaf of the plant. Some experiments have been carried on, notably at the Agricultural Experimental Station, Ottawa, 1900, with a view to the extermination of weeds by spraying. The experiments performed by the writer, and here described, show, as has been shown elsewhere, that leaves of plants are affected differently by the same solution. What one leaf will endure will kill another; and taking advantage of this fact, plants susceptible to the solution may be destroyed. It has recently been learned that the common field grains, such as wheat, barley and oats, are well adapted to the shedding of drops applied in the form of a spray, because of the chemical nature of the surface, because of the shape of the leaf and of its natural position with regard to the stem.

Probably the most troublesome weed now in many parts of the northern United States and Canada, is the wild mustard (Brassica sinapistrim, Boiss.) ; and the means just referred to have been successfully used towards its extermination. A spray of a solution of $CuSO_4$ and of $FeSO_4$ have been used with singular effect, and a certain strength of solution was found which would kill the mustard and not materially injure the grain crop. At Ottawa it was found that the solution of $FeSO_4$ of 10% strength produced some "scorching" of the barley, but stripped the mustard of its leaves and "scorched" the stem to some extent, but did not completely kill it. The 2% $CuSO_4$ solution produced only a slight injury to the barley crop but completely killed

PHOTO. 1.—Acer rubrum ; $FeSO_4$, m/56 ; three days in solution ; veins blackened. Veins blackened to points equally distant from the margin. Area affected symmetrical to whole leaf.

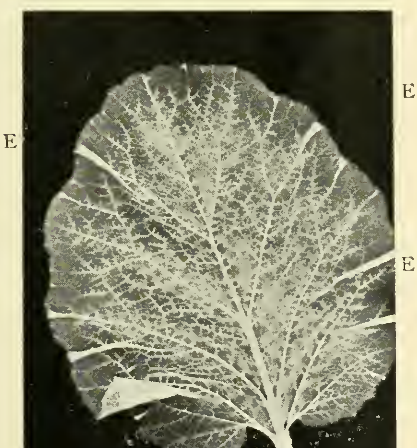


PHOTO. 2.—Primula obconica ; $FeSO_4$; m/56 ; blackening of the veins not complete. E, utmost limit of the darkening of the veins.

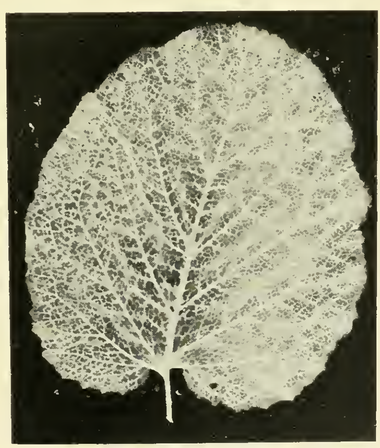


PHOTO. 3.—Primula obconica ; $FeSO_4$; m/56 ; complete blackening of the veins even to the very minute terminations. Almost no diffusion of the salt away from the veins.

PHOTO. 4.—Primula obconica ; HCl ; m/4; two days ; shows clearly the edge of the wave which is entirely independent of the veins.

PHOTO. 5.—Primula obconica ; H_2SO_4; m/4 ; two days. The edge of the wave follows in a measure the contour of the leaf.

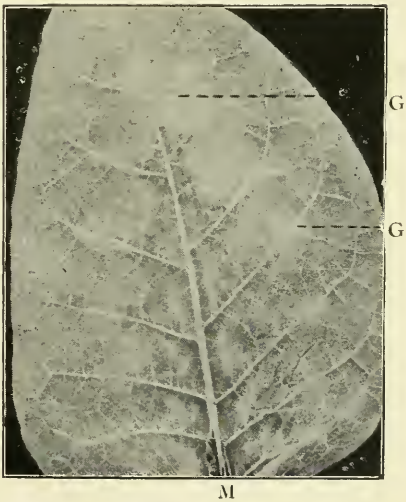


PHOTO. 6.—Nicotiana tabacum ; H_2SO_4; m/4 ; three days ; yellow striped centre, bounded by a green stripe shown at G, beyond which is a brown surface. Xylem (m) laid bare by the acid.

the mustard. This method of weed extermination has been carried on successfully, it is said, in England and France, but in matters pertaining to the farm there is usually very little, if any (Experi. Station Bulletins ref. Chapter II.), literature pertaining to the subject. The amount of liquid used per acre at Ottawa was of $CuSO_4$ solution fifty gallons, made up of two pounds of $CuSO_4$ to ten gallons water No observations seem to have been made as to the actual effect upon the crop, though it is inferred that the barley crop did not suffer materially.

The experiments just referred to, together with those performed by the writer (tobacco spotting) leave no room for doubt whatever that solutions are absorbed by plants. The most important part, however, of this very interesting subject, as has been observed by several writers, to whom reference has been made in Chapter II., is the fact that these solutions which will kill one plant may actually *promote growth* in another. No experiments were performed by the writer with this end in view.

The main objects of the following experiments were to ascertain whether solutions are absorbed by leaves (the test employed by Boussingault was adopted), and to determine the physiological effect.

SERIES I.

Effect of solutions applied to leaf surfaces in the form of drops. July, 1900, leaf Ampelopsis ; solution applied to lower side. Column 1, solution used ; 2, absorbed* ; 3, dry, moist or crystals ; 4, dark ring ; 5, colour of spot ; 6, less apparent effect on the lower surface of the leaf. The strength of the solution used, excepting the nutrient solution and the copper sulphate, was m/4. The copper sulphate was m/56.

1	2	3	4	5	6
$MgCl_2$	Nearly all.	Moist.	Yes.	Yellow.	Less.
$ZnSO_4$	Not all.	Dry.	Slightly.	Yellow.	Less.
Na_2CO_3	No.	Crystals.	No.	Black.	Less, decidedly.
KBr..........	No.	Crystals.	Yes.	Yellow.	Most decidedly.
$NaHCO_3$....	No.	Crystals.	No.	None.	Neither.
Na $\overline{A}$.........	Nearly all.	Moist.	Yes.	Brown.	Less.
K_3PO_4........	Half.	Crystals.	Yes.	Brown.	Less, slightly.
$CuSO_4$	All.	....	Yes.	Brown.	No difference.
Nut. sol......	All.	....	....	.. ...	....
$KClO_3$..	No.	Crystals.	Yes.	Yellow.	Less, decidedly.
KI	No.	Crystals.	Yes.	Yellow.	Little difference.

* Absorbed if no deposit remained after evaporation.

The leaves were in the open air without protection against too rapid transpiration, and in consequence there was a greater amount of crystalline residue than might otherwise have been expected. Those substances recorded as moist were so twenty-four hours after the drop was placed upon the leaf, and they remained so for a longer time, though no further record was made. The fact that a darker ring occurred was noted particularly because of the peculiar formation of the crystalline deposit formed as the drop evaporates.

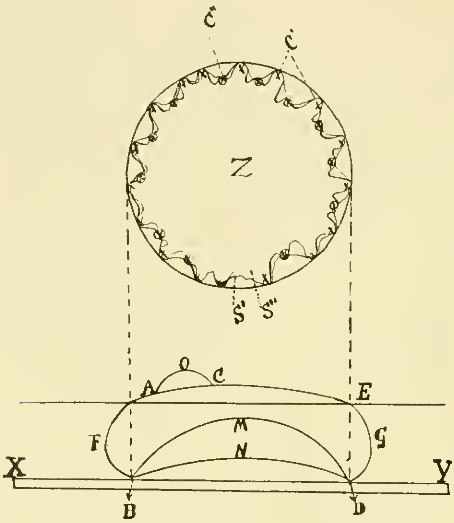


FIG. 10.

XY is a section of a glass slide upon which is a drop of solution BCD, which takes the form roughly of a flattened spheroid touching the glass in the form of a circle with two points on the circumference, as B and D. As the drop evaporates it assumes roughly the form BMD and BND, the points B and D remaining, and the circle of crystals forms roughly as shown in S' S'', C' C''. The curve BFA being sharper than the curve CE, there will be more rapid evaporation and consequently crystals will form first at B and D, where they remain forming the ring. Owing to cohesion and tendency to crystallize, the first crystals formed attract solution until almost all evaporates. Z, ground plan of drop.

Boussingault and Schlössing noticed this ring of crystals, and Boussingault noticed also that in some cases of more dilute solutions, a darker green ring was produced on the leaf by the drop of solution. No attempt was made by Boussingault or any one else to explain this, so it was thought well to make the point clear, although it belongs properly to physics.

When a solution is applied to a leaf surface in the form of a drop, the salt upon rapid evaporation of the water, forms itself in a visible ring, which results in an unusually strong action upon the leaf tissue immediately under this ring of crystals. This ring formation is not confined to leaves, but is true of crystalline substances in general. If a drop of a salt solution be placed upon a clean glass slide and allowed to evaporate, it is found that there is a ring of salt at, or near, the edge of the original drop, while inside the ring there are very minute crystals evenly spread over the surface.

Let XY (see Fig. 10) represent a glass slide, and BCD a section of a drop of solution. Because of the internal cohesion of the particles of the liquid, these tend to form a sphere, but this tendency being in part overcome by the attraction between it and the glass, and of gravitation, it assumes the form of a flattened spheroid, something like the form shown in the diagram. The curves BFA and DGE being sharper than any other curves on the surface of the drop, there will be a greater surface exposure to the air in proportion to the mass of liquid in the immediate vicinity, and consequently a greater evaporation and condensation of solution. There will clearly be less evaporation from the surface AC than from the surface AOC ; and if AOC be a similar curve to a part of BFA, there would be less evaporation from AC than from the part referred to of BFA. Now it follows, as diffusion through liquids is very slow, that the first crystals will form at B and D. These crystals are now no longer floating, but take up a fixed position—from their weight—upon the glass, and remain there. As evaporation proceeds, the distance from C to the plate becomes less and less, while the distance BD along the plate remains almost constant ; BCD becomes BMD and BND. This is due to the fact that the crystal in growing attracts other particles towards it to build it up, and as it cannot now move, the particles move to it ; and also because the substance of the crystal, having a strong affinity for water—otherwise it would not dissolve readily—attracts water towards it, because of a physical affinity now called capillarity, and which some term surface tension. If a drop, half evaporated, be placed under the microscope this may be easily observed, and it will then be noticed that the margin of the area of contact between the drop and the glass is only very roughly the circumference of a circle, because the crystals first formed, (not being formed all at the same time), will be distributed irregularly, the first farthest from the centre. The place of the first crystals laid down will determine the position of the ring of salt found after evaporation has been completed (represented by C' and C'' in diagram). The prominence of the ring will depend upon several things,—affinity of the substance for the solvent, crystal-forming power, and the diffusing power of the substance in solution, etc., which it is not necessary to discuss here.

Though the solution was applied to the lower surface of the leaf, there was an effect more plainly visible upon the upper surface, showing that the solutions had acted especially on the cell contents and upon the chlorophyll granules. The substances were conveyed through the spongy tissue in the lower portion, to the palisade tissue where the chlorophyll is most abundant. The ring, in several instances, was well marked upon the upper, as well as upon the lower surface. Those substances remaining moist, did so because of their strong hygroscopic properties.

When $CuSO_4$ is applied in solutions more dilute, as in strength m/640, one finds it also producing a ring. This is easily recognized by applying the solution to the under surface of a leaf of Primula stellata (Hort.), which has a red colour. When the drop has remained for some hours upon the leaf, one can see by the colour where the tissue has been killed. The red colour disappears, and there appears a blue ring where the tissue has shrunken below the general leaf surface.

Strong acids and alkalies do not produce this ring because these substances dissolve a way regularly through the tissue, permeating in all directions, as is the case when these substances are applied to the cut ends of the petioles. (See photographs 4, 5, 6).

SERIES II.

Experiments with solutions of $CaH_2(CO_3)_2$ and $Ca(OH)_2$ upon leaves of Begonia, Primula obconica, Primula stellata, Pelargonium and Heliotropium.

When drops of each of these solutions were placed upon the upper surface of these leaves, and allowed to evaporate in the open air, there was a slight residue in each case; but if the leaf were placed in a flask (Fig. 6), or if a detached leaf were placed under a beaker to prevent too rapid evaporation, the salts entirely disappeared. (The object of this experiment was to test the effect of the soluble carbonate upon the surface of the leaf). It took between two and three days to absorb completely the drop of solution. If the drop became dry in less than twenty-four hours there was a residue, showing that the loss by evaporation was too rapid. Though the $Ca(OH)_2$ solution left upon evaporation a small amount of crystalline matter, this was undoubtedly not the hydroxide of lime but the carbonate, produced by the action of CO_2 upon the evaporating solution of the hydroxide. The deposit of $CaCO_3$ then upon the leaf vanished upon the application of more water, and especially so if the leaf were kept in a moist chamber as shown in Fig. 6. An interesting fact to notice, was that if a leaf holding upon it the residue from a drop of the solution of the bicarbonate which had been allowed to evaporate rapidly, were placed as in Fig. 6, this white residue vanished after a time. This was due to the moisture in the air in the flask acting just as the water did upon the residue, causing it to become again, in the presence of CO_2, a solution of the bicarbonate, which was then in this condition absorbed. This proves that a part of the calcareous substances found upon the leaves of some plants, may be, upon occasion, in the presence of moisture and CO_2, resorbed by the plant.

From these experiments, and from others not recorded, the conclusions may be drawn that, while a solution applied in the form of a fairly large drop may be harmful, applied as a fine spray it may not be so; that it is the upper surface especially that shows the "scorching," no matter whether the solution be applied to the lower or to the upper surface of the leaf; and that calcium carbonate may be absorbed in the presence of moisture and carbonic acid gas.

SERIES III.

On the action of strong solutions upon Spirogyra.

$CuSO_4$, m/8, one and three-fourths hours, no plasmolysis, chlorophyll band broken up and of a lighter green colour, spiral form lost, protoplasm very granular, and excepting these granules it seemed to be dissolving, membrane not visible.*

$HgCl_2$, m/8, one and a-half hours, no plasmolysis, chloroplast yellow and has lost its form, protoplasm full of dark granular bodies each having a bright red center.

$Pb\bar{A}_2$, m/8, two hours, no plasmolysis, protoplasm full of very fine granules which are not dark, chloroplast shrunken but still spiral.

$FeSO_4$, m/8, one and a-half hours, no plasmolysis, chloroplast yellowish, broken up into fragments each retaining a pyrenoid, the walls between any two cells constricted and of a beautiful deep blue colour, the other walls of the cells of a faint blue tinge giving the whole a blue cast.

HCl, m/8, one and three-fourths hours, no plasmolysis, chloroplast yellow, protoplasm dissolving, no granules, band has kept its form and is very conspicuous.

NaOH, m/8, two hours, no plasmolysis, chloroplast of a light yellow colour and dissolving, protoplasm dissolving.

NH_4OH, 5%, one and three-fourths hours, no plasmolysis, chloroplast in pieces and dissolving, general colour light yellow and the cells swollen.

* Nägeli (1893, p. 1-51) states that copper in very strong solution causes extreme plasmolysis; and that weaker solutions cause a chemical poisonous action with a breaking up of the chlorophyll bands. The strong solutions used by Nägeli are therefore much stronger than m/8.

In all cases the protoplasmic membrane was dissolved, or had not been separated from the cell wall; the strands had disappeared.

The object of this experiment was to discover, if possible, the action of poisonous solutions upon Spirogyra. In the case of $CuSO_4$, $HgCl_2$ and $Pb\bar{A}_2$ there was a peculiar formation of small dense particles in the protoplasmic substance, showing that some chemical change had taken place, resulting in a precipitate; or in an action upon globules, colourless by nature, within the protoplasm. These substances either dissolved the protoplasmic membrane, or passed through it without any apparent obstruction, as no plasmolysis occurred. These dark particles were especially large and dark with the $HgCl_2$ solution, a result which in the case of a mass of cells would produce a darkening of the general colour. Such a change took place in the case of leaves which were acted upon by this solution applied to the cut ends of the petioles. The darkening in the case of the $FeSO_4$ solution was entirely different, being due to the action of the iron solution upon the walls of the cells, producing a blue colour which appears black in mass. This was shown particularly where the cells of the filaments were joined. This dark colour was shown in the leaves whose petioles had been dipped in a solution of $FeSO_4$, the veins becoming dark even to their minute extremities. (See photographs 1, 2, 3). The HCl penetrated the cell wall quickly and dissolved the protoplasm. NaOH and NH_4OH quickly penetrated the cell walls, dissolved the protoplasm and broke down the chlorophyll band.

Poisonous solutions applied to the upper side of a leaf compared with the same solutions applied to the lower side.

SERIES IV.

Detached leaf of Primula stellata; petiole in water; time twenty-four hours; solution strength m/640.

$Pb\bar{A}_2$ upper	Residue slight	No effect visible.
lower	Residue slight	No effect visible.
$CuSO_4$ upper	Residue slight	No effect visible.
lower	Residue none	Red colour destroyed in a ring.
$Ba(NO_3)_2$ upper ...	Residue slight	No effect visible.
lower ...	Residue none	No effect visible.
$HgCl_2$ upper	Residue none	No effect visible.
lower	Residue none	Red colour destroyed in a ring.

It was found that the solutions applied to the lower side were absorbed more quickly.

These experiments show that the leaves of this plant are more sensitive to these solutions when they are applied to the lower surface.

When drops of these solutions were placed upon leaves of Bouvardia, there were distinct spots "scorched" upon the leaves, but when a filter paper was saturated with some of the solution and folded flat upon the leaf, upper and lower side being covered, there was no visible effect. This seemed very peculiar, more especially as the paper was saturated three times upon three successive days, enough of the solution being applied to kill several leaves if placed upon them in drops. This may have been due to the fact that the filter paper* held the solution mechanically in its tissue by capillary action, and as the water evaporated, the salt was retained in the paper; little, if any, could have entered the leaf.

When the solution of $CuSO_4$, and solutions of those substances, which produced dark green rings at the margins of the drops, were applied in sufficiently low concentration to cause no "scorching" of the leaf, but yet strong enough to bring about after some time this darkening of the green colour (see also Griffon, An. Sc. Nat. 8, 1-2, 1899), the action was probably in the nature of a stimulus to growth, and produced a better development of chlorophyll and of protoplasm in the region where the tissue appeared dark to the naked eye. (See Fig. 12). The explanation of this is given in the chapter on "Tobacco Leaf Spot."

The yellow spots and marginal stains noticed by Smith upon the leaves of plants (1872), were due to the poisonous substances held in solution by water clinging to the leaf, either in the form of drops, or held by capillary action on the edge of the leaf. The poisonous substances came, as Smith showed, in the form of fumes, from the neighbouring chemical works. How exceedingly sensitive plants are to these noxious vapours and fumes, is shown in the following statement of Smith :—"When the air has so much acid that two to three grains are found in a gallon of rain water, or forty pints in a million, there is no hope for vegetation in a climate such as we have in the northern part of the country."

In discussing the question of the application of solutions to leaves

* This is in accordance with the common method of purifying distilled water from compounds of copper, etc., by placing pure filter paper in the water.

of plants, one must examine into the condition of the leaf surface, because this surface might exert a mechanical influence upon the solution. Plants whose leaves are coated with an oily or waxy substance will shed the liquid applied, while other plants which apparently shed the drops, actually spread the liquid over the veins by the capillary action of the hairs and striations which occur on the grooves of the epidermis over the veins. This is plainly shown when a harmless solution of considerable strength is used; for after the evaporation of the water the dry salt is clearly seen. Some plants, such as Primula sinensis, have striations radiating from the large trichomes, which would aid in conveying liquid which happened to be brought near the base, up the trichome to a surface less cutinized. (Fig. 5, st.).

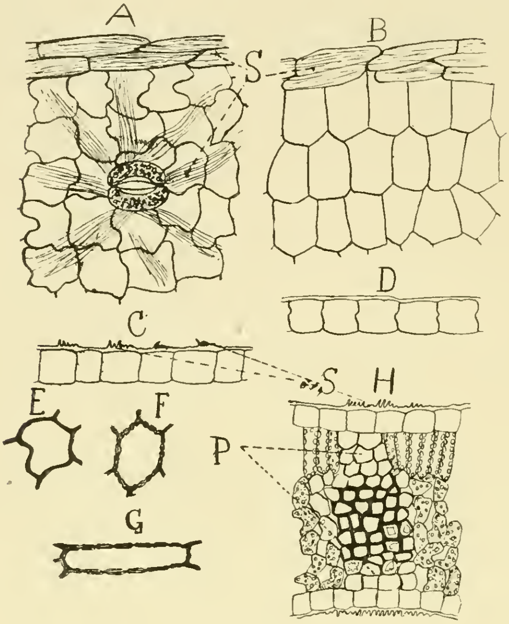


FIG. 11.

A, epidermis, lower surface, showing striations; B, epidermis, upper surface; C, section through epidermal cells, lower surface; D, upper surface, epidermal cells; E, epidermal cell, lower surface; F, epidermal cell, upper surface showing "pits"; G, epidermal cell over vein; H, section through leaf through one of the smaller veins; P, thin-walled parenchyma; S, striations.

The leaf of Ampelopsis has peculiar striations (Fig. 11, H.S.), radiating irregularly from the stomata. Striations are found on both sides of the leaf over the veins. Decrock (An. Sc. Nat. 8, 13, 1, p. 61, 1901) noticed similar striations upon several plants of the Primulaceæ. Corrugations have been observed by Henslow and Stahl, though Stahl assigned to them a far different function from that assigned by Henslow. E. L. Gregory (1886) concluded from experiments performed with many plants having hair coverings upon the leaves, that the basal cells of the hairs were best fitted for the absorption of water.

An interesting instance of the absorption of salts by the leaves of plants is given by Boehm (1875, p. 287), reviewed in Bot. Jahrsber, f. 1875, s. 860.

"Nach einer früheren Untersuchung . . . . . . 'gehen Keimpflanzen der Feuerbohne in distillertem Wasser sehr bald zu Grunde, während dieselben bei Zusatz von Kalksalzen sich vollkommen normal entwickeln. Es sollte nun geprüft werden, ob die Aufnahme des zur Entwickelung nothwendigen Kalksalzes auch durch die Blätter der Feuerbohne geschehen kann. In distillirtem Wasser gezogene Keimpflanzen wurden zu diesem Zwecke täglich drei mal während je 15 minuten mit ihrem obern Ende in distillirtem Wasser mit 2 pro mille Kalksalz eingetaucht. Diese Pflanzen erhielten sich so lang frisch, bis die cotyledonen eingeschrumpft und bei dem Versuchen im Dunkeln sämmtliche Stärke aus dem Stengel verschwunden war, während Controlpflanzen, die nicht in die Kalklosung getaucht wurden, frühzeitig abstarben.'"

This supports the conclusions made by the writer, both in regard to the absorption of water by developing buds and living undetached leaves.

In regard to the question of the absorption of dilute solutions of poisonous substances there is something to be said. It has generally been held that the copper of the bordeaux mixture is not absorbed by the leaves of plants, but by two special experiments, the writer has been able to prove that $CuSO_4$, m/640, or when applied in a more dilute form, is absorbed. Of course $CuSO_4$ is not the compound of copper in the bordeaux mixture, although it is in this form before mixing, still the experiment with $CuSO_4$ will apply to the $Cu(OH)_2$, or whatever soluble copper compound may result from the combination of the substances in the mixture.

If a drop of a dilute solution (m/640) of $CuSO_4$ be placed on the under side of a leaf of Primula stellata, and left for twenty-four hours, no trace of the $CuSO_4$ is found upon the surface, but there is a discolouring, to some extent, of the natural tissue of the leaf. Similar experiments with other salts were performed by Boussingault and by Schlössing, and they concluded that if no salt was visible upon the leaf surface, total absorption took place.

If one were to spot rather freely a similar leaf with a solution of $CuSO_4$, m/640, for four or five successive days, then wash quickly the surface with distilled water, dry, incinerate carefully, collect the ash and dissolve as in qualitative mineral analysis; and if, after getting rid of the surplus acid and evaporating down to a small quantity, this solution be placed in a small test-tube, and in the solution a clean, bright iron wire, in a couple of hours afterwards there will be a coating of metallic copper upon the iron wire even if there be but a small trace of copper present. Piano wire will do for this experiment. Chuard and Porchet (1900, p. 71), state that copper was not found present in the leaves of grapes which had been sprayed with the bordeaux mixture. They state further that the effect of copper upon growth has been exaggerated, but admit that the sugar content of the grape is increased. The deeper green colour of the leaves, they claim, is not due to an increase in the amount of chlorophyll. This statement is not consistent with experiments performed by the writer, nor with those of Griffon (1900, p. 1). A certain stimulus there is, which results in an increase in the size of the chloroplasts, especially in the palisade tissue, and which is referred to in the explanation of the green ring produced by certain solutions applied to the leaves. This green colour may be due in part to a movement of the chloroplasts as noticed by Stahl in the case of other stimuli.

A long series of experiments was performed by Galloway and Woods (1895), to investigate the effects of bordeaux mixture upon the growth of potatoes. They used the substances contained in the bordeaux mixture separately and in various combinations in order to determine which constituent had to do with the stimulus to growth, or whether it was due to the effect produced upon the soil by the solution. They show clearly that, while the bordeaux mixture applied to the leaves undoubtedly increases the growth, it is not the copper compound alone that causes the increase. The lime is found to be a very important factor, and they show that spraying with lime-water causes a large increase in leaf surface, showing that the lime-water is very probably absorbed directly by the leaves and utilized there. The spraying of the soil with lime-water caused some increase, but not so much as the spraying of the leaves.

The writer has shown (Chapter IV.) that $Ca(OH)_2$ in water (lime-water), is absorbed by leaves, and that this substance very probably exerts an active influence upon CO_2 of respiration, retaining it for resorption by the plant.

The experiments of Galloway and Woods show that a spray of water, while not increasing or decreasing the growth of the leaves, produces a decided *decrease* in the gross weight of tubers produced. This may be due to the loss of substances sustained by the leaves due to the drenching with water. (See experiments, Chapters III. and IV.). The experiments just referred to, corroborated by those of De Saussure, Gaudichaud and Sachs, leave no room for doubt that a very considerable amount of inorganic alkaline salts is extracted from leaves by the application of pure water to the leaf surfaces. This may in some cases prove of advantage to the plant, but it also might under certain circumstances prove a decided disadvantage resulting from the too great loss of useful mineral nutrient substances.

Regarding the absorption by leaves of iron in solution, it is easy to speak with certainty ; for, if one secure a plant, chlorotic (from lack of iron in the culture medium), but otherwise in good condition, and place a few drops of a dilute solution of Ferric chloride (m/640) carefully upon a leaf, in twenty-four hours afterwards a green circular area is seen where the drop was applied. Thunbergia alata was the only plant upon which a sufficient chlorosis was obtained to demonstrate clearly this phenomenon. The green area spread over the whole leaf in three days to such an extent that the deep green of the leaf was uniform. Plants chlorotic from starvation, as were some of those referred to in the experiments in Chapter VI., will not become green by the application of an iron salt solution. This fact of the utilization of iron applied to the surface of a chlorotic leaf is referred to by Sachs (1887, p. 285). This same result was obtained some years ago in the case of a Maize plant, by Miss Minns at the physiological laboratory, Botany Garden of Harvard University. Definite information regarding this experiment the writer has not been able to obtain.

Summarizing the results of the experiments here described we may conclude that:—

(1) Salts in solution are absorbed through both surfaces of leaves of plants whether the leaves be detached or not if the surrounding atmosphere be favourable. Absorption generally takes place more readily when the solution is applied to the lower surface.

(2) Dilute solutions applied in drops stimulate the leaf tissue in a ring, whereas if the solutions are concentrated the entire area covered by the drop is affected.

(3) The effects produced by poisonous solutions upon Spirogyra aid in explaining the effects of similar solutions upon foliage leaves.

TOBACCO-LEAF "SPOT."

Tobacco leaves, under certain conditions which are as yet but little known, become somewhat mottled or spotted at a certain period in the growth of the plant, and these spots remain throughout the entire process of curing, and come out on the dried leaf to be used for the wrapper of the cigar. It is generally supposed that leaves having these spots are also characterized by their superior flavour and burning qualities, both of which are valued very highly by those who use tobacco. So important has this become in the marketing of the tobacco leaves that "spotting" has been attempted by means of caustic alkalies, such as carbonate of soda, caustic potash and caustic soda. While this artificial spotting may be only an outward imitation of the natural leaf, yet there are many evidences of its affecting the quality of the leaf from the smoker's standpoint, as will be explained later on. Attempts have been made by some of the smaller tobacco dealers to "spot" the tobacco by means of acids and other chemical irritants, thereby producing a leaf in imitation, to some extent, of the Sumatra leaf, and an article which is more saleable, but which doubtless possesses properties which would render it inferior to the leaf of the same tobacco plant which was not spotted. The only spotting under particular examination, however, is that of the natural Sumatra leaf and of that produced by caustic alkalies.

The real Sumatra leaf when nearly ripe has a peculiar mottled appearance which the leaf retains, more or less, throughout the curing process, and which indicates to the consumer a superior quality. It has been asserted by some who have investigated the cultivation of the Sumatra tobacco that the spot is due to the contact of wood ashes with the leaves during their growth. The plants which produce the best quality of Sumatra cigar wrapper are grown upon recently cleared and burned land. The jungles which contain a large amount of underbrush are made ready for cultivation by fire. The consequence of this is that there is mixed with the top soil a very considerable amount of ash, which, during the course of the summer, is blown by the wind upon the leaves, and which produces certain effects which result in the leaves having this spotted appearance. It is also stated in proof of this, that after two or three years cultivation of this recently cleared land, the tobacco plant does not become spotted to any great extent during its growth; and the leaves are of a thicker and more gummy nature, and are less valuable as a cigar wrapper. If these statements just repeated regarding the cause of the spotting of the Sumatra leaf be true, then it opens up the way to produce it artificially, and that too at little expense. It is well known that ordinary wood ashes contain a high percentage of caustic alkalies, especially caustic potash and caustic soda (if water be added), and that these substances do affect the leaves of plants, causing them to resemble, in outward appearance at least, the leaf grown upon the newly cleared ground in Sumatra.

The Sumatra tobacco when planted early in the year is generally without spots, but when planted late it spots freely. It is grown in Florida quite extensively and becomes spotted similar to the real Sumatra leaf, and in texture and general appearance resembles it closely. It is also cultivated in Cuba.

This tobacco, whether grown in the East Indies, Florida or Cuba, has another rather desirable quality, namely, that of a good "burn." This quality is especially wished for in the wrapper of a cigar. When once ignited it burns away quite steadily without producing flame, and more rapidly than the ordinary tobacco leaf which is used for filling purposes. This can be proved experimentally by placing two or more leaves in similar positions, then by igniting them and comparing the areas burned in a given time. When compared with the bright Virginia wrapper, the writer found that there was little or no difference between them, when the average was taken of a large number of cases. The Virginia leaf was not spotted, but was a thin bright yellow leaf containing apparently very little of this gummy substance generally found in the darker coloured leaves used for filling the body of the cigar.

This quality of burning steadily and rapidly is a desirable one in a cigar wrapper for reasons which the smoker readily understands, and which we need not enter upon here; and it follows that any investigation which results in developing this quality in a plant naturally, or in producing it in a leaf artificially, would be important both from a scientific and from an economic standpoint. The spotted leaf has this quality to a very high degree, and all very thin well-cured leaves possess it also to a considerable extent.

It was shown by Nessler in 1867 that when different kinds of paper, leaves, etc., were impregnated with certain substances the burning quality was affected,—that phosphates and chlorides were detrimental to, and that potassium nitrate aided in the burning.

Adolph Mayer, writing upon combustibility, shows that potassium carbonate was favourable to burning, and that chlorine compounds were unfavourable. If leaves lack potash they have a "poor burn." He shows also that these qualities may be produced in two ways, (1) by using certain fertilizers, chiefly Chili saltpetre, (2) by impregnating the cured leaf with this substance. By saturating tobacco having a "poor burn" with a solution of potassium nitrate or potassium acetate he caused the leaf to have what he called a "good burn." He also found that when Chili saltpetre was used abundantly as a fertilizer for the tobacco crop, a spotting was often produced in the cured leaf, because, as he states, the leaves cured more slowly and unevenly ; and that when the leaves were simply dried instead of cured, a greenish mottled appearance was liable to result.

In Griffon's researches (1900, p. 1), he proves that nitrates in the soil and in nutrient solutions add greenness to the leaves of plants, and that salts of copper in minute quantities augment the dimensions of the chloroplasts and the intensity of the colour, though it may kill the roots. Sodium chloride is uniformly unfavourable, and an excess of lime causes a paleness and a consequent lessening of assimilatory power. The palisade tissue, chiefly, is affected (by the abnormal conditions), both as to the dimensions of the chloroplasts and the cells themselves, and in the colouration of the chloroplast. The writer has found, when decoctions are made of the leaves of several kinds of cured tobacco, by steeping them in distilled water for ten or twelve hours, that these decoctions, to a litmus indicator, show a decided acid reaction in most cases. The litmus paper used was purposely made slightly alkaline in order to insure evenness in quality of the paper and to emphasize strongly the experiment. One can secure litmus paper of an even quality by steeping the paper to be used, in distilled water to which a very few drops of ammonia have been added. The Virginia leaf changed the colour of the deep blue litmus paper in *two* minutes, the Sumatra leaf taking *five* minutes, the artificially spotted leaf *ten* minutes and the leaf artificially spotted then dried, not cured, taking three and a-half hours.

Mr. A. J. Ewart has shown (1896), that plants tend to neutralize both acids and alkalies, and that so long as the substance in which he placed the leaves remained alkaline, the chlorophyll was of a darker green colour. Twenty-four hours produced less effect than sixteen. He experimented with Elodea and Utricularia in a solution of ammonium carbonate (one tenth of one per cent. strength) in water. Darwin, in 1872, noticed that a precipitate in the cell-sap was produced on treatment with dilute alkalies. This shows that the alkali penetrates both the cell-wall and the lining of the protoplasm; only after prolonged exposure were the cells killed. This precipitate consists of tannin and other substances excreted by the ammonium carbonate.

Among the tobacco growers of America there are recognized two diseases, or two forms of the same disease, of the tobacco plant, the "calico" and the "mottled head." "Mottled head" is a condition in which only the uppermost leaves are affected, and where the spotting occurs at a somewhat later stage in the life of the plant. When the plants are affected early in life, the middle and the lower leaves have the characteristic appearance. This is known as "calico." About fifteen years ago there was observed a peculiar mottled condition of the tobacco plant in Holland. Dr. Mayer made some investigations upon this condition and gave to it the name "Mosaic Disease." He concluded that the disease was a bacterial one, but his investigations were not carried on far enough to warrant his conclusion. He proved that a sound plant might be inoculated with infected sap; but that one plant could not infect a neighbouring plant.

In some early works giving a general description of the tobacco plant, we find that one character given is that the plant on reaching its maturity in a natural condition is quite likely to have light coloured spots on its leaves. These spots were in no sense looked upon as a disease, or as being at all hurtful to the plant. The leaves of the suckers from healthy plants or stumps are often found spotted (Sturgis. 1898), and this would tend to show that this feature was not so much a disease as a condition of the plant depending upon soil and atmospheric conditions, or was philogenetic in its nature. It has also been shown by Otto Carl Butterweck that one means the farmer has of knowing when the tobacco is ready to cut is that the leaves begin to turn lighter green, and light coloured spots appear upon them. This is doubtless the time when the plant ceases to draw nourishment from the air and soil, and is now concentrating its nourishment that was scattered through all parts. According to Dunal on Solanaceæ (1852), some species of this genus, Nicotiana, have peculiar grey-spotted, or a dirty green colour naturally; and one species of the genus has its specific name from this character.

About the time that Dr. Mayer was carrying on his investigations in Holland, a disease was described which occurred in South Eastern Russia which resembled very closely the "calico" of America. Some examination was made in detail as to the nature of the disease, as it was called, and the conclusions reached were that the cause was not due to bacteria nor to enzymes, and was therefore in no sense what might be termed an organic disease, but entirely physiological in its nature. It was proved by experiment (Sturgis, 1898) that the lower leaves of a tobacco plant will become spotted when the plant is beginning to starve because of a lack of water; and it occurs similarly when a period of excessive humidity is followed by hot sunshine, which results in a very rapid transpiration. It was, moreover, asserted by some, that spots were produced by drops of water adhering to the leaves and acting as lenses, and in direct sunlight actually burning the leaf. This is probably not true, as tobacco is not one of those plants to whose leaves the water adheres in drops; besides we are unable to bring this about by artificial means upon this plant. Others state that particles of sand, dust, etc., will, when adhering to the leaf, cause a "speck" to be produced as a result of the contact and the irritation brought about by the particle. This is quite probably not correct unless the particle adhering acts chemically upon the leaf cutin and the cells and their contents. It may be that where the land was fertilized freely with ashes, lime, or other rather strongly alkaline substances there is a modicum of truth in the statement; and indeed this would tend to confirm the assertion that the spotting of the Sumatra leaf is due to wood ashes brought into actual contact with the leaves by means of the wind.

A few years ago Marchal described a disease of the tobacco, similar to "calico." He concluded that it was a bacterial disease, as he saw in the spots a bacillus which he was able to grow upon pure culture media, and in turn to inoculate healthy plants with the pure culture. Others have found since Marchal's investigations were made that the mottling is due to a sort of enzymitic action. Now it is obvious that these so called diseases, "mottled-head," "Mosaic disease," "calico," etc., are distinguishable in a general way from the "Tobacco Spot" which is so universally admired in a cigar wrapper; and that so far as investigations have been made upon the subject up to this time, it may be regarded that "Tobacco Spot" is a physiological condition, while the others mentioned are produced by fungi or by enzymes.

This naturally leads up to the producing of the spotted leaf by artificial means. There are various methods employed by the small, but not too scrupulous tobacco dealers to produce an imitation of the Sumatra leaf, as has been already mentioned, but these will not be dealt with here as they have, so far as the writer can learn, no scientific bearing upon the subject under discussion. There is a method, however, of producing a spotted leaf by means of strongly alkaline substances, applied with a sprayer to the leaves, a few days before they are harvested. This process has been investigated in some detail by the writer, and it is found that certain effects are produced upon the leaves, which render them more desirable as a cigar wrapper, for two reasons:—(1) They burn more rapidly and are less liable to become extinguished than similar leaves not so treated. (2) The leaf when cured has a more silky appearance, and is slightly thinner. As to this latter it should be mentioned that a very limited number of leaves were compared, and so too much stress should not be placed upon it. With regard to the former it is found, by experiment with different kinds of leaves, that when saturated with caustic potash, caustic soda or carbonate of soda, then dried and ignited, they will burn more evenly and readily than leaves not so treated. It is the same, but to a much more noticeable degree, with filter paper, writing paper and other kinds of paper. Leaves vary much in the capability of being affected in this way, probably due to differences in chemical constituents of the substances contained in the different plants. Both paper and leaves, when not saturated with the alkali, have a tendency, more or less marked, to burst into flame when ignited; while if first treated, and then dried and ignited, they burn and glow steadily without producing flame.

The next question that naturally arises, is:—Do plants when growing absorb through their leaves any of the alkali which is applied to the leaf surfaces? Certain experiments with the object of answering this question, were performed at the Botanic Gardens, and elsewhere, July, August and September (1899), in order to ascertain whether leaves did absorb substances and transport them to other parts of the plant. Species of the following genera were operated on:—Solanum, Nicotiana, Aralia, Ampelopsis, Fraxinus, Pelargonium and Helenium. The following solutions were used:—Caustic soda in strengths of 1, 2½, 5, 10, 20 per cent.; and sodium carbonate in strengths of 5, 10, 20 per cent. It was found that caustic soda at 10 and 20 per cent. was too strong, producing holes in the leaves after a few days. The strength of solution required to produce a spot similar in appearance to the artificial tobacco spot, differed considerably among the several plants tried, but the ten per cent. sodium carbonate solution seemed to be the most generally successful. Twenty per cent. sodium bicarbonate was used in the Nicotiana, but it was not sufficiently strong to produce a spot and so no further attempt was made with this substance. To each solution was added about one per cent. by weight of lithium nitrate, for the purpose of utilizing the flame test in determining whether the alkali was absorbed and transported to other parts of the plant. It was found in all leaves that were tested by means of the flame and the spectroscope, that the lithia had penetrated to other parts of the same leaf, and also down to the extreme end of the petiole. Lithia was not found in other leaves of the plant, but it is inferred that the strength of the solution of lithia was too low and that the instruments were not sufficiently accurate or delicate to recognize it in such minute quantities, rather than there was no transportation of lithia to other parts of the plant. Lithia was found in few cases in the stem below the leaf, and in a very few cases above the leaf node. Of course it might be objected that the fact of the lithia being found transported did not prove that the caustic soda was conveyed with it. This is a fair objection, because the caustic soda might easily be all decomposed in acting chemically upon the cutin, cell walls and cell contents. However, it is thought fair to assume that at least a certain amount of the caustic soda or the sodium carbonate, as the case may be, would accompany the lithia through the plant tissue. Some of the caustic may be decomposed, as has been proved by the writer in the case of the beautiful liquid colouring matter in the epidermal cells of the red leaves of Ampelopsis in the late autumn. Caustic soda turns the red liquid a deep blue, but this blue rapidly disappears giving place to a greenish colour which soon changes to a yellow. When an acid is now applied, there is no reddening of the liquid, showing that some chemical change has gone on upon the liquid contents by the action of the alkali.

In all the leaves spotted artificially it was found that no spots or mottling appeared except those spots plainly visible when the leaves were lying upon a dark surface; while in all the Sumatra leaves available it was found upon examination with transmitted light that the leaves were all mottled with darker coloured patches, spots and dots, not visible by reflected light. This might suggest the notion that the

Sumatra leaves examined were affected also with a disease similar to the "Mosaic disease," and to "calico."

Now as to the internal effect of the spraying with caustic, the writer has found that the tissue undergoes some changes of importance in the vicinity of the spot. (See Fig. 12, section perpendicular to the leaf surface). It appears that the caustic alkali kills the chloroplast and the protoplasm in the cells where the spot becomes whitish; and that on the darker green ring bordering the light spot, the cells become larger and the chlorophyll bodies more numerous, especially in the palisade cells. There they are found very abundantly along the sides of the palisade cells, which have increased in length very considerably. The guard cells in the ring are well filled with protoplasm and chloroplasts, more so than in the ordinary guard cells of the leaf (see illustrations in Fig. 13). The stomata in the ring present therefore a different appearance in surface view from both the spot and the ordinary tissue. We notice also that the leaf is much thicker and denser in this ring, and that the chloroplasts are larger and more numerous. In the spongy parenchyma there is some enlargement due to the expanded cells and to the increased area of intercellular spaces. In the spot itself the protoplasm was dead, exceedingly pale, and much shrunken. It is quite possible that there is a stimulus to growth produced by the caustic solution used. If this be the case it would increase the activity of the protoplasm in the ring immediately surrounding the part that was dead, and consequently produce more numerous and larger chloroplasts. It has been shown by Griffon, Ewart, Mayer and others, that potassium nitrate and potassium carbonate affected the chloroplasts in some way resulting in increased dimensions and in general in a deeper green colour of the leaves. Griffon shows, too, that it is only the palisade cells that are affected. The writer has found that in the case of Nicotiana and Ampelopsis both

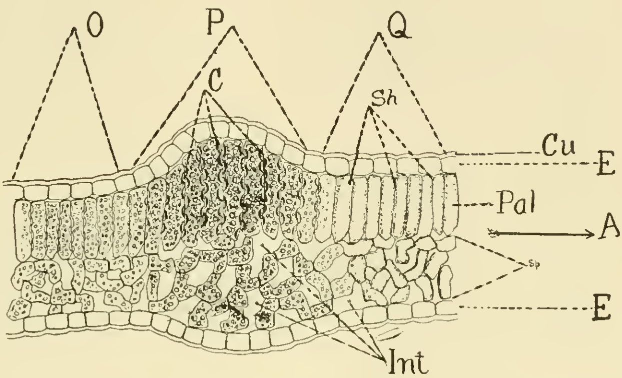


FIG. 12.

Section through tobacco leaf "spot." O, natural condition; P, ring; Q, dead part of spot; C, chloroplasts; Int., intercellular spaces; A, towards centre of spot.

palisade tissue and spongy parenchyma are affected,—including the guard cells of the stomata,—in a manner which is quite noticeable and important (Fig. 13). Outside of this green ring is seen one slightly lighter in colour. This is probably due to the fact that an extra supply of nourishment is now required by the enlarged and more active cells in the green ring, and these cells outside the ring are drawn upon to furnish this supply.

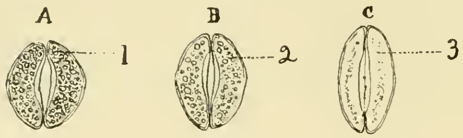
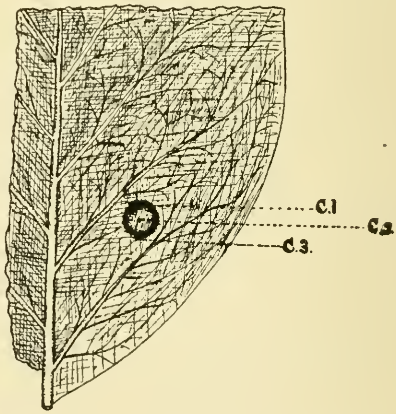


FIG. 13.

Surface view of guard cell of tobacco leaf. A, from area P, Fig. 12; B, natural condition; C, from area Q, Fig. 12.

In experimenting upon Chilomonas, an infusoria, W. E. Garry, (1899, p. 291) shows that when certain solutions are allowed to diffuse gradually toward the colony, a ring of the animals appears. This ring, formed by the crowding of the animals, gradually widens, and with some solutions, after a time, recedes. This would tend to show that the ring is formed of animals, not only those driven away by the irritating liquid, but also of those attracted towards it. In fact H. S. Jennings goes so far as to state that Paramecia are negative to strong acids and positive to weak acids. Whether this offers any explanation to the condition of the leaf around the spot it is not easy to say. At all events the phenomena are similar and in both cases exceedingly remarkable.

The diagrams here given are self explanatory, and are intended to illustrate the general appearance of the spot upon the leaf several days after the application of the alkali to the surface, to show the effect upon the stomata and upon the general tissue of the leaf. The spot does not at first appear light coloured but rather of a rusty brown, gradually becoming lighter. It was also quite noticeable that the thickness of the leaf was somewhat less in the case of cured tobacco leaves, some distance away from the spot than it was in the centre of the spot.



IG. 14.

Tobacco leaf showing spot. C1, deep green ring; C2, dead area; C3, tissue frcm region of O, Fig. 12.

Summary of the results of the investigation :—

There are four causes (in a measure quite distinct), which produce the spotted condition of the tobacco leaf, due to :—

(1) Fungi, (Cercospora, Macrosporium), Bacteria, Enzymes.

(2) Conditions of soil, moisture, temperature, fertilizers, etc.

(3) The recurrence of a philogenetic condition. (This is but a suggestion).

(4) Local applications of chemical irritants.

With regard to the last (4) the important points may be enumerated. The alkali kills the tissue in direct contact with the irritant ; it stimulates to abnormal development the tissue immediately around the spot ; the cells outside of the stimulated area are drawn upon more than ordinarily and are consequently poorer in protoplasm and chlorophyll than that of the tissue in the ring, and in the tissue of the ordinary part of the leaf.

The deductions to be made from this are, that plant growth may be stimulated by local applications to leaves, that leaves can transport the absorbed substance, and consequently the texture of the leaf may be modified by artificial means.

IX.—SOME OF THE EFFECTS OF SEA-WATER ON THE AIR.

The question as to whether the inorganic salts in solution in sea-water ever pass off into the air, in the neighbourhood of the sea, in any measurable quantity, has so far never received much attention from scientific men, but it has long been suspected by many people that sea-water does, in some way or other, enter the air, but little has as yet been done in a scientific way to ascertain the nature of the process, if any, by which the sea salt and other inorganic substances may leave the solution and permeate the surrounding atmosphere. It is obvious that, upon the occasion of storms and winds, the sea spray is blown into the air, and if the air be below the saturation point, as it generally is, much of the water evaporates while suspended in the air, and in consequence, minute particles of salts are left floating in the atmosphere, and later are blown about and deposited upon the surface of the sea, or upon the land and the leaves of plants within a reasonably short distance of the sea shore. Leaving this condition altogether aside, it is still an open question whether the salt may be taken up into the air without the aid of wind or spray and afterwards deposited upon substances that have a physical contactile affinity for the salts ;

and with many of these substances with which these salts come into contact, form chemical compounds of a more or less stable nature. With this object in view, from the standpoint of vegetable physiology, it was thought advisable here to ascertain in an indirect way whether it be possible for substances in aqueous solutions to leave the solution, not with the liquid as a liquid, or as a vapour in the state in which the particles are so large as to refract and reflect light and therefore render it visible; but to leave the solution with the solvent water, or in small particles that are in the layer immediately upon the surface of the water, at the same moment the water particles become taken up by the air.

A series of experiments was arranged, as shown in Fig. 15.

Four kinds of iron were obtained:—(1) Chemically pure iron (98.8%); (2) Fine soft iron wire; (3) Coarse, common iron wire; (4) Piano wire. Each of these specimens of wire was cut up into three approximately equal portions, and one from each specimen was placed under a bell-jar in a watch glass perfectly clean. Under the jar was a crystallizing dish filled with sea-water in one case, salt water in the second case, and pure water in the third. Each jar then had under it four different kinds of iron, and excepting for the liquid used, all conditions were as nearly as possible the same. The specimens of iron were all weighed with the nicest accuracy upon commencing the experiment, and they were in clean watch glasses in order to be able to see if any of the solution happened to be precipitated in any way, or come in contact with the metal while the solution was in the air in the form of a liquid or spray. A liquid containing even the faintest trace of a salt in solution will leave plain evidence of the salt if the smallest part of a drop be allowed to evaporate from the surface of a well-cleaned glass. The surface of the watch glass was very large in comparison with the specimen upon it, and it is fair to conclude that if no salt settled in any way upon the glass none came in contact with the iron in the same way. The jars were each placed upon four glass plates laid

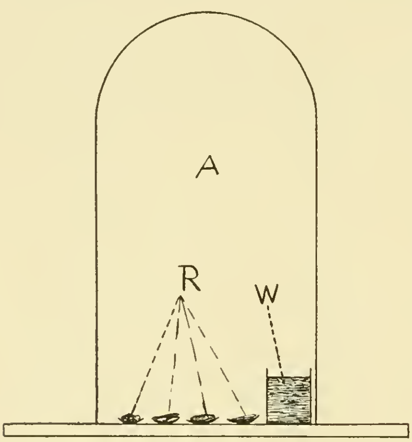


FIG. 15.

R, samples of iron; W, liquid used.

about half an inch apart, leaving an open ventilating canal in the form of a cross, in order to secure some ventilation and yet to have no direct current of air within the jar. Each jar was removed every two days and sprayed upon the inside from a wash-bottle having a fine nozzle. This spray was of a liquid corresponding to that in the receptacle under the jar:—(1) Sea-water; (2) Salt water; (3) Distilled water. The spraying was done at some distance from the specimens, and in no case was there enough of the spray to cause drops to run down and off the side of the bell-jar. The object of this was to secure a moist atmosphere and to aerate fully.

There was one very peculiar phenomenon which developed during the experiment and one which is worthy of note. The jar containing the sea water did not become dry on the inside, even after four or five days, while the others dried completely (during the course of the first experiment) in twenty-four hours or less, consequently the jar containing the sea-water was not sprayed as often as the others, although it was the intention, when the experiment was arranged, to spray them all regularly. Why did the jar containing the sea-water remain moist,—the water clinging in small drops—while the others dried so readily? This phenomenon raises one of the most interesting questions relating to atmospheric conditions, and the effect upon plant and animal life. The experimental work belongs rather to physics and chemistry than to botany, so it will be deemed sufficient to merely answer the question leaving the details to the realm of physics where it belongs. Experiments were performed with some solutions, simple and compound, by placing a drop upon a leaf and leaving it undisturbed for several hours, and in some cases for several days, for the purpose of finding out whether the solution was absorbed by the leaf. During the performance of this experiment, among other things observed was that certain solutions did not seem to evaporate for some days, while others became dry in from one to six hours. Some solutions remained upon the leaf, having the appearance of a drop of water, for a remarkably long time. The same was then tried with glass plates, instead of leaves, and it was found that similar results were obtained—the same solutions remaining moist. A comparison was then made between glass and leaves and results obtained which showed that the moist condition was kept up longer by those drops which were on the leaves. Certain salts are very hygroscopic in their nature, *e.g.*, magnesium chloride, sodium acetate, etc., and it was these salts which remained longer moist,—retaining not only their necessary water of crystallization, but also enough besides to keep the whole in a liquid form. Certain mixed solutions exhibit this property to a greater degree than a solution containing only a single salt, but of equal concentration. It is thus, probably, with this complicated solution known as sea-water.

After the conclusion of the experiment described above, owing to some rather peculiar results as shown by the figures in the tables of weights given, it was thought advisable to continue the experiment for some time longer and to add another,—a fourth specimen of iron. This was a piece of piano wire, bright and clean. The other specimens, being now rusted, were cleaned with emery paper,—the rust being removed in this way—and after being carefully weighed were again placed under the jars. An interesting phenomenon had developed, in consequence, probably, of the change in humidity of the room in which the experiment was conducted, due to the increased firing necessary to heat the building. (The experiments were carried on in the basement of the university museum where during autumn there is considerable humidity in the atmosphere and where, when extra heating is required, the atmosphere in the basement becomes very dry).

Samples A, B, C, were of coarse iron wire, well cleaned.

Samples D, E, F, were of finer soft iron wire, well cleaned.

Samples G, H, I, were of 98.8% pure iron wire, well cleaned.

Samples O, P, Q, were of fine piano wire.

Samples A, E, H, O, under jar No. 1 (sea-water).

Samples B, F, I, P, under jar No. 2 (salt solution).

Samples C, D, G, Q, under jar No. 3 (distilled water).

The weighings are given in grams.

TABLE I.

Specimen.	Weight.		Gain.	Gain %.	Time, *14 days*.
	Oct. 29th.	Nov. 12th.			
A	8.1000	8.1128	0.0128	0.1580	
E	1.9446	1.9480	0.0034	0.1748	
H	0.5710	0.5809	0.0099	1.7340	
B	7.2891	7.2950	0.0059	0.0807	
F	1.9315	1.9336	0.0021	0.1087	
I	0.5230	0.5300	0.0070	1.3380	
C	7.6096	7.6128	0.0032	0.0420	
D	1.8845	1.8856	0.0011	0.0583	
G	0.6764	0.6810	0.0046	0.6800	

TABLE I. a.

Specimen.	Weight. Nov. 12th.	Weight. Nov. 26th.	Gain.	Gain %.	Time, *14 days.*
A	8.1128	8.1145	0.0017	0.0209	
E	1.9480	1.9499	0.0019	0.0975	
H	0.5809	0.5839	0.0030	0.5760	
B	7.2950	7.2950	0	0	
F	1.9336	1.9350	0.0014	0.0718	
I	0.5300	0.5305	0.0005	0.0943	
C	7.6128	7.6159	0.0031	0.0407	
D	1.8856	1.8888	0.0032	0.1690	
G	0.6810	0.6859	0.0049	0.7190	

TABLE II.

	Nov. 27th.	Dec. 13th.			
A	8.0589	8.0642	0.0053	0.0657	*16 days.*
E	1.8471	1.8491	0.0020	0.1082	
H	0.5409	0.5450	0.0041	0.7580	
O	2.6095	2.60995	0.00045	0.0172	
B	7.1251	7.1247	0	0	
F	1.9101	1.9101	0	0	
I	0.5093	0.5093	0	0	
P	2.5559	2.5559	0	0	
C	7.7426	7.7426	0	0	
D	1.8586	1.8591	0.0005	0.0269	
G	0.6581	0.6608	0.0027	0.4100	
Q	2.56455	2.5646	0.00005	0.0019	

TABLE II. a.

	Dec. 13th.	Jan. 4th.			
A	8.0642	8.0661	0.0019	0.0235	*22 days.*
E	1.8491	1.8510	0.0019	0.1027	
H	0.5450	0.5453	0.0003	0.0550	
O	2.60995	2.60996	0.00001	0.0003	
B	7.1247	7.1247	0	0	
F	1.9101	1.9105	0.0004	0.0209	
I	0.5093	0.5093	0	0	
P	2.5559	2.5559	0	0	
C	7.7426	7.7456	0.0030	0.0387	
D	1.8591	1.8613	0.0022	0.1183	
G	0.6608	0.6619	0.0011	0.1664	
Q	2.5646	2.5649	0.0003	0.0117	

As indicated in these tables there are some rather peculiar phenomena, some of which are not easy to explain. Experiment I. a. was simply a continuation of I., as the specimens were weighed and returned to the jars without being cleaned of the accumulated rust. The same is true of II. a.

In both cases where the specimens were put in, well-cleaned of rust, those in the atmosphere under the influence of sea-water had made the largest proportional increase in weight, showing that the sea-water affected the atmosphere, which in turn caused an addition in weight of the iron due to oxidation. In jar No. 2, in which was the salt solution, there was found a greater percentage increase than in that of the distilled water. This, it was expected, would show that salt and water would always produce an increase in weight over that of the distilled water, and that the substances in solution had to do with the increased accumulation of rust, but in all the subsequent weighings this was reversed,—that of the salt and water showing little or no increase in weight. This seemed to upset any preconceived notions, or any conclusions that might be drawn from the first experiment, but when one takes into consideration other atmospheric conditions there is an explanation which may be fairly reasonable. This peculiar phenomenon, as was mentioned before, was associated with the difference in atmospheric humidity in the room where the experiments were performed. Almost all through the course of the first experiment the humidity was from seventy to eighty, and temperature fifty-five to sixty degrees F., while during all the other experiments it was at thirty-five to forty and temperature sixty-five to seventy. At first, when the humidity was about seventy-five, the jars remained moist for about twenty to twenty-four hours, whereas in the later experiments the moisture disappeared in five to six hours in jars two and three.

At first it was thought sodium chloride and other salts *might* pass into the air, but these experiments point rather to another conclusion, namely, that the salts affected the formation of iron oxide, only where it contributed to the atmospheric humidity, and that sodium chloride actually protected the iron from rust by absorbing water vapour from the air and thus reducing still further its humidity. The reason why the sea-water salts did not produce the same effect as the common salt solution was because of its extraordinary hygroscopic properties maintaining an atmosphere with more moisture than would be the case with a sodium chloride solution. It should be mentioned that during the latter part of the experiment the inside of jar No. 2 was covered with salt crystals, due to the residue of solution sprayed upon the inside; and there had been placed inside the jar at the commencement of the experiment a watch glass containing dry salt. Besides this, the salt from the solution in the crystallizing dish had crept up round the margin of the dish, and there had collected in considerable quantity, but only on the half of the margin on the side towards the source of light. No attempt is made to explain this last mentioned phenomenon. The greater increase per cent. of one sample of iron in jar three as compared with the same in jar one in experiments I. a. and II. a. is due to the fact that the specimen in jar one had been fairly well coated with rust from experiments I. and II.; and so the iron having the smallest amount of rust upon it at the commencement of the experiments I. a. and II. a., made, as might be expected, the greatest gain in the succeeding experiments.

From these experiments we might conclude :—

(1) That sea-water causes the atmosphere to produce rust upon iron to a greater extent than does fresh water.

(2) That the presence of salt (NaCl) causes an accumulation of rust upon iron when the humidity is high—about seventy to eighty—but when the humidity is low—about thirty-five to forty-five—it prevents rust formation.

(3) That sea-water affecting the atmosphere in maritime localities may also affect vegetation.

The only investigations of importance relating to the question of chlorides in the atmosphere were carried on at Rothampstead, England, by Lawes and Gilbert (1883), and by Dr. Frankland (1881). Their determinations were made by analyzing the water from rain-falls, and, in the case of Lawes and Gilbert, the experiments extended over a period of six years, 1877-83. The results obtained by these men did not fall into any very regular series and several points were left by them unexplained. Quoting from Lawes and Gilbert :—" In the account given in the earlier results of this investigation it was pointed out that the winter rain-fall was far richer in chlorine than the summer rain-fall; we are now able to take a step further, and show the general character with respect to chlorine of each month of the year. The minimum amount of chlorine occurs in the rain of July. In August and September there is a distinct but not a very large increase in quantity. In October and November a great rise occurs, the quantity of chlorine contained in the air being three times as large as during the preceding months. After the period of maximum there is a fall, but the chlorine remains high throughout the winter months, the diminution towards the summer period not commencing till April. The rain of March has yielded the highest proportion of chlorine per million of water, but this is partially due to the small rain-fall of this month. Rather more than two-thirds of the annual supply of chlorine is contributed by the winter months." And they say further in explanation :—"It would appear that in summer the supply of chlorine is very limited, for a large increase in the rain-fall is attended with but little rise in the quantity of chlorine brought down per acre. In winter, on the other hand, the supply of chlorides in the atmosphere is so constantly renewed, that an increased rain-fall results in a considerable addition to the supply per acre. The rather wide irregularities in the composition of the groups of rain-fall for the whole year, are principally due to the different proportion of summer and winter months which enters into the various groups.

"The large excess of chlorides found in winter rain is probably due in a great measure to the chlorides volatilized during the combustion of fuel; the excess in question is too uniform to be dependent chiefly on the action of strong winds blowing from the sea; it is also remarked in calm months as well as in stormy weather. Exceptionally high results are, however, probably due to storms. When we turn to the nice gradations observed among the winter months, it is difficult not to believe that the temperature of the air has some influence on the results. In the more rarified atmosphere of summer, gaseous diffusion will probably be more active, while the power of transporting minute solid particles will be diminished.

"It is difficult to ascertain the influence which the direction of the wind has had on the composition of a monthly rain-fall; a partial study has been made of the data at our disposal, but with no definite result."

From this it would seem that no satisfactory explanation was then known for the small amount of chlorides in the air in summer in comparison with the winter months, and if we except the volatilizing of sodium chloride by combustion, and the spray of the sea, there is no explanation given for the chlorides being in the air at all. Both of these, however, do not account for it; but from the light of the experiments of Lawes and Gilbert, and of those described in this paper, the conclusion is suggested that the leaves of plants may absorb sodium chloride in solution from the atmosphere, resulting in a decrease in the amount during summer. If it were not for plants, therefore, there would be a greater amount in the atmosphere in the summer than in the winter; and when vegetation is checked in October and November, and leaves have fallen, one would expect the largest amount when the temperature was still comparatively high and the leaves incapable of absorbing much of the chlorides. This is, however, only a suggestion.

In Griffon's researches (1899) it is shown that leaves of plants in the vicinity of the sea differ in assimilating power from those of the same species inland. He shows that for a given unit of area of leaf surface there is less assimilation in the leaf of a plant grown near the sea than of one inland. Whether this is due to salts in the soil or to salts in the air Griffon does not say. This may in part be due to salt water in the soil, but possibly not wholly so, as the chief differences are to be found in the leaf rather than in other parts of the plant.

In the work of Smith (1872), there are many and extensive collections of tabulated results of analyses of rain-water in northern Europe. The principal analyses were made of the rain-fall of England and Scotland, and minute details are given. He shows that chlorides and sulphates, as well as many other substances, are found in the air, and states that the amount of chlorides depends upon two things:—(1) the proximity to the sea; (2) the combustion of fuel in factories. He concludes, however, that the presence of chlorides was not wholly due to spray, for he says:—"The common salt from the sea is not spray, or at least not spray purely; if it were so there would be the relative amount of sulphates to chlorides which we find in sea-water." It has been observed that salt is often found on windows far from the sea when a violent wind is blowing. Now the question naturally arises, did the salt reach the glass as an aqueous solution (in small drops) or as dry particles? If it were carried in the form of small drops that would be simply as rain or mist; but such, however, is not the case as there is neither rain nor mist, but simply a strong wind blowing from the sea. If it were in the form of dry particles one would scarcely expect it to stick to the dry pane of glass. How then was the salt conveyed from the sea? It was this question that the writer attempted to answer by the experiments described at the beginning of this chapter. Though some rather important phenomena were developed in the course of the experiments, yet little light was thrown upon the above mentioned question. That chlorides and sulphates and other inorganic salts are in the air in rather considerable and constant quantities is what mainly concerns us here.

Smith gives :—

London, 1869, chlorides	1.2 grams ;	sulphates	16.45 grams	per	million.
Glasgow, " "	8.97 "	"	70.19 "	"	"

Ad. Bobierre (Smith, 1872), gives :—

Nantes, ammonia	1.997	grams	per	cubic	meter.
salt	13.9	"	"	"	

M. Bobinet (Smith, 1872), gives :—

Paris, calcium sulphate 20 grams per million.

According to Smith the sulphates found in the air are alkaline sulphates, and these sulphates are largely the product of organic decomposition. On a complete analysis he found that a hectare of land at Caen received in rain-fall in one year :—

NaCl	37.5	kilograms.
KCl	8.2	"
$MgCl_2$	2.5	"
$CaCl_2$	1.8	"
Na_2SO_4	8.4	"
K_2SO_4	8.0	"
$MgSO_4$	6.2	"
$CaSO_4$	5.9	"

and besides these he found ammonium salts, iron oxide, and oxide of manganese and nitric acid.

From this it may be seen that rain-water is an excellent nutrient solution in a very dilute form and it is extremely probable that some of the food substances of the plant are obtained directly from this source.

One important conclusion may be drawn from the experiments described in this chapter. Since growth of plants is dependent for energy upon destructive metabolism, and since destructive metabolism is (for aerobic plants) dependent upon the absorption of free oxygen (Vines, p. 332), it follows that because oxidation of iron is hastened in an atmosphere under the influence of sea-water, growth may be hastened by a similar chemical process. What applies to the destructive metabolism of plants applies in a large measure to the same process in animal life.

X.—ON THE EFFECTS OF WATER AND NUTRIENT SOLUTIONS UPON DEVELOPING BUDS OF WILLOW TWIGS.

In order to investigate in some detail the question as to whether nutrient solutions affected the budding and the general commencement of growth of twigs of a plant belonging to the genus Salix, and as to the *manner* in which they affected growth, a series of experiments was performed during the months of March and April, 1900. Apparatus was arranged as indicated in Fig. 16.* The twigs were taken at a season of the year best suited to the purpose, the internal conditions of the plant being then of such a nature as to produce immediate active growth if subjected to right conditions of temperature and of moisture. The twigs were, as nearly as may be, of uniform size and quality. As will be referred to, a comparison was also made between the effects of distilled water and of tap water upon the development of roots and buds. Willow twigs, being of such a hardy nature, and having the capability of sending forth adventitious buds and roots, even under rather unfavourable circumstances, and each small part of the twig being in itself, so to speak, the embryo of a new plant, they lend themselves readily to experiments designed for various ends. The main purpose of the experiments here described was to find out whether the bud, as it is developing, absorbs water or solutions, and whether it is affected by the liquid in which it is immersed. This is the main point. The other results recorded are subordinate to this, so far as this paper is concerned, and will consequently receive less attention, but they may not be subordinate from the standpoint of scientific interest.

Three twigs were placed as in Fig. 16, A B, with both ends in liquid, the first, (a), with both ends in distilled water, the second, (b), with distilled water in the lower vessel and a nutrient solution in the upper, the third, (c), with nutrient solution in the lower and distilled water in the upper vessel. Each twig had two undeveloped buds in each jar of liquid, and several others outside the jars. The lower jar in each case was wrapped with dark paper to exclude most of the light. The upper jars were of transparent glass and had no wrapper. The twigs were all inserted alike in having the top, or smaller end, uppermost. The upper and the lower liquids were renewed from time to time as occasion required ; and in the case of (b), there was a complete

* These diagrams are for the purpose of illustrating accurately the whole conditions under which the experiments were conducted.

change necessary occasionally, owing to the very rapid growth of algae in the solution, which, if not removed, might in a short time become a hot-bed of bacteria injurious to the young buds upon the twig.

Observations were made and results recorded upon five different times during the course of the experiment which lasted over a period of forty-three days.

On March 19th, five days after the apparatus was set up, it was found that upon (a) there were growing buds between the two vessels

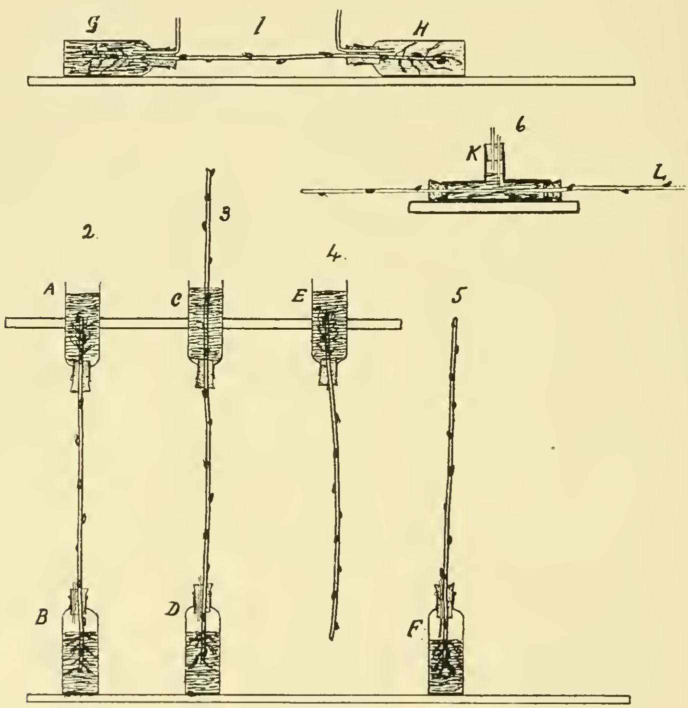


FIG. 16.

G and H contain water or solution as required. A, C, E, bottles with bottoms removed and upturned to hold liquid, cork being made so as to prevent liquid from running out. K, is a large "T" tube filled with water through which a twig passes.

but none in either liquid. Twig (b) had one growing bud *in* the upper liquid but no others growing. Twig (c) had none at all.

On March 26th, twig (a) had no growing buds in the upper liquid but had very healthy buds between the jars; twig (b) had growing buds between the jars and the two in the upper solution were also growing; (c) had growing buds between the jars but none in the upper liquid.

On April 8th, (a) had no buds in the upper liquid while (b) and (c) had.

On April 23rd, twig (a) had roots in the upper liquid but no growing buds; (b) had leaves between the jars, the lower ones especially drying up; those in the upper liquid were living and fresh; (c) had roots in the upper liquid, and one of the buds was developing, seemingly at the expense of the other.

April 26th, (a), roots but no buds developed in the upper liquid; (b), no roots developed in the upper liquid but the leaves were flourishing; (c) had roots and one green branch in the upper liquid.

On April 26th, all were taken from the liquids and the root systems compared. It was found that no buds developed in the solution in the lower jar; the roots of (a) were the most healthy looking and the most flourishing in every way; (b) was second, and (c) was much the poorest in development and had a miserable looking root system in comparison with the other two.

From these experiments we may conclude, (1) that a nutrient solution, such as that used here (in comparison with pure or tap water), does not favour the development of a healthy root system of this plant. It would not be safe, perhaps, to make this conclusion upon two or three experiments alone, but in every case of similar and of different experiments there was no exception to this. On young roots, water, in every case, seemed more favourable to the development of a healthy and extensive root system. (2) Leaves can live and develop in water and in a nutrient solution. (3) The development of roots and leaves is not, as is often stated, confined to the poles,—the one system at the one pole, the other at the other,—certain conditions of moisture supply having an important bearing upon their development. An important point to note, and one which will be discussed later, is that the nutrient solution in some way or other affected injuriously, after a time, the leaves on the plant, other than those immersed in it, and that the solution (as in b) affected the root system. This point is the stronger when compared with other similar experiments.

Coincident in time with the experiment just described was one set up as shown in Fig. 16, CD, in which we have (d) with distilled water in both the upper and the lower vessels; (e) with distilled water in the lower vessel and nutrient solution in the upper (two buds in the liquids in each case).

March 19th, we found small buds developed, *(all)* above the upper liquid in (d); in (e) no buds developed yet.

March 26th, (d), all the buds above the upper liquid were developed; (e) small buds developed in the upper solution which was somewhat green owing to a growth of algae which seemed to thrive vigorously in the solution used.

April 8th, (d), both buds developing in the upper solution and some above but none below; (e) both buds in the upper solution growing, several above and one below were growing.

April 23rd, (d), leaves all living above the upper solution; (e), leaves all dried above the upper solution and below it, but not in the upper liquid.

The one important difference between this experiment and the preceding one is that there was no development of roots in the upper liquid, and almost no development of leaves between the jars where there was a vigorous growth of leaves in Series I.

At the conclusion of the experiment, upon examining the root systems of the twigs, it was found that (d) had a healthier root system than (e). This must have been due to the difference of conditions, which were, that there was a nutrient solution in the upper jar in the one case, and water in the other, the nutrient solution above, having *apparently* entered through the buds and penetrated as far as the roots.

Seriės III. was arranged on the same date as the others, and according to the plan shown in Fig. 16, 5; (f) was in a nutrient solution; (g) in distilled water; and (h) in tap water. (Two buds in each liquid, and several others besides).

March 19th, the roots of (f) were most numerous but short, and the buds of all three twigs were commencing to develop; little if any difference between (g) and (h).

April 8th, the tips of the buds of (f) were dead, while the buds of the others were living.

April 23rd, the leaves of (f) were all withered and the twig appeared dead. Both the others were living. The root systems were compared and it was found that the roots of (f) were rather numerous but stunted and of a yellowish brown colour, while those of the others were long, whitish and healthy-looking. When these results are correlated with those of the preceding it is found that, in a measure at least, they confirm certain conclusions previously stated.

Series IV., shown in Fig. 16, 4, was set up at the same time as the others and consisted of three jars with twigs as shown in the diagram, having in the jar in (i) tap water, and having the "butt" end of the twig in the jar. In (j) there was distilled water in the jar but the twig was placed with the "top" end in the water. In (k) the "butt" end was in the solution, (nutrient).

March 19th, (i) had five healthy roots (one of which was about an inch long), growing equally from all sides of the twig and nearly perpendicularly to its axis. Twig (j) had sent out four short roots and (k) two roots, one of which was about three mm. long.

April 8th (i) seemed to be thriving excellently both as to roots and buds. Twig (j) had a very poor growth; (k) had a few roots in the liquid but they were short and the tips were darkened; a couple of buds were developing near the tip of the twig.

April 23rd, (i) flourishing nicely; (j) not dead but exceedingly slow in growth both as to roots and buds; (k) buds become darkened and seemed to be drying up, the roots were discoloured and of a stunted growth. We note the difference in growth between (i) and (j) when moisture and polarity are contending forces; also the effect again of the nutrient solution upon the growth of both the roots and the buds.

Series V., Fig. 16, 1, in the diagram shows the arrangement of the apparatus. Two sets were arranged—(l) and (m), all the jars being wrapped about with black paper and no observations were made of the ends of the twigs until the close of the experiment on April 23rd. In (l) there was distilled water in both jars; in (m) nutrient solution in both jars. Before the close of the experiment it was observed that both twigs had considerable growth, but that in the case of (m) the leaves had a tendency to curl and to turn black, wilt and die, though the plant was certainly living at the close of the experiment. Upon examining the ends of the twigs on April 23rd it was found that (l) had many roots at the butt of the twig, some of them 75 to 100 mm. in length, that it had ten roots at the "top" and one green bud. Two of the roots at the "top" were each 60 mm. long. The roots at the "butt" of (m) were short and stunted, none being over 25 mm. long. The tips of these roots were brown in colour. It had developed two roots at the "top," and also one bud. These roots, strange to say, were longer than those in the jar at the "butt" of the twig.

To test if water might enter through the bark, an experiment was arranged as shown in Fig. 16, 6, and left from March 14th to April 23rd (forty days). One internode of the twig was kept in water, while both adjacent nodes and the free ends were in the air. Absolutely no growth occurred and the twig dried and shrivelled up at both ends. The middle which was constantly in water shrivelled up considerably.

From all these sets of experiments, varied as they are, some rather important general conclusions may fairly be drawn. The nutrient solution when applied to developing buds of the willow seemed to affect the development of the roots of the twig in the same way as when applied to the place of origin of the roots. It also affected developing buds, other than those immersed in it in the same way as they were affected when the roots were in the liquid. All these results point towards absorption of the liquid by the developing bud.

Water was in all cases more favourable to growth than was the nutrient solution; and distilled water was more favourable than tap water.

XI.—SUMMARY OF RESULTS AND CONCLUSIONS.

Wilted leaves, whether detached from the plant or not, will absorb water, if immersed, or if water be applied to the surface in the form of a spray. Weighing a leaf or a branch to estimate the amount of water absorbed, will be deceptive, because a certain amount of substance is extracted by the water; and unless this substance be taken into consideration in the weighing, a loss instead of a gain *may* result, and yet an absorption of a considerable amount may have taken place.

Special parts of leaves of certain plants seem to be adapted to the purpose of absorption as shown by the surface of the epidermal cells over the veins, at the base of the trichomes, and in other regions. Trichomes in some cases are particularly susceptible to the action of water and of solutions applied to them. Striations and hairs or trichomes aid exceedingly in spreading liquids over the regions which seem to be adapted to absorption; and trichomes prevent a rapid evaporation of the liquid so spread, by retaining air. Absorption of water may take place also through the surface of the petiole.

Guttation drops and dew-drops contain substances in solution which are generally resorbed by the plant. Carbonates as incrustations may serve to store up, in the presence of moisture, CO_2 at night, and utilize the same as the bicarbonate is reduced to the carbonate in the day time. Incrustations may be, therefore, not only an adaptation to retain water in desert countries, but also to utilize to the full the loss of CO_2 caused by respiration. $CaCO_3$, though insoluble in water, may be absorbed if water and CO_2 be present.

Distilled water becomes alkaline, generally, if allowed to remain upon leaves of plants for a shorter or longer period of time.

Certain plants adapted to a moist climate may be made to take in all the food necessary for growth through the leaves. Distilled water used as a spray acts for a time as a stimulus to growth. It may be that it acts as a means of drawing from the plant surplus alkaline salts which, if formed in too large quantity in the cells, might become harmful. Calcium and sodium compounds, and also potassium oxalate have been extracted from leaves by distilled water. Rain water may act as a stimulus in this way.

Solutions if applied to the surfaces of detached leaves, or to leaves upon the plant are generally absorbed as shown by the increased content of ash. Solutions, so applied, often stimulate a certain portion of the tissue to an abnormal development. The ring produced upon a leaf by the application of a drop of solution, is the result of the peculiar action of the evaporating drop.

Solutions applied to the cut ends of the petioles of leaves are generally conveyed to the minute terminations of the tracheids, where they kill the tissue in one of two ways:—(1) by drawing water osmotically from the cells into the intercellular spaces, producing a translucent appearance of the tissue ; (2) by chemical action,—upon the walls of the cells, (b) upon the protoplasmic membrane, (c) upon the protoplasm as a whole. The first determinable reaction after death is alkaline, even though the tissue be killed by an acid. Some leaves will remain green and fresh longer in a dilute solution of the poison lead acetate, than they will in either distilled or tap water. This applies to leaves which ordinarily wilt away in water in a fortnight or so.

Certain substances in solution ascend through the blade of a leaf, at rates which vary as the lengths of the different veins of this leaf, and the area of the part affected is symmetrical to the area of the whole leaf.

The lithium test gives rise to error because the water ascends faster than the lithium, and because the rate of ascent in the same leaf, varies as the length of the vein.

A detached leaf is a living thing which may continue its functions, to some extent, for several months after being detached from the plant.

The food required by woody branches of Salix in the early growth of spring is water. A nutrient solution at this stage proved harmful. Water and nutrient solutions are not absorbed through the bark but affect the developing bud and young leaves in a manner which seems to indicate absorption through the buds.

Since sea-water affects the atmosphere in such a way as to produce an accumulation of rust upon iron, greater than that produced in an atmosphere under the influence of pure water, it is reasonable to conclude that the atmosphere in the neighbourhood of the sea may affect plants, because physiological processes are associated, in large measure, with chemical processes.

The best thanks of the writer are due to Professor Goodale for opportunity and much kindly assistance and encouragement in regard to this paper, to Professor Sharples for material and help in the work on the "spotting" of the Tobacco leaf, and to Mr. Robert Cameron, foreman of the Botanic Gardens, for material cheerfully furnished at all seasons.

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THE WINDWARD ISLANDS OF THE WEST INDIES.

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(Read 2nd November, 1901).

CONTENTS.

INTRODUCTION AND HOW TO REACH THE ISLANDS.

THE West Indies have had a long and thrilling history, including even the small Windward Islands, that separate the Caribbean Sea from the Atlantic Ocean. These lesser Antilles were formerly a source of great commercial wealth. They have been the birth place of many distinguished families, and the scenes of actions of world-wide importance. But most of these things and their literature are more than half a century old. The small amount of scattered knowledge concerning their physical features scarcely amounted to more than a statement that they were volcanic islands or coral rocks. It was even most difficult to get information as to the facilities of travelling about among the islands, especially the smaller ones. Although some popular books of travel have been written, the best account of the features of the islands is that of Élisée Reclus.* But the different islands have varied and most interesting geological and geographical phenomena. It was for the study of these that I visited the Windward Chain in 1896-'97; for previously, I had discovered in the West Indian region the evidence of the great changes of level of land and sea in late

* "The Earth and Its Inhabitants." Vol. II., pp. 431-486. (Appletons, 1893).

geological times, which was published several years ago.* The results of my investigations of the geology of the Windward Islands have only in part been completed, and that portion has recently been published by the Geological Society of London.† The final studies have not yet been made. The object of the present paper

Map of the Windward Chain of Islands.

is to describe the physical geography of the region and the changes it has undergone.

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* "Terrestrial Submergence Southeast of the American Continent." Abs. Bull. Geol. Soc. Am. Vol. V. (1893), pp. 19-22, with map.

"Reconstruction of the Antillean Continent." Ib. Vol. VI. (1894), pp. 103-140.

"Geographical Evolution of Cuba." Ib. Vol. VII. (1895), pp. 67-94.

"Great Changes of Level in Mexico and Interoceanic Connections." Ib. Vol. IX. (1897), pp. 13-34.

"Late Formations and Great Changes of Level in Jamaica," and "Resemblances between the Declivities of High Plateaus and those of Submarine Antillean Valleys." Trans. Canadian Institute, Toronto. Vol. V. (1898), pp. 324-357 and 358-368.

† "On the Geological and Physical Development of Antigua." Quar. Jour. Geol. Soc., Lond. Vol. LVII. (1891), pp. 490-505.

"On the Geological and Physical Development of Guadeloupe." Ib., pp. 506-519.

"On the Geological and Physical Development of Anguilla, St. Martin, St. Bartholomew and Sombrero." Ib., pp. 520-533.

"On the Geological and Physical Development of the St. Christopher Chain and Saba Banks." Ib., pp. 533-544.

"On the Geological and Physical Development of Dominica, with Notes on Martinique, St. Lucia, St. Vincent and the Grenadines." In preparation.

"On the Geological and Physical Development of Barbados, with Notes on Trinidad." In preparation.

A few words may be said as to the means of reaching the different islands. From New York the ships of the Quebec Steamship Company leave on irregular dates, but averaging three or four sailings a month, sometimes first touching at St. Croix, next at St. Christopher (universally called St. Kitts) and then sail onward to the south. On other voyages the ship calls first at St. Martin, and then proceeds as before. Again St. Kitts may be the first stop. While most of the larger of the more southern islands are visited on each trip, this is by no means so certain as on the north bound voyages. After touching at St. Lucia, or St. Vincent, the steamers proceed to Barbados and often to Demerara, and some of the tourist steamers in winter, to Trinidad. Another line sails for Grenada and Trinidad direct. The Pickford and Black Line, from Halifax, sails regularly every four weeks for Bermuda, St. Kitts, and on to Trinidad. Local steamers of the Royal Mail Line sail regularly once a fortnight between the larger islands. There are other occasional steamers by which passage between the islands can be made. But to the smaller islands, one must depend upon small schooners or sloops of perhaps only ten tons capacity, which may usually be found sailing weekly from the larger islands, for carrying the mail, etc. Thus there is a weekly sloop from St. Kitts to St. Martin, Anguilla and Sombrero; from St. Martin to Guadeloupe; from St. Croix to St. Thomas and the Virgin Islands, etc. To and from Barbados and Martinique there are fortnightly steamers to England and France, and other steamers to the South American ports and Colon, as also to Jamaica. The Quebec Line and the Pickford and Black steamers sailing among the islands usually travel at night, so that the tourist can go ashore for the day and get a glimpse of these most beautiful tropical lands. The coasting voyages of the Royal Mail Line give no opportunities for seeing the islands, as they make brief calls, day or night, and then proceed onward.

SOMBRERO.

Sombrero is a lonely sentinel away out in the Atlantic, at the northern end of the Windward Chain, being situated forty miles beyond Anguilla. It is less than a mile long, with a breadth of a quarter its length. Its flat top is pitted by former workings for phosphate of lime. It is about thirty feet above the sea, with vertical walls, so that landing at the foot of a ladder is difficult. It is composed of a coral-bearing soft white limestone, found to be of early Pleistocene age. Pockets on the surface have been converted into phosphate of lime by birds, which, during some portion of the Pleistocene period, made it their home. There is not a tree on the island. The great lighthouse of this region is here, and six men are in attendance upon it. It is a dependency of St. Kitts, a sloop from which visits the island weekly (see map, Plate A, appended).

THE ST. MARTIN ARCHIPELAGO.

St. Martin, St. Bartholomew and Anguilla rise out of the same banks which are submerged from 100 to 200 feet. The whole forms a physical unit, being an isolated remnant of the dissected and submerged Antillean plateau. The margins of the plateau are further indented by deep valleys, heading in amphitheatres, as shown west of Anguilla and St. Martin, and south of St. Bartholomew, where the incisions on the two sides of the drowned tableland have united into a channel across it. These features are shown on the map (Plate A, appended). This mass rises prominently above the broad channel, 2,500 feet in depth, separating it from the Saba banks, but from its eastern side the descent to the Atlantic abyss is not known to be interrupted by other features.

St. Martin (see map, Plate A, appended) is mostly composed of mountain ridges (the highest point of 1,360 feet may be seen in figure 1, Plate I.) and valleys which broaden out rapidly, from the *cul de sac* of each, and terminate in bays, in front of which there are often beaches, such as that shown in the illustration, where the Dutch town of Philipsburg is built. These valleys are formed by the rapid erosion of high lands due to the tropical storms, one of which I witnessed, when eight inches of water fell in three hours. Such rainfalls in the dry season are due to the mountains, even low ones, condensing the moisture out of northeastern trade winds, while neighbouring flat islands, like Anguilla, have a great scarcity of rain. On the western side of St. Martin, Simpson's Inlet is a beautiful bay or lagoon, enclosed by ridges connected by sand beaches. Only an inconsiderable portion of St. Martin could be considered a coastal plain.

The mountains are composed of the old West Indian igneous foundation, probably, in part, older than the Tertiary era, though perhaps, in part, belonging to the earlier Eocene days. There are also volcanic tuffs, and a formation of grey limestone which is composed of calcareous layers intercalated with tufaceous beds, but the calcareous strata are more or less silicified into chert. Such are well seen along the shore, as at Pelican Point, illustrated in figure 2, Plate I., where also boulders three or four feet in length, more or less rounded by the waves, may be seen. The silicified formation contains manganese which has been economically worked, and also iron ore. Similar igneo-calcareous formations have been found in St. Bartholomew to belong to the Eocene period, from the fossils which Cleve* obtained in the calcareous layers. Remnants of the white limestone, or the Antigua formation, (of the Oligocene period) occur about Simpson's bay, and in the French portion of the island.

Tintamarre, an off-lying island, rising to a height of ninety feet above the sea, is a remnant of the former coastal plain of St. Martin. It is composed of two calcareous formations, both of white limestone, but the strata of the lower are more or less upturned, and contain Oligocene fossils ; the upper, substantially horizontal, contains an old Pleistocene fauna.

Anguilla (see map, Plate A, appended) is a low-lying island, separated by a strait of four miles in width from St. Martin, of which it was a former coastal plain. Its highest point, near the northern cliffs which are being encroached upon by the sea, rise to only 213 feet in height. At points the old igneous foundation may be seen near waterlevel, beneath the white limestones. The disturbed lower beds contain an Oligocene fauna, and the upper horizontal beds hold Pleistocene fossils. It is often difficult to distinguish these formations apart, although separated by such a long geological gap. This island is also interesting from the occurrence of Pleistocene bones discovered by Mr. Wager Ray, and found by Prof. Ed. Cope to be those of *Amblyrhiza*,—rodents, as large as a Virginia deer, whose ancestors had migrated from South America in the Pleistocene period, when there was a continuous land connection with that continent.

Gravel formations have been found in these islands belonging to later days of the Pleistocene period. Coral reefs are now flourishing, especially off the coast of Anguilla, but they are not raised above the sea level.

The roads of the flat island of Anguilla are well made, as also those of St. Martin, which, however, have to pass over several high hills. St. Martin is politically divided between Holland and France. St. Bartholomew is French. Both French colonies are dependencies of Guadeloupe, and both are free ports. Anguilla is an English dependency of

*"On the Geology of the Northeastern West India Islands," by P. T. Cleve. Trans. Roy. Swedish Acad. Sc., IX., No. 12, p. 26.

St. Kitts. From St. Kitts there is a weekly schooner to St. Martin, Anguilla and Sombrero, from St. Martin a fortnightly schooner to St. Bartholomew and Guadeloupe; from St. Martin to Saba and Curaçao sails a monthly schooner. At St. Martin the Quebec Steamship Line calls about once a month on the outward passage from New York.

Anguilla has practically no exports and is a very poor island, the negroes living on "ground" provisions (tubers), some fruits, etc. St. Martin formerly produced sugar, but this industry has almost disappeared. Some cattle are raised for export, but the salt production is now the principal source of wealth. The few thousand people of these islands are almost entirely black or coloured, with only a few whites, mostly the descendants of the planters of slave days. But many of the old families have disappeared. English is the language spoken in the Dutch portion of St. Martin, but it is also generally understood on the French side of the island and in St. Bartholomew, whose inhabitants are largely of French origin. Before leaving this subject, I wish to express my very high appreciation of the Honorable Diedric C. Van Romondt, K.N.O., and formerly Governor, and his family, and to thank them for the princely hospitality shown to Mrs. Spencer and myself, such as characterized the palmy days of the West Indies. His charming suburban home is in the beautiful valley of Cul de Sac, opposite the highest point of the island, both of which may be seen in figure 1, Plate I.

THE ST. KITTS CHAIN, MONTSERRAT AND THE SABA BANKS.

Here we find three elevated remnants of the dissected Antillean plateau rising up as tablelands to 3,000 feet or more above the floor of the drowned valleys. But the channels separating them have a depth of 15 to 100 feet below the surface of the sea. On the St. Kitts remnant, St. Eustacia, St. Kitts itself and Nevis rise as the mountainous back-bone of the region, with the Saba banks, as a slightly submerged coastal plain, to the south. (See map, Plate A, appended). Montserrat is a repetition of the central mass. Saba is simply an extinct volcanic cone, rising precipitously from the floor of the sunken Antillean ridge, but at the foot of a submarine tableland now forming the Saba banks. Its height above the sea is 2,830 feet, with the water 2,250 feet or more in depth. On the floor of an extinct crater, at the height of several hundred feet, is perched the town of Bottom. It is a small Dutch settlement where the inhabitants are engaged in boat building, or as mariners.

Saba banks have an area of 800 or 900 square miles, rising to within 100 or 150 feet of the surface of the sea. This is a fine example of a submarine tableland not surmounted by any mountains. Its surface has been levelled over by coral growths and sands derived from them. It is the only conspicuous remnant of the coastal plains on the Caribbean side of the mountainous back-bone of the Antillean ridge until the Grenadines are reached. (See map, Plate A, appended).

Saba and Statia (the colloquial name of St. Eustatius), are both within sight of St. Martin, and can occasionally be reached by sloop from St. Martin and St. Kitts.

Statia, St. Kitts and Nevis are all situated on a narrow submerged ridge. The north-western end of Statia and the south-eastern end of St. Kitts are the remains of the old dissected and degraded mountains composed of the ancient trappean foundation of all the Antillean islands of the Windward chain, but the remaining portions of these two islands and Nevis are surmounted by volcanic ridges, belonging to geological days more recent than the early Pleistocene epoch, with the volcanic activity continuing down so recently that some of the craters are still preserved, such as that of Mount Misery (4,314 feet above the sea), with one side removed to a depth of 600 feet. It is about a quarter of a mile in diameter, with a lake partly filling the depression (according to my friend, Dr. Christian Branch, who then resided in the island). Statia has also a perfectly preserved crater, called the "Quill," rising to a height of 1,950 feet. (See view, figure 1, Plate II.). From the central ridges, the surface slopes in the form of a *glacis*, which is deeply dissected by valleys, as shown in figure 2, Plate II. At only one point on the north-eastern side of the island did I see any lava, and that belonged to a Pleistocene eruption, but Dr. Branch informed me that some black rock had been reported from near the summit of Mount Misery. The soil is made up of volcanic ashes of great fertility, which is constantly creeping down the slopes. At Basse Terre, St. Kitts, in 1880, a cloud burst upon Monkey hill, back of the town. (See figure 3, Plate II.). Over thirty inches of rain fell within three hours. The floods from such storms carry ruin before them. Great, deep valleys are rapidly excavated out of the loose, volcanic soil, while the material removed from them settles upon the more level land, in this case filling the streets and gardens with mud to several feet in depth. On the sloping ground every structure is washed away by the sheets of water, and people overtaken by them are whirled into the sea. Sometimes where the bodies are caught by an obstruction, this impediment to the current causes a deposition of mud, so that they may be quickly buried on the plains before reaching the sea. A similar flood, though not so severe, yet drowning many people, occurred in Montserrat during my stay on a neighbouring island. These floods give some idea of the very rapid changes in the physical features of this tropical region. But in addition the Atlantic ocean is encroaching upon the eastern sides of all the islands, owing to the waves being thus driven by the north-eastern trade winds. Accordingly, almost all the ports are upon the leeward sides of the islands. In St. Kitts, there are wild monkeys, but they belong to an African species, and it is not known who imported them.

Nevis is a nearly circular island radiating from a volcanic dome which rises to an altitude of 3,596 feet. It is nearly always wrapped in a cloud, like the summit of Mount Misery in St. Kitts. Its sloping surfaces are similar to those of St. Kitts, of which it is now a political dependency, though formerly the more important. In the seventeenth century there were several thousand white settlers who were forced to leave owing to the concentration of the lands into the hands of a few owners. Now the whites are few and poor. Here was born Alexander Hamilton, one of the fathers of the American republic. So also the wife of Lord Nelson, who, at his marriage here, was attended by Prince William (afterwards King William IV.), as best man. The island is separated from St. Kitts by a strait only a few miles wide, and very shallow.

The old eruptive foundation of these islands belonged to the very beginning of the Tertiary era, or to a little earlier geological time. During the Miocene, and until about the close of the Pliocene period, this region was a land surface, and no formations were accumulated beneath the sea. But in the Pleistocene period a most interesting phenomenon occurred. A volcanic upheaval raised Brimstone hill on the flanks of St. Kitts to a height of about 700 feet, without having produced a crater. (See view, figure 1, Plate IV.). In the outburst, the floor of the sea was thrust up so that a limestone veneer, about thirty feet thick, covers the sides of the hill, which is about half a mile in diameter, to a height of 400 feet, on which a strong fort was formerly raised. The formation thus lifted up contains fossils which show that it was formed at the close of the Pliocene or beginning of the Pleistocene period. The same phenomenon was repeated twelve miles away in Statia (see figure 1, Plate II.), but there the limestone mantle occurs to a height of over 900 feet, and on the summit a well preserved crater was formed.

Montserrat (see map, Plate B, appended), shows the old igneous foundation, small remnants of the earlier Tertiary (Oligocene) limestone, and the surface accumulations from two volcanic cones of apparently the same age as those of the other inner islands of the Windward chain.

Most of the roads in these islands are well made. Very fine sugar estates cover the slopes of St. Kitts and Nevis, but the industry is paralyzed, and prevailing poverty has succeeded the luxuriant wealth of a generation or less ago. In Montserrat, great quantities of lime juice and citric acid are produced. The people are mostly negroes, with a considerable number of Portuguese, descendants of labourers imported some time ago into St. Kitts. The old English white families are disappearing from different causes, the final being the intermarriage of those in reduced circumstances with people of colour, that is to say, with those whose blood is very slightly coloured. These in their turn become commingled with others of darker shades, so that eventually you find descendants of the most distinguished white families appearing like full-blooded negroes, in spite of the very strong prejudice against the mixing of the races, which socially ostracises the slightest trace of African blood.

ANTIGUA AND BARBUDA.

These two islands (see map, Plate B, appended) form another distinct tableland, rising 2,000 feet or more above the floor of the submerged Antillean plateau (see map, Plate B, appended). The island of Antigua impressed itself upon me as a little continent, with all the features necessary to complete one, and indeed this impression is not far wrong, for here may be studied all the geological and physical history of the dismembered and drowned tableland between North and South America, except the phenomena of the later volcanic activity. It is the starting point of investigation. Moreover, it is a fertile island and suggests prosperity, until one looks beneath the surface and finds that the prices paid for the sugar now are no more than the smallest pittance required for sustaining slave labour. The south-west quarter of the island is mountainous, the highest peak rising to 1,330 feet. This district is broken up into narrow ridges, with the valleys rapidly increasing in size, so that their lower reaches are broad flats extending into the shallow bays, where corals grow in profusion. In these valleys are small rivers, but over most of the other sections of the island the drainage is underground without water courses. The central belt of the island is a low depression, out of which rise several hills. The north-eastern part is somewhat higher and undulating. The mountain district is characterized by the old Antillean igneous foundation dating back to the beginning of the Tertiary era, or in part a little older. Even the latest trace of volcanic activity does not appear to have been as late as the Miocene period. The central portion of the island is underlaid by tuffs derived from the older volcanic remains, but contain some beds of silicious limestone and others of fresh-water origin with silicified wood and land shells. It belongs to the Eocene formation. The north-eastern part of the island is composed of white limestone—the Antigua formation belonging to the Oligocene period. But over this is a mechanical limestone, composed of the broken *débris* of an older one, dating back to the close of the Pliocene or beginning of the Pleistocene epoch, and probably still another series of late origin composed of the same material, but distinguished by unconformity and the contained fossils. There is also a still newer formation of gravel belonging to a later Pleistocene epoch.

Barbuda is a flat limestone island, with lagoons on the west. The highest point rises to only 115 feet. It is the remains of the old Antillean coastal plain extending seaward from the mountains of Antigua.

The termination of the central plain in the harbour of St. John's is illustrated in figure 1, Plate III., where the cliffs of the eastern rolling country are shown in the distance. Figure 2, Plate III., shows a fragment of the dissected coastal plain at Hodges' hill, which appears in the background to the right.

The population and the present conditions of Antigua are similar to those of St. Kitts. The roads are nearly always excellent. Being generally low, the island is rather dry and is not subject to the same rain-fall as the more mountainous islands. Dr. Christian Branch could find no remains indicating the permanent occupation of Antigua by the Caribs, who were numerous in all the other islands, and he attributes the fact to the scarcity of water at certain seasons of the year.

THE GUADELOUPE ARCHIPELAGO.

This is another remnant of the dissected Antillean plateau, of which the lower lands are now submerged. The summit of the ridge connecting it with Antigua is covered by about 2,000 feet of sea, but both sides of it are indented by deep embayments (see map, Plate B, appended). The tableland has been deeply dissected, so that being now sunken there are deep channels between the islands. The archipelago is underlaid by the old igneous foundation common to the Windward group, but on Guadeloupe proper this is surmounted by tuffs and by volcanic accumulations, which have been ejected during the time extending from the close of the Pliocene period to the present. There are several cones, the highest of which is 4,863 feet. Several eruptions have been recorded in the eighteenth and nineteenth centuries. Grande Terre is a rolling coastal plain, separated from the main island by a narrow strait, called Salt River. Its general characteristics are those of the limestone section of Antigua, being underlaid by white calcareous marl of the Antigua formation, with the remains of a mantle of mechanical limestone above, and also another calcareous formation belonging to the beginning of the Pleistocene epoch, while on the mainland, as at Petit Bourg, there is a mid-Pleistocene deposit of sand and gravel. Marie Galante and Petite Terre are also limestone islands like Grande Terre, forming part of the old coastal plains in front of the mountain section. The Saintes are remnants of the old igneous basement. Remains of a small elephant which emigrated from South America in the Pleistocene period have been found in Guadeloupe.

The roads in this French island are good. A coasting steamer sails round the island and to the dependencies. The main industry is sugar, which is principally raised on Grande Terre. Fine coffee is also cultivated, as well as some cocoa and vanilla. The people are mostly coloured, with a larger white population than in the English islands, but the coloured population is more unsatisfactory from our point of view, and dislikes the intrusion of foreigners. And in their policy they have done much to impair the prosperity of the island. In disembarking or embarking at Basse Terre, the capital, one is liable not merely to the imposition of the boatmen, but one's life may be imperilled by them, practically, without redress. So also one may be insulted, or even assaulted, as was the case of even an American Consul. The successful revolution in Haiti has left here a bad effect which has not disappeared. But from the white people with whom I came in contact I received only the greatest courtesy.

DOMINICA.

Here is a repetition of the mountainous part of Guadeloupe, from which it is separated by a depression about 2,000 feet below sea-level (see map, Plate B, appended). It (see map, Plate C, appended) has no coastal plains like Antigua and Guadeloupe, unless we so regard the banks, some twenty miles to the south-eastward, as the remnant of the

Antillean tableland, now dissected and submerged. Again one finds the old igneous basement, over the denuded surface of which are several igneo-sedimentary deposits (of the older Tertiary era) surmounted by the newer volcanic formations, which culminate in cones, one of which is 4,747 feet above the sea. The earliest eruptions occurred about the commencement of the Pleistocene period, and the last in 1880. After the renewal of volcanic activity, there was an early Pleistocene deposit of coral rock, preceded and succeeded by gravel accumulations; all except the last of these formations have been mostly removed by denudation so that only fragments are now to be found, on the small remnants of the coastal slopes, the best example being the Grand Savanna, as shown in its relationship to the mountains in figure 2, Plate IV. Some little flat land is found in the rapidly widened valleys, such as that at the mouth of the Layou river (figure 3, Plate IV.). Fragments of terraces in their natural condition are few, but one may be seen at Roseau, on which the church is built, (illustrated in figure 1, Plate V.). Back of the town is an erosion plain (Morne Bruce), at 400 feet above the sea, which was once a coastal feature. The corresponding terrace, on the other side of the valley, shown in figure 2 (which is almost a continuation of figure 1, and might be joined at letters A B), may be seen sloping outward, owing to the local elevation of the volcanic centres, and not to the regional rising of the land.

Dominica is the most beautiful of the islands. Portions of it have never been cultivated, and in some of the valleys one may see a tropical vegetation, among which are tree ferns of great size and loveliness. The situation of the town of Roseau, the capital, at the mouth of the Roseau valley, and in front of the terrace of Morne Bruce with the sloping terraces on the other side of the valley, which is headed in a high mountain (not seen in Figures 1 and 2 owing to cloud at the time of photographing them), is unsurpassed in its graceful beauty, in spite of the dilapidated appearance of the town. The valley itself becomes enlarged, after passing above its mouth, which is, in fact, a cañon or gorge cut by the river since the recent elevation of the land. It is shown in figure 3, Plate V., the view being taken from the summit of Morne Bruce.

On account of the floods of the swollen mountain streams, the coastwise roads are badly cut up, and because of the almost impossible undertaking of maintaining bridges, one is compelled to travel on horseback, except for a few miles near Roseau. There are a few degenerated descendants of the freedom-loving Caribs, who were so ruthlessly destroyed alike by the Spanish, French and English during the early bloody history of the West Indian region. As if by an irony of fate, the islands have ceased to be of commercial value to the conquerors, and their descendants have mostly disappeared, or sometimes have become lost in the admixture with the negroes, whom they imported to supplant the natives on their own soil. The negroes here are mostly from French settlements, and speak a jargon, almost unintelligible to the English or French visitor. Hardly any industries flourish. A little sugar is still cultivated, cocoa and limes are grown quite extensively. Mr. Frampton has started the cultivation of the kola bean.

MARTINIQUE, ST. LUCIA, ST. VINCENT AND THE GRENADINES.

These islands (see maps, Plates C and D, appended) form a continuation of the mountain chain of Guadeloupe and Dominica and are composed of the same old igneous foundation and overlying tuffs, and later gravel deposits, and probably of some remnants of the old Pleistocene limestone (as in Guadeloupe and Dominica), though I did not see them. The older basement is more exposed than in the more northern islands, and the old traps are decayed to considerable depths. In fact, as we go farther south, the physical features assume more mature forms. Thus Martinique is deeply indented by the Bay of Fort Royal, and the hills to the south of it are erosion features. But the northern part of the island is surmounted by the more recent volcanic ridges and cones, the highest of which rises to a height of 4,438 feet. Martinique is more or less flanked with sloping surfaces (due in part to the sloping beds of tuffs underlying them) as in St. Kitts (illustrated in figure 1, Plate VI.). Remains of base level erosion benches may be seen as in figure 2, near St. Pierre.

Martinique is the most important of the French islands, but, unfortunately, it is so often placed in quarantine, on account of yellow fever, from which the other islands are generally free, that one is uncertain of being able to visit it, for if even a single case of fever breaks out, the traveller cannot leave, except by going on a French steamer bound for France, or by chartering a sloop and lying at sea for sixteen days, an experience which, for even a few days, one does not desire to have repeated. On this account, although lying in front of the island we did not land. This was the home of Josephine Beauharnais, afterward the wife of Napoleon I.

St. Lucia (see map, Plate C, appended) is surmounted by a cone rising to 4,000 feet. The igneous rocks, belonging to the ancient basement of the island, are deeply eroded and also decayed. Here is the best harbour among the islands, generally the only anchorage being in the open roads. We also find here the only pier in the Windward Islands, at which the ocean steamers can land, and this was built on account of the great coaling station. There is also a fine botanical garden at Port Castries, the capital. The slopes of the southern side of the island are largely cultivated for sugar cane. As elsewhere, the coloured population greatly predominates. The *fer de lance*, one of the most poisonous of the snake family, is as common as in Martinique. On the south-western coast, the Pitons rise on one side out of the sea (to 2,619 feet) as shown in figure 3, Plate VI. They are remains of a crater, partly blown away, partly carried off by the waves, and denuded by torrential rains. Travellers frequently mention them.

St. Vincent (see map, Plate C, appended) repeats the features of St. Lucia. The highest cone in the Soufrière mountains rises to an altitude of 4,048 feet. Just south of it, the large crater is occupied by a lake at an altitude of 1,930 feet, but the rim of the crater is from 3,000 to 3,600 feet above the sea. The volcanic eruption of 1812 sent the ashes to Barbados, more than a hundred miles away, where the dust obscured the sun for three days. Some of the valleys have a mature form, as that of the very beautiful Buccament, illustrated in Plate VII. This, however, was desolated by a hurricane about four years ago. The valley crosses the island to the sea, so that a little submergence would separate the hills, to the right in the picture, from the main portion by a strait. This feature is constantly appearing among the Windward Islands. There was a very fine and far-famed botanical garden before the hurricane, which carried every tree-top away, blew every insect off the land and covered the island with showers of fine earth.

The Grenadines (see map, Plate D, appended), represent the most complete subaërial dissection of the ancient volcanic foundation, so that a large number of islets and rock rise above the extensive banks which have a length of over a hundred miles, and are submerged 100 or 150 feet. But Grenada, as large as St. Vincent is surmounted by the later volcanic ridges with the highest point attaining an altitude of 2,749 feet. Grenada is the most celebrated of the islands for tropical fruits of fine varieties. The Trinidad steamers from New York stop here, but not the Windward Island lines, except the regular fortnightly Royal Mail steamer.

Trinidad.

This is not an Antillean island, (see map. Plate D, appended), but a part of South America, being situated on the continental shelf, and separated from the mainland by only a shallow strait.

Along the northern coast there is a range of mountains containing crystalline schists, and rising to points 3,000 feet above the sea. Isolated ridges occur in other parts of the island, in some cases having a height of 1,000 feet. Elsewhere the island is generally low, with occasional extensive swamps. Apart from the northern mountains, the sandy, shaly, and calcareous strata are of a much more clastic nature than the formations of the Windward Islands, for these materials have been supplied by the South American rivers, such as the Oronoco;—but they belong to the same geological periods as those of the Antillean chain. There are no volcanic accumulations as in the other islands. During the long Miocene-Pliocene period, land surfaces prevailed and gave rise to most of the present topographic features. These, however, were thinly covered by subsequently deposited mantles, so that the general changes of level of land and sea, the connection with North America, and the drowning of the region again, are phenomena common to the history of the other islands. Among the strata certainly no more recent than the Eocene period, are radiolarian and foraminiferal organisms that were accumulated at abysmal depths of the ocean, of perhaps two miles or more. These are of importance in showing that where there had been shallow seas, or even land, the region had sunken to the great depth mentioned, and been raised again, so that other shallow water formations could cover them and constitute the foundation of the modern land features.

Trinidad is a beautiful island of large size, but its fertile plains are only partly cultivated, as much of the island is still covered by primeval forest. The roads of the cultivated districts are excellent. Sugar cane is the principal product. Pitch Lake is most valuable, and is far famed. It lies on a flat plain a mile from the sea and 110 feet above it, and has an area of about a hundred acres. No high land occurs within sight of it. It is immediately surrounded by a small open wood. It is a dreary spot. The pitch rises and overflows a loose sandstone, which is covered by a red earthy loam. The surface of the pitch is hardened, and only plastic near the centre, so that it can almost everywhere be walked upon. But through the fissures of the crust are numerous springs of water and sulphuretted hydrogen, sending an offensive odor through the whole district.

Situated near one of the mouths of the Oronoco, Port of Spain is a city of commercial importance, and having been almost destroyed by conflagrations, it has been rebuilt with a modern appearance. Near it is located the largest and most celebrated botanical garden in the West Indies, except that of Jamaica.

In Trinidad there is a larger percentage of whites than in the other islands described, but they embrace several nationalities. Besides the negro labourers, there are many Hindoos imported to the sugar estates. Being on the edge of the belt of Trade Winds, with the intervening mountains, the island seems much warmer than Barbados, which is separated by less than two degrees of latitude. Trinidad has direct steamship communications with New York, but not by way of the Windward Islands. The Halifax steamers touch it once every four weeks. The Royal Mail line gives a fortnightly connection with Barbados. Occasional vessels of various lines also stop here, by which one is able to reach the Venezuelan ports, the Isthmus of Panama, and Jamaica.

BARBADOS.

This outlying island (see map, Plate E, appended) is situated somewhat more than a hundred miles east of the main Antillean Chain, but on the same submarine plateau (see maps, Plates D and E). The surface of Barbados rises in terrace steps, or slopes, to an altitude of 1,104 feet. Except in, and adjacent to the Scotland valley, on the northeastern side of the island, the surface rocks are composed of a white limestone, or a coral formation. But in the valley referred to, and adjacent to the coast, there are beds of sand, accumulated when this region was connected with the continent, and received the sands carried down the rivers—perhaps the ancient Oronoco. This deposit cannot be newer than the early Eocene days, and I am inclined to regard it as belonging to the Cretaceous period. Upon its surface is a marly deposit containing foraminifera and radiolaria, like similar accumulations in Trinidad. These were formed in oceanic abysses at a depth of two miles or more, in a geological epoch that may be referred to the Eocene period. The region, after having sunken to such a great depth, rose so that upon the oceanic beds we find a shallow-water formation of white limestones, as in Antigua and the other islands mentioned before, belonging to the Oligocene series. During the long

Miocene-Pliocene period the land underwent changes of level, at times very high, so that the broad valleys between the island and the main group were being modified into rolling features by atmospheric agents, acting for a very long time. About the close of the Pliocene period, the region was depressed, so that only a very small islet remained away out in the Atlantic. Then followed the great elevation of the region, when all the islands and the continent were united. This was succeeded by another subsidence, so that the terraces round Barbados were cut out of the new coral reefs, as the land was again rising. Since their elevation the streams and rains have begun to excavate small cañons into the margins of the terrace steps. The occurrences of these terraces and little valleys give diversity to the surface features, for there are no mountains here. (See figure 1, Plate VIII.). The sea is encroaching upon the east coast, as in all the other islands, on account of which all the ports are on their western or leeward side. An illustration of the encroachment is shown in figure 2, Plate VIII., where the raised coral reef is breaking away and great blocks are lying along the coast.

The fertile sugar estates have been occupied by numerous owners, which has given rise to social conditions somewhat different from that found in the other islands. Within twenty years after the arrival of the first planters (1625), the population rose to 50,000, including many cavaliers, Irish labourers, and Indians stolen from other islands. The population now numbers 200,000, of whom one-fourth are whites. As the area is only 166 square miles, it is the most densely settled country in the world, so much so that the labourers on the estates get only three days' work a week, and another set work the remaining time—and that at twenty cents a day. More than half of the coloured infants die within a year, but even this does not keep down the increase. These conditions intensify misery caused by the ruined sugar trade. The people live on "ground" provisions (sweet potatoes, at fifteen cents a bushel, when bought; yams, a large tuber); bread-fruit, used green in place of potatoes; sugar cane and some fruits. They also get a small quantity of fish at times, of which the flying fish is most delicious.

In the church of the parish of St. John is the tomb of Theodore Paleologos, the last representative of the Christian Emperors of the Eastern empire, he having died here an exile in the seventeenth century.

GENERAL CHANGES OF LEVEL OF LAND AND SEA.

The primary object of my three visits to the West Indies and to the Central American region, and another to be begun in a few days, has been to carry on my investigations of the great changes of level which have occurred in late geological times, for there one finds the most complete evidence at present obtainable. In visiting the Windward Islands, I wished to extend the observations made elsewhere bearing upon the time of the great earth movements, and in doing so I had to investigate the geological formations, the results of which have been published in papers mentioned in the foot-note. Besides the geographical descriptions, with some geological results given here, I have included the charts of the region from which much more information can be gathered.* On these I have also drawn certain lines of soundings to bring out the drowned valley-like features.

While a somewhat less elevation of the region would connect the more northern of the Windward Islands, as we have seen, a rise of 3,600 feet would unite Dominica and Martinique (by way of the banks shown on map, Plate C), with an embayment between them reaching to a depth of 6,600 feet, miles within the line connecting the two islands. An elevation of 3,300 feet would bring the ridge between Martinique and St. Lucia to the surface, with another deep indentation to the west, a tributary of which heads in an amphitheatre, incising the island mass north-west of St. Lucia to a depth of 6,624 feet, within the line where the shelf is sunken only 600 feet. Between St. Lucia and St. Vincent, the connecting ridge is mostly within 1,200 feet of the surface, except for about five miles where the channel across the col reaches to a depth of about 3,000 feet below sea level. From this point the drowned valley rapidly deepens to nearly 6,600 feet within the line of the islands. The deepest part of the drowned valley crossing the ridge between St. Vincent and the Grenadines is only a mile wide and does not exceed a submergence of 1,300 feet. The Grenadine banks, really a submarine tableland, are covered by 100 or 150 feet of water, and the western slopes show the indentations, amphitheatres or cirques reaching to the same great depth, and still further away the soundings suggest that the submerged Antillean plateau in part rises more than 9,000 feet without quite reaching the surface of the sea. The cirques or amphitheatre-valleys on the eastern sides of the islands have not been so fully shown as on

* The charts are a reduction of Chart 40, U. S. Hydrographic Office. The various larger charts of the different islands should be consulted.

the western, on account of the eastern slopes being the more precipitous, and the soundings fewer in number. Between the Grenadine and Trinidad banks (see Plate D), the connecting plains may not be submerged to more than 750 feet, except in a narrow channel. The various forms of the valley-like indentations of the border of the great submarine Antillean plateau are similar to those upon the slopes descending from the high tablelands of Mexico and Central America, which have been fashioned by the rains and streams, and accordingly their occurrence is interpreted as evidence that the former altitude of the now sunken plateau was as great as the present submergence of the valleys now drowned. This conclusion is only to be modified, in referring to the islands, or their districts, which have been the scene of Pleistocene or more recent volcanic activity, for here we find local elevation due to plutonic forces which have not affected the great earth movements of the region. Among the Windward Islands the evidence of the full height at which the land stood has not been determined, as among the Bahamas and on the southeastern margin of the North American continent, where we have found that it exceeded 12,000 feet. At the time of the great elevation of the Antillean plateau, the region west of the Caribbean sea—Central America—was low.

The valleys are the result of two periods of erosion,—namely that of the Miocene-Pliocene, with the production of broad rounded forms, and that of the early Pleistocene days when the elevation reached the maximum height and all the islands were united so as to connect South and North America. This last epoch was of the shorter duration with the deepening of the old valleys, the formation of cañons, and the excavation of cirques or amphitheatres, at the heads of the narrow valleys, as they were dissecting the tablelands.

In the remains of elephants, and the large rodents of Guadeloupe and Anguilla, we have confirmatory evidence of the great elevation during the early Pleistocene epoch, for these mammals migrated from the continent about that time. But all the Pleistocene animals have disappeared from this region, and the modern species have not found their way to these islands, for since the very general subsidence which exterminated the former species, there has been no connection between the islands and the continent.

Beyond the proper limits of this study, between St. Croix and St. Thomas, of the Virgin Island banks, there is a remarkable basin attaining a depth of 15,000 feet (see map, Plate F), unlike any other feature in this region. But the col at the head of the valley connecting it with the channel leading to the Atlantic basin is submerged only 6,402 feet, or about the same depth as the cirque of St. Lucia, or the indentations within the lines of the Windward Islands, and consequently this St. Croix indentation can be brought within the same investigations of changes of level as the Antillean Chain. By the rise of the land to this amount the migration of South American mammals could have even reached the North American continent. Such was the explanation of the occurrence of the numerous South American types of bears found at Port Kennedy, near Philadelphia, upon the remains of which Professor E. D. Cope was at work when overtaken by his untimely death, for these types had no geographical distribution that would suggest a connection with South America by way of Mexico and Central America.

In conclusion, I must thank the many kind friends, whom we made in all the islands, for their lavish hospitality, so that our scientific trip was converted into a social holiday and a pleasant memory never to be forgotten.

FIGURE 1.—View of the Valley of Cul de Sac. Beach on which is built the town of Philipsburg, with salt pond in rear, Island of St. Martin.

FIGURE 2.—Pelican Point, St. Martin, with sea rolled boulders, some four feet long.

FIGURE 1.—The "Quill" of Statia, from Brimstone Hill, St. Kitts.

FIGURE 2.—*Glacis* of volcanic ashes, dissected by rains, St. Kitts.

FIGURE 3.—Monkey Hill, and town of Basseterre, St. Kitts.

FIGURE 1.—Harbour and City of St. John's, Antigua.

FIGURE 2.—Coast of Antigua, near Hodge's Hill (on the right).

FIGURE 1.—Brimstone Hill, 700 feet high, on the flanks of the volcanic ridge, St. Kitts.

FIGURE 2.—Grand Savanna and Mountains, Dominica.

FIGURE 3.—Layou Valley, Dominica.

A

B

Figure 1.—Roseau Town and Valley, Dominica. Morne Bruce in foreground (400 feet high).

A

B

Figure 2.—Nearly joins Fig. 1 at A B. Sloping terraces back of Town.

Figure 3.—Roseau Valley, from Morne Bruce.

FIGURE 1.—View of the north-western side of Martinique.

FIGURE 2.—View near St. Pierre, Martinique.

FIGURE 3.—The Pitons, St. Lucia.

Plate VII.

Buccament Valley, St. Vincent.

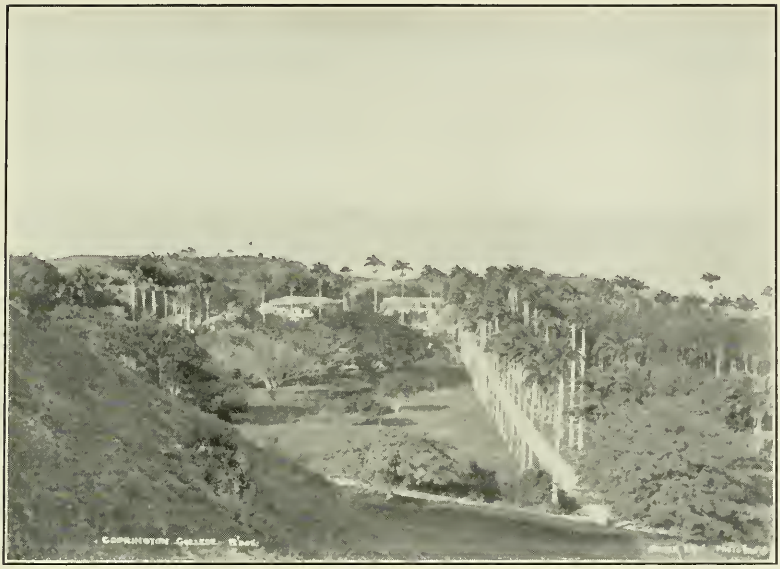
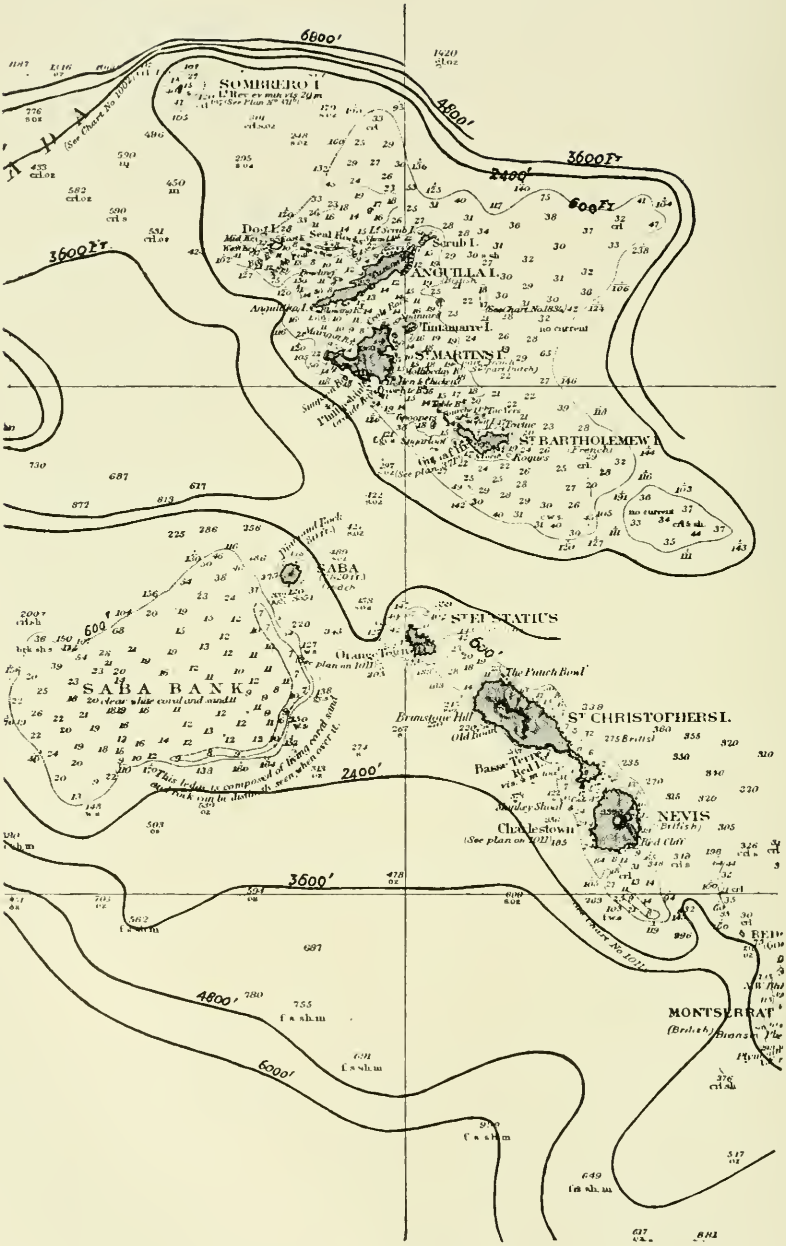


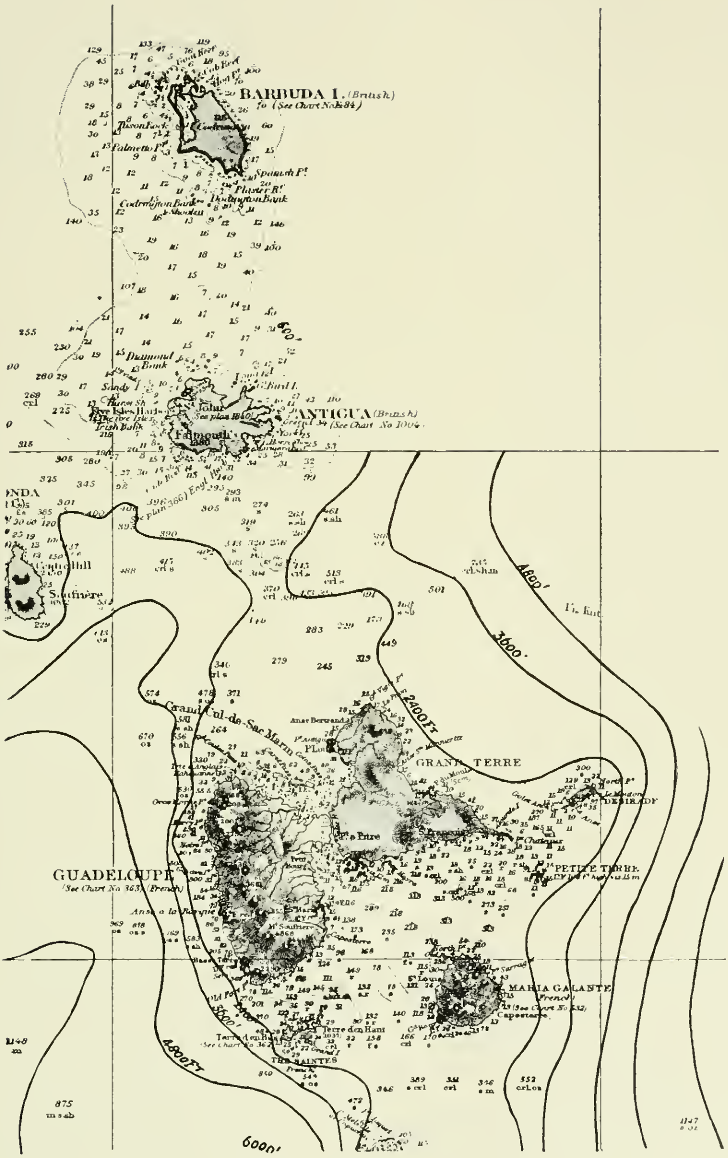
FIGURE 1.—View about Codrington College, Barbados.

FIGURE 2.—East Coast of Barbados, showing Terrace at 150 feet altitude, and fallen masses of coral rock.

Plate A.

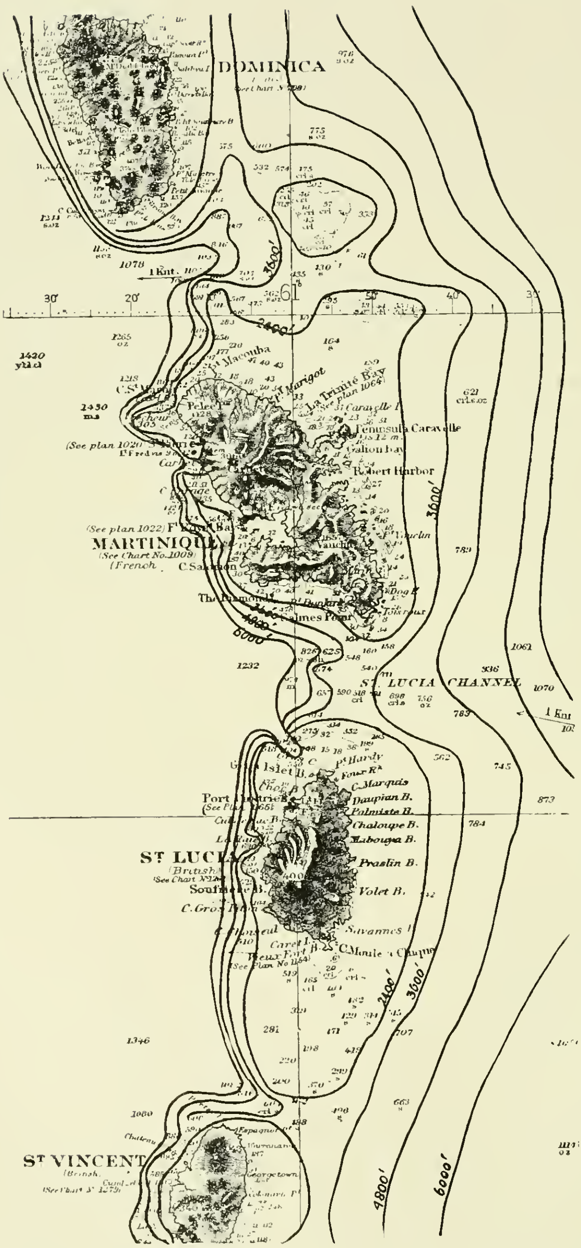


Scale—24.5 English miles to one inch; heavy contours in feet; ordinary soundings in fathoms of six feet. All the maps (F, A, B, C, D, E) can be joined into one.

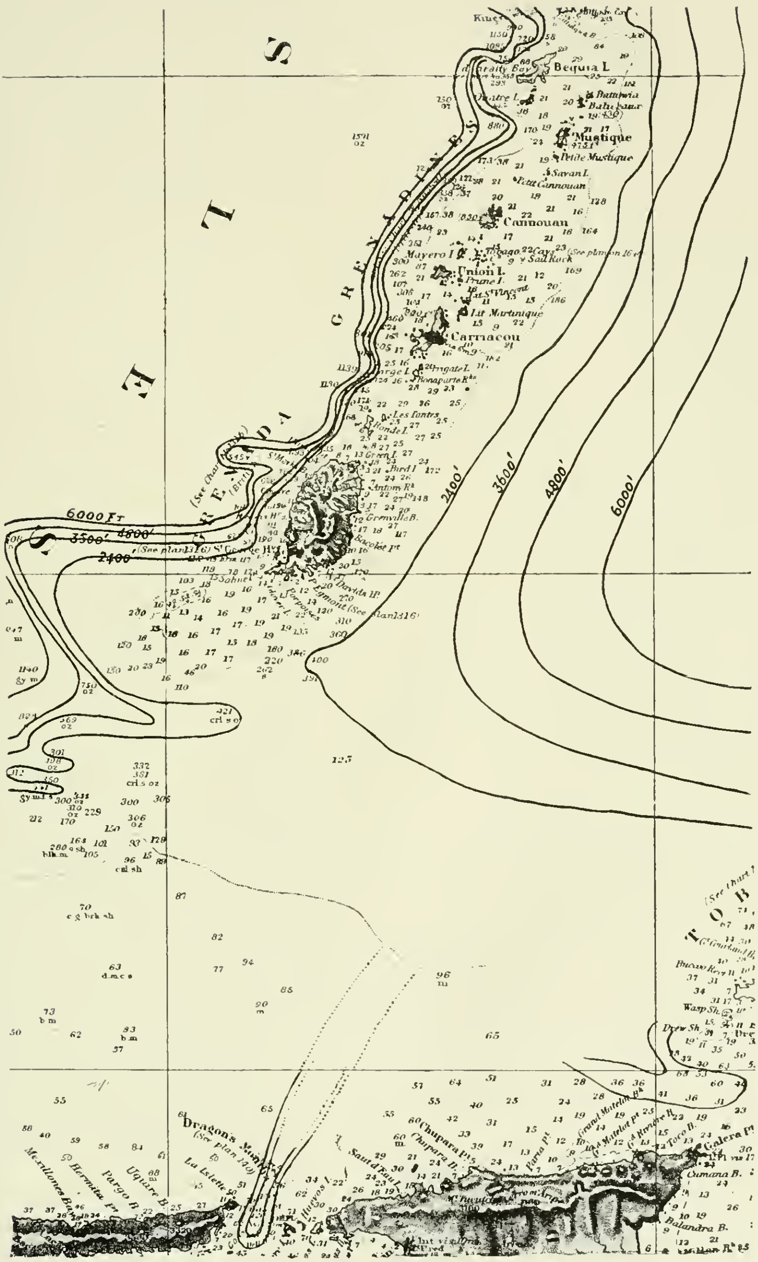


SCALE—24.5 English miles to one inch; heavy contours in feet; ordinary soundings in fathoms of six feet. All the maps (F, A, B, C, D, E) can be joined into one.

PLATE C.

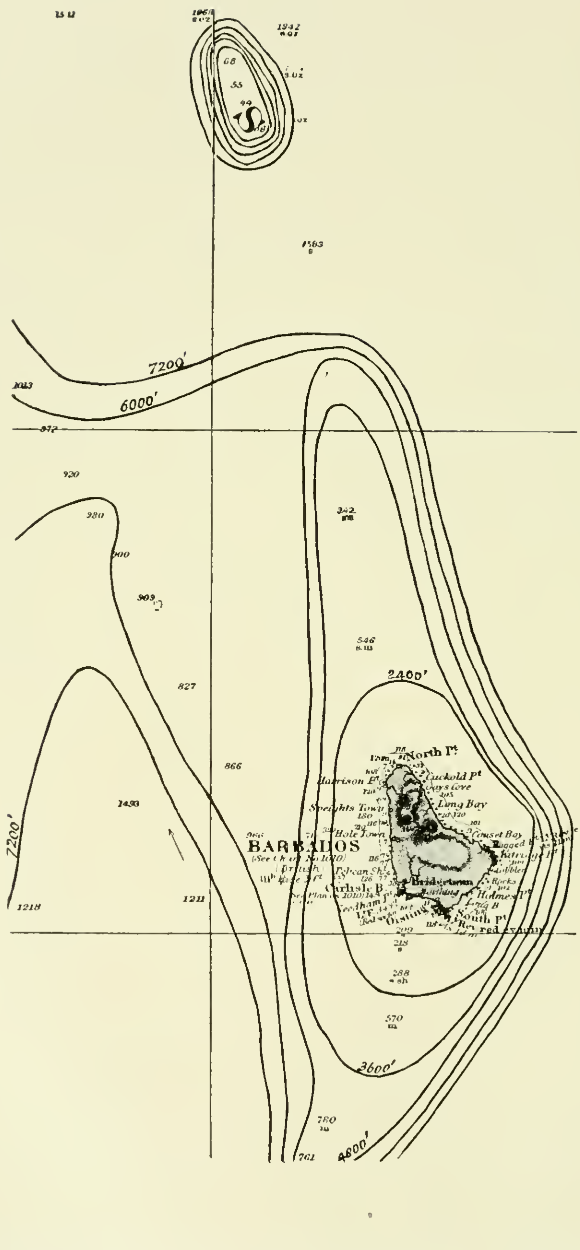


SCALE—24.5 English miles to one inch; heavy contours in feet; ordinary soundings in fathoms of six feet. All the maps (F, A, B, C, D, E) can be joined into one.

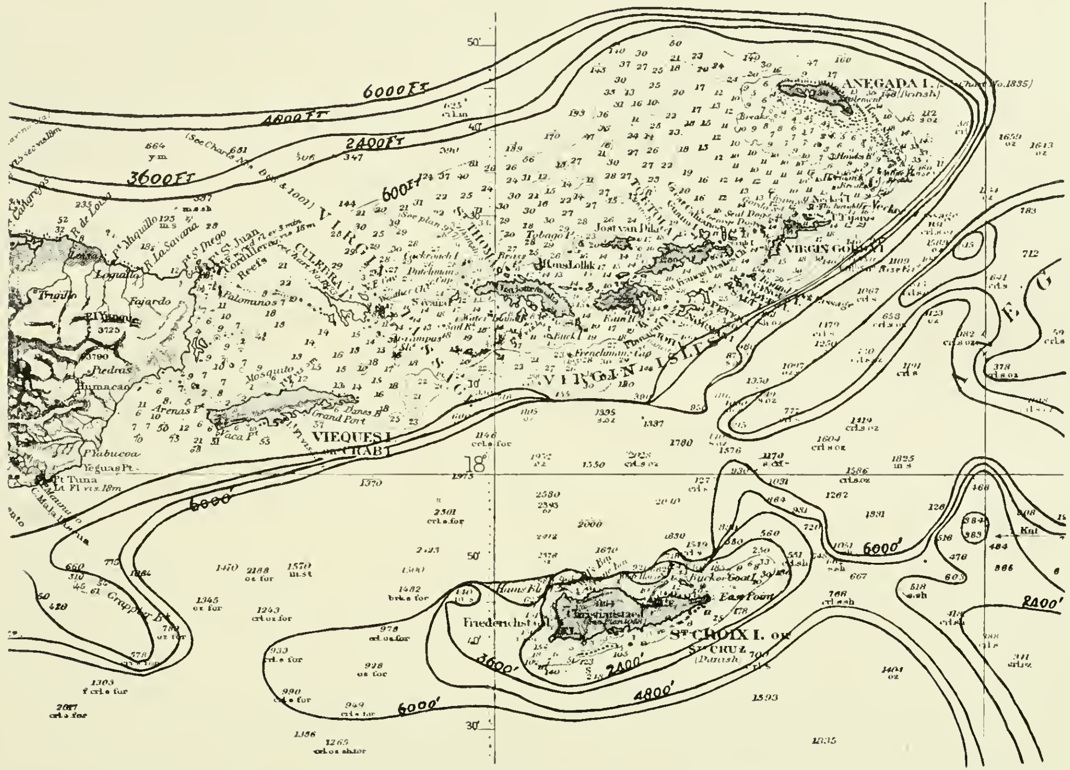


SCALE—24.5 English miles to one inch ; heavy contours in feet ; ordinary soundings in fathoms of six feet. All the maps (F, A, B, C, D, E) can be joined into one.

Plate E.



Scale—24.5 English miles to one inch; heavy contours in feet; ordinary soundings in fathoms of six feet. All the maps (F, A, B, C, D, E) can be joined into one.



SCALE—24.5 English miles to one inch; heavy contours in feet; ordinary soundings in fathoms of six feet. All the maps (F, A, B, C, D, E) can be joined into one.

PHOTOGRAPHY IN NATURAL COLOURS.

BY J. S. PLASKETT, B.A.

(Read 15th March, 1902.)

THE subject to be discussed in this paper has, ever since the discovery of photography some sixty years ago, excited the keenest interest and attention, not only among photographers and scientists, but among the lay public as well. From time to time, results have been obtained which have led many people to believe that the problem was approaching a solution. Again and again glowing reports have been published stating that the long-looked-for process had at last been discovered. But in nearly every case it has proved that the colours obtained were either unlike, or, if like, were not dependent upon the colours of the light waves which produced them ; and it is very doubtful whether any real progress towards realizing a practical solution of the problem of obtaining a direct photograph in colours has been made.

The nearest approach to such a solution is reached by the Lippmann process, in which the colours are produced by the interference of light, this interference giving rise, in the taking process, to what are known as standing waves in the photographic film. These standing waves cause a peculiar, laminated structure in the deposit of silver on the plate, the position of the laminae corresponding to the lengths of the waves, and hence to the colours, that give rise to them. The explanation of the colours seen, when such a plate is viewed by reflected light, is quite similar to that accounting for the colours of thin films such as soap bubbles. The theory is not, however, perfectly complete and satisfactory as the cause of certain abnormalities in the process is not evident. The true colours can only be seen when the heliochrome is viewed by reflected light at normal incidence, and are hence not very easy to observe. Probably the most satisfactory way of viewing it is to strongly illuminate the surface and, by means of a lens, form an image of this surface upon a screen. The technical difficulties of the process are very great, so great, indeed, that, during the ten years it has been discovered, only comparatively few good examples of interference heliochromy, as it is termed, have been produced.

Even this process does not give us a direct photograph in colours, taken in an ordinary camera, like an ordinary monotone such as the world is looking for, and still less does the three-colour process, which is an indirect and composite method, fulfil such an ideal. It is, however, the only really practical method at present available, and is the one I shall attempt to describe in this paper. It was, in its inception, based on the Young-Helmholtz or three-colour theory of vision, and, although the principles of the three-colour process are independent of any visual theory, yet a short statement of the essential points of this theory may be of service in simplifying the succeeding explanations.

The facts of colour vision are accounted for in the Young-Helmholtz theory by assuming that there are three fundamental colour sensations, a red, a green, and a blue-violet; and that all colours, except deep spectrum red and the extreme violet, according to Abney's latest researches, are compound sensations, produced by the excitation simultaneously of two, or sometimes even of the three colour sensations. Although this theory has been very generally discredited by physiologists and psychologists, it still possesses many strong advocates on the physical side, and will always retain considerable interest on account of the historical associations connected with it. It has the merit of giving a simple and direct explanation of the main facts of colour vision, while those not explainable on this hypothesis meet with little better fate at the hands of the other theories advanced.

Maxwell supported this theory and, by means of a modified form of spectroscope which he called a colour box, made measurements to determine the ranges of these colour sensations. These measurements placed in the form of curves, can be projected upon the screen (Fig. 1). They are of great interest to all students of the three-colour process, not only for their historical value, but principally by reason of the fact

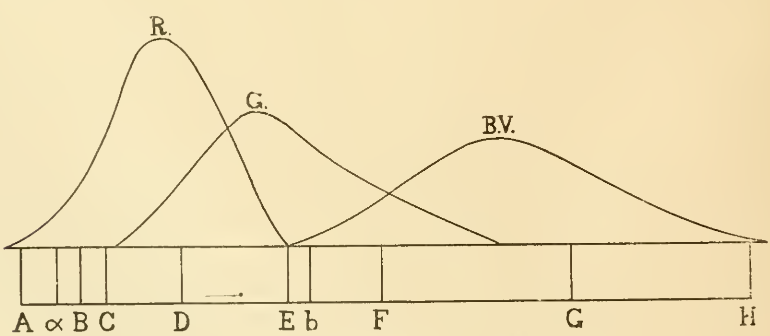
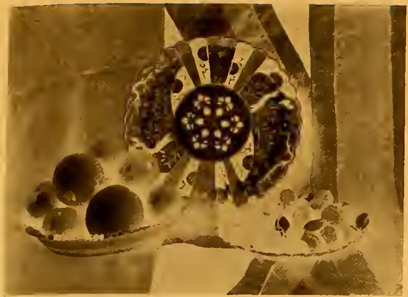
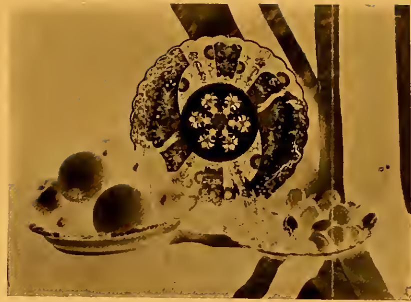


FIG. 1.—Maxwell's Colour-Sensation or Colour-Mixture Curves.

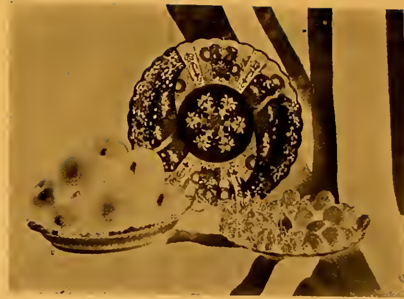
PLATE.



Negative taken through Red Filter.



Negative taken through Green Filter.



Negative taken through Blue-Violet Filter.

Positive from above Negative.
Blue-Green Complementary to Red.

Positive from above Negative.
Magenta-Pink Complementary to Green.

Positive from above Negative.
Yellow Complementary to Blue-Violet.

The Three Positives Superposed.

that they were and are used by Ives as the basis of his method of obtaining photographs in colour. These curves indicate graphically what Maxwell believed to be the amount of action produced on each of the colour sensations by any particular part of the spectrum. It has long been known, however, that, although these curves give a fair approximation, they do not exactly represent the ranges of the sensations and they are now superseded by the new measurements of Sir Wm. Abney, probably the most widely known authority on colour photometry and on photography. He spent some nine months in 1898-99, in redetermining the colour-sensation curves on the Young theory, and his paper treating on the subject was published in the Transactions of the Royal Society for 1899. A diagram of these new values can next be shown (Fig. 2) illustrating the difference between the two sets of curves. It will be noticed that, although the general

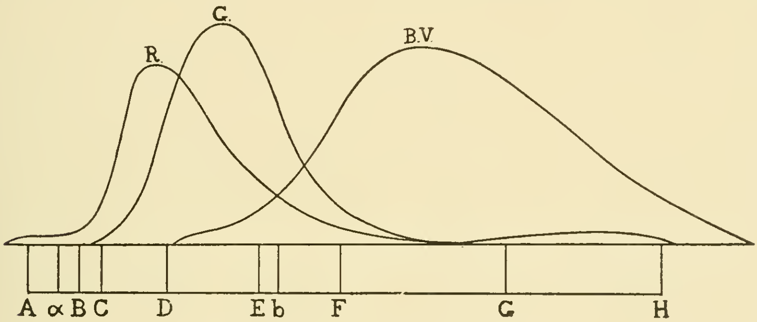


FIG. 2.—Abney's Colour-Sensation Curves.

forms are similar, Abney's sensation curves include each a longer range of the spectrum than Maxwell's but are not so long as those of Helmholtz where each sensation embraces the whole spectrum. But Abney's values, determined by the aid of more modern methods, and in the light of recent researches must be regarded as giving the closest approximation to the truth. From the method of their determination, and as will be seen later, Maxwell's curves could more appropriately be called colour-mixture than colour-sensation curves, and are very essential in both the theory and the practice of the three-colour process.

The three-colour process of photography is based on the experimental fact, which is probably most readily explained by the Young theory, that all spectrum colours, and hence all the colours of nature, can be imitated, to a very close degree of approximation, by mixing in varying proportions colours taken from three narrow sections of the spectrum. These sections or colours coincide very approximately with the dominant hues of the sensation curves; that is they are a particular red, green, and blue-violet. Abney has chosen as the most suitable position for these primary, fundamental, or reproduction colours about $\lambda 6700$ in the red, $\lambda 5120$ in the green, and $\lambda 4600$ in the blue; these particular wave lengths, which are in ten millionths of a millimetre, coincide very approximately with the wave lengths due to the red and blue lithium and the green magnesium lines and are hence easily located in the spectrum.

Before discussing and illustrating the mixing of spectrum colours, it will be advisable to clearly distinguish between two methods of colour mixture commonly employed which will be frequently referred to in this treatment of the subject. These are positive and negative mixture, or mixture by addition and mixture by subtraction. The former is effected by adding coloured light to coloured light, and the latter by adding absorption to absorption; while the resultant colours produced in the two cases are, in general, essentially different. Perhaps the commonest fallacy on the subject of colour mixture is the prevailing belief that the mixture of yellow and blue gives green, but it can easily be shown that the mixture of yellow and blue lights can not by any possibility give green. If a piece of yellow glass be placed in one lantern, forming a yellow patch upon the screen, and a piece of blue glass in another lantern, forming a blue patch, the overlapping of these patches and the consequent mixture of the coloured lights, as is at once seen, [*shown*] gives us white light, which, although it may be yellowish or bluish in hue, is without any approach to a greenish tinge; by no variation in the intensities of these colours, provided the hues remain yellow and blue, can green light result. If, however, one lantern be stopped and the yellow glass placed in front of the blue glass, a green patch at once appears on the screen, [*shown*] showing that the colour produced by superposing the absorptions, as is always done in the mixture of paints or pigments, is essentially different to that resulting from the mixture of coloured lights. To prevent confusion, it is very necessary that this distinction be carefully borne in mind in the subsequent treatment; and it will at once be evident that positive mixture is the only kind that can be employed in mixing spectrum colours.

The mixing of the three primary spectrum colours is effected by a modification of Abney's well known colour patch apparatus. This consists, first of all, in a means of forming a pure spectrum, and, secondly, of an arrangement for combining any of the spectrum colours, in any desired proportions, to form a patch upon the screen, visible to the entire audience. The light from the crater of the electric arc is converged, by the condensers of the lantern, upon the slit at one end of a collimating tube which contains at the other end a lens whose principal focus coincides with the slit. The beam of light from the lantern hence emerges from the collimating tube parallel, and will form a distinct image of the slit at the principal focus of any lens inserted in its path. The interposition, between this and the collimating lens, of direct vision prisms, constructed to give dispersion without deviation, breaks up the single uncoloured image of the slit into a number of overlapping coloured images forming a pure spectrum [*shown*] in the plane containing the principal focus of the lens. If a large condensing lens be placed beyond this plane, its function will be to collect all these coloured images, and form an image of the last surface of the prisms. This image is, however, too small to be visible at any distance, and an enlarged image of this image may, by means of another large lens, be formed upon a screen beyond, and of such a size as to be plainly visible to all. By causing this image to fall upon a small square of white card on a black velvet background, the effect of coloured edges can be eliminated ; and the colourless nature of the image formed in this case is evidence that the union or recomposition of all the spectum colours gives white light [*shown*]. If a card containing a narrow slit be moved along in the plane of the spectrum, the patch on the screen will assume each spectrum colour in turn, isolated, of course, from its fellows and hence uninfluenced by contrast. The substitution for this single slit of a brass frame containing three slits, whose relative positions and apertures can be varied at will, and which was specially constructed for this experiment, enables us to determine the resultant colour produced by the mixture of any two or three spectrum colours, in any desired proportions. If the positions of these slits be made to coincide with the primary colours, as determined by Abney, which can easily be done by burning lithium and magnesium salts in the arc, thus "scaling" the spectrum, the resultant colours produced by the mixture of these primary colours can be at once determined.

The union of the three primaries, red, green, and blue-violet, in certain definite proportions, easily determined by trial, and measured by the relative apertures of the slits, produces an uncoloured patch on the screen [*shown*] ; and this white light, although not optically equivalent, being produced by the mixture of three narrow isolated bands instead of the whole range of the spectrum, can not be distinguished from ordinary white light. Nor can colours, produced by the mixture of these spectrum primaries, he distinguished visually from spectrum or natural colours. By closing the green and blue-violet slits, the red of the spectrum, through the third slit, colours the patch red ; the gradual opening of the aperture in the green produces, by the mixture of red and green, orange-red, orange, and yellow ; the green aperture remaining open and the red being closed gradually, gives yellow-green and green. The same procedure followed with the green and blue-violet slits produces blue-green, blue, and blue-violet ; and with the blue-violet and red slits forms violet, purple, and red, the whole range of spectrum colours including also the purples. *(Matching of colours shown).* Any colour in nature may be matched in like manner, the tints being produced by first matching the hue, and then opening all the slits sufficiently to add the required amount of white ; the shades being produced by making the slits narrow enough to sufficiently diminish the luminosity.

The correct proportions of the primary colours necessary to reproduce the colours of the spectrum are indicated graphically by Maxwell's sensation or, more properly, colour-mixture curves (Fig. 1). These curves, determined by the aid of his colour box, a modified form of spectroscope, and in principle similar to the experiment just described and shown, do not so nearly represent the stimulation of the fundamental nerve sensations or processes of the eye to produce any colour sensation, as the amount of the primary colours required to match the same colour. Although the positions of Maxwell's primaries are not quite correctly chosen, the amount of error introduced in his colour-mixture curves is practically negligible ; and hence the amount or intensities of the primaries necessary to reproduce any spectrum colour are immediately given by measuring the lengths of the corresponding ordinates to the curves. The relative intensities of the primaries necessary to produce any colour whatever can be determined by matching the colour by the three slits, and then measuring their relative widths ; unit width being determined by the relative apertures required to give white light.

The possibility of matching any colour by the mixture of the three primary or reproduction colours forms the basis of the three-colour process of photography. If, by any photographic process, three coloured images of any coloured object can be obtained, one red, one green, and one blue-violet ; and if each image contains in its various parts, the correct proportions of its own colour required to match the colours of the object, then the optical superposition or mixture of these images should give us a correct representation of the colours of the object. The red, green, and blue-violet images of some fruit against a red background can be shown side by side upon the screen by means of three lanterns. On bringing these images to one position or superposing them on the screen [*shown*], their mixture produces, as is seen, a very good reproduction of the original colours.

The method of obtaining these coloured images, as will be at once observed, is to back with coloured glass photographs on glass or transparencies in black and white, exactly similar to ordinary lantern slides. Consider the red image only for simplicity. A transparency of the object must be obtained in which the relative transparency of the various parts corresponds exactly to the amount of the primary red required (in union of course with the green and blue-violet), to match the colours of the object. Working backwards from the transparencies, the negatives from which they are made must have the relative opacities of their parts corresponding exactly with the amounts or intensities of the reproduction colours required to match the colours of the object. Negatives fulfilling these conditions will be correct, not only for the method of positive superposition already referred to and shown, but for every method of synthesis.

The colour-mixture curves of Maxwell (Fig. 1), indicate graphically the amounts of red, green, and blue-violet light required to match the

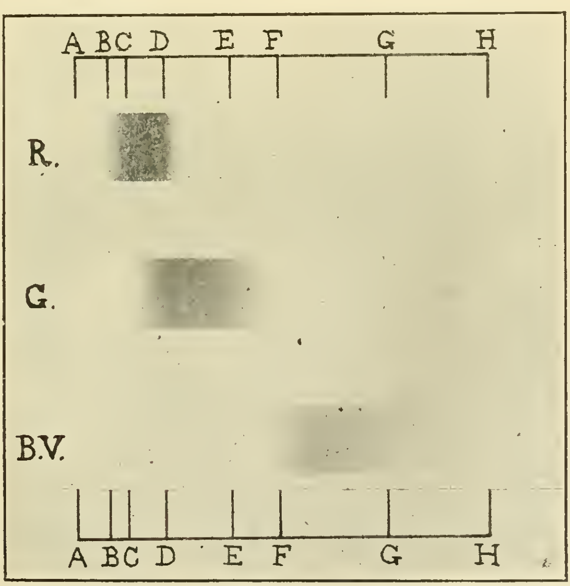


FIG. 3.—Colour-Record Negatives of the Spectrum.

colour of any part of the spectrum, and, from what has just been said, can therefore act as standards indicating the correct opacity patches on a set of negatives of the spectrum. The projection, upon the screen, of three negatives of the spectrum on one plate (Fig. 3) shows the approximate agreement of the deposits with the curves; while a transparency from these negatives shows the agreement between their relative transparencies and the curves.

The method of obtaining such negatives is that of selective absorption. The effect of interposing red, green, and blue glasses or stained films in a beam of white light is to absorb, roughly speaking, all but the red, green, and blue light, respectively; this can be easily shown by interposing them in the beam producing the spectrum [*shown*]. If

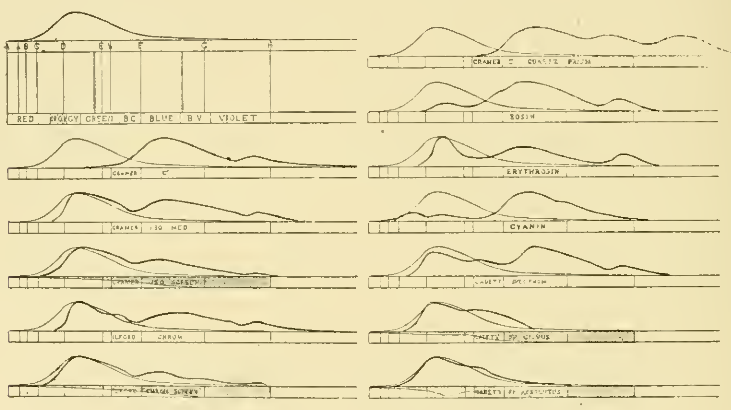


FIG. 4 —Action of the Spectrum on Photographic Plates.

they be placed in the path of the light entering the camera, near the lens say, they will perform the same office of picking out or selecting, hence the name, the red, green, and blue parts of the object, and allowing only these parts to be registered on the respective plates. This seems simple enough, but in practice the question of colour screens or filters. as they are termed, is much complicated by the fact that no photographic plate yet produced represents the spectrum in degrees of monotone corresponding to the visual intensity. It will probably be remembered, by those who heard the paper presented by the writer to the Institute last year on "Colour Values in Monochrome," that, only after tedious experimenting, could orthochromatic plates be made to render the spectrum approximately correctly. The curves representing the action of the spectrum on such plates are shown in Fig. 4, by the irregular curves in each part of the diagram, the regular curves indicating the luminosity or visual intensity. It is the compensation of these irregularities by the filters that renders their adjustments so difficult.

The spectrum method of adjusting filters, which are almost universally composed of stained films or liquid solutions, consists in so altering their absorptions that the opacity patches produced on negatives of the spectrum obtained through them agree with the colour-mixture curves (Fig. 1). Photometric measurements of the densities of the negatives are required, and the tedious nature of this process renders it impracticable for commercial use. It would I am satisfied give results unexcelled by any other means, and this statement I hope to prove at some future date by constructing a set of filters by such a method.

The ideal method has, so far, only been considered ; the use of pure red, green, and blue-violet spectrum light for the primary or reproduction colours, and the theoretical colour curves, based on measurements made with such light. In practice, however, such conditions can not prevail ; the nearest approach to the pure colours obtainable by selective absorption must necessarily be used for the reproduction colours, and the colour curves must be changed slightly to compensate for the errors thus introduced. The stained films that were used for backing the transparencies already shown are examples of such monochromatic colours. Their analysis in the spectrum shows that all but a fairly narrow band of the spectrum is absorbed [*shown*], but they do not approach the purity of colour obtained by the three slits. These reproduction colours, which must, first of all, fulfil the same condition as the three spectrum primaries that, when mixed positively, white light shall be produced, can be used in the same way as the three pure colours to match any colour. This is effected by backing with these colours three circular openings one in each of the three lanterns and causing the three coloured patches thereby produced to overlap on the screen [*shown*]. The intensity of the colours can be diminished, not, as in the spectrum experiment, by narrowing the aperture but by interposing patches of developed grey of different densities. By suitably varying the intensities of these reproduction colours, in a similar degree to the spectrum primaries, the whole range of spectral and extra-spectral colours can be produced [*shown*] exactly as in the former experiment.

The use of these reproductions, instead of the pure colours naturally leads to the use of an artificial instead of a pure spectrum as a test object in the adjustment of filters. A number of small squares of coloured glass, representing the principal colours of the spectrum as red, yellow, green, blue, violet, and also including purple and white, are mounted on another piece of glass forming the artificial spectrum. The amount of the reproduction colours necessary to match each of these glasses in luminosity and hue is accurately measured, giving the analogue of Maxwell's colour-mixture curves. The photometric measurement of the densities of the resulting negatives is avoided by reducing the luminosities of these glasses sufficiently, by means of rotating sectors or patches of developed grey, to render the quantity of red light, say, coming through each glass equal. A negative of such an apparatus, taken through a correctly adjusted red filter or screen, should give a series of patches of equal density ; and, since the equality of the densities of adjacent patches can be accurately estimated by the eye, no photometric measurements are necessary, and the adjustment of filters is much facilitated. A similar apparatus is, of course, required for the green, and for the blue-violet filters.

This device, due to Sir Wm. Abney, is usually called an Abney sensitometer, and is of very great service in three-colour work ; a simpler form of the same instrument is also very largely employed in the adjustment of compensating filters for orthochromatic plates. The set of filters employed in making the examples of three-colour work to be presently shown were adjusted by an Abney sensitometer, and were supplied by Mr. Sanger Shepherd, of London, who was awarded the medal of the Royal Photographic Society in 1899 for his three-colour filters. They were adjusted for use with the Cadett Spectrum plate, and will evidently, when the varying colour sensitiveness of different plates is remembered, only give correct results with this plate. The Cadett Spectrum and the Lumière Panchromatic are the most suitable plates for three-colour work. There are other panchromatic plates manufactured, but these have hardly, as yet, entered into serious competition with the above named brands. The Cadett plate is not so sensitive to red as the Lumière and requires a longer exposure through the red filter, but has the decided advantage of giving a much longer scale of correct gradation, and, in subjects with much contrast, will be more likely to produce correct results. It is possible to use a different brand of plates with each filter, but not advisable where good work is desired as it will be found almost impossible, on account of the different qualities of the plates, to secure a harmonious set of negatives. In three-colour photo-mechanical process work, this last procedure is frequently followed but the negatives and positives require and receive considerable retouching and etching.

On comparing the absorptions of these taking screens with those of the reproduction glasses [*shown*], it is at once seen that the former pass much broader bands of the spectrum than the latter. This distinction is very important, and was first pointed out by Mr. F. E. Ives, of Philadelphia, perhaps the most familiar name in the literature of the three-colour process. It is mainly owing to his genius and perseverance that photography in colours occupies the position it does to-day. The necessity for this distinction can perhaps be most clearly seen in attempting to make a colour photograph of the spectrum. If the reproduction glasses are used as taking filters, the evident result will be three narrow isolated bands of colour instead of the continuous spectrum ; while if the taking screens are used as reproduction glasses, unnecessary impurity and degradation of colour will result from the mixture of colours, other than the primaries, in the taking filters.

The negatives obtained through these filters will not be alike, but in some cases the differences are not marked. This, of course, is due to the fact that the colours in nature are, in general, of a very complex character, and pass, when analysed by the spectroscope, nearly all the spectrum colours; hence all three negatives will frequently be influenced by the same colour though not of course to the same degree. The projection of three such negatives side by side upon the screen [*shown*] will illustrate, more fully than can be described, the various points of difference between them [Plate]. The differences are most marked in negatives of the spectrum and least marked in outdoor subjects where the colours are, as a rule, very impure or mixed in character. The purer the colours the more the filters differentiate them into their three classes, and the greater the differences in the negatives. The negatives may be called colour-record negatives, and are quite similar in appearance to ordinary negatives [Plate], possessing no colour whatever in themselves. The slight variations in the densities of the corresponding parts are the means employed, in the various methods of synthesis, of obtaining the positives in colours; the same negatives being suitable for every kind of synthesis.

The two principal methods of obtaining colour positives from the colour-record negatives are by positive synthesis and by negative synthesis. Positive synthesis includes triple projection and the Kromskop, and is the method followed by Mr. Ives. Triple photographic prints on paper, the three-colour photo-mechanical process, and triple superposed transparencies all come under the heading of negative synthesis. The latter subdivision embraces most of the work done by the writer which has, so far as he knows, not before been undertaken in Canada. Positive synthesis depends on positive colour mixture or the superposition of coloured lights, while negative synthesis depends on negative colour mixture, the addition of absorptions, or the superposition of coloured dyes or pigments.

In positive synthesis, a transparency in black and white, an ordinary lantern slide in fact, is made from each of the three negatives; and each transparency is illuminated by its own reproduction colour. That is to say the transparency from the negative taken through the red filter is illuminated with pure red light, that from the green filter with pure green light, and that from the blue-violet filter with pure blue-violet light; this is effected, practically, by backing each transparency with its corresponding reproduction glass. Since the relative transparency or redness of the red positive is proportional to the amount of the primary red, of the green positive to the amount of the primary green and of the blue-violet positive to the amount of the primary blue-violet required, when united, to match the colours of the object, evidently any method

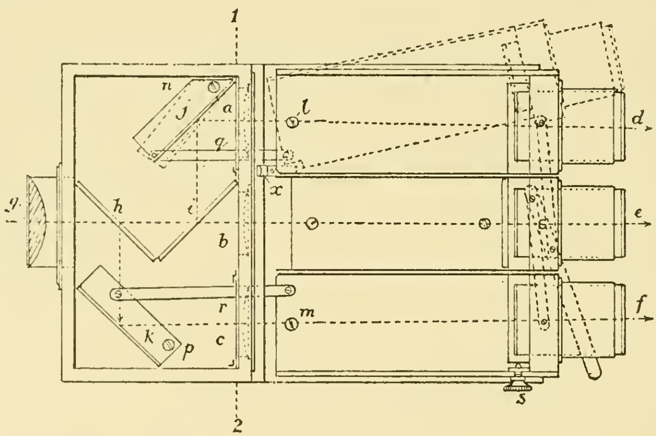


FIG. 5.—Ives' Lantern Kromskop.

of seeing these three images at one time and in one position on the retina should reproduce the colours of the object. One method of effecting this is by triple projection in which the three images are superposed and registered on the screen. The three lantern slides from the three negatives are placed one in each lantern and backed with their suitable reproduction glasses, thus giving the required red, green and blue-violet images on the screen. Each of these images, it may be mentioned in passing, according to the Young-Helmholtz theory, stimulates its respective process to the same degree as the original object. Again, the superposition of the green and blue-violet images, on the same theory, would, to the red colour-blind person appear similar to the original object, the superposition of the red and blue-violet would appear natural to the green colour-blind, and of the red and green to the violet colour-blind.

The superposition and registration of the three images [*shown*], give us as you will agree, a very good reproduction of the original colours. It may be as well to point out, however, that it is a mere optical illusion, for the colours though visually similar are not optically the same. In the one case we have, collectively at any rate, the whole range of the spectrum and in the other only three narrow bands in the red, green, and blue. The principal drawbacks to this method of synthesis are the necessity of using three lanterns and the difficulty of registration. These can be partially overcome by means of Ives' lantern Kromskop a diagram of which may be shown (Fig. 5). A beam of light from the source, preferably the electric arc, is rendered parallel by the condenser *g*, and divided into three approximately equal portions by the unsilvered reflectors *h* and *i* and the silvered mirrors *j* and *k*; these three beams then pass through the reproduction filters *a*, *b*, *c*, and the three transparencies and are focussed and registered upon the screen by the objectives *d*, *e*, *f*, which are adjustable for this purpose. The three transparencies are made on a single oblong plate from negatives on a single plate, these negatives being taken in a special camera which insures the same relative position of the three images in every case. Registration for one slide then will be registration for all and this difficulty is lessened. Only a comparatively small picture can be successfully shown, however, owing to the loss of light in the colour screens, a diameter of three or four feet being the limit for a brilliant image.

Instead of superposition on the screen the three images may be united by superposition on the retina. This can be effected by another device of Ives, called the Kromskop or photo-chromoscope, a diagram of which is given in Fig. 6. It depends upon the principle of transparent or partially transparent reflectors. In Ives' instrument A. B. C. are the positions of the three transparencies from the red record negative at A, from the blue-violet record negative at B, and from the green record negative at C. At A is the red reproduction screen and at B is a violet screen; D is a transparent mirror of blue-green glass and E a transparent mirror of yellow-green glass. The red image from A falls upon the mirror at D and is reflected to the eye, any entering the mirror being absorbed since its colour, blue-green, is complementary to red. The violet image at B is reflected from the yellow-green mirror at E and transmitted through D, becoming blue-violet and thus reaches the eye. The transparency at C, being transmitted through yellow-green and blue-green, reaches the eye as green. The red, green, and blue-violet images are superposed on the retina, reproducing, in a very faithful manner, the original colours.

The writer was unable to obtain either a lantern attachment or a Kromskop. The difficulty was overcome in the former case, as has just been seen, by the use of three ordinary lanterns ; in the latter case by constructing a modification of Ives' Kromskop. This differed in principle by the substitution, for the coloured glass reflectors, which could not be obtained, of transparent mirrors silvered on the front surface. If unsilvered or uncoloured glass were used the reflected images would be doubled and registration could not be obtained.

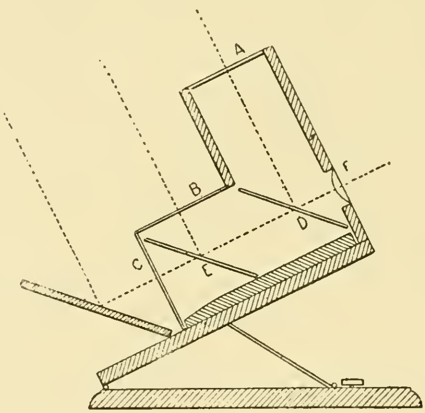


FIG. 6.—Diagram of the Kromskop.

Considerable difficulty was experienced in getting a satisfactory film of silver upon the thin plate glass used. It must be remembered that the construction of a mirror, silvered on the front surface, even when the coating is opaque, is by no means an easy matter; moreover that the mirrors for the Kromskop require a transparent and uniform coating of silver on the front surface, which must be of such density as to transmit a definite proportion of the incident light, the quantity transmitted being determined by the condition that the field of the instrument remains uncoloured. Evidently the silvering process was not a simple matter, and several trials were necessary. Success was, however, finally obtained and the instrument constructed gives excellent results.

The great objection to these methods of positive synthesis lies in the fact that the colours can only be seen by means of special apparatus, and although effects are produced by the Stereo-Kromskop, embracing both the natural colours and stereoscopic relief, which can not be excelled by any other method, yet some process of producing slides to be projected by an ordinary lantern or viewed like an ordinary transparency, would be decidedly more useful.

Such slides and transparencies are examples of the second method of producing coloured positives, *i.e.* by negative synthesis. They depend upon the superposition of coloured transparencies or pigments, and the basis of the method is essentially different. It is, in fact, exactly complementary to the former method, in which the effects were produced by adding coloured light to coloured light, hence the name, positive. This latter method depends upon the addition of absorption to absorption, or the addition of colours (by negative mixture remember) which are obtained by subtracting the positive colours from white light, and is called negative by reason of the subtractive nature of the superposed absorptions.

From the three negatives, transparencies are made as in positive synthesis, but, instead of being in black and white, they are in colours complementary to the reproduction colours. That is, from the red-record negative, a transparency is made in minus red or blue-green, from the green in magenta-pink, and from the blue-violet in yellow. The colour and appearance of these three transparencies are represented approximately, although not exactly, by the three coloured positives shown in the Plate. The superposition and registration of the three transparencies produces a slide similar in external appearance to an ordinary lantern slide but exhibiting the colours of the object.

Let us examine into the reason for the complementary nature of the printing and reproduction colours; that they are very nearly complementary is at once evident by the spectroscopic test [*shown*]. Consider the transparencies, by the two methods, from the negative taken through the red filter. In positive synthesis its lights are coloured red, or red is transmitted, while its shadows are dark or red is absorbed. In negative synthesis its lights are white or red is transmitted and its shadows blue-green, complementary to red, and hence red is absorbed. Thus red is transmitted by the lights and absorbed by the shadows in the two cases ; and the same thing will be true for the green and blue-violet records, so that the superposition by the two methods should give identical results.

A simple concrete example may perhaps help to render this somewhat difficult point more clear. Consider the effect of photographing a pure green patch on a white ground. The grounds in the three negatives will be opaque; the patch in the red and blue-violet record negatives transparent, and in the green record negative opaque. The corresponding transparencies will have transparency of the grounds from all three, transparency of the patch from the green and opacity of the patch from the red and blue-violet record negatives. In positive synthesis the ground will be composed of red, green, and blue light superposed, giving white; while the patch will be green since the opacity of the other two prevents red or blue-violet from reaching the retina or screen. In negative synthesis, in the superposed transparencies, the three grounds will be colourless giving white, while the patch will be coloured blue-green in the transparency from the red, and yellow in the transparency from the blue-violet record negative, there being no colour from the green record negative. The superposition, by negative colour mixture, of blue-green and yellow gives pure green. The results are identical, visually, although they may be and, in fact, are essentially different spectroscopically.

An illustration of the three prints, in the complementary colours, and of the finished picture resulting from their superposition is found in the Plate. The method of formation of any colour in the picture by the negative mixture of varying quantities of the three printing colours will illustrate, better than any description, the reason for using complementary colours in the printing or staining of the components. The principal steps in making a transparency or print by the three-colour process are illustrated in this Plate. The three negatives, without colour of course, taken through the red, green, and blue-violet filters illustrate the essential differences in densities due to different colours, and indicate the general character of colour record negatives. The separate prints are quite similar to ordinary prints from the above negatives, but are in colours complementary to the reproduction colours. The superposition of these three, or the printing of one over the other, produces the finished picture. In the Plate the printing was effected by three process blocks made from the three negatives, but it could be accomplished photographically by using carbon tissues of the required colour, and transferring them to a common support in accurate register. Such a process would be exceedingly tedious and somewhat uncertain, and an easier method is offered in three-colour transparencies or lantern slides. Here three separate coloured films are made and superposed giving, when bound together, the finished slide. The examples of colour prints made by the writer are by this latter method of superposing three transparencies, and can be projected upon the screen. In the majority of cases, as will be seen later, the colours are fairly true to nature, and, where such is not the case, the reason is generally obvious. It must be remembered, however, in criticising three-colour work that the slightest change in conditions during one stage of the process may be the cause of considerable change in the colours produced. Hence absolute accuracy should not be expected, but a range of colours agreeing very well, in general, with the originals can be, with care, always obtained.

The three methods of negative synthesis, above referred to, follow exactly the same principle. In the three-colour transparencies, part of the transmitted white light is absorbed by the colours giving the resultant effect. In three-colour prints on paper, whether photographic or photo-mechanical, the white paper reflects all the colours, part of them being absorbed by the dyes or inks in the same manner as in the transparencies; hence similar results should be obtained by the three methods.

As a matter of fact, however, leaving three-colour photographs out of the question, as being not yet in a practical stage, the range and gradation of colours obtained in three-colour transparencies is superior to that usually reached by the three-colour photo-mechanical process. The causes of this inferiority of the latter process are not far to seek. In the first place, printing inks fulfilling the theoretical conditions of absorption, transparency, and permanency required for this purpose have not yet been produced; and certain shades of green and brown cannot be obtained by these already in use. Fugitive inks, more nearly complying with the two first conditions, can be obtained giving better colour renderings, but the employment of these for most purposes is undesirable. In the second place, the difficulty, I think, lies in the process worker trusting too entirely to empirical methods, and not attempting to place his work on a true scientific basis, without which complete success can not be hoped for. It is unfortunately true that some sort of coloured effect can be obtained with incorrect screens and printing colours, but delicate and accurate colouring is impossible without the use of scientifically adjusted filters and colours.

Before concluding, a short description of some of the principal technical details of the process may prove of interest and possibly even of some service to those undertaking this fascinating branch of photography.

The first step in the process of making a three-colour transparency is to obtain the correct ratios of exposure through the three filters. Remembering that in the synthesis equal quantities of the reproduction or printing colours gave white or grey, it is evident that objects free from colour must be represented by equal density in the three negatives. The ratios of the exposures made, in a steady light, on such a test object as a piece of crumpled white blotting paper, must be so adjusted that the density of the resulting negatives, after development for the same time in one tray, will be equal. Each batch of emulsion requires a test of this nature as the relative colour sensitiveness of different batches varies slightly; for example, in one emulsion the ratios of exposure were found to be red, 60; green, 8; blue, 1; and in another red, 30; green, 6; blue, 1.

The exposure through the red filter is about five hundred times the normal exposure without a filter, so that, using ordinary apparatus, and allowing for the changing of plate holders, filters, etc., a set of three exposures, under the best possible conditions, of a well lighted outdoor subject will require at least a minute; while for indoor exposures ten minutes and upwards, depending upon the light, will be required. Any movement of camera or object, any error or omission in the sequence of operations, about fifteen in number, or any change in the quality of the light, unless correctly allowed for, renders all three negatives useless. The use of a special camera, in which all three exposures are made simultaneously, considerably reduces the time of exposure, and, by lessening the number of operations, diminishes the liability to error; but such a camera is expensive to purchase and troublesome to keep in order.

It is advisable before development to mark or letter a corner of the film on each plate, as it will be frequently found difficult to distinguish the negatives from one another. The plates are developed together in one tray in a metol developer, without bromide; with any other developer, especially hydroquinone, the images will not appear equally, and a different range of gradations will be obtained giving faulty results. Short development producing soft delicate negatives is required; over development bleaches out the light tints of colour. Under exposure causes excessive colour contrasts, while over exposure weakens the contrasts. The negatives are fixed and washed in the same manner as ordinary negatives, and after drying are ready for making the transparencies.

For positive synthêsis, whether for triple projection or the kromskop, three ordinary lantern slides are required. These must also be soft, delicate, of a neutral tint, and of the same density.

The three-colour transparencies, by negative synthesis, are composed of three coloured positives, one on glass and two on celluloid. From the negative taken through the red filter an ordinary lantern slide in black and white is made, and the black silver image is changed to a blue-green or minus red colour by an iron process. The positives from the green and blue-violet record negatives are made, by a modification of the carbon process, on transparent celluloid coated with a soluble emulsion of silver bromide in gelatine. These films, which are quite similar to the regular Kodak film, are sensitised by soaking for three minutes in a solution of bichromate of potash; and when dry are exposed under the negatives to daylight, the celluloid side being next to the negatives. The image, being partially visible, forms a guide to the length of exposure required, which averages about five minutes in diffused light. The parts of the bichromated gelatine unexposed to the light dissolve in warm water, and, when the silver bromide is also dissolved out in a solution of hyposulphite of soda, there is left an image in colourless gelatine in relief.

The colour is given to these images by soaking in baths of aniline dyes; the print from the green-record negative in minus green or magenta-pink, and from the blue-violet-record negative in minus blue or yellow [Plate]. The gelatine being in relief, varying depths of colour in the images are obtained, reproducing all the gradations. Perhaps the most tedious part of the process is to obtain the correct depth of colour in the prints. This can be tested by observing the roughly superposed prints. A general blue, pink, or yellow tinge throughout the picture shows that the corresponding prints are too deeply stained. The colour given to black or grey objects also forms a delicate test; rusty brown blacks indicate that the pink print is too strong, greenish blacks too weak; violet blacks indicate that the yellow print is too weak. Prints too strong are quickly reduced by soaking in water, while if too weak are strengthened, to a certain limit, by a longer immersion in the dye bath. This, of course, does not apply to the blue-green image which when once made cannot be changed in intensity, and, if not suitable, the only remedy is to make another.

When the colours are correctly adjusted, the prints must be mounted in register. The registration is not difficult if the prints are all of the same size, and, when effected, a cover glass may be put on and the slide bound in the ordinary way. The numerous internal reflections diminish the brilliancy of the image, and it is preferable to seal the components in optical contact with Canada balsam. Although this is a disagreeable and troublesome process, one is amply repaid by the superior brilliance of the slides. These may be viewed in the hand or projected by an ordinary lantern similarly to the regular slides and require no special care in their handling.

The natural colour slides made by the writer, some twenty in number, embrace reproductions of the spectrum, colour charts, and coloured pictures ; flowers, and fruit ; views around the University and some other subjects. These, which can now be exhibited, illustrate fairly well the capabilities of the process [*Slides shown*]. Many instances, in which a slide in the natural colours, if obtainable, would prove of very great value, will at once suggest themselves and the process has now reached the stage where any such slides can be made ; and the further simplification of the details ought to render such slides a regular commercial product.

The three-colour process is an indirect, and, to a certain extent, cumbersome method, and not at all what is usually looked for in colour photography. The ideal process would be such that a photograph in colours could be produced similarly to, and with little more trouble than a photograph in monochrome. This problem, however, seems no nearer a practical solution than it has for the last twenty or even fifty years, and there is no process at present in sight which holds out any hope of realizing such an ideal. But, in consideration of what has already been accomplished in science and the arts, he would be foolish who would venture to put a limit to man's achievements in this or any other branch of science.

JOSEPH BRANT IN THE AMERICAN REVOLUTION.

BY LIEUT.-COL. E. CRUIKSHANK.

SECOND PAPER.

(Read 26th April, 1902).

THERE can be no more convincing proof of the extreme importance that was attached to the subjugation of the Indians than the fact that Washington was willing to detach an entire division of his best troops upon this service for the greater part of a year and weaken his own army so much in consequence that he was obliged to remain almost wholly on the defensive during their absence. Preparations for this expedition began in the winter of 1778–9. As early as February 11th, 1779, Washington had instructed Major-General Philip Schuyler to collect intelligence for that purpose.

"It will be necessary immediately to employ proper persons unacquainted with each other's business to mix with the hostile Indians that the most unequivocal information may be gained of their strength, their intentions, and what ideas they may have acquired of our design. We should also learn what support or assistance they expect in case our intention should be known to them or, what precautions they are taking to oppose our operations.

"The Indians in friendship with us may be sent on this purpose. The half-tories also, if they can be engaged and will leave pledges as a security for their fidelity, might prove very useful instruments. Similar investigations should be carried into Canada and the garrison at Niagara.

"I shall likewise depend on your having the routes to the object of the expedition critically explored both by Indians and others so that a complete knowledge of distances, natural difficulties, and the face and nature of the country may be obtained."

On March 3rd, 1779, letters were addressed to President Reed, of Pennsylvania, and Governor Clinton, of New York, informing them of the proposed invasion of the Indian country and requiring the assistance of a body of militia from each State, suggesting at the same time that as great a proportion as possible of the troops to be furnished by them should be composed of persons who had been driven from the frontier by the Indians.

"This class of people," Washington observed, "besides the advantages of a knowledge of the country and the particular motives with which they are animated, will be most likely to furnish the troops best calculated for the service. They should be a corps of active rangers who are at the same time expert marksmen and accustomed to the irregular kind of wood-fighting practised by the Indians."

A few days later the command was offered to Major-General Gates who possessed the highest reputation of any officer in the Continental army in consequence of his success at Saratoga.

"The objects of the expedition," he was informed, "will be effectually to chastise and intimidate the hostile Indians, to countenance and encourage the friendly ones, and to relieve our frontiers from the depredations to which they would otherwise be exposed. To effect these purposes it is proposed to carry the war into the heart of the country of the Six Nations, to cut off their settlements, destroy their next year's crop, and do them any other mischief which time and circumstances will permit."

The force it was intended to place at his disposal was stated at 4,000 Continental troops, rank and file actually fit for service, and as numerous a force of militia as might be deemed necessary.

But Gates absolutely declined to undertake the service on the plea that he no longer possessed the "requisite youth and strength" for such an enterprise, and the choice of Congress then fell upon Major-General Sullivan, an officer of considerable military experience and ability.

Rumours were then spread abroad designedly of an intended expedition against Quebec by way of Coos, N.H., to prevent the British garrisons in Canada from affording any support to the Indians with regular troops.

On March 25th, Washington definitely stated the proposed plan of operations in a letter to General Schuyler.

"The route by the Susquehanna appears to be more direct, more easy and expeditious, and more secure. There is very practicable navigation for boats of eight or ten tons all the way from Sunbury to Tioga, about 140 miles, and for small boats as far as Shemung about eighteen miles beyond Tioga. The distance from Shemung to the heart of the Seneca settlements is not above 60 or 70 miles through an open and travelled country very susceptible of the passing of a body of troops with artillery and stores."

After announcing his intention of dividing the invading force into three columns to move simultaneously up the Mohawk, Susquehanna, and Allegany rivers, he added :—

"These different attacks will terrify and distract the Indians, and, I hope, facilitate our project. It is also to be hoped in their confusion they may neglect in some places to remove the old men, women, and children and that these may fall into our hands. If they attempt to defend their country, we may obtain some decisive advantage, if not, we must content ourselves with distressing them as much as possible and destroying this year's crop."

The Congress of the State of New York promptly furnished a thousand militiamen under General James Clinton, a brother to the Governor, who was instructed to assemble this force at Canajoharie on the Mohawk river by May 12th. The first blow was directed against the Onondaga village, the most accessible settlement of the hostile Indians. On April 20th, Clinton advanced swiftly from Fort Schuyler with about 500 soldiers and surprised the village in the absence of most of the men, captured thirty-four women and children and burnt every building there.

The preparations for the main expedition under Sullivan's command went on without interruption. Hundreds of boats built for the purpose were incessantly employed for several months in transporting supplies to Wyoming, which was selected as the base of operations.

"The expedition you are to command" Washington wrote to Sullivan on May 31st, "is to be directed against the hostile tribes of the Six Nations, their associates and adherents. The immediate objects are the total destruction and devastation of their settlements, and the capture of as many prisoners of every age and sex as possible. It will be essential to ruin their crops now in the ground and prevent their planting more. . . . .

. . . .

. . . . Parties should be detached to lay waste all the settlements around with instructions to do it in the most effective manner that the country may not be merely *overrun*, but destroyed.

" After you have very thoroughly completed the destruction of their settlements, if the Indians should show a disposition for peace, I would have you encourage it on condition that they will give you some decisive evidence of their sincerity by delivering up some of the principal instigators of their past hostility. Butler, Brant, the most mischievous of the Tories that have joined them or any others that may be in their power that we are interested to get in ours. They may possibly be engaged by address, secrecy, and stratagem to surprise the garrison of Niagara and the shipping on the lakes and put them in our possession."

The inroads by Brant upon the Minnesink and by Macdonnell on the west branch of the Susquehanna were mainly designed to divert Sullivan from prosecuting his invasion of the country of the Senecas, but he resolutely refused to detach any portion of his force for the defence of the frontiers. Finally when all his preparations were complete he advanced on August 11th, to Tioga Point at the confluence of the Tioga with the Susquehanna where he formed a fortified depot for his supplies. About the same time Brant with his party returning from his attack on Minnesink rejoined Colonel Butler at a place called Chuckmet, or New Town, fourteen miles from Sullivan's encampment, where he was endeavouring to assemble the Indians for the defence of the Seneca towns. A letter written by Brant about this time to Colonel Daniel Claus has been preserved.

SHIMONG, August 19th, 1779.

" I am deeply afflicted; John Tayojaronsere, my trusty chief, is dead. He died eight days after he was wounded. Five met the same fate. I am very much troubled by the event as he was of so much assistance to me. I destroyed Onawatoge a few days after. We were carrying off two prisoners. We were overtaken and I was wounded in the foot with buckshot but it is of small consequence. I am almost well.

" We are in daily expectation of a battle which we think will be a severe one. We expect to number about 700 to-day. We do not quite know the number of the Bostonians already stationed about eight miles from here. We think there are 2,000 besides those at Otsego, represented to consist of two regiments. This is why there will be a battle either to-morrow or the day after. Then we shall begin to know what shall become of the people of the Long House. Our minds have not changed. We are determined to fight the Bostonians. Of course their intention is to exterminate the People of the Long House. The Seven

Nations will continue to kill and devastate the whole length of the river we formerly resided on.

" I greet your wife. I hope she is still well and that you yourself may also be well."

On the same day that this letter was written Sullivan was joined by Clinton's brigade which floated down the Tioga on the crest of an artificial freshet they had created by damming that river near its source, and increased his force to above 5,000 effective men. The Indians became panic-stricken at the appearance of such an overwhelming army which was attended by a multitude of packhorse drivers and boatmen, and the majority seemed to think only of placing their families and moveable property in a place of safety. Butler bitterly complained that he was unable to assemble more than 300 warriors to resist the enemy's advance when their chiefs had promised to join him with at least a thousand. He had brought with him from Niagara to their support about three hundred of his corps of rangers and fourteen volunteers from the detachment of the 8th or King's Regiment then stationed at that post. With this comparatively small force be kept up a show of confidence and assured the faltering chiefs that he hoped to repel the invaders with the rangers, assisted only by their brethren led by Brant even if they declined to come to his assistance.

On August 27th, he advanced a few miles nearer the enemy's camp and occupied a position selected by the Delawares as the place where they should await an attack. It was a ridge extending from the river to the foot of the mountain and covered in front by a large creek, but was much too extensive to be held by so small a force. The defence of the right flank in the low ground next the river was entrusted to Captain John Macdonnell with sixty rangers assisted by Brant with thirty volunteers from the loyalists and Indians.

Two days later Sullivan advanced and after cannonading their position for several hours turned their left flank when the Indians in that part of the field made such a precipitate retreat that the rangers and Brant's volunteers were nearly surrounded before they became aware of this movement and forced to disperse to effect their escape which they succeeded in doing with slight loss.

The Indians were so thoroughly dispirited by this affair, which was called by the Americans the battle of Newtown, although they had lost only five men killed and nine wounded, that they could not be induced to make another stand even by the influence and example of Brant and the Seneca chief Sangerachta, who are described by Butler as having behaved on all occasions with great courage and determination.

The victorious army destroyed forty Indian villages with their adjacent orchards and cornfields but did not succeed in taking more than a dozen prisoners, and failed to lay waste a considerable part of the fertile valley of the Genesee, whither the Indians retreated. Brant continued during these operations to watch their movements with great vigilance but his sole success was the destruction of an isolated party of thirty men under a Lieut. Boyd.

On September 8th, Butler reported that the enemy had taken possession of Canadasaga, the principal Seneca village the day before. "Joseph Brant who stayed to reconnoitre says that to all appearances they cannot be less than 3,000."

Two days later he stated that Sangerachta had gone with Brant to meet the chiefs at Genesee. "There is a very good understanding between them and they concur with each other on every occasion."

"Shortly after Lieut.-Colonel Mason Bolton, the commandant at Niagara, reported that the Indians are extremely dissatisfied that troops have not been sent to Oswego or this [post] notwithstanding all the efforts of Major Butler, Sangerachta and Joseph Brant to keep them in line."

In his next letter (September 17th) Bolton remarked that "Joseph Brant who upon all occasions deserves everything that I can say in his favour, has just arrived. Sangerachta has behaved extremely well. He has great weight with the Six Nations. Joseph some time ago was not on the best terms with him. They had their quarrels like other great men."

Early in October Brant again returned to Niagara from the Seneca country and reported that the invaders had retired to Wyoming. By this time, Sir John Johnson had arrived there with a reinforcement of four hundred troops from Montreal, and after a consultation with Colonel Bolton sailed for Oswego with the intention of attempting a raid upon the Mohawk valley. Bolton stated that "Joseph with a number of the Six Nations went by land to the Three Rivers, the place of rendezvous, determined, I believe, to cut off the Oneida village and attempt something more if opportunity offers, but as the season is so far advanced I think it scarcely possible."

His forecast proved correct. Foul weather set in and continued until the expedition was abandoned and Brant returned to Niagara on November 15th.

Snow fell that winter to an extraordinary depth and many Indian women and children perished miserably from cold and famine mainly in consequence of the devastation of their settlements. The animosity of the Indians against their enemies was thus greatly intensified.

Guy Johnson arrived at Fort Niagara late in the autumn and superseded Butler in control of the Indian Department. About the middle of February Brant was despatched to the frontier with Captain Hendrick Nelles, Lieut. Joseph Clement, twelve white volunteers, and 220 Indians. This was much the largest force he had yet commanded and he began operations by forming a close blockade of Fort Schuyler, by which the garrison was reduced to great distress. There is no record of his movements for several months, but the following manifesto has been preserved which is dated at the Delaware on April 10, 1780.

"That your Bostonians (*alias* Americans) may be certified of my conduct towards all those whom I have captured in these parts know that I have taken off with me but a small number. Many have I released. Neither were the weak and helpless subjected to death, for it is a shame to destroy those who are defenceless. This has been uniformly my conduct during the war. These being my sentiments you have exceedingly angered me by threatening or distressing those who may be considered as prisoners. Ye are (or once were) brave men. I shall certainly destroy without distinction does the like conduct take place in future."

At the end of April Brant was still out, but had sent in Lieut. Clement for supplies, and he does not seem to have returned to Niagara until the end of June when he reported that the Oneidas were prepared to abandon the rebels on the first favourable opportunity.

Early in July he marched out again "with a strong party of warriors" with the intention of raiding the few remaining settlements in the Mohawk valley. On August 8th Bolton reported his first success.

"Joseph has paid a visit to the Oneida village which with the fort he set on fire. One hundred of them are now on their way to this post and the rest thought it proper to retire to Fort Stanwix which obliged him and his party of 370 men to march with all expedition possible towards the Mohawk river as there was no doubt but that an express would be sent off to alarm the inhabitants."

Three days later Guy Johnson furnished additional details.

"Captain Brant has already effected a very good piece of service and is advancing against the rebel frontiers. On his march from hence he came upon the only remaining Indian village, sixteen miles from Fort Stanwix. He found the village abandoned but met some Indians who told him they had returned through fear of parties of strange Indians with many other particulars in which it appeared they had deceived him for they soon deserted and gave notice to the garrison of Fort Stanwix. Captain Brant then burnt the rebel fort at the village with other buildings and marched to the Indians below Fort Stanwix where he met the Oneidas in camp and called upon them to follow the example of the rest of their people and return to the British Government. About 100 replied that it was their desire and they are now partly come into this place. The small remainder ran towards Fort Stanwix which they reached except two who were shot. He then drew towards the fort where he proposes to remain a few days to deceive the rebels and then proceed against the frontiers. The fort burnt by that party was a great inconvenience to us, and its destruction with the return of the Indians to their true alliance will distress the rebels and lay that route open to our parties.

"Lieut. Clement has just arrived express from Captain Brant. He has destroyed and taken so many cattle with what may be expected from his having subdivided his party who are gone against other places, must be severely felt by the rebels along that country. This occurred about the 2nd inst.

"Lieut. Clement reports that Captain Brant has burnt and destroyed the Oneida village of Conowaroharie with the rebel fort and village, and retired somewhat to deceive the enemy. They proceeded to the Mohawk river with about 300 Indians and arrived at the settlement called the Kley's Barrack about 10 o'clock a.m., on August 2nd, which having reconnoitred, he and the chief warriors thought proper to detach David Karacanty with the greater part of the Indians to make a detour and suddenly attack Fort Plank, while Joseph and the remainder should come on directly and prevent any scattering parties from taking shelter in the fort. In this they were disappointed by the too great eagerness of the Indians to take prisoners, who scattered and alarmed the settlement, by which a considerable number of men got into the fort which made an attack inexpedient, as it was well fortified and had two pieces of cannon mounted. Disappointed in this they advanced to the upper part of the settlement where the rebels had a fort at the house of Hendrick Waldrod which they abandoned. This they immediately burned, and scattering, the Indians destroyed houses till they came to Elias Map's where they had another picketted fort which they likewise burned. The extent of the settlement destroyed was on the Mohawk river in length above two miles and above five in breadth, and containing above 100 houses, two mills, a church and two forts. They took and killed about 300 black cattle and 200 horses besides hogs, poultry, etc., and destroyed a considerable quantity of grain of different kinds. The number of rebels killed and prisoners amounts to about 45. Captain Brant released a number of women and children and having effected this he retired to Butler's Mills about three days since. With the greater part of the Indians he intends to pay the rebels another visit before their return, for which purpose they have divided into seven parties."

These detachments marched by separate routes against Schoharie, Cherry Valley, and the German Flats, where they took many prisoners, destroyed buildings, and created intense alarm.

By one of these parties Brant transmitted a threatening message to a militia officer at Schoharie which has been preserved in the Clinton Papers.

"I understand my friends Hendrick Nuff and Cook are taken prisoners near at Esopus. I would be glad if you will be so kind as to let those people know that took them not to use my friends too hard, for if they will use them hard and hurt them I will certainly pay for it, for we have several rebels in our hands [which] makes [me] mention this for it would be disagreeable for me to hurt my prisoners. Therefore I hope they will not force me."

Early in September General Haldimand determined to despatch two strong expeditions against the frontier of New York, which were designed to advance simultaneously, one from Crown Point towards Albany, the other from Oswego upon the Mohawk valley. The objects of these movements, he stated, were "to divide the strength that may be brought against Sir H. Clinton, to favour any operations his present situation may enable him to carry out as well as to destroy the enemy's supplies from the late plentiful harvest and to give His Majesty's loyal subjects an opportunity of retiring to this Province."

Lieut.-Colonel Butler, with 200 rangers and 220 regular troops from the garrison of Niagara, was directed to join Sir John Johnson at Oswego and act under his orders. His instructions forbade him to take "a single man who is not a good marcher and capable of bearing fatigue. I hope Joseph is returned," the Governor added, "as I would by all means have him employed on this service."

Contrary winds prevented Butler from arriving at Oswego until October 1st, and by that time the garrisons on the Mohawk were warned by their Indian spies that he had sailed from Niagara on an expedition of some kind. It was not until daybreak on the 17th that the weary column, commanded by Sir John Johnson, passed the fort at the head of Schoharie, having made a long detour through the wilderness for the purpose of attacking the enemy in an entirely unexpected quarter, and swept along the west bank of that stream down to the Mohawk, burning every building and stack of grain as they went along. Sir John then "detached Captain Thompson of the rangers and Captain Brant with about 150 rangers and Indians to destroy the settlement at Fort Hunter on the east side of Schoharie Creek, which they effected without opposition, the inhabitants having fled into the fort." Advancing swiftly up the Mohawk the invaders laid waste the country on both sides until midnight, when utterly exhausted they halted at the narrow pass called "the Nose" to snatch a few hours' sleep. Before daybreak they were again on the march and soon encountered Colonel Brown with 360 men from Stone Arabia who attempted to check their further progress. While the detachments of the 8th and 34th Regiments advanced directly upon the front of the enemy's position, Brant with a party of Indians made a circuit through the woods to turn their right flank, and Captain John Macdonnell led a body of rangers in the opposite direction to turn their left. The position was carried with trifling loss to the assailants, while Colonel Brown was killed and about a hundred of his men killed or taken. Johnson reported that "Captain Macdonnell and Captain Brant exerted themselves on this occasion in a manner that did them honour and contributed greatly to our success. Captain Brant received a flesh wound in the sole of his foot near his former wound."

Before night they were forced to fight a sharp rear-guard action with a pursuing force of more than a thousand men under General Van Rensselaer. They turned upon their assailants, drove them from their position and crossed the river unmolested. During their raid they had destroyed thirteen gristmills, many sawmills, a thousand houses and about the same number of barns, containing, it was estimated, 600,000 bushels of grain. The severity of the blow from a military point of view was freely acknowledged by their enemies.

James Madison wrote from Philadelphia on November 14th, 1780:—

"The inroads of the enemy on the frontiers of New York have been most fatal to us in this respect. They have almost totally ruined that fine wheat country which was able, and from the energy of the government was likely, to supply magazines of flour both to the main army and the northwestern posts. The settlement of Schoharie which alone was able to furnish, according to a letter from General Washington, 80,000 bushels of grain for public use has been totally laid in ashes."

Brant returned to Niagara where he remained about two months to recover from the effects of his wound, but on the first day of February he again marched for the Mohawk river at the head of 185 Onondagas and Oneidas, accompanied by thirty rangers under the command of Lieut. John Bradt, a nephew of Colonel Butler, and Volunteer Hare. He was instructed to blockade Fort Stanwix and observe the motions of the enemy generally. The perils and hardships of such an expedition had been vastly increased by the destruction of the Indian villages and the devastation of the border settlements, as Colonel Johnson pointed out.

"This post (Niagara) is unluckily at a great distance from the rebels settlements, which not only occasions delay, but causes each party to carry three weeks or a month's provisions as (since the loss of the Indian towns,) none can be had by the way. The Mohawk river has ceased to be an object as being almost totally ruined."

They arrived one day too late to intercept a convoy of provisions, but cut off a party of soldiers sent out from the fort to cut wood of whom was one killed and sixteen captured. After lurking in the vicinity for several weeks, they returned to Niagara on March 17th.

By this time reports had been received from Detroit that parties of frontiersmen from Pennsylvania and Virginia had been directed to assemble at various stations on the Ohio river, under the command of Colonel George Rogers Clark, with the avowed intention of invading the territory of the Western Indians and possibly attacking that post. Colonel Guy Johnson accordingly determined to despatch Brant with an escort of seventeen young Seneca warriors to deliver "a speech and belt [of wampum] to the Indians there and also to the Shawanese villages, to encourage them to act with vigour and to watch the enemy's motions, with the promise of such aid as time and circumstances will permit from hence."

For some time Brant had received pay as commanding officer of a corps known as "Captain Brant's Volunteers," composed partly of white men and partly of Indians, but soon found that this station impaired his influence among his own people.

Brigadier General Watson Powell, who succeeded Colonel Bolton in command at Fort Niagara, wrote to General Haldimand on May 15th, 1781 :—

"I do not think Captain Brant is quite pleased with his situation, as he told me the day before he went off that he wished to give up his company. I believe he would be happier and have more influence with the Indians which he in some measure forfeits by their knowing that he receives pay."

He added that Brant was very anxious to go to Oswego, where a British post had lately been established and make it his base of operations as soon as he returned.

Brant and his party sailed for Detroit in one of the armed vessels on Lake Erie about the middle of April, and on the 26th of that month Major De Peyster, attended by the officers of the garrison, held a council with a number of the chiefs and warriors of the Hurons, Ottawas, Pottawatonies, and Miamis at which they declared their firm determination to support the Shawanese who were believed to be the first object of the enemy's attack.

Brant's speech at this council is thus reported:—

"The-ya-en-dinega (*alias*) Capt. Brandt addressed himself to the several Indian nations and said :—

"I am pleased to find that you are ready to assist your brethren the Shawanese. You see me here. I am sent upon business of importance to your several nations. I shall follow you and your father to the camp that is to be formed at Sandusky at which place I shall deliver to you the speeches of the Six Nations in presence of the Ohio Confederacy who will be there. I hope when you are acquainted with the contents of my embassy, it may furnish means to unite you more strongly in the cause we are mutually engaged, and continue our friendly intercourse as the meeting will be general." (Haldimand Papers, B 123, p. 27).

Scarcely had the council dissolved when a messenger arrived from Sandusky with the alarming report that a strong body of the enemy, under Colonel Brodhead, had surprised and destroyed the Delaware village of Cooshocking and had then divided into two parties which were supposed to be advancing upon Sandusky by different routes. Brant immediately marched with his small party to the assistance of the Hurons at that place and was soon followed by Captain Andrew Thompson with a company of Butler's Rangers.

Shortly after Brant's arrival at Upper Sandusky he addressed the following letter to Captain Matthew Elliot and Isidore Chene, the Huron interpreter, who had remained at the village near the mouth of the river known as Lower Sandusky.

"UPPER SANDUSKY, *May 19th, 1781.*

SIR,—This is to acquaint you that we received an account last night from Moravian Town that there is two thousand rebels coming to this place in four parties, each of them five hundred. They intended to meet together about two days' journey from this place. Two of the Moravian Indians brought this news. If this is true they can't be now far off from this place. But I think you had better remain still where you are till you hear from us again, because the news is not certain yet until our spies return. Sir, I will be very glad if you can send me five gallons of rum by the bearer, I mean if you can do it conveniently, also, I wish you to spare me eight pieces of pork. George Girty and an Indian just arrived from the Shawanese towns brought a string of wampum message from those different nations, that they would be glad if they could get some of the ammunition as soon as possible, which Major De Peyster promised them, and also would be happy if the Major would send some of his men to assist them, because they are now sure the enemy will soon get into this country. They think if he does not send men immediately it will be too late as it happened last summer. They have sent four different parties for spies but [they] are not yet returned. They are doing according to the Major's desire. This is the purport of their message. I leave it to yourselves whether you will let Major De Peyster know this news or not. Do what you think is best. It would not be amiss if you could get a few horses and send some of the ammunition to this place.

I wish you to do all you can to encourage the Indians that came from Detroit who are not yet tired of staying there for it won't be long before we shall meet the enemy. No more at present.

I remain your

Sincere friend and humble servant,

JOS. BRANT.

(Haldimand Papers, B 101, p. 73).

Brant appears to have remained at Sandusky for several weeks until it was definitely known that a considerable body of the enemy was descending the Ohio in boats with the design, it was supposed, of attacking the Shawanese villages near Chillicothe and thence advancing upon Detroit. Captain Thompson with his company of rangers and Brant and McKee at the head of a body of Mingos and Hurons hastened to the assistance of the Shawanese. From Chillicothe Brant and George Girty went forward with a small party to the confluence of the Big Miami and the Ohio, to watch the movements of the enemy.

Their initial success was reported by Brant in a letter to McKee dated "about ten mile below the mouth of the Big Miami river, August 21st, 1781."

"Three nights ago we layed at the mouth of the Miamies river. We heard a number of boats pass but we could not tell how many for it was dark. When they go past the mouth [they] fired cannon. We was going to attack them but we could not. We suppose [them] to be Clark's army. I [have] been at the Bone Lick yesterday to see whether he was there, but I could see no sign of it.

"This morning we saw a boat coming down the river and got ready ourselves and took the boat with seven men, one major amongst them, of militia, Cracrath, who was following Clark as he is gone down sure enough and has about three hundred and fifty men with him. They [are] deserting from him very fast. The prisoners do not know who far Clark is gone down the river. They suppose [him] to be at the Falls. Likewise the prisoners says there is one hundred and fifty men coming down the river with ten small boats, one large one and one still larger horse boat [with a] number of them in it, which is expected to be here next day after to-morrow the longest. They was at the Three Islands five days ago. We are about ninety strong at present with different tribes. These Indians and chiefs particular desires you and the Indians that is with you to come on as fast as possibly you can to join this party. Whilst the enemy are scattered we can easy manage them. And further desires [an] express should be sent to different Indian villages, for every man of them should come immediately to this place for there is no signs, any other party can go against the Indians except Clark as the prisoners say there is no other can be sent. No more at present, please to excuse my writing, I wrote in a hurry.

"Please let all the Indians know if they don't come to assist us we [are] determined to attack the enemy as well as we can." (Haldimand Papers, B. 182, p. 424.)

The detachment that they then lay in wait for consisted of one hundred of the " best men of Westmoreland County " in Pennsylvania, including a small company of rangers, under the command of Colonel Archibald Lochry, the lieutenant or commandant of the militia of that county. On August 24th, Lochry's flotilla arrived in the vicinity of the spot where Brant's party lay in ambush and observing an inviting natural meadow on the Indiana shore he ordered his men to land there for the purpose of cooking provisions and cutting grass for their horses. While thus employed they were surprised and driven to their boats but their escape was prevented by a party of Indians in canoes. The entire corps was cut off. Lochry and six other officers and thirty privates were killed, and twelve officers and fifty-two men were made prisoners including Craycraft's party.

A few days later Brant was joined by Thompson and McKee with all the men they could assemble at the Shawanese villages, and the united force proceeded down the Ohio in the hope of overtaking Colonel Clark. They had advanced within thirty miles of Louisville where it was reported that he was awaiting Lochry's arrival, with the intention of attacking him in his camp when they found that so many of their Indian followers had deserted and returned to their homes, that they were obliged to abandon this design and resolved to attack some of the smaller forts in Kentucky instead. On arriving at the main road leading from Louisville to the upper forts, a party of Miami Indians who formed their advance guard surprised and captured a convoy of wagons escorted by a party of horsemen, several of whom were killed. An ambush was formed near the scene of this attack and next day they entrapped a strong party of Kentucky militia led by Colonel Floyd, the lieutenant of the county. Floyd and forty of his men were killed and a number taken prisoners with the loss of only four Indians.

During this expedition Brant accidentally wounded himself in the leg with his own sword and was consequently obliged to remain for the winter at Detroit where he was joined by his wife who came from Niagara to nurse him.

Late in April, 1782, he arrived at Fort Erie in the first vessel from Detroit, accompanied by the small band of Seneca warriors who had followed him during his western campaign. Soon after his return to Fort Niagara he consented to go to Oswego although he stated that he would have preferred to join the Shawanese again with whom he thought his services would have been more effective, and on June 24th he embarked in one of the armed vessels on Lake Ontario for that post with about 200 warriors.

At this time his relations with the officers of the Indian Department do not seem to have been very satisfactory, as General Watson Powell wrote a few days after his departure :—

"I am sorry to say there have been frequent complaints since I came here that Captain Brant was a great expense to the Government and more difficult to please than any of the chiefs, and more particularly since his return from Detroit."

After Brant's arrival at Oswego there was practically a cessation of hostilities all along the New York frontier and the garrison of that place undertook no offensive operations. In September, 1782, he accompanied Sir John Johnson and Colonel Hope in a tour of inspection by way of the Ottawa and French rivers to Mackinac and Detroit. From the latter post he went with Captain Potts of the 8th Regiment to the mouth of the Miami river on Lake Erie to select a site for a new military post and proceeded to Sandusky to meet Captain McKee on his return from another raid into Kentucky which had culminated in the battle of the Blue Licks.

After his return to Niagara his dissatisfaction seems to have greatly increased as he wrote to Sir John Johnson in the following terms on Christmas Day, 1782 :—

"I have been very uneasy since we had the news of the Shawanese' misfortunes who fell into the hands of the white savages, the Virginians, and did alarm the Five Nations greatly and made them to hold councils about the matter and make speeches to the General but badly translated into English. We, the Indians, wish to have the blow returned on the enemy as soon as possible, but I am afraid it will again be a trifling affair when our speech gets below, which is too often the case, which will be a very vexatious affair, because we think the rebels will ruin us at last if we go on as we do, one year after another, doing nothing only destroying the government goods and they crying out all the while for the great expenses, so we are, as it were, between two hells. I am sure you will assist all you can to let us have an expedition early in the spring, let it be a great or small one. Let us not hang our heads between our knees and be looking there. I beg of you, don't tell us to go hunt deer and find yourselves shoes because we shall soon forget the war for we are gone too far that way already against the rebels to be doing other things. I have changed my mind since my arrival here.

You know I was very sparing of the Indian officers to be struck off. I am writing now to be ; so if you do leave few, a little department, you will save so much money. Government may be able again to give the warriors proper clothing as they formerly had. You never saw such confusion in the department, nothing equal to at present.

" My complaint in the ear is still bad but I hope I shall be able to get out this winter to the Mohawk river. I will try to be at Oswego in thirty days' time or little more. I am as much forward to go to war as I ever did but I am not so well contented as I used to be formerly, because the warriors are in want. They are treated worse instead of better. I shall tell you the particulars if you should want to know why I write you so."

Some months later Colonel Allan Maclean, of the Royal Highland Emigrants, who had succeeded General Powell in the command of Fort Niagara, observed that " Captain Joseph Brant, though a brave fellow who has been a faithful, active subject to the King, has been the most troublesome because he is better instructed and more intelligent than any other Indian."

The dissatisfaction of the Indians had by that time greatly increased upon learning the proposed terms of peace by which they considered that their interests were sacrificed.

The war was at an end. Brant's reputation as a successful partisan stood high both among friends and enemies. None of the leaders of the Indians had been so actively and continuously engaged, and none had been so uniformly distinguished by courage and ability.

THE BEGINNING OF MUNICIPAL GOVERNMENT IN ONTARIO.

BY PROF. ADAM SHORTT, QUEEN'S UNIVERSITY.

(Read 12th April, 1902.)

AFTER the conquest of Canada, very satisfactory progress was being made towards the converting of the country into an English colony, by methods very similar to those which had worked so successfully in the colony of New York, originally a Dutch settlement. But, unfortunately for this promising development of a united Canada, difficulties arose between the mother country and the older colonies. The nature of the rights claimed by the colonists, proved to the majority of the ruling party in Britain that full British rights and liberties, even as they were in those days, were quite inconsistent with the retention of the colonies in that condition of submissive dependence, which was called for by the colonial system of the time. The object of this system was to foster the colonies, not with a view to their own good, but with a view to the good of the mother country. Nevertheless, it was honestly believed by many of its advocates, that, in serving the purposes of the mother country, the colonies would share in her prosperity and greatness, and obtain all the benefits that were possible to people who had abandoned the political. social, and other privileges of the home land for the greater material gain, but necessarily inferior life of the colonies.

As the difficulties with the colonies increased, the conviction grew that the colonists had been permitted to usurp many liberties, which were quite inconsistent with their dependent position. It was freely admitted by many that France and Spain, not England, had dealt wisely with their colonies in keeping them in due subjection.

Possessed of such convictions, and with a view to employing the joint French-Canadian and Indian forces as a rod of correction to bring the arrogant colonists to a due sense of their inferior status in the empire, every one of the numerous measures, then either in operation or preparation, for the Anglicizing of Canada was abandoned. All the ordinances passed after the Conquest were repealed by the Quebec Act, which re-established in its purity the French-Canadian civil laws and institutions. Only the English criminal law was retained, since it would afford, it was thought, a better hold upon the people, it being much more severe than the French criminal law.[1]

This reactionary attitude on the part of the British Government, which was responsible for the loss of the American colonies, requires special attention in connection with the subject before us, since it serves to account for the peculiar attitude of the colonial governors towards the self-governing aspirations of the loyalists and others who afterwards settled in the western districts.

The more intelligent loyalists and early settlers, while refusing, from circumstances or conviction, to break with the Home Government, were not by any means prepared to endorse its views with reference to the entire subordination of the colonies to the wishes of the mother country. No sooner, therefore, were the loyalists settled in the newer districts of Canada, than the Government began to be bombarded with their claims for British rights and British institutions.[2]

Haldimand, Dorchester and Simcoe, the governors who first had to deal with them, all complain of the independent spirit which they manifested. While it cannot be denied that the governors had much cause for resentment at the turbulent arrogance of many of the loyalist troops and some of their officers, when disbanded, yet they showed no little suspicion as to the real loyalty of the best of them.[3] Both Haldimand and Dorchester strongly favoured the retention of the whole colony, including the loyalist settlements, under the French-Canadian laws and institutions established by the Quebec Act. They professed to fear another revolution as the natural and inevitable consequence of granting them British freedom ; and in this they were probably not far astray, assuming that their views of what a colony should be were correct. Dorchester's experience, however, at length compelled the reluctant admission that, owing to the temper of the people, together with the example before them of the late colonies now enjoying their British institutions in independence, it would be impossible to retain the loyalists and others under the French-Canadian system.[4]

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1 See the correspondence of Governors Carleton and Cramahe with the Home Government. Canadian Archives, Vols. Q. 5 to Q. 11. Also the Debates on the Quebec Act, from the notes of Sir Henry Cavendish, London, 1839.

2 Numerous references to this subject are scattered throughout the state papers of the period. See more particularly, Canadian Archives, Vols. Q. 24, 25, 27.

3 Among many official papers in the Canadian Archives bearing on this subject, see the collections of despatches relating to the settling of the loyalists, in the Haldimand papers, Vols. B. 63 and 64.

4 See various despatches in Canadian Archives, as in Vols. Q. 39 and 42.

He took part, therefore, in framing, or at least revising the Constitutional Act of 1791, which made possible the adoption of English laws and institutions in Upper Canada. But when we come to look into that act, we observe that the greater part of it is taken up with provisions for establishing an hereditary political aristocracy and an episcopal state church.

It was a firm conviction with the high Tory party of the time, that had the colonies been supplied with an hereditary landed aristocracy and a well-endowed state church, there never would have been any revolution in them.[1] Hence, great care was taken by the authors of the Constitutional Act, to make very special provision for these two bulwarks of monarchical rule.

Having thus briefly outlined the situation to be occupied by the early settlers of Western Canada, we are in a better position to understand the peculiar circumstances attending the attempts to introduce local or municipal government in Upper Canada.

It was at once natural and inevitable that those loyalists who had really exercised full citizenship in the colonies, should seek to reproduce here the various customs and institutions economic, religious, social and political, to which they had been accustomed in the colonies before the Revolution. While, therefore, the Government might look askance upon these institutions as the real cause of the Revolution, the loyalists, nevertheless, cherished them as the very essence of the British system as they had known it. To these things they had remained loyal, for these they had fought and suffered. We can imagine their feelings, therefore, on being told that all these laws and institutions were illegal, that to be good British subjects they must give up all that they had hitherto valued as the very essence of the British system, and adopt a French-Canadian system, which they had hitherto regarded as of all things the most alien. To accept the laws and institutions of a conqueror might indeed be hard, but must be expected as the natural sequel of conquest. But how little to be expected that those who had followed the British flag should lose more completely the essentials of British freedom than those who had remained in the revolted colonies? The supposition could hardly be taken seriously, it was too bad to be true.

Being for some time engaged in procuring the merest necessaries of life, and laying foundations for future property, interpretations of the

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1 See, among other literature of the period, Knox's Extra Official State Papers, Vol. II., pp. 21, 30, 33, etc.

civil law, and the services of a local administration were not much in request, and their want was not felt. Criminal law was more in demand for the punishment of breaches of the peace, and the criminal law was English. Only on one point did the first British-American settlers come into direct contact with French-Canadian institutions, and that was in the tenure of their lands. These were granted only on the French-Canadian feudal basis, with all the obligations and restrictions which that involved. This, therefore, was the burden of the first complaints which poured in upon the Government. A little later the settlers began to express a desire for those institutions of local government to which they had been accustomed, and which in some sections they were tentatively reproducing before there was any legal sanction for them.

What, then, was the nature of those municipal institutions which the people sought to introduce?

There were two quite distinct types of local government developed in the American colonies. Their differences chiefly depended upon the character and circumstances of the immigrants from Britain, who laid the colonial foundations.[1]

The New England colonists of different migrations were almost entirely drawn from the middle classes of the mother country, and especially from those districts in which the Puritanic spirit had been strongly developed. But Puritanism is simply a general expression for a type of mind characterized by independence of thought, and consequently of action, in the various departments of practical life, whether religious, social, economic or political. Where almost the whole of the social fabric was composed of such people, and especially in a new country, self-government in general, and local government in particular were inevitable.

The settlers of New England, being very largely of the same social class, and having few or no servants, maintained their individuality, and exercised with the freedom of a new country those local rights and privileges which they had introduced from England.

In the southern colonies, on the other hand, of which Virginia was the special type, the settlers were mainly of the middle and upper classes, with less of the Puritanic strain. Moreover, they soon obtained from Britain a large element of the lower class in the capacity of servants. These were much inferior to their masters in moral fibre,

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1 For an excellent summary of the municipal institutions of the American Colonies, see "Town and County Government in the English Colonies of North America, by Edward Channing, Ph.D., Johns Hopkins University Studies in History and Political Science, Second Series, Vol. X., 1884.

intelligence, enterprise and strength of character generally. Later the planters obtained negroes as servants, and the whites, gradually emancipated from service, yet remained for the most part in a backward condition as a permanent lower social order. Under these circumstances the superior white minority asserted and secured the right to rule in local as well as central affairs.

The other colonies exhibited various modifications or combinations of these two types, according to their social structure, or their contact with New England or Virginia.

The majority of the loyalists and other settlers in Upper Canada came from New York colony, with smaller proportions from the adjoining colonies of Pennsylvania, New Jersey and New England. In these regions the New England type of local government prevailed.

The local unit of the New England system was the old English parish or town, with officers who were at once civil and ecclesiastical, and who combined in themselves legislative, executive and judicial functions.

The New England parish or town, in its town meeting, had power to legislate on all matters of purely local interest, affecting the everyday life and comfort of the people ; and its legislation, expressed in by-laws, was executed by officers of its own. The town had certain duties with reference to the parish church, the relief of the poor, and the oversight of the public morals. These duties were performed by the wardens, usually termed church wardens, though they were really civil officers. The town clerk kept a record of the proceedings of the town meetings. The constables looked after the peace and protection of the town, and raised hue and cry in pursuit of offenders.

There were also overseers of the highways who attended to the maintenance of the roads and levied a labour tax for that purpose. Other officers were the assessor and collector of the taxes, fence viewers and pound keepers. There was also a body of select men, corresponding to our present town or township councillors, having a general oversight of town matters, with power to act in emergencies. All these officers were elected annually at the general town meeting.

The Court of Quarter Sessions, which was early established in the colonies, was mainly a court of justice in New England. Nevertheless, all regulations made by the select men of the town had to obtain the sanction of the Court of Quarter Sessions before being regarded as legal, The Quarter Sessions also had the duty of laying out all the highways in the county, though the several towns undertook their opening up and maintenance. From this body, too, licenses were to be obtained by the keepers of public houses. It also levied certain rates for the support of its own officers and functions, and apportioned these rates to the several townships. The executive officer of the Court, for the county, was the sheriff, who was appointed by the governor. For militia purposes the colony was divided into shires, for each of which a lieutenant was appointed, whose duty it was to call out the militia.

In Virginia the shire, or county, was the central unit of local government, presided over by a lieutenant, corresponding to the Lord Lieutenant of an English county, and appointed by the governor-in-council. There was also a sheriff, with sergeants and bailiffs as required. Then there were the county courts, composed of justices of the peace, corresponding to the Courts of Quarter Sessions of New England, but with much more extensive municipal authority. They exercised most of the powers allotted in New England to the town meeting and the select men. Thus the county court, which met monthly, had power to erect and keep in repair the court house and jail, it had sole charge of the highways and bridges, and let contracts for their construction ; it divided the county into walks or precincts, over which surveyors were appointed, corresponding to our pathmasters, who called upon the people for their quotas of labour. The court also appointed constables, and as in New England, licensed inn-keepers. The poor were looked after in Virginia by the Church, through its vestry and church wardens, the latter being two in number, appointed by the vestry from among themselves. In this connection it is interesting to note that New York, and after it Upper Canada, combined both systems, the town meeting electing one church warden, and the parson appointing the other. Inasmuch as the justices of the peace were appointed by the governor-in-council, the local administration of Virginia was in no way dependent upon the will of the people in general, whereas, in New England, local government was very directly dependent upon the people. In Upper Canada, as we shall see, the people sought to obtain the New England system, while the governor and council endeavoured to fasten upon them the Virginia system.

Having thus briefly summed up the two typical forms of local government in the American colonies before the Revolution, we are in a position to understand whence came much of the peculiar combination which was afterwards found in Upper Canada.

In New York colony the population was made up of very heterogeneous elements, as regards nationality and creed, hence the ecclesiastical features of the New England system are wanting. On the other hand, the powers of the Court of Quarter Sessions were more extensive than in New England, though much less so than in Virginia. While, therefore, in most parts of New York the town meeting was a very important institution, yet it had a narrower field of operation, being encroached upon in this respect by the Court of Quarter Sessions. It is this modification of the New England system which we should naturally expect to find reproduced in Canada.

After the Quebec Act, which uprooted all previously planted British institutions, and the American invasion, which prevented the operation of almost any civil government, the governor and council once more set to work to build up a system of courts and local administration, in accordance with the re-established French-Canadian laws. Little progress, however, was made in the latter field before the arrival of the loyalists in 1785. Throughout the war, a steady stream of refugees sought the protection and aid of the British Government in Canada. The first regular body of loyalists, strictly so called, was brought in and settled under military leadership. Governor Haldimand had expected to superintend their settlement himself, but being engaged in other quarters, he assigned the task to Sir John Johnson in May, 1784. In a couple of private letters to Johnson, he stated that he intended to recommend him for the position of Lieutenant-Governor and Commander of the Western District, and Superintendent General of all the refugee loyalists to be settled there.[1] Though this plan was not realized, yet in July, 1784, Johnson was appointed to superintend the settling of the loyalists and Indians in the new district.

In order that the leaders might have adequate authority to deal with such legal matters as were connected with the settlement and the keeping of the peace, magistrates' commissions were given to Sir John Johnson, Maj. De Lancey, Maj. Holland, Maj. Ross, Maj. Jessup, and Mr. Collins, who were thus constituted the first justices of the peace for the new settlements.[2]

As already stated, these settlements were to be established under French-Canadian law, and the lands granted under the French feudal tenure. The dividing of the district into townships had nothing to do with legal administration or local government, but was entirely a matter of convenience in surveying the territory and recording the lots of land.

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1 Canadian Archives, B. 65, pp. 22 and 29.

2 Canadian Archives, B. 65, p. 28.

Express instructions were issued that the townships should not be named, but merely numbered ; they were not even to be referred to as townships, but as Royal Seigneuries.[1] Thus did the government seek in advance to head off the distrusted town meeting.

In dealing with the malcontents among the loyalists, Haldimand, writing on August 20th, 1784, recommends to Major Ross, then in command at Cataraqui, to employ the civil power as far as possible, and adds that he will send up commissions of the peace for Major Van Alstine and Captain Sherwood, which he believes, in addition to those already sent, will make a sufficient number.[2]

As yet these justices were merely peace officers, there were still no Courts of Quarter Sessions. In all matters not permitted to be disposed of in a summary manner by one or more magistrates, recourse must be had to the courts at Montreal. But in the following year, 1785, an ordinance was passed "for granting a limited civil power and jurisdiction to His Majesty's Justices of the Peace in the remote parts of this Province."[3]

Meanwhile, the magistrates and chief men of the settlements, headed by Sir John Johnson, began to send in those petitions already referred to, for relief from the French-Canadian system, and for more extended local administration. In their petition of April 11th, 1785, sent directly to the King in London, they submitted a plan for the government of the new settlements. In brief, it provides for the forming of the territory, from Point Beaudet westward, into a district distinct from the Province of Quebec. It was to be placed under the direction of a lieutenant-governor and council, subordinate, however, to the governor and council in Quebec. This district, having Cataraqui as its metropolis, was to be subdivided into smaller districts or counties, with courts of justice appropriate to each. The petition expatiates at length upon the advantages of such an arrangement, and upon the hardships of the present situation. Those who signed this petition, ten in number, were all officers who had served in the late revolutionary war.[4]

The following year, the magistrates at Cataraqui and at New Oswegatchie (Prescott), being requested to do so, sent their views as to the needs of the Western settlements, in a memorial addressed to Sir John Johnson, the superintendent of the district. In these memorials, in addition to the usual prayer for deliverance from the French-Canadian

1 Canadian Archives, B, 65, p. 34.

2 Canadian Archives, B. 64, p. 182.

3 Laws of Lower Canada, Vol. 1, p. 103.

4 Canadian Archives, Q. 24-1, pp. 76-84.

system, they show a growing anxiety with reference to local government, education and facilities for trade. In the Cataraqui memorial, after referring to the need for local courts of justice aud increased powers for the magistrates, they continue, "The election, or appointment, of proper officers in the several townships, to see that the necessary roads be opened and kept in proper repair, we conceive would be of great utility, by facilitating the communication with all parts of the settlement. Humanity will not allow us to omit mentioning the necessity of appointing overseers of the poor, or the making of some kind of provision for persons of that description, who from age or accident may be rendered helpless. And we conceive, it would be proper that the persons appointed to this charge, as well as the road masters, should be directed to make regular reports of the state of their districts to the courts at their meetings, and be in all cases subject to their control."[1] Here, we observe that the magistrates, naturally favouring the conservation of their own power, lean to the side of the Virginia system, in which the Court of Quarter Sessions, and not the town meeting, should be the centre of local administration.

The New Oswegatchie memorial, though briefer, is to the same effect. It prays that the new settlements may be formed into separate counties or districts, from Point Beaudet upwards, each having its own courts, judges and civil officers.[2]

These memorials and many others affecting the whole judicial and local administration of the Province of Quebec, were referred to a committee of the Council, composed of one English and two French-Canadian members. Their report is very exhaustive and very interesting, but only certain portions of it bear directly on the question in hand. It brings out, however, the utter inadequacy of such local administration as existed under the French-Canadian system, for the regulation of the loyalist settlements, where the quasi-civil machinery of the French Canadian Church was entirely wanting. Mr. Finlay, the English member of the committee, strongly supported the claims of the loyalist settlements to be erected into separate districts, and recommended an ordinance to be passed authorizing this division. But the two French-Canadians on the committee strongly opposed any weakening of the French-Canadian system, and supported their views with most interesting and subtle argument based on legal, social, political and international grounds.[3]

1 Canadian Archives, Q. 27-2, pp. 510-518.

2 Canadian Archives, Q. 27-2, pp. 519-520.

3 Canadian Archives, Q. 27-1, pp. 199-206.

However, the outcome of the matter was the ordinance of April, 1787, making further provision for the administration of the new settlements.[1] The most important section bearing on our present inquiry is the following, "Whereas, there are many thousands of loyalists and others settled in the upper countries above Montreal, and in the bays of Gaspe and Chaleurs below Quebec, whose ease and convenience may require that additional districts should be erected as soon as circumstances will permit, it is enacted and ordained by the authority aforesaid, that it may be lawful for the Governor or Commander-in-chief for the time being, with the advice and consent of the Council, to form by patent under the seal of the province, one or more new districts, as his discretion may direct, and to give commission to such officer or officers therein as may be necessary, or conducive to the ease and convenience of His Majesty's subjects residing in the remote parts of the province." In accordance with the authority granted in this ordinance Lord Dorchester issued a proclamation, dated July 24th, 1788, dividing the western settlements into four districts, named Lunenburg, Mecklenburg, Nassau and Hesse.[2] On the same day appointments were made to the following offices in each of the new districts: judges of the Court of Common Pleas, justices of the peace, sheriff, clerk of the Court of Common Pleas, and of the Sessions of the Peace, and coroners.[3]

Courts of Quarter Sessions were thus organized, and began their sittings the following year. The first court for the district of Mecklenburg was held at Kingston on April 14th, 1789,[4] and the first court for the district of Lunenburg was held at Osnabruck, on June 15th, in the same year.[5]

Except as regards the criminal law, the justices were still required to administer the French system in accordance with the Quebec Act. But as this immediately led to difficulties, the justices of the district of Mecklenburg submitted certain problems to the Government at Quebec. For instance, proclamations to be legal were required to be made at the church doors of the parish, and to be published in the *Quebec Gazette*. But in the whole of the western settlements there were only two church doors, and no one was known to take the *Quebec Gazette*. The justices, therefore, made a characteristic suggestion, namely, that as most of the settlers had to go to one or other of the two grist mills of the district, at

1 Laws of Lower Canada, Vol. I., p. 121.

2 Canadian Archives, Q. 37, p. 178.

3 Canadian Archives, Q. 39, pp. 134-139.

4 Early Records of Ontario, *Queen's Quarterly*, Vol. VII., p. 55.

5 Lunenburgh, or the Old Eastern District, by J. F. Pringle, Cornwall, 1890. p. 47.

Kingston and Napanee, the proclamation should be posted there, a suggestion which was accepted. Again, having no officers corresponding to the French notaries, mortgages and other documents requiring registration could not be registered. Further, under the French system the public highways were under the direction of the officers of the militia, subject to the supervision of the grand voyer. But this arrangement could not be carried out in the English districts. The granting of licenses to keep taverns was in the hands of the Secretary of the Province or his agent, and could be arranged only in Montreal. And finally it is prayed that if Government will not grant them any relief from the French system, then, inasmuch as they are entirely ignorant of the requirements of that system, the Government may send them full instructions as to the laws and how they are to be enforced.[1] But by this time a change in the constitution had been recognized as inevitable and was then being prepared, hence no action was taken on this memorial. In default of instructions the justices in civil matters simply followed the laws and customs which they had known, and decided cases on the good old English principle of equity and good conscience.

The duties of the Court of Quarter Sessions, as interpreted, were partly judicial, as in connection with the maintenance of the peace, partly legislative, as in prescribing what animals should not run at large, or what conditions should be observed by those who held tavern licenses, and partly administrative, as in appointing certain officials, and in laying out and superintending the highways.[2] We find, for instance, that before 1789 the magistrates had appointed church wardens for the township of Fredericksburg, and doubtless for several others, and that these church wardens were exercising their powers as if they were living in an English colony under English laws.[3]

It is noteworthy that most of the civil or municipal administration undertaken by the justices of the peace was based upon the old English law and custom as it was in the days of Queen Elizabeth and the Stuarts, and not as subsequently modified in Britain.[4]

The first loyalist settlers were chiefly military men, many of them not having been actual settlers in the colonies, and some of them being German auxiliaries. They came to Canada under command of their officers, who, as we have seen, were appointed the first magistrates of the districts. As might be expected, most of these settlers did not take

1 Canadian Archives, Q. 43, 1 and 2, pp. 404-415.

2 Early Records of Ontario, *Queen's Quarterly*, Vol. VII.

3 Early Records of Ontario, *Queen's Quarterly*, Vol. VII, p. 58.

4 Early Records of Ontario, *Queen's Quarterly*, Vol. VII., pp. 58, 243, 327.

a very strong interest in introducing and maintaining the self-governing institutions of the former colonies. Yet some of the first and nearly all of the later arrivals, being largely farmers and civilians, such as those settled in Fredericksburg, Adolphustown and the Prince Edward peninsula, at once attempted to reproduce in Canada their familiar institutions. Thus, while in the townships in the immediate neighbourhood of Kingston, there appears to have been little anxiety with reference to town meetings, yet in the townships named, town meetings were established before there was any legal warrant for them, as, for instance, the record of Adolphustown will show.[1]

But we must turn now to that change in the fortune of the western settlements which came with the passing of the Constitutional Act of 1791. By it the western districts were formed into an independent province, with a representative assembly and an opportunity to introduce English laws and institutions.

To preside over the formative period of this new Government, General John Graves Simcoe arrived in Upper Canada. Simcoe was a man whose life had been spent in the profession of arms. He was, from all accounts, a most efficient officer, saturated with the military spirit. A man of simple, straightforward ideas, devoted to military methods, when in authority he was accustomed to give his commands to go and come and find them obeyed without question. Almost incapable, by temper and experience, of recognizing any other form of administration, he sought to organize his Government as nearly as possible on a military basis. Self-government by the people at large he fervently and frankly abhorred. Aristocratic military and ecclesiastical rule he considered to be the only possible form of stable government for a decent and respectful people and a well-meaning ruler. As governor of Upper Canada he felt that the whole responsibility for the successful administration of the colony rested upon his shoulders. His sense of responsibility, however, was felt not towards the colonists, but towards the Home Government, hence his extreme unwillingness to share with the colonists the administration of the country which they occupied. Canada did not belong to the colonists, but to Great Britain; the governor was not appointed by the colonists, or in any way responsible to them. He was sent out to administer a British colony in the interests and for the glory of the country which sent him. True, those interests and that glory were to be expressed in a happy and prosperous condition of the colony, but the proper methods and means for accom-

1 Early Municipal Records of the Midland District, in Appendix to the Report of the Ontario Bureau of Industries, 1897.

plishing that result were not matters upon which the colonists could be expected to have sound ideas, and a little experience of them proved to Simcoe's own satisfaction that his conviction was well grounded. Only men of military training were fit to be trusted to carry out with loyalty and discretion the commands of their superiors. Hence, while still in London,[1] Simcoe surrounded himself with a band of military men, chiefly fellow officers in the late American war, and took them with him as his Executive Council, and afterwards as the chief members of his Legislative Council. The minor officials he expected to select in the colony from among the officers already settled there. He had also arranged to have the Assembly composed of military men, trusting that the loyalists would, under his direction, aided by the influence of Sir John Johnson, select as their representatives the half-pay officers in the Province. Here, however, he came upon his first disappointment. Writing from Navy Hall to the Colonial Secretary, on November 4th, 1792,[2] he states that in his passage from Montreal to Kingston, while the first election was in progress, he discovered that the general spirit of the country was against the election of half-pay officers, but that, to use his own words, "the prejudice ran in favour of men of the lower order who keep but one table, that is, who dine in common with their own servants." Only by stopping over at Kingston, and specially exerting his personal influence, did he manage to bring in his attorney-general, Mr. White. If such was the attitude of men but lately disbanded from the ranks in which they had fought against the advocates of self-government, what might be expected from later arrivals who were merely loyalist in name? No wonder that Simcoe should gravely attempt to put into practice a scheme for maintaining a number of military companies scattered over the colony, into which he intended to recruit crude republicans from the neighbouring states, and there, on soldier's pay, by salutary drilling, useful manual labour, and friendly lectures on the evils of self-government, convert them into well affected British subjects, fit to be trusted with a bush farm in a back township.[3] No doubt the broth would have been well flavoured had he been able to catch his hare.

The settlers having preferred men of the lower order to Simcoe's half-pay officers, we are prepared to find some assertions of popular claims which did not meet with the approval of the governor. We come upon one such at the very threshold of the new legislation. The

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1 His plans for the government of Upper Canada are detailed in his letters to Dundas, written in London. See, for instance, letters of June 2nd and August 12th, 1791, Canadian Archives, Q. 278, pp. 228-255, and 283-307.

2 Canadian Archives, Q. 279-1, p. 79.

3 Canadian Archives, Q. 278, p. 287.

first bill introduced into the Assembly of Upper Canada, in the first session of the first parliament, September 19th, 1792, was a bill, "to authorize town meetings for the purpose of appointing divers parish officers." But, after passing its second reading, it was ordered that the further consideration of the bill be postponed for three months. On the same day another bill was introduced to authorize "the justices of the peace to appoint annually divers public officers." This, again, was followed by a bill to authorize "the election of divers public officers." None of these, however, managed to get through the House.

In these proposals we observe the conflict of the two rival American systems typified by New England and Virginia, the one seeking to vest in the people the election of their local officers and the regulation of their local affairs, the other seeking to confine these rights to the justices of the peace in Quarter Sessions, who again derived their positions from the Governor-in-Council.

Simcoe, in his report on the session to the Home Government says that the lower House "seemed to have a stronger attachment to the elective principle in all town affairs than might be thought advisable."[2] The following session the bill with reference to town meeting was once more introduced and passed, but with such modifications as made it quite harmless as a measure of local self-government. Writing to Colonial Secretary Dundas, in September, 1793,[3] Simcoe says that he managed to put off the bill of last session on town meetings as something that should not be encouraged. But as regards the opposite measure proposed, he says that "to give the nomination altogether to the magistrates was found to be a distasteful measure." Many well affected settlers were convinced that fence viewers, pound keepers and other petty officers to regulate matters of local police would be more willingly obeyed if elected by the householders, and especially that the collector of the taxes should be a person chosen by themselves. "It was therefore thought advisable not to withhold such a gratification to which they had been accustomed, it being in itself not unreasonable, and only to take place one day in the year." When we turn to this act[4] we find that it merely permits the ratepayers to elect certain executive town officers, whose duties were either prescribed by the act, or left to be regulated by the justices in Quarter Sessions. Beyond the permission to fix the height of fences, the town meeting had not legally any

1 See Journals and Proceedings of the House of Assembly of the Province of Upper Canada, 1792, Canadian Archives, Q. 279-1, pp. 87 *et seq.*

2 Canadian Archives, Q. 279-1, p. 83.

3 Canadian Archives, Q. 279-2, pp. 335 *et seq.*

4 33rd Geo III., cap. 2.

legislative function. The town officers were independent of each other, and responsible, not to those who elected them, but to the magistrates. By an act passed the following year[1] a slight additional legislative power was given to the town meetings, permitting them to fix the limits of times and seasons for certain animals running at large, but even this power was afterwards curtailed. This first act, therefore, while authorizing town meetings, effectively strangled all interest in them, except where, as in Adolphus and neighbouring townships, the limitations of the act were to a certain extent disregarded. For years to come the Court of Quarter Sessions remained the only living centre of municipal affairs.

Recognizing the democratic tendencies of the people, Simcoe reports to the home Government that, "in order to promote an aristocracy, most necessary in this country, I have appointed Lieutenants to the populous counties, which I mean to extend from time to time, and have given to them the recommendatory power for the militia and magistrates, as is usual in England."[2] He selected them as far as possible from the Legislative Council.

With the same object in view he proposed to erect the towns of Kingston and Niagara into cities, each with a corporation consisting of a mayor and six aldermen, to be justices of the peace, and a suitable number of common councillors. This was a standard arrangement in Britain, as it was afterwards in the first chartered cities in Upper Canada. But the members of Simcoe's corporations were advised "to be originally appointed by the Crown, and that the succession to vacant seats might be made in such manner as to render the election as little popular as possible, meaning such corporations to tend to the support of the aristocracy of the country."[3]

In 1795, the Duke of Portland, writing to Simcoe, discourages his projects for the incorporation of cities, and disapproves of his appointment of lieutenants of counties. He is afraid that the effect may be the very opposite of what Simcoe intended, that instead of strengthening the power of the central government, it may weaken it by scattering its functions, while it requires to be strong to check the influence of the popular assembly.[4]

What we find, then, as the result of the various influences brought together in Upper Canada is, that the Virginia type of local or municipal

1 34th Geo. III., cap. 8.

2 Canadian Archives, Q. 279-1, p. 85.

3 Canadian Archives, Q. 287-1, p. 164.

4 Canadian Archives, Q. 281-2, pp. 328 *et seq.*

government, and not that of New England, was practically brought into operation in this province. This was mainly through the influence of Governor Simcoe, aided by the justices of the peace already in the field, who naturally wished to enlarge their powers. Hence the municipal administration of the country centered in the Courts of Quarter Sessions, whose members being appointed by the Governor-in-Council were responsible to the Executive alone.

The various acts passed for local administration simply enlarged the powers of those courts to deal with municipal matters. In course of time certain towns obtained special charters and with them a measure of local self-government varying in range from town to town. But it was only after the struggle for responsible government had resulted successfully, that representative municipal institutions, such as we now know them, were introduced and applied to the whole Province.

SAWDUST AND FISH LIFE.

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(*Read 14th February, 1903.*)

CONTENTS.

NOTE.—The trees mentioned in the following report are : White Pine (Pinus Strobus, L.), Red Pine (Pinus Resinosus, Ait.), British Columbia Cedar (Thuya Gigantea, Nuttall), Ontario Cedar (Thuya Occidentalis, Linn.), Hemlock (Thuya Canadensis, Carr.), Maple (Acer Saccharinum, Wang.), Elm (Ulmus Americana Linn.), Ash (Fraxinus Sambucifolia, Lam.), Oak (Quercus Rubra, Linn), Spruce (Picea Alba, Linn).

PART I.—HISTORICAL.

THE following is a continuation of my preliminary report upon the effects of polluted waters on fish life. The work was first begun at the Dominion Biological Station, St. Andrews, N.B., in 1900, and has been continued since then at the biological laboratory of Queen's University, Kingston, and along the saw dust beds of the Bonnechere River in the county of Renfrew, Ontario.

The investigation was begun at the suggestion of Professor Prince, the fish commissioner for the Dominion of Canada, and has been carried on largely through the encouragement which he has given from season to season.

The question, "Is sawdust injurious to fish life?" has been before the Canadian public for over forty years. The *Fishery Act* of 1858 for the two Canadas provided that fish ways should be erected upon dams that obstructed the passage of anadromous fish to their spawning grounds in the shallow head waters of rivers; and it forbade also throwing lime, chemicals, and other poisonous material into such rivers. It did not mention sawdust or mill rubbish, but it provided for the making of regulations by the executive, and in the exercise of this power we find that on May 16th, 1860, a by-law was passed making it illegal to throw "slabs, edgings, and mill rubbish into any river or stream which may have been leased or reserved by the Crown for propagation, or where fish ways have been erected."

This by-law was embodied in the amended Act of 1865, the clause relating to sawdust reading as follows:—

"Lime, chemical substances, or drugs, poisonous matter (liquid or solid), dead or decaying fish, or any other deleterious substance shall not be thrown into, or allowed to pass into, be left, or remain in any water frequented by any of the kinds of fish mentioned in this Act, and sawdust and mill rubbish shall not be drifted or thrown into any stream frequented by salmon, trout, pickerel, or bass under a penalty not exceeding a hundred dollars."

Immediately after confederation the Act was further amended, and a very important proviso was attached to the foregoing clause, viz.:—

"Provided always that the Minister shall have power to exempt from the operation of this sub-section, wholly, or from any portion of the same, any stream or streams in which he considers that its enforcement is not requisite for the public interests."

Evidently the promoters of this legislation either did not feel sure that sawdust was poisonous, or they thought it just, in the interests of the lumber industry, to exempt from the operations of the Act certain large rivers in the maritime provinces, Quebec and Ontario. Exemptions were continued by the minister from year to year down to 1894, when they ceased by Act of Parliament. Parliament itself, however, extended these exemptions down to 1899.

In 1873 an Act was passed making it illegal to throw mill refuse into navigable rivers, on the ground that in some parts of the Dominion rivers once navigable had ceased to be so on account of the accumulation of mill rubbish. The Otonabee River in Ontario, and the La Have in N.S., were two rivers which were obstructed in this way.

Most of the Eastern United States have legislated against throwing sawdust into streams containing protected fish; but so far as I have been able to discover, the promoters of the legislation have never been able to prove conclusively the poisonous action of sawdust. At any rate, the scientists of the United States Fish Commission have not been unanimous in their opinions regarding the matter.

For example, in the Fish Commissioner's report for 1872-3, part i., "Inquiry into the Decrease of Food Fishes," Mr. Milner, one of the investigators, says (page 49): "In a number of rivers entering into Green Bay, the white fish was formerly taken in abundance in the spawning season. Saw mills are numerous on all these streams at the present day, and the great quantity of sawdust in the streams is offensive to the fish, and has caused them to abandon them. In one or two rivers of the north shore (Michigan) they are still found in autumn."

In this same report another scientist, Mr. Atkins, referring to the Penobscot River, says (page 303): "The extensive deposits (of sawdust) have in some instances so altered the configuration of the bottom as to interfere with the success of certain *fishing stations;* but beyond that I see no evidence that the discharge of the mill refuse into the river has had any injurious effect on the salmon. It does not appear to deter them from ascending, and being thrown in below all the spawning grounds it cannot affect the latter."

In the Fish Commissioner's report for 1872-3 and 1873-4, vol. I., we meet with another confident statement, but no proof. Mr. Watson, in an article on "The Salmon of Lake Champlain and its Tributaries" (page 536), says: "The sawdust stained and polluted the water, and the sediment and debris of the mills settled largely on the gravelly bottoms, which had been so alluring to the salmon, changed their character, and revolted the cleanly habits of the fish."

Four years after this the Commissioner inserts in his report (1878) a translation of an article by Professor Rasch, of Norway, on "The Propagation of Food Fishes": "That the rivers on which there is considerable cutting of timber gradually become more and more destitute of salmon is an undeniable fact; but while it is asserted that the sawdust introduced into the river from the saw mills causes the salmon coming from the sea either to forsake the foster stream because of meeting the sawdust, to seek another river not polluted, or else when the fish attempts to pass through the areas quite filled with sawdust then this by fixing itself in the gill openings, or between the gills causes its death, yet later experience seems to entitle us to the assumption that sawdust neither causes the salmon to forsake its native stream, nor produces any great mortality among the ascending fishes. The hurtfulness of the sawdust to the reproduction of the salmon is not so direct, but is exceedingly great in this, that it partly limits and partly destroys the spawning grounds of the river."

In his report for 1879, the Commissioner gives a translation from another Norse writer, W. Landmark, on "The Propagation of Food Fishes." This scientist mentions four objections to sawdust:—

1. "Sawdust gradually sinks to the bottom, and thus fills the very place where the fish eggs are to develop, with impure and injurious matter."

2. "When eggs are brought into contact with sawdust or any other rotting wooden matter for any length of time, the eggs are overgrown with a species of fungus, which invariably kills the germ."

3. "When the water rises and causes the masses of sawdust which have gathered in the river to move, a large number of young fish are carried away with it, and are gradually buried in the newly-formed piles of sawdust." In a foot-note he says: "It has been said that sawdust will drive the salmon entirely away from a river, but I think that

this is very improbable, and could only be possible in cases where a river has been completely filled with it."

4. "The refuse from the saw mills, in many places, interferes with the fisheries."

For the next eight years we find little or nothing in the reports of the United States Fish Commissioner regarding the ill-effects of sawdust. In an appendix to his report for 1887, entitled "Fisheries of the Great Lakes in 1885," we find the following expression of opinion from Hugh M. Smith and Merwin Marie Snell: "The fishermen appear to be considerably hampered in their operations by the presence of great quantities of drift wood and sawdust from the mills. At times this debris covers the lake (Michigan) for miles around, and very seriously interferes with the seining and netting. The most disastrous effects, however, are seen on the fish themselves, especially during the spawning season. Spawning grounds formerly existed in this vicinity, but they have been deserted for some years owing to the deposit of sawdust thereon."

On November 29th, 1888, there was started in *Forest and Stream* a very remarkable correspondence, which lasted nearly a year. The general topic was the effect of sawdust upon trout. The writers lived in Canada, the New England States, and some in the west as far as California. Both sides of the question were presented with great vigor. Most of the correspondents were evidently keen sportsmen and close observers of nature, and the only regret one feels in reading through these letters is that some of the men did not test their observations and conclusions by experimenting with sawdust. The following is a typical letter:—

A CENTURY OF SAWDUST.

Editor FOREST AND STREAM.

I was delighted with the intelligent way in which your correspondent "Piscator" handled the sawdust question in your issue of December 27th. It is a comfort to listen when a well-informed person speaks, but in these days of callow pretension experience is usually elbowed back from the front.

In my opinion the famous Mill Brook, of Plainfield, Mass., which has a record of a century as the finest trout water in the Hampshire hills, supplies those very conditions and corroborative data which "Piscator" declares are essential to determine what pernicious effect the presence of sawdust has upon the denizens of mill streams. Here is a water power which carried no less than thirteen manufactories fifty years ago. These included a tannery, a sawmill and factories for making brush and broom handles, whipstocks and cheese and butter boxes, all of which discharged, more or less, sawdust and shavings into the streams, to say nothing of three satinet factories and a felt hat factory, whose waste must have been deleterious to fish life.

Most of the buildings have since been destroyed by fire or tumbled into pieces by decay, but the old foundation, walls, and dams remain, and untold tons of tanbark and sawdust still cover the beds of the abandoned mill ponds knee deep, all of it in a perfect state of preservation, as I happen to know from wading the stream last summer. Nevertheless, the brook continues fairly stocked with small trout, despite the supplementary fact that it has been unmercifully fished ever since the memorial days of the "Mountain Miller," fifty fingerlings per rod being not unusual now for a days' catch. Besides, at no time within my recollection have there been less than three sawdust-producing mills on this stream at once, so that it may be asserted that its waters have not been normally clear for a century. Where the current is rapid and the water broken by ledges and boulders, the presence of the sawdust is scarcely perceptible, but at mill-tails, and in the basins above the dams, it accumulates in quantity and remains, becoming water soaked and sinking to the bottom.

Obviously, in localities where the entire bottom is imbedded by sawdust, fish can neither spawn nor feed ; but it happens that such deposits do not form on their breeding places, nor is the area of their foraging ground appreciably diminished by their presence. Even in the half-emptied and now useless ponds, the current constantly scours out a central channel through the sawdust, leaving the bottom clear and pebbly ; so that, in fact, these local beds are of no more detriment to the fish than so many submerged logs. The trout can range far and wide without encountering them at all. Yet, strange to say—that is, it must seem strange to those persons who take it for granted that sawdust kills fish—the most likely places for the larger trout are these self-same pebbly channels in the old ponds, along whose edges, despite a hundred freshets and ice-shoves, the persistent sawdust and tanbark lie in wind-rows so deep that the wader feels as if he were going to sink out of sight whenever he puts his foot into the yielding mass, every movement of which stirs up a broadening efflorescence which spreads for rods away, distributing itself throughout the stream.

From these sawdust beds I can always fish out three or four good trout with a cautious fly, and at certain times the surface is fairly dimpled with breaking fish, which presumably are after larvæ and insects which the sawdust has harboured, though careful investigation might discover other inducements for their congregating there.

In passing I would remark that this Mill Brook is fed by seven lateral brooklets, which tumble into it from the adjacent hillsides at intervals between dams, and are so effectually protected by overgrowth that they must always serve as prolific breeding places, secure from predatory birds and small boys, as well as places of refuge to trout which wish to escape the sawdust of the main stream. I have seen trout streams, especially in the pine barrens of Northern Wisconsin and Michigan, which were by no means as favoured as this Mill Brook, the current being comparatively sluggish, and not so capable of purging itself of sawdust ; yet I know ot few trout streams in any lumber region where its denizens cannot avoid the sawdust if they will, by withdrawing to the headquarters or lateral tributaries, provided fishways are supplied to enable them to surmount the dams where the accumulations chiefly occur. What I remark as most singular in the Mill Brook is, that the trout gather most where the sawdust is thickest, both on old mill sites and on sites where mills are running now. I take my best trout right from under the flume of a whipstock factory and sawmill, where the refuse is dumped as fast as it forms.

But I recall to mind a still more striking example of the innocuousness of sawdust. There are in Hampshire county, Massachusetts, a series of three large natural reservoirs, varying from half a mile to two miles in length, which for fifty years have abounded in pickerel, perch, eels, and bullheads.

It is said that they originally contained trout, but the water is dark and discolored by the drainage of spruce and cedar swamps. At the outlet of the lowest pond once stood a village called Hallockville, which operated a grist mill, sundry sawmills, and what was then the largest tannery in Massachusetts. It was burned in 1846 and never rebuilt, and the dams and foundation walls are now almost destroyed and buried by a new growth of forest. But the sluice and flood stream below are still clogged with the sawdust and tan bark deposited a half century ago, and the water is black and forbidding, though much broken into swirls and rapids by boulders and ledges. But for the colour of the water, it is a most likely-looking place for trout, though it has been tested time and time again without successful results. It has always been maintained, from the date of the building of the tannery, that there were no trout in it. I used to fish it myself when I was a boy. Last summer I took therefrom five small trout with a worm. They had doubtless worked their way up from the Buckland streams below, for they never came through the dam from the pickerel ponds above. Nevertheless, the lower streams are occupied by many sawmills, and carry their proportion of sawdust, that substance which some of your correspondents maintain is fatal to fish life. I leave your readers to draw their inferences, and trust that Mr. Fred. Mather will feel himself sustained by this testimony of the streams. That gentleman is not apt to make mistakes. He is grey with the experience of years, and that is better than guess work.

WASHINGTON, *December 29th.* CHARLES HALLOCK.

In this same year (1889) a very remarkable report on this subject was sent to the Hon. C. H. Tupper, the Minister of Marine and Fisheries, Ottawa, by W. H. Rogers, late Inspector of Fisheries for Nova Scotia. The report did not appear among the State papers, and it was consequently published in Halifax under the title of "*The Suppressed Sawdust Report.*" No one can read this pamphlet without being staggered with the mass of information which is supplied to prove the harmlessness of sawdust, and the marvel is that the Minister did not order a thorough investigation to be made into the whole subject.

Of course, diametrically opposite views were expressed by other fishery officers, in whose judgment, no doubt, the Minister had perfect confidence. For example, Mr. S. Wilmot, the Superintendent of the Dominion Fish Hatcheries, wrote a very vigorous report denouncing the deadly effects of sawdust, and his opinions were certainly entitled to some weight. But there was this marked difference between the reports of the two officers : Mr. Rogers' was bristling with facts and observations based evidently upon first hand knowledge of the subject, whereas Mr. Wilmots' report showed no close acquaintance with it.

Turning again to the reports of the United States Fish Commissioner, we do not find any further reference to sawdust until 1892, when Mr. Hugh M. Smith again reports upon "The fisheries of the Great Lakes." At page 404 he says :—"At first white fish and trout were both abundant. . . . Since 1881 or 1882 they have been comparatively scarce. . . . The gill-net fishermen lay the blame on the small meshed pound-nets. The pound-net fishermen, on the other hand threw the responsibility on the saw mills and the gill-net men. The saw mills, they say, pollute the waters with sawdust and vegetable refuse, and the gill-net men lose a great many nets, which with the fish in them soon decay and become a putrid mass, which contaminates the fishing grounds, and causes the fish to leave for other places."

Comparing this with his report for 1887 it will be seen that Mr. Smith refrains from asserting any ill effects from sawdust, and places the responsibility for such statements upon the fishermen. A similar remark applies to the International Fish Commissioner's report for 1893, and to the report of Mr. Richard Rathbun in 1899 on the "Fisheries in the Contiguous Waters of the State of Washington and British Columbia." "Attention," he says, "has been especially called to the Skagit river, on whose banks there are numerous shingle mills, from which a very large amount of refuse is allowed to enter the water. According to the statements of the fishermen in that region this practice has caused a great deal of damage to the spawning grounds of the salmon and has affected the fishery in other ways."

Coming to 1899 we find a very important report from the Dominion Fish Commissioner, Professor Prince, and one from the Deputy Commissioner for the Province of Ontario, Mr. Bastedo. Both reports command attention from the fact that they take opposite sides upon the sawdust question. Professor Prince says: "So far as our present knowledge goes, sawdust pollution, if it does not affect the upper waters, the shallow spawning and hatching grounds, appears to do little harm to the adult fish in their passage up from the sea." . . . "There is no case on record of salmon, or shad, or any other healthy adult fish being found choked with sawdust or in any way fatally injured by the floating particles."

Again, in summing up his conclusions upon all forms of pollutions: "In the first place it is evident that circumstances modify the effects of all forms of pollutions, so that waste matters which would be deadly in one river will pass away and prove of little harm in another, where the conditions are different. In the second place it shows how varied are the effects of various waste products under the same conditions upon different species of fish. Salmon will survive unharmed where shad and gasperaux would be killed off. Further, these notes indicate how little is actually known of the effects upon fish life of these various pollutions from accurate and thoroughly scientific experiments."

Contrast with this Mr. Bastedo's opinion as published in his report for the same year: "There can be nothing more destructive of fish life than the depositing of sawdust in the rivers and lakes. It is said to absolutely kill all vegetation, and it is well known that in waters where there is no vegetation fish life is noticeably absent. Minute crustacea of various kinds feed upon the juices of the plants which are to be found at the bottom. These afford food for the smaller fish, and again these furnish food for others of larger size."

Such was the state of our knowledge in 1900, when at the suggestion of Professor Prince, I undertook some experiments at St. Andrews, N.B., for the purpose of ascertaining whether or not sawdust was injurious to fish life.

PART II.—EXPERIMENTAL.

The results of these experiments were published in the report of the Minister of Marine and Fisheries, Ottawa, in 1901, and went to show that brook trout were not injured by living for two weeks in a water tank largely filled with sawdust, so long as a copious supply of water was allowed to run into and out of the tank. These results were abundantly corroborated this summer (1902) in a series of experiments carried on for several weeks in the biological laboratory of Queen's University, Kingston. Perch, rock bass and black bass fry were all used. In fact, the tests this season were, if anything, more exacting than they were in 1900. The volume of pine and of cedar sawdust used was 20 per cent. of the whole volume of the tank, and both adult fish and black bass fry (these latter only about six weeks old and an inch long) were kept for four or five days in the mixture, without any apparent injury.

When, however, sawdust was allowed to lie in still water, or in very slowly running water, entirely different results were obtained. Then, the most disastrous effects followed the immersion of different animals in the poisonous mixture. Not merely did adult fish die in it, but fish eggs, fry, aquatic worms, small arthropods, animalcules and water plants. Nor was the cause of death due to suffocation from lack of oxygen, because when air was made to bubble rapidly through the solution the final results were the same, the only difference being that death was somewhat delayed. No one could paint too vividly the deadly effects of strong solutions of pine or cedar sawdust when soaked in standing water. Adult fish died in two or three minutes; fish eggs in a few hours; fry and minnows in from ten to fifteen minutes; aquatic worms and insects, eight to twenty-four hours; aquatic plants, a few days. Every living thing died in it, and if one were to judge of its effects by laboratory experiments alone, then the prohibitory legislation needs no better defence.

Without anticipating further the results of these experiments, I shall proceed to describe them, so that the reader may be in a position to draw his own conclusions, if he differs from mine.

THE SINKING OF SAWDUST.

As regards the sinking of sawdust, the following experiment was typical of a large number which were carried out, in order to determine how much and how quickly sawdust sank after being thrown into the water at the tail end of a mill.

A litre measure was filled up to 900 c.c. with tap water, and then 100 c.c. of moderately packed pine sawdust was poured upon the water. The moment the sawdust touched the surface, particles began falling to the bottom, and continued to fall for nearly twenty minutes. During this time the water had penetrated 100 c.c. of the floating sawdust, and this volume of it began to sink very slowly *en masse*. Figure 1 represents the conditions in the experiment at the end of the 20 minutes. No less than 70 c.c. of the sawdust lay at the bottom; 100 c.c. were between the 700 and 800 marks, and about 20 c.c. only were floating. The 100 c.c. of sawdust at the beginning of the experiment had swollen to nearly 200 c.c. On giving the vessel a slight tap, the 100 c.c. of water-logged sawdust, lying between the 700 and 800 c.c. marks, suddenly upset and most of it sank to the bottom. The large particles, however, rose again to the top, so that in less than three minutes more, only 30 c.c. were floating, and the rest, swollen to 170 c.c., were lying at the bottom.

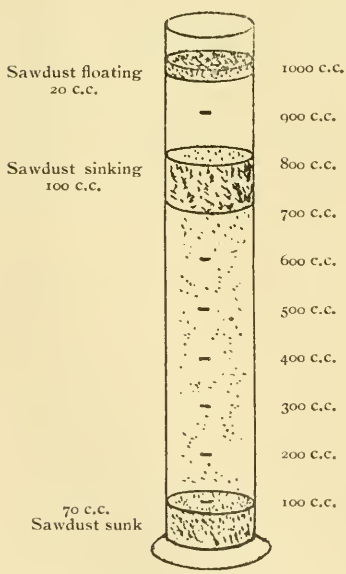


FIG. 1.

Litre measure at end of 20 minutes.

The following conclusions are based upon the results of many similar experiments: From 50 per cent. to 80 per cent. of white pine sawdust sinks in standing water, in from two to three minutes. The variations in quantity and time depend upon, (1) the size of the particles (2) upon the manner in which they are made, (3) upon whether the water is perfectly still or agitated, and (4) upon whether the particles are dry or moist.

Large particles sink much more slowly than small ones, because the latter are more easily penetrated through and through by the water.

Dust made with a hand-saw sinks more slowly than sawdust made with a large mill saw. The difference seems to be due to the difference in the force with which each is made. A large upright or circular lumber saw strikes the log with great force, squeezes out the imprisoned air from the wood fibres, renders them denser, and as a consequence they sink more quickly than particles of a similar or smaller kind which have been made by a hand-saw.

When water is slightly agitated, sawdust thrown upon it sinks more quickly than when the water is perfectly still. Consequently, in the swells of a steamer, in the waves made by wind, and in the ripple of a slight rapids, all the sawdust excepting the largest particles would sink to the bottom in a few minutes.

If thrown into a rapidly flowing stream, sawdust is carried downwards until it reaches comparatively still water, and then the finer particles sink ; the coarser may be carried for miles and miles down a river and out into the bays of a lake or sea.

In laboratory experiments the coarser particles would float for days, because the water is unable to penetrate the fibre and displace the imprisoned air, which gives to wood its buoyancy. Wood fibre is, of course, heavier than water, and therefore sinks ; and pine logs would sink much more quickly than they do only that the water cannot penetrate their interstices and drive out the air. Yet they do sink in considerable numbers, as every lumberman knows.

Hardwood logs cannot be floated to market at all, because the water of the cell-sap permeates them, rendering them heavier than water and they sink. A very simple experiment illustrates how pine logs sink after being in the water some time. Throw a piece of blackboard crayon into a dish of water. At first it floats, but soon bubbles of air escape from the chalk, and in a few moments it sinks to the bottom. So is it with sawdust and logs.

Sawdust from cedar takes a longer time to sink than that from pine. In fifteen minutes 66 per cent. only had sunk, probably because it contains more resin and consequently water-logs more slowly. Maple sawdust ranged half way between pine and cedar—66 per cent. sinking in eight minutes. Elm sawdust differed from pine, maple, or cedar in that only about 30 per cent. sank in twenty minutes; 75 per cent. of oak sawdust sank in six minutes. So that as far as my experiments went the different kinds ranged as follows: oak sank most quickly, then white pine, maple, cedar, elm. But it must be remembered that the particles in my experiments differed from each other in size and in the moisture they contained, and consequently different results might easily be obtained. The important point is that all kinds sink in a few minutes, especially in agitated water, but not, of course, in a stream with anything like a rapid current.

EXTRACTS FROM SAWDUST.

The first experiments of the season were performed for the purpose of determining the effects of sawdust upon fish eggs. The St. Andrew's experiment had shown that adult trout were not injured by sawdust in rapidly running water; but two other points remained to be determined: (1) Whether sawdust killed fish eggs, and (2) whether it destroyed the food of young, or full grown fish.

Perch eggs were collected along the shallows of Collins Bay, just west of Kingston, and brought to the laboratory on May 12th. They were placed in a clean aquarium with a stream of tap water (from Lake Ontario) running into and out of the vessel. On the same day a bag made of bleached cheese cloth, and filled with a peck of white pine sawdust was placed in an aquarium, 40½in. x 15in. x 16½in. It was weighted with stones to keep it on the bottom. Water entered the aquarium very slowly, so that the conditions of the experiment approximated somewhat to those in the pools of a sluggish stream.

Next morning it was noted that as a result of the bag of sawdust being in the aquarium all night, the water had dissolved out a sufficient amount of material from the sawdust to turn the bottom layer of water a yellowish brown color. This layer measured 1¾in. in a total depth of 16½ inches. Above the yellowish brown layer, and separated from it by a well-defined surface, the water was as clear as that of Lake Ontario. Only about $\frac{4}{5}$ths of the bottom of the aquarium

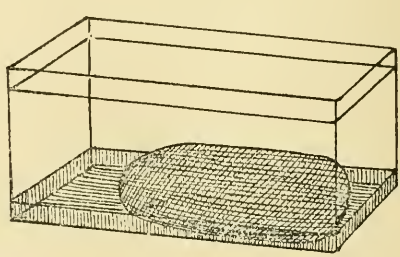


FIG. 2.

was covered by the bag; its upper surface stood about half an inch above the brownish liquid. These conditions are represented in figure 2. Four batches of eggs were placed in the aquarium at 10 a.m. of the 13th of May, viz.: two batches on the very bottom of the aquarium in the brownish water, and two on the surface of the bag of sawdust, well within the clear water.

Next morning at 9.00 a.m. every egg in the yellowish brown water was dead; and every egg in the clear water was alive.

Assuming that the brownish water was a saturated solution of material extracted from sawdust, two other solutions were made from it, —one of 25 per cent., and one of 50 per cent. strength, in tap water. Fresh batches of eggs were placed in each of them. In twenty-four hours the eggs in the 25 per cent. solution were all alive; half of those in the 50 per cent. solution were dead. In twenty-four hours more some of the fry had hatched out, but eggs and fry in both solutions were all dead.

In order to ascertain whether the death of both larvæ and fry was not due to lack of oxygen, rather than to poisonous extracts dissolved from the wood, air was made to bubble rapidly through some of the brown water. This experiment was begun at 12.30 p.m., and 800 c.c. of air per minute were passed through 230 c.c. of the discoloured water. At 5.30 p.m. of the same day, a batch of 60 eggs was placed in this aërated water, and air was passed continuously through it all night at the rate of 400 c.c. per minute. Next morning at 10 a.m. every egg in the batch was dead. The conclusion, therefore, is quite clear. The eggs were killed, not by lack of oxygen in the water, but by the poison contained in the water and evidently dissolved out of the sawdust.

The water had changed during the night to a much darker shade of brown. This marked change in colour will be discussed in a subsequent report.

SOURCE OF POISON.

The source of the poison given off by sawdust is undoubtedly to be found in the contents of the wood cells. Sugar, starch, oil, resin, gum, jelly, alkaloids, and acids are all examples of material stored in different parts of plants.

In the older parts of trees the protoplasm and sap disappear completely from the cells, and they may then contain nothing but the stored material. In the pine family there is stored in the wood and bark cells an abundance of crude turpentine and resin. The Norway spruce of Europe furnishes in this way turpentine and Burgundy pitch. The yellow pine of the Southern United States yields spirits of turpentine by distillation of the crude turpentine which runs away from the tree by cutting into it. The residue after the distillation is resin.

Now the poisonous material in sawdust must be either the cell wall or the stored material. It cannot be the cell wall, for this is just the wood fibre or material used in making paper, and pure paper is certainly not harmful to fish life. The poison can scarcely be anything else than the turpentine and other substances stored in the cells.

Different trees, such as tamarack, pine, cedar, spruce, etc., generate and store different kinds of reserve material. When a log from one of these trees is cut into boards, the sawdust gives off proportionately much more poisonous matter than the slabs, edgings and bark. The reason of this is easily understood. As each cell or vessel is microscopic, and contains only a very small quantity of poison, and as the cell wall must be broken open in order to let out the contents, it follows that the greater the number of cells that are opened, the greater will be the quantity of turpentine, resin, etc., poured out. Hence, a saw log converted into sawdust, or ground into shreds, as in a pulp mill, gives out the maximum of poison; whereas a similar log sawn into boards, edgings and slabs, will give out a much less quantity. The minimum will be given out by a saw log floating in the water.

The total waste in manufacturing saw logs into boards is sometimes stated as equal to the lumber obtained for market; but this is a gross exaggeration. Prominent manufacturers like the Rathbun Co., W. C. Edwards, M.P., and J. R. Booth estimate the waste as varying between 25 per cent. and 35 per cent. of the whole log. The proportion of refuse varies with the size of the logs, with the kind of lumber into which the log is cut, and with the kind of saw used in the mill. The old-fashioned gang saw and the large circular saw produce a higher percentage of waste than the more modern band saw. There is more waste in cutting a log into inch boards than into 3-inch deal, and small logs produce proportionately more waste in bark, slabs and edgings than large logs. The waste in sawdust alone varies from 10 per cent. to 20 per cent.

PULP INDUSTRY.

There are other industries in Canada, which in preparing their products for market grind up plants and trees, and thus let out their cell contents. One of these is the pulp industry—likely to become very extensive in the near future. Two processes are in vogue in this industry. In one, the logs are macerated with chemicals, the mills being known as sulphite mills. In the other process, the logs are ground into shreds in what are known as mechanical mills. Both processes liberate the greatest possible quantity of stored material from the wood cells, and if this material is equally poisonous with that liberated from sawdust, then the waste water discharged from a pulp mill should be much more poisonous than from a sawmill. The St. Andrew's experiments determined the percentage of poison from a sulphite mill which is fatal to fish life, but, so far as I know, the percentage of poison from a mechanical mill has never been determined. A provisional conclusion, however, may be based upon some of the experiments to be described later in this paper.

BEET SUGAR INDUSTRY.

The manufacture of sugar from the maple and from the beet depends upon the fact that sugar is one of the reserve materials stored in the cells of these plants. In order to liberate the sugar from the beet roots they must be thoroughly ground into a mash, so as to rupture the cell walls. The more effectively this is done, the higher is the percentage of sugar obtained from the beet. It is easily conceivable that the water that escapes from beet sugar factories may contain matter that is poisonous to fish life.

Professor Prince called attention to both these sources of pollution in his report for 1899, and they are referred to now merely for the purpose of emphasizing the fact that other industries may pollute the streams of Canada to even a greater extent than lumbering. In all three industries the source of pollution is the contents of the wood or plant cell.

There is a similar action going on in nature all the time. Leaves, branches, and trunks of dead trees are decomposing continuously; their cell contents are being dissolved in rain and melting snow, and are in part carried away in streams and rivers. The only difference is that in this latter case the poisons come away so slowly that air (oxygen), sunlight, and bacteria have ample time in which to change their poisonous character ; whereas in the saw mill, the pulp mill, and the beet sugar factory, the poisons are quickly discharged into running water, and tend at once to produce their effects upon fish and other life.

STRENGTH OF SAWDUST EXTRACTS.

As already explained, the first experiments were made with solutions obtained by soaking white pine sawdust for at least twenty-four hours in tap water from Lake Ontario. When the sawdust was soaked for four days in tap water, 1,000 c.c. of the yellowish-brown solution already described as oozing out from the bag of sawdust, and lying at the bottom of the aquarium, yielded 1,240 milligrams of solid matter after evaporation in a platinum crucible. The ash from this weighed 80 m.gs., which was found to be exactly the same as that from tap water. Deducting this from 1,240, leaves 1,160 m.gs. as the weight of the material stored in the pine cells of the sawdust, and dissolved out in 1,000 c.c. of water in four days.

After filtering off the first water, and adding fresh water to the same sawdust, and allowing the mixture to stand five days longer, it was found that 1,000 c.c. of this second solution yielded a total of 360 m.gs. of solid, or allowing for the ash in tap water, a net residue of 260 m.gs. of reserve material was dissolved out the second time.

The corresponding figures for cedar (Ontario) sawdust were as follows :—

1. Solid from 1,000 c.c. soaking four days............... 1,300 m.gs.
2. Same sawdust with first water filtered off, fresh water added and allowed to stand five days .... =550 m.gs.
3. Same operations repeated, soaking five days .... =350 m.gs.

No allowance is made in these figures for the ash from tap water, viz., 80 m.gs.

These figures indicate clearly enough that the reserve material stored in the wood cells comes away in diminishing quantities every time fresh water is added to the sawdust.

The next point sought to be determined was the number of times that fresh water could be added to a fixed weight of sawdust and continue to produce solutions which would be poisonous to fish life. For the purpose of getting information on this point two series of experiments were carried on, one with cedar sawdust and one with white pine.

EXTRACTS FROM CEDAR (ONTARIO).

On the second of June 400 grams of cedar sawdust were placed in a cheese-cloth bag and sunk to the bottom of a small glass aquarium (12 in. x 8 in. x 6 in.) containing 7,000 c.c. of tap water. Next day there had formed at the bottom, to a depth of two inches, a dark yellowish brown solution. The uppermost four inches were tinged a light yellow by diffusion, but there was a perfectly distinct surface of a greyish colour separating the upper from the underlying dark water. These characters became still more marked during the week, at the end of which time 1,400 c.c. of the lower liquid were siphoned off into a shallow circular dish and a perch weighing seventy grams immersed in the solution. In thirteen minutes it was lying on its back moribund, but revived when returned to fresh water. The control animal was kept twenty-four hours in 1,400 c.c. tap water in a similar vessel and then returned to the aquarium.

A perch weighing twenty-five grams was placed in 400 c.c. of this extract and air bubbled rapidly through it all the time. In twenty-eight minutes it was dead. The control animal in tap water under similar conditions was alive at the end of seventy hours.

Three *Daphniæ* in this extract died within two hours.

Fresh water Hydra died almost instantly in it. Paramœcia were unaffected by either cedar or pine extracts. These scavengers were often observed apparently feeding upon the dead bodies of Hydra that had died in the poisonous extracts.

On the 13th, all the water was poured off and fresh water added. On the 14th, a perch weighing seventy grams was placed in 1,400 c.c. of the solution formed during the preceding twenty-four hours. It was moribund in six minutes. With air bubbling rapidly through some more of this solution, another perch placed in it was moribund in seven minutes.

Pond silk appeared to be unaffected by an immersion of four days in this water.

June 15th. A perch moribund in fourteen minutes in a third solution—all the water being poured off and fresh added.

June 16th. A perch moribund in eight minutes in a fourth solution from the same sawdust.

June 16th. A perch moribund in ten minutes in a fifth solution formed by soaking this same sawdust in fresh water for seven hours.

June 17th. A perch moribund in ten minutes in a sixth solution. With air bubbling through more of this solution, another perch moribund in twenty-one minutes.

June 18th. A perch moribund in twelve minutes in a seventh solution. The water was poured off twice to-day, making an eighth solution.

June 19th. A perch moribund in eight minutes in a ninth solution. At 11 a.m. a pond leech was placed in this solution. It had eighteen young ones, five large and thirteen small, attached to its back. Three of these young at once detached themselves from the mother's back. The adult showed every symptom of discomfort by swimming rapidly round the vessel, then pausing and rolling itself up into a wheel as if to escape the effects of the water. It tried to leave the vessel, but was put back again. Gradually the smaller young detached themselves from the mother until only two or three of the smallest (about ¼ inch in length) remained on her back. The larger young ones were about an inch long, but when extended they were an inch and a half. Finally the smallest dropped off and wriggled about with the others on the bottom. The mother came to rest in about three-quarters of an hour. The young were all dead at 4.30 p.m.; the mother was moribund at 6 p.m., and died during the evening.

Two pond snails lived just twenty-four hours in this solution. The larva of an aquatic insect lived five and a half hours in it.

At 11.30 a.m. three bunches of vorticellæ were placed in the solution. Their cilia at once stopped moving in all the individuals, and they assumed the spherical form. By 5 p.m. most of these animals had dropped from their stalks and lay quite motionless at the bottom of the glass. Apparently they were dead. Returned them to fresh water, but found them all apparently dead next morning.

A bunch of embryos of the pond snail placed in this extract at 5.30 p.m. to-day were all found dead the next morning.

June 20th. Placed a perch weighing seventy grams in 600 c.c. cedar water drawn off for the eleventh time; air bubbling through it very rapidly. Moribund in ten minutes.

Placed in this extract about a dozen worms gathered from the mud in water about three feet deep. These animals were massed together and lying among the roots of aquatic plants. The moment the solution touched them they separated all over the bottom of the watch glass, wriggling in all directions and voiding their faeces. In three hours they were all dead. So were two small phyllapod crustaceans which happened to be along with the worms.

June 24th. Placed a batch of about fifty aquatic worms in cedar extract drawn off for the twelfth time. At first great wriggling ensues with evacuation of faeces ; then constrictions occur in each segment of the body, making the animal look somewhat like a string of beads ; then the hinder end appears to disintigrate but leaves the front end living and moving ; finally the head dies. In two hours most of them were dead, but in a few the head was still alive.

June 25th. At 9.15 a.m. placed a tadpole one inch long in cedar extract drawn off for the twelfth time. Apparentty dead in fifteen minutes, but revived in about an hour when returned to fresh water.

Up to this time all experiments with cedar extract had been conducted with what might be considered as saturated solutions ; that is, the solution used was siphoned off from the bottom of the aquarium where the sawdust was lying and where the dark colour showed the extract to be the strongest. From this date the experiment was varied by throwing out all the water, filling up the aquarium with fresh water and allowing the bag of sawdust to float at the top of the water. In this way the solution was uniform in colour and strength throughout the aquarium. The animals were then as a rule placed in the aquarium and usually swam about below the floating sawdust bag.

June 27th. Mr. Halkett, an officer of the Department of Marine and Fisheries, arrived to-day from Belleville, bringing with him about 100 black bass fry. The weight of one of these of medium size was found to be 135 milligrams ; its length one inch.

Placed two of these fry in cedar extract drawn off fourteen times. Both appeared to be dead in two minutes. Placed in fresh water they did not revive.

June 28th. Two black bass fry in cedar extract drawn off sixteen times from the same sawdust, died in two hours.

June 30th. Changed all the water on the cedar sawdust to-day no less than five times. Immediately after adding the fresh water black bass fry were placed each time in the aquarium, and in each case the animal was dead in from half an hour to forty minutes.

July 7th. The last experiment with this sawdust was performed to-day. The water was changed this morning at 9 a.m. for the thirty-first time, and immediately afterwards a black bass fry was immersed in it. It swam about below the floating bag which contained the sawdust. The odour of the cedar was scarcely perceptible in the water. The strength of the solution was, of course, increasing all the time. At 11 a.m. the fry was dead.

Some of the water that was drained off from the sawdust at 9 a.m. was found to contain 235 m.gs of solid matter per 1,000 c.c. Allowing for the residue after ignition, there would still remain 155 parts per million of poisonous extract dissolved out of the cedar cells in the thirtieth withdrawal. This is quite remarkable when it is remembered that the sawdust had been soaking continuously for five weeks, and the water on it changed thirty times.

Comparing the solid in this solution with that in a saturated solution already given, viz., 1,240 per 1,000 c.c., we conclude that there has been a continuous withdrawal of poisonous extracts from the cedar. The question, therefore, of whether a river is polluted with sawdust or not, simply becomes a question of determining the quantity of sawdust poured into a known volume and flow of water, and the further question of determining whether the resulting solution is poisonous enough to kill fish eggs, fry, adult fish or fish food.

Warm water was found to extract the poison from wood cells much more quickly than cold water.

EXTRACTS FROM WHITE PINE.

The general effect of pine extracts upon fish eggs has already been described. It only remains to point out some special effects under varying conditions. One of these is that eggs live longer in aërated sawdust water than in unaërated. This is quite clear from the following experiment: At 9.45 a.m. of May 18th, two batches of eggs were placed in pine water at the bottom of the aquarium. At 5.30 p.m. every egg but two was dead.

At 11.15 a.m. of May 17th two batches were placed in pine water through which air was bubbling at the rate of 400 c.c. per minute. At

9.45 a.m. of the 18th, twenty out of one batch of thirty-three were dead; and in the other, thirteen out of seventy-three were dead. At 5.30 p.m. of the same day all of the first batch of thirty-three were dead, and only seven were alive in the other.

The effect of aërating the pine water was made apparent in another way. At 10.25 a.m. of the 18th, 120 eggs were placed in pine water in a shallow dish so that the water was only three-eighths of an inch deep. At 5.20 p.m. only a few were dead; all the rest were very quiet. At 9 a.m. the next morning forty-seven were dead; at 6 p.m. all were dead except five. At 10.30 a.m. of the 20th four of these five had hatched out and were quite lively. This experiment shows that the large surface exposed to the air absorbs oxygen, and therefore tends to prolong the life of both larvæ and fry. In contrast with this it is interesting to note that the same quantity of poisonous water put into a tall jar at 6 p.m. of the 19th had killed every egg in a batch of nineteen by 10.30 a.m. the next morning. In this case, the depth of the water and the small surface exposed to the air prevented the diffusion of the oxygen downwards to the eggs, lying at the bottom of the vessel.

There can be no doubt that fish instantly perceive the poisonous character of pine or cedar extracts. A minnow was placed in the large marble aquarium already described, and being driven to one end of the vessel, it sank through the clear water and into the yellowish brown extract lying at the bottom. The moment his head touched it he started towards the surface. I drove him back several times, and each time he sank into the coloured water he made frantic efforts to escape from it. He refused finally to be driven into it. Immersed in the pine extract in a separate vessel the minnow was moribund in three minutes and could not be resuscitated in fresh water.

A perch placed in 900 c.c. of pine water, in a shallow dish, was moribund in three minutes.

Another perch in pine water with air bubbling rapidly through it lived three and a half hours.

Two limicolous worms died in thirty minutes; two rotifers lived only ten minutes.

One tadpole half an inch long lived two hours. Another tadpole of the same size died in half an hour in a weak solution. A similar animal in strong cedar extract lived only six minutes.

A copepod placed in it at 11 a.m. was alive at 3.30 p.m., but died during the early evening.

Daphnia and the larva of an aquatic insect lived three days.

One hydra immersed at 10.40 a.m. was dead at 5 p.m. Its body was partly disintegrated and many paramœcia appeared to be feeding on it. Another hydra on being placed in the extract contracted its tentacles, detached itself from its support, contracted the lower half of its body, voided the intestinal contents, and appeared to be dead in two hours. It revived in fresh water.

A colony of vorticellæ at first showed no signs of discomfort ; the cilia kept on moving, and the stalks contracting spirally. Soon the stalks ceased their movements ; a little later the cilia stopped ; the animals took the spherical form and within one and a half hours all were apparently dead.

A rock bass weighing seventy grams when placed in 1,300 c.c. of the extract became moribund in twelve minutes. All of the fish revived in from five to twenty minutes when returned to fresh water.

A perch weighing thirty grams when placed in a jar containing 400 c.c. of the pine water with air bubbling rapidly through it was moribund in ten minutes.

June 16th. Up to this time my experiments with this extract had been made with strong solutions. To-day a series of experiments were begun for the purpose of ascertaining, if possible, how long the same sawdust would continue to give off poisonous solutions when the saturated water was drained off and fresh water added from time to time.

With this end in view 360 grams of white pine sawdust were placed in a cheese cloth bag at 9 a.m. and sunk to the bottom of a small glass aquarium, 12 in. x 8 in. x 6 in., containing 7,000 c.c. of tap water. Two hours after, 800 c.c. were siphoned off from the extract at the bottom of the vessel. This was found to be very slightly poisonous to adult fish. Next morning at 10 a.m. 800 c.c. more were siphoned off. A rock bass lived in this one hour and twenty minutes. Another fish lived six hours in it when air was made to bubble rapidly through it. The third and fourth withdrawals of 800 c.c. each were thrown away.

June 21st. The larva of an aquatic insect lived twelve hours in the extract drawn off for the fifth time : vorticellae lived twenty hours, limicolous worms twenty hours, a pond snail seventy hours. The sixth and seventh withdrawals were thrown away.

June 27th. The eighth withdrawal of 800 c.c. made by soaking the pine eighteen hours, killed a perch in three hours, and three black bass fry in half an hour.

June 30th. A slight modification was made in this experiment. In place of siphoning off the strong extract at the bottom of the aquarium, the whole 7,000 c.c. of water were drained off, and the aquarium was filled up with fresh water. The weights were removed from the bag, which at once rose to the top of the water. Consequently the extract coming off from the sawdust, being heavier than the fresh water, fell towards the bottom and became uniformly diffused throughout the vessel. This was the twelfth withdrawal. Black bass fry lived five hours in this water, which was, of course, becoming more poisonous all the time.

July 7th. The last experiment with this sawdust was made to-day. The bag is still floating. The water was changed for the twentieth time at 9 p.m. last evening. At 9 a.m. to-day a black bass fry was immersed in this solution. In two hours it was dead. Some of this solution was evaporated and was found to contain 160 m.gs., or, allowing for the residue after ignition, eighty parts per litre. That is, pine sawdust soaking continuously since June 16th, with the water on it changed twenty times furnished in twelve hours eighty parts per million of poisonous extracts from its wood cells.

Comparing these figures with those for a saturated solution already given, viz., 1,160 parts for 1,000 c.c., we see that there has been a continuous withdrawal of poisonous material from the sawdust. The question, therefore, of determining whether any stream is polluted with pine sawdust or not is largely the question of determining the minimum amount of sawdust extracts which will kill fish eggs, fry, adult fish, and fish food. Needless to say, such determinations would have to be made for every sawmill stream in Canada, and for each separate kind of fish.

OTHER WOOD EXTRACTS.

A number of experiments were made with extracts from other woods besides pine and cedar. Norway, or red pine, British Columbia cedar, maple, hemlock, oak, ash, elm were all used, but it was soon discovered that the most poisonous extracts were obtained from the pines and cedars. Consequently experiments with the hard woods were soon discontinued.

From all hard woods, however, the saturated yellowish-brown extract was found to be very poisonous to both adult fish and fish eggs.

The following experiments give typical results in the case of each of these woods.

MAPLE SAWDUST.

A dark orange liquid oozed out from maple, and lay at the bottom of the aquarium. This was separated from the clear liquid above by a perfectly well-defined greyish surface. At the top of the water (16½ inches deep) intake and outflow pipes allowed tap water to flow into and out of the aquarium at the rate of 600 c.c. per minute. A perch having sunk into this extract once or twice could not afterwards be driven into it. The animal soon found where the fresh water inlet was, and when driven to other parts of the aquarium would always come back to the fresh water.

Aquatic plants in maple extract lost their chlorophyl in three days. Returned to fresh water they regained their colour, but the tips of their leaves had died.

HEMLOCK SAWDUST.

Hemlock has always had a bad reputation, but does not deserve it.

On July 27th, six black bass fry were placed in a mixture of five volumes of water to one volume of hemlock sawdust. The vessel was covered with four layers of cheese cloth, and a copious stream of water was made to fall upon it from a tap about a foot above it. The fry were all alive and well at the end of three days, when they were returned to the aquarium.

As a control experiment, five black bass fry were kept for the same length of time in the same volume of water, viz., 600 c.c., with air bubbling through it all the time. These animals also were quite lively and well at the end of the experiment.

BRITISH COLUMBIA CEDAR SAWDUST.

This sawdust sank rapidly, 75 per cent. falling to the bottom of perfectly still water in two minutes. It gave off a very poisonous extract. Two black bass fry lived only one minute in a solution made by standing five and a-half hours. The colour was a beautiful amber with a strong smell of cedar. A solution made by one gram of sawdust standing in 500 c.c. water for three hours rendered a black bass fry moribund in two hours. A solution from one gram in 750 c.c. water for twenty-seven hours, killed another fry in two and a-half hours. Even as homeopathic a solution as one gram in 1,500 c.c. killed fry in less than eighteen hours.

If much of this sawdust is poured into British Columbia streams, the stockholders of British Columbia Fish Canning Co's will need to look closely into the future prospects of their industry.

NORWAY, OR RED PINE.

Within three minutes, 90 per cent. of the sawdust from this wood had sunk. A strong solution made in eighteen hours rendered a black bass fry moribund in one hour. This water when aërated, but not filtered, rendered another fry moribund in exactly the same time. In both cases the gills of the animals seemed to be affected by fine particles of the wood fibre clinging to the filaments and preventing respiration. This was not observed to be the case with any other kind of sawdust.

A solution made by soaking one gram of this sawdust for nine hours in 250 c.c. of water killed a fry in less than an hour.

Another fry lived fifteen hours in a solution made by soaking one gram in 850 c.c. water for six hours.

OAK.

Contrary to expectations, oak sawdust was not so poisonous as pine and cedar. It communicated an orange colour to the water just as other woods did. A tadpole lived three days in a strong solution, and was quite lively at the end of that time.

ELM.

A few experiments were made with elm sawdust. Here again a dense yellowish-brown layer forms at the bottom of the aquarium. This kills adult fish in from half an hour to two hours. A tadpole lived over an hour in it. When this water was thoroughly aërated a perch lived twenty hours in it, and was then active and apparently well.

EXTRACTS QUICKLY SOLUBLE.

The experiments hitherto described would seem to indicate that some considerable time was required for the water to dissolve out the poisonous extracts from white pine sawdust, but such is certainly not the case. This was clearly shown in the following experiment, Fig. 3. Two minnows were confined in a bottle containing 600 c.c. water and eighteen grams of white pine sawdust. Fresh water was made to enter and leave at the rate of 100 c.c. per minute. The inlet tube passed straight to the bottom of the vessel, and its lower end was therefore buried in about an inch of sawdust. One animal lived forty minutes, the other fifty. When the incoming water was reduced to 80 c.c. per minute three minnows lived only from three to five minutes. When the fresh water entered at the rate of 125 c.c. per minute, minnows lived from twenty to ninety minutes. The control animals were kept for a week in a similar bottle, without sawdust, of course, and with water coming in at the rate of 110 c.c. per minute. In these experiments the poisonous extracts must have been coming away all the time. The moment the bottle was full of water the minnows were slipped into it. Consequently, when the fish were killed in five minutes, the 600 c.c. at first in the bottle. and 400 c.c. additional water were poisoned. When they were killed in ninety minutes, no less than 11,250 c.c. were poisoned. That is, the percentage weight of sawdust to poisoned water was .16 per cent. This determination is important, as we shall see later, when we come to compare it with the percentage of sawdust thrown into the Bonnechere River.

FIG. 3.

FISH AT MILL-ENDS.

Millmen and anglers alike testify that many kinds of fish are taken by hook and line at mill-ends, no matter how excessive the sawdust may be. The sawdust does not kill the fish so long as there is a rapid and abundant flow of water. Why do fish thus congregate at mill-ends? To answer this question we must remember two things : first, rapidly running water is better aërated than sluggish water ; and secondly, some fish, such as trout and salmon, ascend streams until they reach suitable spawning grounds, or are stopped in their ascent by high falls or mill-dams. In ascending a river these fish are but obeying a law of their nature ; in congregating at mill-ends they are equally obeying a law of their nature, and are instinctively seeking water which furnishes their blood with a plentiful supply of oxygen. This instinct is well illustrated in the experiment just described in Fig. 3. The experiment was repeated a number of times, and in every instance the fish discovered where the fresh water came in. In one instance, in order to get close to the incoming water, a minnow stood on its head for fifteen minutes with more than half of its body buried beneath the sawdust. It was thus acting under the impulse of two fundamental instincts, viz., the instinct to avoid poisoned water on the one hand, and to seek fresh water on the other. The experiment seems to throw light upon the experience of anglers who have found that trout desert the main stream when saw mills are running, and betake themselves to the unpolluted branch streams lower down.

A STAGNANT ARTIFICIAL POOL.

Reference has already been made to the fact that black bass fry, minnows and perch, when placed in an aquarium, invariably avoided the poisonous sawdust water at the bottom. Having sunk into it once or twice, it was found almost impossible to drive them into it again. Here was a conflict between two fundamental instincts. On the one hand was the natural instinct to hide in deep water ; on the other hand, the equally natural instinct to avoid the poisonous solution at the bottom. Which instinct would the fish obey if compelled to make a choice ?

The following experiment was designed for the purpose of seeing which instinct was the more powerful, and for the further purpose of imitating what might possibly occur in a stagnant pool along the course of a sawdust polluted stream.

A glass aquarium 12 in. x 8 in. x 6 in. was placed in a much larger vessel and a mixture of ice and salt packed in the latter so as to surround the aquarium. The aquarium was then half-filled with white pine extract which had been forming for three weeks, and which killed adult fish in from one to three minutes. After the extract had been cooled down to 8° c., tap water at the temperature of 13° c. was slowly admitted to the aquarium so as not to disturb the underlying poisonous water. The tap water, being warmer, floated clear and transparent on the dark purplish extract below. The clear water entered and left the aquarium at the rate of 150 c.c. per minute. The arrangement of apparatus is represented in Fig. 4.

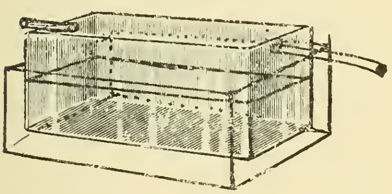


Fig. 4.

At first two minnows were placed in the aquarium. They at once dove to the bottom, encountered the poisonous water, immediately came up again, repeated the operation a few times, and finally remained swimming about in the clear water. Three black bass fry, liberated one after the other, went to the bottom and never came up—suffocated and poisoned in the dark stagnant water at the bottom. Of two other minnows dropped into the aquarium, one large one never came to the surface ; the other joined its fellows in the clear water above. All three soon found the end at which the fresh water was entering and remained there facing the stream.

This experiment shows what might possibly happen in pools partially filled with sawdust. Wood extracts would form, and being cooler and heavier than the clear water, would lie at the bottom of the pool. Of course, fish already in the pool would be driven away, but those coming up or down stream through shallow stretches, and trying to hide in the deeper waters of the pool, might be suffocated or poisoned.

COMPARATIVE RESULTS.

After obtaining the general results detailed in the preceding part of this paper, it seemed desirable to plan a series of experiments that would show comparative results at a glance. With this end in view, two grams each of different kinds of sawdust were placed in shallow circular dishes containing respectively, 300, 400, 500, 600, 700, 800, 900, 1,000, 1,200, 1,500, and 1,700 c.c. of fresh water. After soaking for about five hours in each case, a minnow was placed in each of the dishes. The length of time each animal lived was carefully noted, except in those cases where death occurred during the night. The results are given in the following tables:—

WHITE PINE SAWDUST.

Weight of Sawdust.	Volume Water c.c.	Time Soaking.	Time at which minnow was immersed.	Results.
2 grams.	300	From 10 a.m.	2.43 p.m.	Lived about 9 minutes.
"	400	"	"	" "
"	500	"	"	" "
"	600	"	"	" "
"	700	"	"	" "
"	800	"	"	" 10 minutes.
"	900	"	"	" 13 "
"	1000	"	"	" 15 "
"	1200	"	"	" 20 "
"	1500	"	"	" 29 "
"	1700	"	"	" 29 "

ONTARIO RED PINE.

2 grams.	300	10 a.m.	2.47 p.m.	Lived 47 minutes.
"	400	"	"	" 50 "
"	500	"	"	" 50 "
"	600	"	"	" 1 hour and 28 minutes.
"	700	"	"	" 1 " 14 "
"	800	"	"	" 1 " 14 "
"	900	"	"	" 1 " 53 "
"	1000	"	"	" 2 hours and 20 "
"	1200	"	"	" 2 " 50 "
"	1500	"	"	" 3 " 45 "
"	1700	"	"	" 3 " 45 "

ONTARIO CEDAR.

Weight of Sawdust.	Volume Water c.c.	Time Soaking.	Time at which minnow was immersed.	Results.
2 grams.	300	From 10 a.m.	2.33 p.m.	Lived 8 minutes.
"	400	"	"	" 9 "
"	500	"	"	" 19 "
"	600	"	"	" 20 "
"	700	"	"	" 21 "
"	800	"	"	" 22 "
"	900	"	"	" 27 "
"	1000	"	"	" 27 "
"	1200	"	"	" 1 hour.
"	1500	"	"	" 1 " and 48 minutes.
"	1700	"	"	" 1 " " 55 "

BRITISH COLUMBIA CEDAR.

2 grams.	300	10.15 a.m.	2.51 p.m.	Lived 6 minutes.
"	400	"	"	" 6 "
"	500	"	"	" 15 "
"	600	"	"	" 53 "
"	700	"	"	" 43 "
"	800	"	"	" 1 hour and 9 minutes.
"	900	"	"	Jumped out of dish unnoticed.
"	1000	"	"	Lived 1 hour and 32 minutes.
"	1200	"	"	" 1 " 36 "
"	1500	"	"	" 3 " 50 "
"	1700	"	"	" 3 " 29 "

HEMLOCK BARK.

Bark. 2 grams.	300	10.10 a.m.	2.36 p.m.	Lived 55 minutes.
"	400	"	"	" 1 hour and 32 minutes.
"	500	"	"	" 1 " 43 "
"	600	"	"	" 1 " 49 "
"	700	"	"	" 2 hours.
"	800	"	"	" 1 hour and 32 minutes.
"	900	"	"	Jumped out of dish unnoticed.
"	1000	"	"	Lived 2 hours and 18 minutes.
"	1200	"	"	" 3 " 24 "
"	1500	"	"	" 4 "
"	1700	"	"	" 4 " 15 "

HARD MAPLE SAWDUST.

Weight of Sawdust.	Volume Water c.c.	Time Soaking.	Time at which minnow was immersed.	Results.
2 grams.	300	From 10.38 a.m. July 15th.	July 15th. 3.30 p.m.	Lived 2 hours and twenty minutes.
"	400	"	"	July 21st, 10 a.m. Still alive.
"	500	"	"	" 16th. Died last night.
"	600	"	"	" 21st, 10 a.m. Still alive.
"	700	"	"	" 16th. Died last night.
"	800	"	"	" 21st, 10 a.m. Still alive.
"	900	"	"	Lived only 2 hours.
"	1000	"	"	July 18th. Died between 4 p.m. and 8 p.m.
"	1200	"	"	Lived 3 hours and 30 minutes.
"	1500	"	"	July 18th. Died between 4 p.m. and 8 p.m.
"	1700	"	"	" 20th. Died 3 p.m.

This experiment was discontinued July 21st, 10 a.m.

ONTARIO CEDAR BARK.

2 grams.	300	10.20 a.m.	2.41 p.m.	Lived 37 minutes.
"	400	"	"	" 1 hour and 20 minutes.
"	500	"	"	" 50 minutes.
"	600	"	"	" 50 minutes.
"	700	"	"	" 1 hour and 20 minutes.
"	800	"	"	" 1 " 31 "
"	900	"	"	" 1 " 40 "
"	1000	"	"	" 1 " 57 "
"	1200	"	"	" 2 hours 10 "
"	1500	"	"	" 4 "
"	1700	"	"	" 4 " 20 "

ELM SAWDUST.

2 grams.	300	10.44 a.m. July 15th.	3.30 p.m.	Lived 4 hours and 30 minutes.
"	400	"	"	Died 10 a.m. July 16th.
"	500	"	"	Lived 1 hour and 30 minutes.
"	600	"	"	" 2 hours and 30 "
"	700	"	"	" 1 hour and 30 "
"	800	"	"	July 21st, 10 a.m. Still alive.
"	900	"	"	" 18th. Died last night.
"	1000	"	"	" 21st. Died last night.
"	1200	"	"	Lived 1 hour and 30 minutes.
"	1500	"	"	" 4 hours and 30 "
"	1700	"	"	" 1 hour and 30 "

This experiment was discontinued July 21st, 10 a.m.

OAK SAWDUST.

Weight of Sawdust.	Volume Water c.c.	Time Soaking.	Time at which minnow was immersed.	Results.
2 grams.	300	Since 10.15 a.m. of 23rd.	July 23rd. 2.30 p.m.	Lived 2 hours and 30 minutes.
"	400	"	"	" 2 " 30 "
"	500	"	"	" 3 " 30 "
"	600	"	"	" 7 " 30 "
"	700	"	"	" 2 " 20 "
"	800	"	2 animals	One lived 2 hours and 20 minutes. July 24th. Died last night.
"	900	"	2 animals	One lived 7 hours and 30 minutes. July 24th. Died last night.
"	1000	"	"	July 25th. Jumped out unnoticed.
"	1200	"	"	" 30th, 9 p.m. Still alive. Released.
"	1500	"	"	Lived 3 hours and 30 minutes.
"	1700	"	"	July 25th, 3 p.m. Dead.

ASH SAWDUST.

2 grams.	300	10.48 a.m. of July 15.	3.30 p.m. July 15th.	July 21st, 10 a.m. Still alive.
"	400	"	"	Lived 1 hour and 30 minutes.
"	500	"	"	July 21st, 10 a.m. Still alive.
"	600	"	"	Lived 1 hour and 40 minutes.
"	700	"	"	" 2 hours and 10 "
"	800	"	"	July 21st. Died last night.
"	900	"	"	Lived 1 hour.
"	1000	"	"	July 21st, 10 a.m. Still alive.
"	1200	"	"	" 21st. Died last night.
"	1500	"	"	" 21st, 10 a.m. Still alive.
"	1700	"	"	" 19th. Died to-day.

This experiment was discontinued July 21st, 10 a.m.

HEMLOCK SAWDUST.

2 grams.	300	10.15 a.m. of 23rd.	2.30 p.m. July 23rd.	July 26th, 9.30 a.m. Dead.
"	400	"	"	" " "
"	500	"	"	July 30th, 9 a.m. Released.
"	600	"	"	" " "
"	700	"	"	" " "
"	800	"	"	July 26th. 9.30 a.m. Found dead.
"	900	"	"	Lived 45 minutes.
"	1000	"	"	July 26th, 11 a.m. Dying.
"	1200	"	"	" 28th, 3.00. Dead.
"	1500	"	"	Lived 1 hour and 45 minutes.
"	1700	"	"	July 26th, 9.30 a.m. Dead.

SPRUCE SAWDUST.

Weight of Sawdust.	Volume Water c.c.	Time Soaking.	Time at which minnow was immersed.	Results.
2 grams.	300	10.30 a.m. of 23rd.	2.40 p.m. July 23rd.	Lived 3 hours and 30 minutes.
"	400	"	"	July 24th, 9.30 a.m. Found dead.
"	500	"	"	" " "
"	600	"	"	" 26th, " "
"	700	"	"	" 24th, " "
"	800	"	2 animals	{July 24th, 9.00. Dying. {" 25th, " Found dead.
"	900	"	"	July 26th, 9.30 a.m. Found dead.
"	1000	"	"	" 30th, 9.00 a.m. Released.
"	1200	"	"	" " " Dying.
"	1500	"	"	" 27th, 7.30 p.m. Dying.
"	1700	"	"	" 26th, 9.30 a.m. Found dead.

The reader will, of course, understand that in all experiments due. allowance must be made for constitutional differences in individual fish. Some men survive the effects of cold, hunger or poisonous drugs longer than others. In the same way some species of fish, and some individuals in each species, are naturally more hardy than others, and can survive in poisoned water a longer time. Some are more delicately organized and are, therefore, more easily killed. For example, black bass fry lived longer than minnows in some of my experiments. Consequently too much importance must not be attached to the exact number of minutes or hours that a fish will live in any given strength of sawdust solution. When we are dealing with vital phenomena, all we can consider is the general average of a number of experiments. Keeping this in view, some conclusions may fairly be drawn from the foregoing results.

1. White pine sawdust is by all odds the most poisonous substance.

2. Next comes Ontario cedar.

3. Then British Columbia cedar.

4. Red pine, cedar bark and hemlock bark are moderately poisonous.

5. Maple, oak, ash, elm, hemlock and spruce may all be grouped together as only slightly poisonous.

EXPERIMENTS WITH BARK.

From the frequent references to the pernicious effects of bark which may be found in the literature of sawdust pollution, one would naturally expect to find that bark solutions were very destructive to fish life and fish food. The very opposite was found to be the case. Compared with the wood extracts, the bark solutions were comparatively harmless. Even tan bark, much execrated by fishermen and anglers alike, was not so poisonous as one might expect, but the experiments must speak for themselves.

WHITE PINE BARK.

Only 11 per cent. of sawdust from this bark sank in ten minutes. A black bass fry seemed perfectly unharmed after being three hours in a solution of this bark that had been forming for twenty-two hours (one gram in seventy-five c.c. tap water). The animal was then returned to the fresh water aquarium.

This same bark, after soaking two weeks, gave a solution that killed solely by suffocation. This was quite apparent from the fact that two minnows when placed in this water (freely aërated) lived for fwenty-four hours and were then liberated. When the solution was unaërated the minnows died in an hour or two. Pouring the solution several times from one vessel to another arëated it sufficiently to enable two minnows to live three days in it without apparent harm.

After standing six weeks a scum formed on the surface. This was removed and the solution aërated by pouring it several times from one vessel to another. A minnow now lived in it for two days and was liberated, apparently as well as ever.

HEMLOCK BARK.

A solution made by soaking one gram of this sawdust bark for fifteen hours in 100 c.c. of tap water killed a minnow in six minutes. After soaking for two weeks this water killed a minnow in one hour, even when thoroughly aërated. But these were very strong solutions compared with the ones obtained from wood.

CEDAR BARK.

Only 5 per cent. sank in fifteen hours. In two days it had all sunk excepting about 1 per cent. A 1 per cent. solution (one gram in 100 c.c.) made in fifteen hours, rendered a minnow moribund in fourteen minutes. Here again the solution was a very strong one compared with those obtained from wood sawdust and used in the experiments previously described.

As bark extracts, therefore, are not more poisonous than those from pine and cedar woods, it seemed useless to conduct separate experiments upon their effects.

DECAYING SAWDUST.

One objection frequently urged against the practice of throwing sawdust into streams and rivers is that the decaying sawdust imparts such a disagreeable odour to the water that sensitive fish are driven away to other waters not so polluted. It seemed to me, therefore, that some progress might be made towards a definite conclusion in this matter, if sawdust were allowed to stand for several weeks in an aquarium and tested from time to time as to the changes going on in it, and the influence of these upon fish.

With this end in view about 1,000 grams of white pine sawdust were placed in an aquarium three feet four inches long, fifteen inches wide, and filled up to sixteen and a half inches deep with fresh water. This was done June 24th. No water was allowed to enter or leave the vessel. No direct sunlight fell upon it.

The usual results followed, viz., a well defined layer of pale, yellow water one and three-quarter inches deep formed in a few hours and lay at the bottom. On top of this was the perfectly clear layer about fifteen inches deep.

After soaking for two days, bubbles of gas began to rise to the surface of the water, but no attempt was made to analyze it. The bottom yellowish layer had become so dense that no object could be seen across it—a thickness of fifteen inches. Its upper surface was sharply marked off from the overlying transparent water by a thin greyish layer. Microscopic examination of this layer showed it to be swarming with bacteria.

At the end of a week, only about an inch at the bottom had retained the original yellow colour ; the next inch had changed to a yellowish brown ; then came a greyish layer about one-sixteenth of an inch thick ; above this, what had at first been fourteen inches of perfectly clear water had turned to a dark grey, though still quite transparent. Black bass fry placed in the aquarium at this time at first sank to the bottom, but after meeting the poisonous extract once or twice could not subsequently be driven into it. On the contrary they swam along the top with their nose just touching the surface of the water, and behaved as if suffering from lack of air. They lived only about two hours.

Four days after this, black bass fry placed in the upper fourteen inches lived only about one hour. They also swam along the surface and appeared to be gasping for air. That they were suffocating in both cases was proved by the fact that when fry were placed in a wash bottle of this water with air bubbling through it, they lived on for twenty-four hours, and were then apparently well and exceedingly active. On being transferred from the wash bottle to the aquarium the animals at first plunged downwards to the bottom, paused there a moment, but soon came towards the surface breathing very rapidly. Evidently they were suffering from lack of oxygen. They swim along the top with noses upwards and body inclined at an angle of about thirty degrees with the surface. Gradually they tire ; sink towards the bottom ; rise again ; swim convulsively towards the surface ; jump clear out of the water with gaping mouth ; become exhausted by their convulsive efforts and finally sink to rise no more. Of all the fish killed in this extract not one ever rose to the surface after death.

It would be difficult to say whether this experiment throws any light upon a point much discussed in the literature of sawdust. The point is this : if sawdust kills fish, why are they not found dead in considerable numbers along the course of the stream ? In my experiments the dead bodies of the fish never rose out of the poisonous liquid.

AROMATIC COMPOUND.

The foregoing experiments show that the oxygen naturally dissolved in the upper fourteen inches of water had, at the end of a week, all disappeared. It was used up either in supporting the life of the bacteria, or in oxidizing the wood extracts through the agency of the bacteria. Bacteria were abundant in every part of the aquarium, but especially in the underlying solution. Moreover, either by their action on the pine extracts, or by the chemical decomposition of these extracts, an aromatic compound of a sweetish pleasant smell had begun to form. At the surface the smell was faint ; but in the water siphoned off from the bottom the perfume was strong and agreeable. The production of this compound is possibly due to micro-organisms, and if the special bacterium could only be isolated and used upon the extracts without admixture with other forms, it might be possible to manufacture a perfume from pine which many people would find agreeable. Alcohol, lactic acid, acetic acid, etc., are all formed by the action of bacteria upon vegetable substances in solution ; the quality, too, of butter and cheese is determined by the action of bacteria on the constituents of milk ; and it would, therefore, be only in accordance with well known facts to find that aromatic compounds, some pleasant, some unpleasant, could be formed from pine extracts by the action of different kinds of micro-organisms.

Some of the bottom water was distilled for the purpose of seeing whether this aromatic compound could be thus separated from the water, but the attempt failed. The distillate had the aromatic odour of the original water, but mixed with it was a disagreeable burnt smell. This distilled water killed minnows in half an hour, both when aërated or unaërated.

At the end of three weeks the uppermost fourteen inches of water had gradually become a steel grey or slaty colour and was quite opaque. The outlines of a window sash ten feet away could not be seen through it. The extract at the bottom still killed by its vegetable poison ; the slate coloured water above still killed by suffocation.

At the end of five weeks these conditions were but slightly changed. In place, however, of the pleasant aromatic odour previously arising from the surface, a musty, disagreeable smell had taken its place. As the laboratory windows were always open, mosquito larvæ became numerous and appeared to be feeding upon the bacteria. These larvæ died in sawdust solutions only when prevented from coming to the surface to breathe.

The water at the very bottom was still of a yellowish tinge ; the uppermost was smoky or slate coloured, as already explained. About 6,000 c.c. of this slate coloured water was siphoned off from the middle, on July 31st, and placed outside of the laboratory in direct sunlight. The object of this was to compare changes taking place in the slaty water placed in sunlight and breeze, with changes taking place in the slaty water which remained in the aquarium.

Dr. W. T. Connell, Professor of Bacteriology, made cultures from these two waters and compared them on three different occasions. His report which will be found in the appendix to this paper, shows that while the number of colonies from water in the shade increased from 3,435 per cubic centimetre to 7,870 per cubic centimetre ; the number of colonies from water in sunshine increased from 3,435 per cubic centimetre to 37,070 per cubic centimetre. These latter were different bacteria from the former. Sunlight and air had killed off those kinds of bacteria which flourish in shade and in absence of oxygen, and had stimulated the growth of other kinds of bacteria which flourish in sunshine and moving water. As a result of sunlight, warmth and breeze, what had been exceedingly disgusting water was changed in a fortnight to water brownish in colour, without any odour, and perfectly transparent. A heavy precipitate lay at the bottom. Minnows were able to live in it, and soon made havoc with the mosquito larvæ. In short the water had, within the fortnight, changed to normal water, while that in the shade still retained all its disagreeable and poisonous characters. The decaying mass of sawdust and water was kept for three months, and up to the very last showed no improvement. Slimy, a dark slate colour, foul smelling, teeming with anaerobic bacteria and mosquito larvæ, it was utterly unfit to support any kind of fish life.

NUTRITIVE RELATIONS.

However, the connection between a few links in the chain of animal life was apparent enough, viz., wood extracts supported bacteria, bacteria supported mosquito larvæ, and these again (after aëration of the water such as would occur in running water) supported fish life. These observations dispose to some extent of the oft repeated charge against sawdust that it destroys the food of young or newly hatched fish. When minnows relished mosquito larvæ as food, and I frequently saw them eating the larvæ, it requires no great stretch of the scientific imagination to understand how fish fry of different kinds, such as trout and salmon, might subsist upon the larvæ of mosquitoes and other aquatic insects, these latter in turn subsisting upon bacteria, and the bacteria subsisting upon the organic matter derived from the decaying vegetation of the forest.

Another thought comes up in connection with the presence of organic matter in streams and rivers. The organic matter which passed into a river when Canada was covered with forest must have been quite different in character from that which this same stream receives to-day from the vegetation of the farms along its valley. The surface drainage from a forest must differ in kind from the surface drainage of a farm, and the bacterial life in each must differ also. Moreover, the waters of our smaller streams were, years ago, shaded by trees, and the varieties of their bacterial life must thus have been quite different from the bacterial life in sunlit streams of to-day. Consequently, it may fairly be argued that the insect life, in and along the streams of an agricultural district, differs both in kind and number from what characterized these same streams 100 or 200 years ago. And if larval and adult insect life has dwindled or disappeared, so must the fish life which subsisted upon it.

The Anglo-Saxon has always been a disturbing factor in the balance of life. Forests, game and fish all disappear with his arrival. To get good fishing or good hunting now-a-days one must travel back to unsettled districts. No one expects game to be plentiful along the shores of Lake Ontario, but many people are amazed that fish are not abundant in it. They still hug the pleasing delusion that if brooks have been overfished, the fish hatchery can restock them. But with the disappearance of our forests it is exceedingly doubtful whether we can ever again, by all the help of hatchery, overseers and fish commissioners, re-people the streams which have been depleted by man through overfishing and deforestation. He has upset the balance of life ; it can only be fully restored by a return to primitive conditions. When game, therefore, becomes plentiful on the streets of Ottawa city, fish will be equally abundant below the saw mills of the Chaudiere Falls.

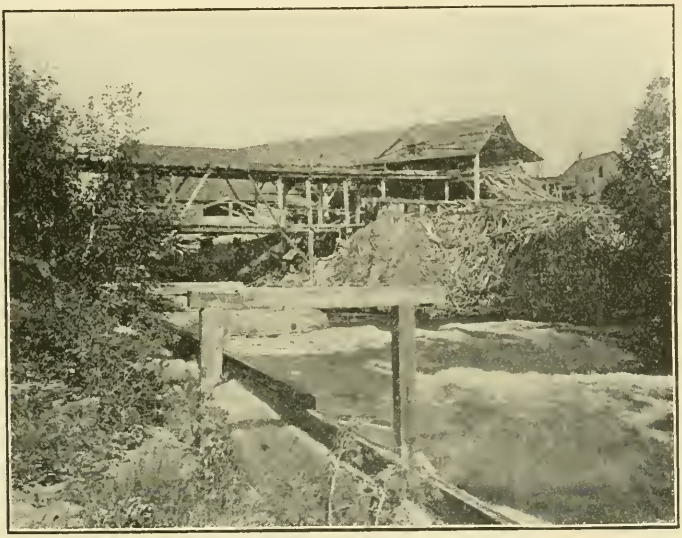
Such, at least, is the conclusion to which my experiments point, notwithstanding the indisputably poisonous effects of strong solutions from sawdust near the source of pollution. As I have already pointed out the question of whether any particular stream is sufficiently polluted with sawdust to kill fish life is simply the question of determining whether enough sawdust is passed into the stream to poison its waters. The forestry engineer will soon be trained to determine the strength of sawdust solutions, and will then be able to settle this question of pollution beyond the possibility of doubt.

ON THE BONNECHERE RIVER.

At present, however, a final judgment cannot be pronounced upon the poisonous effects of sawdust. These effects must be studied near the mills and along the sawdust beds of our rivers. A three weeks' study of the Bonnechere river, a tributary of the Ottawa much polluted with mill rubbish, led me to modify very considerably the conclusions which I had based upon my laboratory experiments. I visited the mill represented in two of the illustrations of this report fully expecting that not one fish could survive in such surroundings. But pike were abundant for miles below the mill, and fish (chub) could be caught any day along the side of the submerged driftwood. Stranger still, the fish so caught lived for three hours in a pailful of sawdust water drawn from the very centre of a sawdust bed. A few brook trout had been caught earlier in the season just below the mill when it was running. At the date of my visit, August 20th, 1902, the mill had been closed for seven weeks and no sawdust was then passing into the river.

The owner of the mill furnished the following information: The water passing over the dam is a stream nineteen and a half feet wide, by one and one-half feet deep, and moving two feet per second. This would mean that about sixty cubic feet of water were passing over the falls every second. Add to this, leakage through the dam, mill, and timber slide, estimated as equal to what passes over the dam, or sixty cubic feet more, a total of 120 cubic feet per second. The total water, therefore, passing down the river in July, August and September, would average 10,368,000 cubic feet per day, and weigh 642,816,000 pounds.

The mill cut an average of 375 logs per day. The logs averaged twelve inches in diameter and were chiefly sixteen feet long, but many



Sawmill on the Bonnechere river, a branch of the Ottawa. Sawdust and edgings pass into the river from the end of the mill.

were thirteen feet. Taking the specific weight of wet pine as .75, each log would weigh about 560 pounds. Of this weight about 13 per cent. would pass into the river as sawdust. This 13 per cent. was obtained as the average of five estimates furnished by such lumbermen as E. W. Rathburn, Esq., J. R. Booth, Esq., and W. C. Edwards, Esq. Consequently about seventy-two pounds of sawdust would pass into the river from every log cut into inch boards, or a total of 27,000 pounds of sawdust per day. Expressing this as percentage of water (642,816,000 pounds) we get .004 as the percentage strength of sawdust in this water.

During the high water of April, May and June the strength of the solution would be considerably less than .004 per cent., and as chub and brook trout were caught on and off all summer below the mill, this strength of sawdust solution was certainly not strong enough to kill off all the fish, though it is quite conceivable that it might drive fish down the river into tributary streams where there could be no sawdust pollution.

Comparing this percentage with that in two of the laboratory experiments described on pages 450 and 452 we find that in one case two grams of white pine sawdust in 1,700 c.c. of fresh water, *i.e.*, .12 per cent. strength, soaking for five hours, killed a minnow in twenty-nine minutes; and in the other case a percentage of .16 killed in ninety minutes.

Slabs, edgings and sawdust, half-a-mile below the mill.

Of course, these figures are mere approximations, but they point unmistakably to the conclusion that the sawdust poured into the Bonnechere river is not destroying its fish life. Moreover, in Golden Lake, an expansion of this same river, and ten miles above any saw mill, lake trout used to be very abundant. Every October large numbers were caught in nets along their spawning beds. Now these spawning grounds are reported to be deserted by the fish, and certainly sawdust cannot be blamed for their disappearance. Higher up the river, in Round Lake, the October fishing is still good, solely because there are fewer settlers and less fishing.

CONCLUSIONS.

1. Strong sawdust solutions, such as occur at the bottom of an aquarium, poison adult fish and fish fry, through the agency of compounds dissolved out of the wood cells.

2. The overlying water in such an aquarium does not at first kill fish. After about a week it does kill, but solely through suffocation, the dissolved oxygen having all been used up.

3. Bacteria multiply enormously throughout all parts of such an aquarium, and through oxidation change the poisonous extracts to harmless compounds. Mosquito larvæ live on the bacteria. No doubt, in natural pools, other aquatic insect larvæ live on bacteria also.

4. Subsequent aëration and sedimentation of sawdust water purify it, so that fish can live in it without injury.

5. Since adult fish and black bass fry both refused to be driven into pine extracts in the bottom of an aquarium after they had experienced its poisonous effects, we may infer that fish would desert a river much polluted with sawdust, going down stream and into tributaries to escape from the disagreeable influence of sawdust extracts.

6. No stream can be pronounced off hand as poisoned by sawdust. Each stream must be studied by itself and the varying conditions must be thoroughly understood before a judgment can be pronounced. The chief things to be considered are (1) the quantity of sawdust, and (2) the volume of water into which the sawdust is discharged. Subordinate conditions are the rapidity or sluggishness of the stream, the amount of sunlight or shade, and the character of the water, whether from agricultural lands or from primitive forests.

7. Further observations and studies along sawdust polluted streams and rivers of Canada are urgently needed before more definite conclusions can be reached.

ACKNOWLEDGMENTS.

Acknowledgment is due to Toronto University, the Public Library, Toronto, and the Canadian Institute, for the privilege of consulting their libraries in order to write the historical part of this report.

I am under special obligations to my colleague, Prof. J. C. Connell, M.A., M.D., for the large number of minnows which he procured for me, and which were so indispensable for the laboratory experiments.

Dr. John Waddell and Mr. C. W. Dickson, M.A., both of the School of Mining, Kingston, rendered valuable aid in determining the amount of solid matter in sawdust water.

The Ontario Fisheries Department facilitated my task on the Bonnechere by instructing their overseers to assist me in every way possible.

APPENDIX TO DR. KNIGHT'S REPORT ON SAWDUST AND FISH LIFE.

BACTERIOLOGICAL EXAMINATION OF SAWDUST WATER IN SHADE AND IN SUNSHINE.

Examination of sawdust water in aquarium made July 31st, 1902.

Two agar plates made. The *first* averaged 3,300 colonies of bacteria per cubic centimetre. None of the colonies were spirilla which were present in large numbers in direct microscopic examination of the water. The chief colonies were those of a spore bearing bacillus, a variety evidently of B. Subtilis ; also a few sarcinae, particularly one like Sarcina Lutea. The second plate averaged 3,570 colonies per cubic centimetre. In general characters they were the same as in the first plate.

AUGUST 4TH, 1902. *Water in aquarium.* Agar plates averaged 3,570 colonies per cubic centimetre. These were in all respects like those of July 31st.

Same water in sunlight since July 31st. Agar plates average 4,200 colonies per cubic centimetre. These colonies contain the same bacteria as in the aquarium water, but in fewer numbers. Further, there is present a fluorescent bacillus, making up half the number of colonies present.

AUGUST 8TH, 1902. *Water in aquarium.* Agar plates develop 7,870 colonies per cubic centimetre. These colonies are of the same type as those found on previous plates with the addition of about 1,000 colonies of B. Mesentericus Vulgatus per cubic centimetre.

Water in sunlight. Agar plates develop 37,070 colonies per cubic centimetre. These consist mainly of B. Fluorescens Liquescens ; also of Sarcina Lutea, and an occasional colony of B. Subtilis.

W. T. CONNELL,
Prof. of Bacteriology.

THE BACTERIAL CONTAMINATION OF MILK AND ITS CONTROL.

BY F. C. HARRISON, PROFESSOR OF BACTERIOLOGY, ONTARIO AGRICULTURAL COLLEGE.

(*Read 28th February, 1903.*)

MILK as sold in cities, towns or villages contains a varying number of bacteria according to its age, the amount of sediment in it, and the temperature at which it has been kept. Soxhlet,[1] Uhl,[2] Backhaus[3] and others have shown that the more dirt or sediment, the more bacteria there will be, and Renk[4] has given us some interesting experimental data on the amount and kind of filth present in ordinary market milk. This filth is largely made up of excrementitious matter, vegetable fibres, epithelial debris, hairs of the cow, dust particles, etc., and the amount of filth contained in a litre of milk furnishes a positive index of the degree of cleanliness observed in the dairy stable. Renk found the following amount of dried impurities in the milk supply of the following German cities: Leipzig, 3.8 milligrams; Munich, 9 milligrams; Berlin, 10.3 milligrams; Halle, 12.2 milligrams per litre; and Hird[5] found in the Washington, D.C., milk supply, 5.30 milligrams of filth per quart.

Backhaus[3] has also shown that 50 per cent. of fresh manure dissolves in milk and does not appear as sediment; and therefore the weight of undried filth in all these samples would have been more than doubled. This investigator has also determined by actual tests that the daily milk supply of Berlin, Germany, contains about 300 pounds of dirt and filth. Further, many of the bacteria derived from such sources are very harmful, for not only are such fecal bacteria concerned in the intestinal troubles of infants, but they also give rise to abnormal fermentations in butter and cheese, producing taints, off-flavours, and decomposition products in these foods.

For the guidance of the dairyman who buys milk for sale, and for the housewife, Renk[4] suggests the following rule: If a sample of milk shows any evidence of impurity settling on a transparent bottom within two hours, it is to be regarded as containing too much solid impurities.

When we examine the results relative to the number of bacteria in European market milk, we are at once struck by the enormous numbers that are frequently present.

Thus, Clauss[6] found that the number of germs per c.c. of Wurzburg milk ranged from 222,000 to 2,300,000. The average was between one and two millions per c.c. Knopf[7] found from 200,000 to 6,000,000 per c.c. in the milk of Munich. Bujwid[8] examined the milk of Warsaw, where there was an average of 4,000,000 per c.c. In the milk immediately after it was drawn from the cow he found 10,000 to 20,000 per c.c. In Amsterdam, Geuns[9] found 2,500,000 per c.c. in fresh milk. Renk[10] examined the market milk of Halle and found from 6,000,000 to 30,700,000 per c.c. Uhl[11] in 30 tests of Giessen milk found from 83,000 to 169,600,000 per c.c. In the month of June he found an average of 2,900,000 per c.c. The average in May was 22,900,000. Uhl explains this difference by the supposition that the cows and stables were kept clean during this latter month, and there was less night's milk mixed with the morning's.

Knochenstiern[12] examined more than 100 samples of the milk of Dorpat. He divided the samples into four classes, according to their sources. The averages of the numbers in the several classes ranged from 10,000,000 to 30,000,000 per c.c.

The milk supply of Helsingfors was studied by Hellens,[13] who found in samples taken in the summer from 20,000 to 34,300 bacteria per c.c., while in the winter the bacterial content ranged from 70,000 to 18,630,000 and averaged 2,111,000 per c.c. About 60 per cent. of the summer samples contained over 1,000,000 bacteria per c.c., against 35 per cent. in the winter samples.

Rowland[14] found in twenty-five samples of London, England, milk an average of 500,000 bacteria per c.c.

Sacharbekoff[15] examined more than eighty samples of St. Petersburg milk. The number of bacteria ranged from 400,000 to 115,300,000 per c.c., with an average of 16,596,000.

Conn[16] has already pointed out the fact that the market milk of American towns and cities contains fewer bacteria than are to be found in European supplies ; and he has explained as the reason for this difference, the free use of ice in North America.

Sedgwick and Batchelder[17] examined a number of specimens of milk from Boston. They found, as an average of several tests that milk obtained in a clean stable from a well-kept cow, milked into a sterilized bottle, contained 530 bacteria per c.c. ; but when the milking was done under the ordinary conditions of farm practice, the number of bacteria reached on the average, 30,500. From fifteen samples of milk obtained from the houses of people in the suburbs of Boston, the average was 69,000 germs; from fifty-seven samples from milkmen the average was 2,350,000, and from sixteen samples secured at groceries, the average was 4,577,000 bacteria per c.c.

In 1901, Park[18] reported on the milk supply of New York. He found that during the coldest weather the average was about 250,000 bacteria per c.c., during cool weather about 2,000,000, and during hot weather about 5,000,000. Regarding the harmfulness of these bacteria the writer cites the universal clinical experience "that a great many children in cities sicken on the milk supplied in summer, that those who are put on milk that is sterile, or that contains few bacteria, as a rule, mend rapidly, while those kept on the impure milk continue ill or die."

Leighton[19] determined the number of bacteria in the milk supply of seventeen dairies at Montclair, N.J., the investigation extending over a period of three years. In dairies of the most approved type the average number of bacteria per c.c. was below 15,000. Poorly equipped dairies, in which the owners had endeavoured to do their utmost to produce a pure product with the crude means at hand, gave an average of between 40,000 and 70,000 per c.c.; and in those dairies in which neither good equipment nor good intentions prevailed, the average number of bacteria was over 180,000.

McDonnell[20] sampled 352 lots from eleven American cities. The worst samples were found in restaurants, and with small retail dealers. Twenty-eight per cent. of all samples contained less than 100,000 bacteria per c.c., while 34 per cent. had less than 500,000 per c.c.

Loveland and Watson[21] found in the supply of Middletown, Conn., from 11,000 to 85,500,000 per c.c.; and milk as delivered by milkmen to their private customers in the city of Madison, Wis.,[22] ranged from 15,000 to 2,000,000 organisms per c.c., varying mainly with the seasons of the year.

The writer[23] examined about twenty samples of Guelph market milk, a few years ago, and found an average of 650,000 bacteria per c.c.

Eckles[24] has made a bacteriological study of the milk supply of a creamery. He found from 1,000,000 to 5,000,000 organisms per c.c. in winter, and in summer from 10,000,000 to 80,000,000 per c.c.

During the summer of 1901,[25] whilst investigating an affection known as bitter milk in a large cheese factory, the writer had the opportunity of analysing the milk of ninety-six patrons who delivered milk to the factory. The results of this examination showed that the mixed milk of each patron contained from 4,000,000 to 30,000,000 of micro-organisms per c.c. Astonishingly large numbers (from 100,000 to 5,000,000) of the Colon bacillus were found in many samples.

Having seen from this review of the bacterial content of European and American milk supplies, let us now examine the sources of this contamination.

The bacteria which find their way into milk come from :—(1) The fore-milk; (2) The animal and milker; (3) Dusty air; (4) Unclean utensils. And, as Russell remarks, "the relative importance of these various factors fluctuates in each individual instance."

I.—CONTAMINATION FROM THE FORE-MILK. (*See Fig. 1*).

The constant presence of bacteria in freshly-drawn milk is a matter of considerable importance, and this fact helps to explain the ineffectual attempts to obtain milk in commercial quantities uncontaminated by bacteria. At the same time it has been but very recently that investigations as to the number and nature of the organisms that gain access to the milk through their localization and multiplication in the milk ducts, have been made. The first recorded experiments are those of Leopold Schultz[26] in 1892. He examined milk bacteriologically at the first of the milking, in the middle of the milking and at its close. This examination consisted merely in counting the number of bacteria present, and as a result, the following figures were determined:—The first milk contained from 55,000 to 97,200 germs per c.c.; the middle milk from 2,000 to 9,000 germs per c.c.; and the last milk was in some cases sterile, and sometimes contained about 500 germs per c.c. The number of germs in the last milk, he says, depended upon the quickness with which the milking was done. When done quickly, all the germs were washed out, so that "the last milk was often, but not always, sterile."

Gernhardt[27] investigating the same subject found a larger number in samples from the middle of the milking than at the beginning. To explain this result, as well as to explain irregularities in the numbers, he suggested that the bacteria made their way up through the milk-ducts of the teats, through the cistern, and into the smaller ramifications of the ducts which connect the cistern with the ultimate follicles. As many of the colonies so formed are not easily removed, they are not found in the first milk, but appear later when they have become broken up by the persistent movements of milking.

Von Freudenreich,[28] on the other hand, states that when in the udder milk is free from bacteria, except when the milk glands are in a diseased condition.

H. L. Bolley and C. M. Hall,[29] in their studies of the bacterial flora of the milk of ten healthy cows, isolated sixteen distinct species of bacteria, some of which were common to both the first and last milk, and others to only one of these. All the micro-organisms found were bacteria, and none were found which produced gas.

Russell[30] in his text-book on Dairy Bacteriology, published in 1894, states that he has found an average of 2,800 germs per c.c. in the fore-milk, while the average of the remainder of the milk only had 330 germs per c.c. In characterizing this, he says that " the number of species is usually small, one or two kinds usually predominating to a large degree. Those that are commonly found are those that produce lactic acid, as these microbes find in milk the best medium for their growth."

Gosta Grotenfelt,[31] however, in his text-book on the " Principles of Modern Dairy Practice," reasserts the statement of Von Freudenreich that when the milk is drawn from the udder of a healthy cow, it is germ-free or sterile.

Rotch[32] concludes, from an examination of the bacteria found in four cows' milk, that the bacteria do not necessarily come from external sources, but that they may also come from some part of the milk-tract between the udder and the end of the teat. The few colonies, however, obtained in the plates from the latter half of the milkings, are considered as possible contaminations between the " cow " and the " plates."

Moore[33] states that in investigations made upon this subject, he found that, in addition to the bacteria in the fore-milk, the last milk from at least one-quarter of the udder in every case contained bacteria.

Conn,[34] reviewing this subject, says that the different results of many of these early experiments are due to the small quantities of milk taken, while in the latter experiments large quantities have been taken. He adds, " Undoubtedly the milk-gland of the healthy cow produces milk which is uncontaminated with bacteria, but the large calibre of the milk ducts makes it possible for bacteria to grow in the duct to a considerable extent, so that it becomes a matter of extreme difficulty to obtain milk from the cow, even with the greatest precautions, which will not be contaminated."

Harrison,[35] in 1897, in a report of investigations upon this subject stated. " When milking is done there remains in the teat of the cow a little milk that affords nourishment to any bacteria that may come in contact with it through the opening at the end of the teat." The average of a number of analyses made by him shows the presence of 18,000 to 54,000 germs in the fore-milk, and 1,000 to 3,000 in the after-milk.

Experiments have also been conducted on human milk by Palleske,[36] Honigmann,[37] Knochenstiern,[38] and Ringel,[39] but all of these have independently found it impossible to get human milk from the mammary gland in such a way as to be sterile.

The most recent work upon this subject has been done by Moore and Ward,[40] of Cornell University. They investigated the source of a gas and taint producing bacterium in cheese curd for a certain factory that was troubled with " gassy curd." They easily located the trouble in the herd of a particular patron. On inquiring into the history of the herd it was ascertained that at the time of parturition, the placentae had been retained by a number of cows, and these had been allowed to decompose in the uterus. It was soon after this that the "gassy curd" began to appear. A thorough bacteriological examination located the bacilius which was the cause of the "gassy curd" in the udders of the cows of the herd; and it seemed very probable, though, of course, not demonstrable, that it had gained access to the udders from the decaying placentae.

Subsequent to this, Ward conducted further experiments, and in an article on " The Persistence of Bacteria in the Milk-Ducts of the Cow's Udder,"[41] he concludes, (1) " certain species of bacteria are normally persistent in particular quarters of the udder for considerable periods of time, and (2) it is possible for bacteria to remain in the normal udder and not be ejected along with the milk." These conclusions controvert the statement previously made by Von Freudenreich and Grotenfelt that the milk-ducts are always sterile at the close of milking, becoming tenanted from the outside alone by organisms which chance to come into contact with the end of the duct.

The results of still later investigations by the same author are published in a bulletin on the " Invasion of the Udder by Bacteria."[42] In these investigations a bacteriological examination was made of the udders of milch cows slaughtered after reacting to the tuberculin test. In all cases the udders were perfectly normal. Just before slaughtering the animals were milked as thoroughly as possible and samples of the milk taken, and a bacteriological examination made. After slaughtering, a similar examination was made of the tissues of the udder. In all cases, even in the upper third of the udder, bacteria were found, and they were identical with those found in the milk. He concludes that "milk, when secreted by the glands of the healthy udder, is sterile. It may, however, immediately become contaminated by the bacteria which are normally present in the smaller ducts of the udder." However, "the bacteria so far found in the interior of the udder do not affect milk seriously. This, however, does not preclude the possibility that forms more injurious to milk may invade the udder."

From the above resumé it is apparent that widely different results have been obtained by different investigators, and it has been a very interesting study to see whether the experiments conducted at Guelph would throw any light upon these divergencies in results.

The plan of experiment has been as follows: For a number of days samples were taken from the fore and after milk of a number of cows on the College Farm. The samples were collected in sterile test-tubes, and previous to taking the milk, the flank, udder, and teats of the cows were thoroughly washed with a 1-1000 solution of mercuric chloride. Gelatine plates were then made from these samples, and afterwards the number of colonies counted and the different species isolated and cultivated on the various media. It soon became apparent that while several species were more or less constant in the udders of all the cows, yet there were many variable species present in the milk of some cows that were not present in that of others, and not even in the same udder on two successive days. Therefore, in making a systematic study, it was deemed best to confine our attention to those species that were more or less constantly present in the milk of all the cows, and to make a complete study of those existing in the udder of one particular cow.

The number of bacteria present in both the fore and after milk of the various cows, and of the same cow, and even in the different quarters of the udder of the same cow, were so widely different that little stress can be laid upon an exact enumeration.

The following samples, which are typical of many others, will illustrate the point:

Cow No. 1.	*Determination 1.*
Fore-milk, right front teat	86,400 per c.c.
" " hind "	120,000 "
Strippings, " front "	40,800 "
" " hind "	57,000 "

	Determination 2.
Fore-milk, right front teat	48,000 per c.c.
" " hind "	24,080 "
" left front "	22,400 "
" " hind "	35,100 "

Cow No. 2.	*Determination 1.*
Fore-milk................................	200-500 per c.c.
After-milk................................	0-100 "

The results of a large number of determinations, of which the above are typical, showed on an average 25-50,000 germs per c.c. in the fore-milk. The numbers in the strippings or after-milk varied greatly with the manner of taking. For example, when the milking was done quickly, but very few and sometimes no colonies were found in the "strippings," whereas, when the milking was done slowly and some time lost before the samples from the last milk were taken; the number of bacteria was very variable, being in one case as high as 57,000.

The important point, therefore, is not the exact number, but the fact that bacteria were found in large numbers, not only in the fore, but in the middle and last milk of nearly all the cows tested.

The number of species present in the udders of cows is very small. Of this number some are more or less constantly present, whereas others are very variable in their presence. Of those species which are present, the characters are in many cases so slightly marked that their identification proved a very difficult matter. In fact, with the exception of *Bacillus acidi lactici*, not a single species discovered was strongly characterized. A number had a very little or no effect upon milk, and even the digestors were in every case very slow digestors.

B. acidi lactici (Conn No. 206), *B. acidi lactici* (Conn 202), and *B. lactis aerobans* (Conn 197) are the only ones that have been found constantly present in the samples, and in every case they have composed at least 95 per cent. of the germs present. The following species have been only more or less variably present, and in no cases have been found in large numbers.

B. halofaciens (N. Sp.). This bacterium approaches in characteristics *Bact. annulatum* (Wright), but differs from it in several details, so that we do not hesitate to call it a new species. The name refers to the characteristic halo found in gelatin cultures. As this bacterium was of quite frequent occurrence, we made butter from cream ripened with a culture of it, and found that the flavour of the butter, while not strong was quite disagreeable. At the same time, its presence in relatively small numbers, and the fact that the flavour was not strongly marked, make this an inconsiderable item so far as the "natural ripening" of such milk is concerned.

Micrococcus varians lactis (Conn 113 and 104). Conn, in speaking of this coccus, considers it one of the most important of dairy species, and suggests that it likely exists in the milk-ducts.

Bacillus No. 18, Conn. This species appeared very frequently in all samples examined, but never in very large numbers. In old gelatin and agar cultures, spores appeared at the ends of the bacilli. Two cultures from these heated for ten or fifteen minutes at a temperature of 85° to 90° germinated in one day. The species is very similar to *Bacillus No. 18 Conn*, but differs in that growth on potato is not spreading, and no spores are found in potato culture.

Bacillus No. 7. This bacillus was quite constantly present for some weeks, but afterwards disappeared. It seems to resemble *B. cremoris* (*43*) (*B. lactis No. 9 Flugge*), but differs in its effect on milk, so that it would appear to be an allied species.

Bacterium No. 8. The gelatine plate colony appears very similar to No. 7, but the organisms otherwise differ, both morphologically and culturally in many particulars. Like No. 7 it appeared quite constantly for some weeks and then disappeared.

B. exiguum (Wright).[44] This bacterium was found almost constantly present in the milk of one of the cows tested, but was never found in that of any other. There were never more than from one to four colonies per plate present, so that its effect on the milk was very inconsiderable. Its similarity to *Bact. exiguum* (Wright) is most marked. Wright isolated this bacterium from water, but the fact that he found its optimum temperature to be 36° and that it is a facultative anaerobe does not make it at all surprising that it should be found in the udder of a cow.

Micrococcus No. 10. This coccus comes in the same class as Conn's 167, and may be identical with it. The most marked variations are the gelatin colony, and the fact that in no culture of this germ was there the slightest indication of a yellow colour. At the same time it agrees in morphological characters, and especially in the fact that, although a liquefying coccus, it fails to curdle milk.

Like several of the forms previously described, this coccus was found to be present for some time in the samples taken, but afterwards, in a few weeks, completely disappeared. In no cases was it present in large numbers.

Comment has already been made upon the fact that by far the largest number of species determined in all the samples tested, were lactic acid species, and that other species, although more or less constantly present, were not invariably so, and never in very large quantities. This is a most important, practical consideration, for it means that, although by the most scrupulous care, it may not be possible to procure milk free from germ-life because of those that are present in the udder of the cow, yet the species that gain access to the milk through this source are, for the most part, beneficial ones. In Bulletin No. 21, 1900, of Storr's Agricultural Experimental Station, Conn speaks of a method, now widely adopted in American dairies, for procuring what is known as a "natural starter." The method consists in drawing milk, just as has been done in all the examinations we have made, into sterilized flasks, and using cultures from these as starters. Conn says, "there can be no question that the use of natural starters thus made has been a very decided advantage to the buttermaker," for the reason that "the bacteria which are within the cleanly cow's udder and thence get into the milk, are most commonly of the desired character." There is, no doubt, some uncertainty about this method, but so far as all examinations conducted by us are concerned, the cultures so obtained would be good ones, being largely composed of lactic acid species.

While the large per cent. of lactic acid species present is the paramount characteristic of the bacterial flora of freshly drawn milk, yet there are other peculiarities of considerable, if not equal, importance.

By reviewing the description of the species determined, it will be noted in every case that, although each species would grow at room temperature, yet the optimum temperature was in the neighbourhood of 37°. This fact was well demonstrated in comparing gelatin plates, made from the general milk supply, with those made from the aseptically drawn milk. These plates cannot be kept at a temperature higher than 22°, and it was most marked that when plates from the former were quite covered with bacterial growth, those from the latter were still clear. On the other hand, when agar plates were used and kept at a temperature of 37°, the order of growth was slightly reversed. This explains facts that were noted in reference to the keeping quality of the aseptically drawn milk, as compared with that of the general milk supply; for, when kept at room temperatures, the former remained good considerably longer than the latter; whereas, when kept at 37°, both became curdled in less than twenty-four hours.

Another marked characteristic of all the species was that they were facultative anaerobes. The anaerobic faculty was especially marked in the two species, Nos. 1 and 2, that were found to be so uniformly present in all the milk tested. This is just what one would naturally expect, for the conditions in the udder must be largely anaerobic conditions. In virtue of these conditions, and possibly other undetermined conditions, the udder, so to speak, exerts a selective action upon the bacteria which may be temporarily present in it. In this they are, of course, aided by the mechanical expulsion of bacteria in the process of milking.

In order to throw a little light upon this problem, experiments were conducted in inoculating udders with well-marked but harmless bacteria, which could be easily recognized by their cultural characteristics. *Bacillus prodigiosus* and *B. exiguum*, both of which were marked by their pigment production, were the ones experimented with. Cultures of these were smeared upon the ends of the teats, so that the bacteria might work their way up into the udder, just as any other germ might which comes in contact with the ducts of the teat. In the case of *B. prodigiosus*, about 20,000 per c.c. were present in the fore-milk at the first milking eight hours after inoculation. By the third milking only a few were present, and after that it disappeared completely. The experiment was repeated with *B. exiguum* with similar results, although a smaller number—240 per c.c.—were present in the first milking, and by the fourth milking it had disappeared. No doubt the small number was due to the fact that this germ grows more slowly than *B. prodigiosus.*

In view of Ward's discovery of *B. fluorescens liquefaciens* in the udders of certain cows, it seemed advisable to attempt to colonize this germ in the udder, and a bouillon culture was smeared upon the ends of the teats of a cow in the manner already described. This bacillus was discovered in the fore-milk six hours after the teats were smeared, but was not found in the fore-milk of the second and third milkings.

It does not seem probable that an aerobic bacterium of this character is able to live and compete with facultative anaerobic bacilli. Further, the optimum temperature for the fluorescing bacterium is not 37°.

Possibly, by continuous experimentation, we might have finally discovered a species which would persist in the udder, but at the same time, the bacteria chosen have evidently fared much the same as other more or less injurious forms which may occasionally find temporary lodgment in the udder. Exception, however, may occur, as we have the gas and taint producing bacillus located by Ward and Moore in the udders of the cows of a particular herd.

Another fact that may have a bearing on this problem is that normal healthy organs, taken from the body immediately after death, may contain bacteria which are capable of development. Thus, Ford[45] has shown that 80 per cent. of healthy organs, removed from killed guinea pigs, rabbits, dogs and cats, contained living bacteria. No udder tissues were examined by him, and in order to ascertain if bacteria existed in the udder of healthy animals a few experiments were made along these lines, but they are open to criticism because it is impossible to say, with any degree of certainty, that the bacteria found came from the animal's glands or blood, or from infection through the teat. However, by selection of cows which had been dry for several weeks before slaughter, the latter objection is to some extent overcome. The liver was examined at the same time, and its bacterial content, if any, noted.

The methods employed in this work were essentially those used by Ford—a large piece of the tissue to be examined was excised with a sterilized knife, placed in a sterile jar, and immediately taken from the slaughter house to the laboratory. Small pieces of tissues were then cut from *the inside* of the large piece with sterilized knives, and then held in the flame of a Bunsen burner with sterilized forceps, until the whole of the outside of the piece was well scorched. The piece was then transferred to beef bouillon or peptone whey bouillon, and the preparations placed in the incubator at 37°. On the fourth day gelatine plates were made from the different pieces of tissue, and the bacteria, if any, were isolated.

I. An aged cow, dried up five weeks before slaughtering, udder small, with considerable fatty tissue. All organs perfectly healthy.

	Bouillon.	*Peptone Whey.*
Liver	+	+

Subsequent plating and sub-cultures gave :—

B. mesentéricus vulgatus.

B. subtilis, and a Micrococcus, identity not established.

	Bouillon.	*Whey Peptone.*
Udder	+	+

Plates gave colonies of :—

B. subtilis.

Micrococcus (sp ?).

II. An aged cow, dry for some time, the butcher not knowing the exact length of time. Udder of a fair size, and well formed. All organs apparently healthy and normal.

	Bouillon.	*Whey Peptone.*
Liver	+	+

Plates gave colonies of :—

B. lactis aerogenes.

Proteus.

	Peptone.	*Whey Peptone.*
Udder	—	—

III. An aged cow, dry for four weeks previous to slaughter. Udder fair size. Organs normal and apparently healthy.

	Bouillon.	*Whey Peptone.*
Liver	+	+

Gelatine plates gave colonies of :—

B. subtilis.

A spore-bearing bacillus, which produces no effect in milk.

	Bouillon.	*Whey Peptone.*
Udder	+	+

Gelatine plates gave cultures of :—

Micrococcus varians lactis.

These results, whilst agreeing with Ford's, are not sufficiently authoritative to allow us to assert positively that the bacteria found in the udders of the two cows came from the blood or lymph stream, rather than through the teat, but in conjunction with the results obtained by Ford, they threw doubt on the supposition that all udder infection comes originally through the orifice of the teat. It is also noteworthy that a spore-bearing bacillus belonging to the *subtilis* group, and several micrococci were isolated by Ward from udder tissue. Another fact, which is difficult to explain and which may possibly have some influence on the bacterial content of the udder in its normal condition, is the strong germicidal power of freshly drawn milk. This property was first noticed by Fokker,[46] and subsequently confirmed by Freudenreich,[47] and quantitative studies of freshly drawn milk, inoculated with various bacteria, show that an actual destruction of bacteria took place. This germicidal property has been shown to exist in the milk-serum, and is evidently allied to the similar bactericidal property of blood, for Brieger and Ehrlich[48] and Wassermann[49] have found that the milk of immune animals can confer immunity.

If then, this germicidal power exists in fresh drawn milk, it is certain to be present whilst the milk is still in the udder, and may inhibit or prevent the rapid multiplication of adventitious bacteria, which penetrate up the opening of the teat. Although we have frequently found large numbers of lactic acid bacteria in freshly drawn milk, yet the reaction of this milk is never acid.

De Freudenreich[47] has also shown that the bactericidal power is not the same for all species, that whilst the cholera vibrio, the typhoid bacillus, and even *B. Shafferi* (a colon bacillus), are destroyed in large numbers, the bactericidal action is less pronounced on lactic acid bacteria. These facts may possibly explain why the germs of the colon type are so seldom found in the healthy udder, for we know that the teats and udder of the cow are constantly brought in actual contact with particles of manure, and even the hands of the average milker are soiled with stable filth, which undoubtedly contains colon bacteria.

It might be reasonably asked if the advice, commonly given to those who wish to procure milk as near sterile as possible, to milk the first few streams in a separate utensil, is good. And in reply we would say, decidedly yes, for not only is the number of bacteria in the fore-milk much in the excess of the bacteria found in the rest of the milk, but frequently the number of species found in the fore-milk is considerably larger than that in the after-milk. (*See Fig. 2*).

In reviewing the subject there can be no doubt that the number of bacteria present in the milk as it exists before being drawn from the udder is somewhat startling, and were nothing more than an enumeration of the germs given, there might be some occasion for alarm.

However, a systematic study of the germs proves that, with the possible exception of rare cases, this source of bacterial life is much more beneficial than baneful to the average consumer of milk and its products.

CONTAMINATION FROM ANIMAL AND MILKER.

A prolific source of contamination is from the animal and milker, and the following realistic statement,[50] describes a condition which unhappily is only too common on many farms :—

"The day has gone by when a pretty milkmaid went, in clean white apron and with shining milk pail, to milk the cow with the crumpled horn, out among the buttercups on a dewy morning. Instead, some old fellow stumbles out of the house and to the barn, with the stump of a clay pipe in his mouth, and wearing overalls and boots saturated and covered with the filth acquired by a winter's use. When he reaches the barn he selects some recumbent cow, kicks her until she stands up dripping and slimy, and as he is a little late and the milk will hardly have time to cool before the man who carries it to the city will come along, he does not stop to clean up behind the cow, but sitting down on a stool, proceeds to gather the milk, and whatever else may fall, into a pail which perhaps is clean and perhaps is not. Of such refinements as washing the udder of the cow or wiping her flanks he has never heard. If he has, it is only to scoff. Then he stands the milk behind the cows. That is bad enough, but it is not all the story. Everyone knows that in straining the milk the strainer becomes obstructed, more or less, with dirt and filth, and when the milk does not run fast enough, he would be a rare milker who hesitated to scrape away a place with his fingers so that the milk might run more freely. Those who have seen certain fingers, as I have, know what that means."

This description seems hardly credible, but when one visits an ordinary cattle stable, he is prepared to believe almost anything under this head. The hair of cows, even those that are kept very clean, swarms with bacteria. (*See Fig. 3*). Many hundreds may be isolated from a few particles of hair, and this fact alone shows the importance of keeping cows clean, well carded and well brushed. When in this condition, they are not so liable to lose hairs, nor are the hairs so easily dislodged during the movements of milking. The particles of manure and filth which cling to the sides, flank, udder and tail of animals are laden with germ-life. Wüthrich and Freudenreich[51] have found far more bacteria in manure when the animals are given dry food than when kept upon grass, and the most numerous species present were the colon bacillus, the hay bacillus, and other species able to liquefy gelatine, and peptonise casein. Great care should be taken in the construction of cow stalls. If they are too long the hind-quarters of the animal are apt to be plastered with manure when she lies down; and when too short, the hind-quarters and tail find their resting place in the gutter. (*See Fig. 4*).

The milker, too, is not always above reproach. Clothed in dust-laden garments, used for all kinds of farm and stable work, without even washing his hands, he does the milking as he would do any other job on the farm. Too often the milker has the filthy practice of moistening his hands and the cow's teats with milk. Freudenreich[28] reports some experiments in which the germ-content of milk was reduced from several thousands to 200 where the hands were well rubbed with vaseline before milking, and as pointed out by Russell,[52] a pinch of vaseline not only helps the milker to obtain a firmer grasp, but also prevents scales or dirt from being rubbed from the teats, and its effect on sore or chapped teats is healing.

METHODS OF PREVENTION.

Contamination from the milker can, however, to a very large extent, be prevented by moistening thoroughly the flanks and udder of the cow before milking. Germs cannot leave a moist surface, and the dust-like particles are thus held in place. (*See Fig. 5*). The following instructive experiment is cited by Russell[52]:—

"When the animal was milked without any special precautions being taken, there were 3,250 bacteria per minute deposited on an area equal to the exposed top of a ten-inch pail. When the cow received the precautionary treatment, as suggested above, there were only 115 bacteria per minute deposited on the same area. This indicates that a large number of organisms from the dry coat of the animal can be kept out of the milk if such simple precautions are carried out." It has been frequently found that the germ-content of the milk in the pail is increased from 20,000 to 40,000 bacteria per minute during the milking period by the dislodgment of organisms from the animal.

By diminishing the exposed surface of the milk-pail, (*see Fig. 6*), a considerable amount of dirt may be excluded, as it is obvious that less dirt will fall in a pail with a small opening. A number of different types of such pails are now in use, and Eckles[53] and other investigators have given us experimental data on the subject. Thus 43,200 bacteria per c.c. were found in the milk drawn in a common pail, as against 3,200 per c.c. in the covered pail. The milk soured in forty-three hours in the first case; sixty-four hours in the latter case.

The milker should put on a clean, loose, cotton or linen smock over his clothes, and invariably wash his hands immediately before milking. The milking smock should be washed frequently and should be kept, not in the open barn or stable, but in some place as far removed as possible from all kinds of dust and dirt. The practice of moistening the hands or cow's teats with milk should be scrupulously avoided.

MILKING MACHINES.

Of late years several milking machines have been introduced to obviate the difficulty in obtaining milkers, and to lessen the time taken in milking. Such machines, in order to be a success, must do the milking naturally, quickly, thoroughly and without any annoyance to the cow. Further, milk drawn by such a machine must be of good keeping quality, and the machine must be adaptable to the requirements and arrangements of an ordinary dairy farm.

When first introduced many dairymen expressed the opinion that these machines would guard against the admittance of all dirt, but unfortunately this requirement has not been fulfilled by a machine installed in the Dairy Stable of the Ontario Agricultural College.[54] This machine, called the " Thistle Milking Machine," (*see Fig. 7*), was in more or less constant use in our stable during the summer of 1899, and we took advantage of this opportunity to make bacteriological analyses of the milk milked with the machine as compared with milk drawn by hand milking. We found that the machine milk had a far larger germ content than that milked in the usual manner, and after making a direct comparison between the number of bacteria in machine milk and the number in hand milk, we found that the proportion varied greatly, from three to twenty times as many bacteria being found in the machine milk as in the hand drawn milk. The averages were as follows :—

Machine Milk.		*Hand Milk.*	
No. of Analyses.	No. of Bacteria in c.cm. of *morning's milk.*	No. of Analyses.	No. of Bacteria in c.cm. of *morning's milk.*
161	141,600	78	10,600
	Evening's Milk.		*Evening's Milk.*
74	165,000	16	12,900

These results were greatly in favour of hand-milking, and the large number of bacteria found in machine milk was attributed to three causes:

1. When the rubber teat-cups are fastened on the cow, a small portion of the hairy coat of the udder is included in the cup, and no matter how clean the animal is, germs are sure to be present on this coat in considerable numbers, depending upon the cleanness of the udder.

When the suction of the machine is applied, the force exerted naturally draws any loose or dry particles that may be on the teats and that portion of the udder within the cups, down into the milk. In this way, many germs on these particles gain access to the milk, and find in it suitable conditions for their growth and multiplication.

2. The teat-cups and connecting tubes to the milk pail are made of rubber, and consequently cannot be scalded or steamed, as scalding water or steam would crack and spoil the rubber ; hence it is impossible to cleanse them thoroughly from germ life. They may look clean after being rinsed in warm water and kept in cold water, but they are certainly not bacteriologically clean, *i.e.*, free from germs ; and in the process of milking many of the germs on the inside of the rubber and in the crevices of the tubing are washed into the milk. Conclusive evidence on this is afforded by the fact that, time and again, germs that were constantly present in water in which the rubber tubing was kept between milkings, were also found in the milk.

3. In detaching the cups from one cow and putting them on another, attendants sometimes let them fall upon the floor of the stable, and in this way germ-loaded particles of dust and dirt get into the teat-cups and find their way into the pail as soon as the milking of the next cow begins. Of course, this may be put down to carelessness on the part of the attendants ; but in our experience, no matter how careful the transfer was made from one cow to another, instances of the cups falling occurred from time to time, and each time undoubtedly made a large addition to the germ content of the milk.

In 1898, the Highland and Agricultural Society of Scotland[55] offered a prize of £50 for the best milking machine. Only two makers entered their machines for competition, viz., Mr. W. Murchland of Kilmarnock, (the Murchland Milking Machine Company), (*see Fig 8*), and the Thistle Mechanical Milking Machine Company. The judges, after an exhaustive trial, awarded the prize to the Murchland Milking Machine, it having in every respect most effectually filled the conditions which they originally agreed should guide them in making their awards. In every instance the samples of milk drawn by this machine were found to keep satisfactory. After a lapse of forty-eight hours, they were found in no respect inferior to the samples of milk drawn by hand, in fact, if anything, rather superior in point of flavour. The judges regarded the Murchland machine as a practical success. On the other hand, the chief defect in the Thistle Milking Machine was the effect it had upon the keeping qualities of the milk. Most of the samples from it developed sourness in twelve to twenty-four hours, while samples drawn by hand from the same cows at the same time, and kept under precisely the same conditions, remained perfectly sweet for from thirty-six to fifty hours.

The Murchland Machine was also placed in competition for a prize of £50 at the York meeting of the Royal Agricultural Society[56] at York. In the opinion of the judges it presented such difficulties for efficient cleaning, that they were unable to report that it adequately fulfilled the requirements set forth in the regulations for these trials. No award was made in the competition.

CLEANING MILK BY THE USE OF A GRAVEL FILTER.

The gravel filter in most general use is the model used by the Copenhagen Milk Supply Company, (*see Figs. 9 and 10*). It consists of two enamelled iron tanks placed at different levels; a pipe in the form of a siphon has its long limb connected with the bottom of the upper tank, and its short limb with the bottom of the lower tank, so that the milk poured into the upper tank comes up as a kind of spring at the bottom of the lower one. On the bottom tank there are three layers of gravel, that in the lower layer being about the size of a pea; in the middle layer somewhat smaller, and in the third or top layer, a little larger than a pin's head. The layers are separated from each other by perforated tin trays. On the top of the uppermost layer of gravel are five thicknesses of fine cloth. The whole is kept in position by a pyramidal frame-work which presses down the tin trays. As the milk rises to the top of the tank, it passes into a large storage or mixing receptacle. These filters require the most careful management, and are generally taken to pieces immediately after use, when the gravel is washed in hot water until the water comes off clean. It is then steamed at a temperature of 202° F., after which it is spread out in shallow trays, and baked at a high heat. For the concluding operation, the gravel is placed in a winnowing machine, which drives off all particles of fine dust.

Schuppan[57] seems to have first called attention to the use of gravel filters as a means of reducing the number of bacteria in milk. He reported in 1893 that the bacterial content of milk was reduced 48 per cent. by sand filtration ; and at the meeting of the German Dairy Association in 1893, he strongly recommended the use of sand filters for removing dirt and germs from milk. The use of sand filters was, however, questioned by other members.

Backhaus,[58] whilst giving no numerical data, reports that these filters have no effect in reducing the number of bacteria in the filtered milk. The mechanical separation is good, all coarse particles, such as hair, straw, manure, etc., are arrested ; but the bacteria are washed out of the manure, and the milk contains more bacteria than before filtration.

In 1899, Dunbar and Kister[59] made an exhaustive study of the working of this class of filter. In twenty-two analyses of raw and filtered milk there were in seventeen cases more bacteria present after filtration, and in four cases fewer bacteria. A few examples of their results will suffice :

Raw Milk.	*Filtered Milk.*
80,000 per c.c.	60,000 per c.c.
793,000 "	44,100 "
95,000 "	49,400 "
819,000 "	94,000 "
Average 446,700 "	Average 61,800 "

In these cases filtration diminished the bacterial content of the milk.

Of the seventeen other samples, the bacterial content after filtration was increased, thus :—

Raw Milk.	*Filtered Milk.*
350,000 per c.c.	600,000 per c.c.
650,000 "	950,000 "
650,000 "	1,260,000 "
320,000 "	620,000 "
3,900,000 "	14,300,000 "

The average of the seventeen analyses was :—

Raw Milk.	*After Filtration through Gravel.*
1,300,000 per c.c.	5,567,000 per c.c.

CLEANING MILK BY CENTRIFUGAL FORCE.

Clarified milk, (*see Fig. 11*), or milk that has been passed through a separator, has been recently quite extensively advertised. The effect of this method of cleaning milk is similar to that of the gravel filters, and according to Backhaus,[58] 95 per cent. of the mechanical impurities (hairs, manure particles, etc.), are eliminated. The separator divides the milk into three parts, the slime which adheres to the bowl of the machine, the skim-milk and the cream. Several investigators have given us data of the number of bacteria which are found in these three products. Thus Popp and Becker[60] found the germ content per c.c. of the whole milk, to be 72,954; of the cream of this milk, 58,275; the separator skim-milk, 21,735; and the separator slime, 43,891.

Scheurlen[61] found in one litre of milk 2,050,000,000 of bacteria, and after separation 1,700 in the 200 c.c. of cream, 560,000,000 in the 800 c.c. skim-milk, and 18,000,000 in the 6 c.c. of slime.

Other investigators have also shown that centrifugation does not decrease the number of bacteria in milk. Thus, Fjord and Fleischmann[62] claim that centrifugal separation has little value as a means of purification, and Conn[63] states that " milk after passing through a centrifuge, although it contains less gross impurities, shows more bacteria than before. This is explained by the fact that masses are broken up, and large numbers of bacteria liberated, " and again," the same writer says, "centrifugal purification does not materially affect the bacteria, for there seem to be about as many after treatment as before."

Niederstadt[64] obtained similar results, for he found that by the centrifugal treatment of 300 litres of milk, about 130 grams of sediment were obtained. The cream was richer in bacteria than the sediment. The separator effected no purification of milk from bacteria, and 75 per cent. of the bacteria went into the cream.

Dunbar and Kister,[59] in an exhaustive series of experiments, found in four instances fewer bacteria after separation, the average of these four instances being as follows :—

Raw Milk.	*Centrifuged Milk.*
446,000 per c.c.	146,000 per c.c.

But in the remainder of the experiments, twenty-four in number, more bacteria were found in the separated milk, the averages in this case being :

Raw Milk.	*Centrifuged Milk.*
1,400,000 per c.c.	2,200,000 per c.c.

It would seem from these figures that the smaller the number of bacteria present in the whole milk, the more efficient was the separator in reducing their numbers.

Eckles and Barnes[24] have also investigated the purification of milk by the centrifugal separator. They found a large proportion of the bacteria removed by centrifuging, but no enhancement in keeping quality.

Russell in a private communication to the writer expresses his opinion thus :—" I do not think clarification is worth the trouble, unless the milk is exceptionally dirty.'

At the suggestion of the Ontario Department of Agriculture, we (my assistant, Dr. Streit and myself) have reinvestigated this subject.

A power belt separator was used, run at the speed indicated by the manufacturers. The milk came from farms in the vicinity, and was of average quality, similar to the ordinary factory supply. About 150 pounds of this milk were thoroughly mixed in a sterilized can with a sterilized stirrer. A half-pint sample of the milk was taken in a sterilized jar, the rest of the milk being put through a separator. The cream and skim-milk were caught together in a sterilized can, and were again thoroughly mixed with a sterilized stirrer, and another half-pint sample of the clarified sample was taken. Both samples were immediately carried to the laboratory, where suitable dilutions were made and plates poured.

The culture medium used was whey gelatine, with one per cent. of peptone. The plates were kept at 20° C. and counted at the end of forty-eight or seventy-two hours, depending on the size of the colonies. In most cases the plates were counted by each of us independently, so as to reduce the personal equation.

Each result given in the table is the average of four plates, and thus the gross average represents the numerical results obtained from 240 plates or analyses.

THE BACTERIAL CONTENT OF MILK BEFORE AND AFTER SEPARATION.

DATE.	BEFORE SEPARATION.		AFTER SEPARATION.		
	Total No. of Colonies.	Liquefying Colonies.	Total No. of Colonies.	Liquefying Colonies.	More bacteria after Separation + or less —
April 8	447,000	25,000	775,000	64,000	+
" 8	391,000	23,300	1,000,000	196,000	+
" 10	491,000	6,500	529,000	18,700	+
" 10	442,000	7,500	469,000	16,000	+
" 12	1,351,000	88,500	2,495,000	271,000	+
" 12	1,990,000	67,500	2,070,000	110,000	+
" 17	1,958,000	......	4,250,000	21,600	+
" 17	3,000,000	3,800	3,750,000	9,000	+
" 19	1,850,000	6,600	2,700,000	30,700	+
" 19	2,500,000	6,000	2,800,000	25,700	+
" 22	1,100,000	4,200	1,160,000	10,850	+
" 22	1,200,000	10,850	1,200,000	18,750	+
" 24	2,000,000	15,000	2,000,000	10,000	—
" 24	2,000,000	11,000	2,250,000	13,000	+
" 26	996,000	6,000	1,100,000	12,600	+
" 26	1,100,000	11,000	994,000	8,600	—
" 24	2,700,000	4,800	2,900,000	12,000	+
" 24	3,000,000	13,000	2,700,000	7,600	—
May 1	714,000	22,800	790,000	56,000	+
" 1	646,000	30,000	730,000	32,000	+
" 3	950,000	38,000	908,000	36,000	—
" 3	832,000	26,000	964,000	38,000	+
" 7	530,000	30,000	710,000	40,000	+
" 7	480,000	13,000	805,000	22,000	+
" 17	2,250,000	31,000	2,470,000	61,000	+
" 17	2,060,000	6,000	3,000,000	61,000	+
" 20	2,300,000	......	2,750,000		+
" 20	2,800,000	.....	2,300,000	.....	—
" 22	16,000,000	20,000	15,000,000	19,000	—
" 22	12,000,000	26,000	17,000,000	26,000	+
Average	2,359,000	19,800	2,759,000	44,540	+

400,000 bacteria per c.c. more in centrifuged milk.
24,740 liquefying bacteria per c.c. more in centrifuged milk.

A perusal of the table will show that on six occasions there were fewer bacteria after separation than before, and on twenty-four occasions more bacteria present after clarification than in the raw milk.

Another striking fact brought out by this investigation is the large increase of liquefying colonies in the separated milk. The bacteria which liquefy gelatine are usually harmful, some are spore-producing germs and they give rise to off flavours in both cheese and butter. Many of this class are present in manure, on particles of fodder, etc., and our results seem to show that these bacteria exist in clumps or masses in such material and the centrifugal process breaks these up and distributes them through the milk.

These results obtained at Guelph are identical with those obtained by Dunbar and Kister, and go to show that centrifugal purification, as far as bacteria are concerned, is ineffectual.

CONTAMINATION OF MILK FROM THE STABLE AIR.

Although it is difficult to separate contaminations from animal and milker and that from the air, it is better to consider the latter source of infection separately, as the number of germs floating in the air depends to a great extent upon the amount of dry fodder and straw that may be used in the stable. If manure is not frequently and thoroughly cleaned out, it gets dry and small particles from it help to swell the number of microbes in the air. The greater disturbance of these dusty fodders at any time, the greater will be the germ content of the air at that time. The following data show the number of bacteria per minute deposited in a 12-inch pail. In series A. (*see Fig. 12*), the exposure was made during bedding; in B. (*see Fig. 13*), one hour after this operation.

Series A.	16,000	13,536	12,216	12,890
	15,340	19,200	23,400	27,342
	42,750	27,820	18,730	12,210
Average...	20,100			
Series B.	483	610	820	715
	1,880	1,987	2,112	1,650
	990	1,342	2,370	1,750
Average....	1,400			

These results indicate that many bacteria are attached to particles of considerable weight, as they soon settle on the floor.

Cows are frequently bedded with dusty straw at the very time when milking is going on, a forkful of straw in some instances that have come under my observation, having been thrust under the cow that was being milked. Dusty fodders are often thrown down from the loft when the milking is in progress, filling the stable with dust, every particle of which carries spores of moulds and bacteria. It must be remembered also that very undesirable spores which are very difficult to kill, even by long-continued steam heat, abound in straw and hay.

Much benefit would ensue either from moistening the fodder or from feeding and bedding an hour or so before milking commences, to allow the dust, etc., of the air time to settle. In many of the more modern dairy farms, the stables are thoroughly cleaned and ventilated, the floors sprinkled, and the manure removed from the building before milking commences, or a milking room is provided, into which the cows, one, two or three at a time, are brought for milking. This room is supplied with water, conveniently located, and kept in an absolutely clean condition.

CONTAMINATION OF MILK FROM DAIRY UTENSILS.

Probably more trouble is caused to butter and cheese makers by the use of dirty utensils than any other way. Every article that is brought into contact with milk is at once infected with bacteria. When milk is left in storage cans for some time, a tremendous number of microbes develop, and a vast number of spores or latent forms of bacteria are produced. In this way, vessels are infected, and the bacteria find lodgment in all the cracks and crevices of pails, cans, dippers, strainers, etc. Take any milk-can and run the point of a pen-knife along the seam of the can, and you will find a stinking, cheesy mass, composed very largely of bacteria, all ready to grow and re-produce when fresh milk is poured in the can. Nothing is more difficult to clean than these dairy utensils, with the facilities at hand on the average farm. Scalding with hot water is often insufficient to kill the bacteria on the inner surface of the can, and in the cracks and crevices which are usually present. The following experiments will suffice to show the importance of utensils as a factor in milk contamination. Thus Russell[52] took two cans, one of which had been cleaned in the ordinary way, while the other was sterilized by steaming. Before milking the udder of the cow was thoroughly cleaned and special precautions taken to avoid the raising of dust; and the fore-milk was rejected. Milk drawn into these cans showed the following germ-content:

	Number of Bacteria.	*Hours before Souring.*
Steamed pail........	165 per c.c.............	28½
Ordinary pail........	4,265 "	23

The writer[35] has also shown the great differences in the bacterial content of cans by a bacterial analysis of the can washings. Cans were rinsed with 100 c.c. of sterile water, and numerical determination of this rinsing water was made.

The following data are from cans poorly cleaned, (*see Fig. 14*), cans washed in tepid water and then scalded—the best farm practice—and cans washed in tepid water and then steamed for five minutes (*see Fig. 15*).

BACTERIAL CONTENTS OF CANS CLEANED IN VARIOUS WAYS.

	Number of Bacteria per c.c. of Can Washings.		
Poorly Cleaned.......	238,500	342,800	215,400
	618,000	806,000	510,000
	230,000	600,000	418,000
Average..442,000.			
Ordinary Method......	89,000	84,000	26,000
	24,000	38,000	76,000
	15,000	44,000	93,000
Average..54,300.			
Approved Method.....	1,100	1,800	890
	355	416	725
Average....880.			

All cans should be constructed so as to facilitate cleaning. Stamped pails, without seams, may now be purchased, but if seams are present they should be examined to see that they are well flushed with solder. The bottoms of all cans should be concave, and not convex, to expedite cleaning.

After thorough rinsing to remove organic matter, cans should be washed in hot water, to which borax or soda may be added. After washing, rinse with boiling water, and if available place the can over a steam-jet for a few minutes.

THE EFFECT OF TEMPERATURE.

After the milk is infected with bacteria, the temperature at which it is kept exerts an important influence on the rate of growth or multiplication of the bacteria in the milk. Freudenreich[65] obtained some milk, which when delivered at his laboratory two and a-half hours after the milking, had 9,300 germs per c.c. Samples were stored at 15°, 25° and 36° C. (or 59°, 77° and 95° F.), and the results were as follows per c.c.:

	15°	25°	35°
3 hours.......	16,000	18,000	30,000
6 "	25,000	172,000	12,000,000
9 "	46,500	1,000,000	35,280,000
24 "	5,700,000	577,000,000	50,000,000

If we analyze this table we find that at 15° the increase during the first half hour was 700, or 7 per cent., which would indicate that the average duration of a generation is thirteen hours. In three hours more, the increase is 15,000, or 150 per cent., which gives the average duration of a generation as two hours. In the next three hours, the increase is 21,500, and the average duration of a generation about 3½ hours. From the ninth to the twenty-fourth hour, the average is about 2½ hours. At 25°, the times required for a doubling of the number are for the successive periods, about half an hour, about an hour, and about 7½ hours. At 35° the time occupied in a generation was about twenty minutes at first, then about forty-five minutes, and at last thirty-seven hours. These results are curious, but could only be explained by a fuller knowledge of the species concerned, and of the cause influencing the changes.

The most important point brought out in this experiment is the tremendous rate of increase at the higher temperatures; therefore, much may be done to restrain this rapid multiplication by cooling the milk as rapidly as possible. Milk allowed to cool naturally takes some time before it reaches the temperature of the air. Hence, measures should be promptly taken to reduce the temperature quickly.

Certified Milk.

Of late years a number of sanitary, model dairies have been established in the vicinity of large cities in various parts of the United States and Canada, (*see Figs. 16 and 17*), which have placed on the market, milk with a relatively low bacterial content. Such milk is known as "hygienic," "sanitary," or "certified." It is interesting to note that these establishments prosper, an indication that the discriminating public appreciate the honest endeavour of these dairies to produce milk which will fulfil the requirements of the most exacting sanitarian.

These establishments put into practice the suggestions made by various experimenters and investigators, as the result of their experimental inquiries, and these have been more or less briefly outlined in this paper. The freedom from bacteria obtained in these dairies depends on the thoroughness with which all details are carried out. Russell[52] has shown that when samples of milk are secured under as nearly aseptic conditions as possible, the germ content was 330 organisms per c.c.; but when drawn under ordinary conditions, the bacterial content was 15,500 organisms per c.c. Marshall[66] gives similar results, for example, milk drawn under aseptic conditions averaged 295 bacteria per c.c.; under ordinary conditions, 786,000 per c.c.

In certain cases milk coming from sanitary dairies is endorsed by a board of examining physicians and experts. Thus, the Milk Commission[67] of the Medical Society of the County of New York endorses milk from various dairies when the acidity of the milk is below 0.2 per cent., and when the milk contains less than 30,000 bacteria per c.c. The Milk Commission of the Philadelphia Pediatric Society give their endorsement for milk free from pus and injurious germs, and having not more than 10,000 germs per c.c.

Such milk naturally has enhanced keeping qualities, and milk and cream from several such hygenic dairies in the United States were shipped to the Paris Exposition in 1900, arriving in good condition after 15 to 18 days in transit.

The adoption of a numerical standard seems a very necessary step. Bitter suggests that 50,000 organisms per c.c. should be a maximum limit in milk intended as human food. Park thinks that any intelligent farmer, with sufficient cleanliness and a low temperature, can supply milk averaging not over 100,000 bacteria per c.c., when twenty-four hours old, and suggests that the sale of milk should be so regulated that that containing more than this number per c.c. should be excluded from the market. Rochester, N.Y., has already tried the enforcement of this standard, with good results. In the opinion of Russell, " the practical difficulties to contend with in establishing a milk standard based upon a quantitive bacterial determination are such as to render its general adoption extremely problematical." On the other hand, this investigator advocates the employment of the acid test, and postulates that milk should not contain more than 0.2 per cent. figured as lactic acid, and if possible the acidity should be brought down to 0.15 per cent.

To conclude, from what has been brought up before you, it is undoubtedly easy to see the reason for cleanliness in all operations connected with the dairy business. " All the results of scientific investigation," says Fleischmann, " which have found such great practical application in the treatment of disease, in disinfection, and in the preservation of various products, are almost entirely ignored in milking," and the only remedy for this state of affairs is " a campaign of education among the farmers who produce milk, concerning, first, the simple protection of a readily putrescible fluid from pollution with dirt or other elements of decay ; and, second, the sanitary protection of milk from infection."[68]

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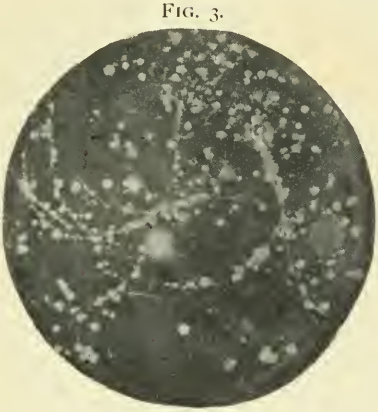
Fig. 1—Contamination from the Fore Milk.

Gelatine Plate shewing colonies of bacteria in $\frac{1}{15}$ c. c. (about 2 drops) of the fore milk. (First few streams from all four teats).

Fig. 2—Bacterial Content of Milk, the Fore Milk being Rejected.

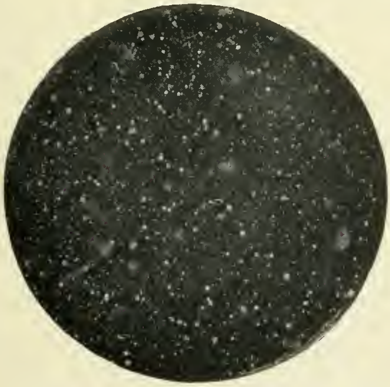
Gelatine Plate shewing colonies of bacteria in $\frac{1}{15}$ c. c. (about 2 drops) of milk taken from the middle of the milking.

Fig. 3.



Shewing the bacterial contamination from hairs.

Fig. 4—Contamination from Animal and Milker.



Gelatine plate exposed under the udder of a cow for one minute, while milking under ordinary conditions.

Fig. 5—Contamination from Animal and Milker.

Gelatine plate held under cow for one minute while milking. The udder and flanks well moistened with water.

FIG. 6—SANITARY MILK PAIL AND MILKER PROPERLY CLOTHED.

FIG. 7—THISTLE MILKING MACHINE.

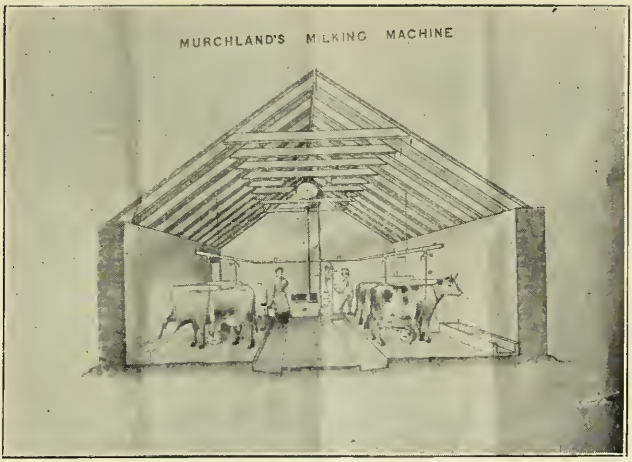


FIG. 8—MURCHLAND MILKING MACHINE.

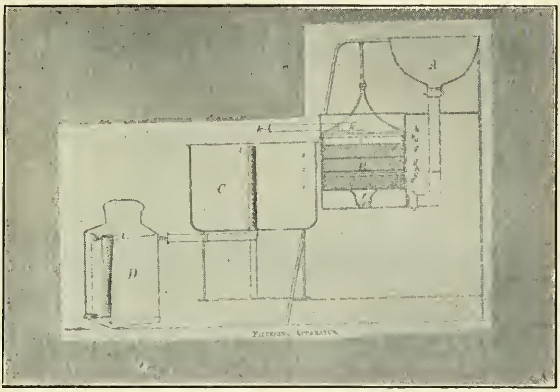


FIG 9—SECTION OF COPENHAGEN GRAVEL FILTER.

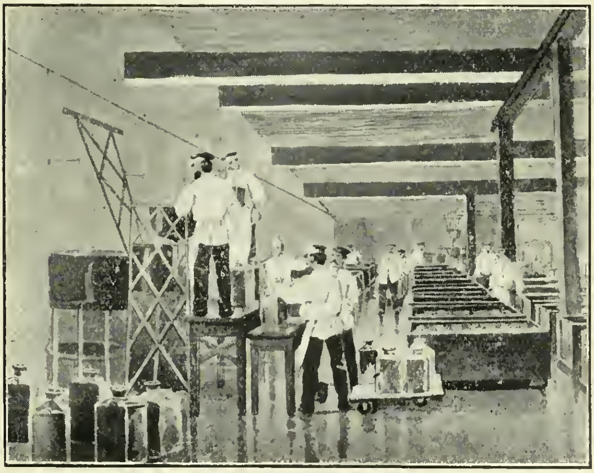


FIG. 10—FILTERING MILK THROUGH GRAVEL.

Fig. 11—Cleaning Milk by Centrifugal Force. S. Separators.

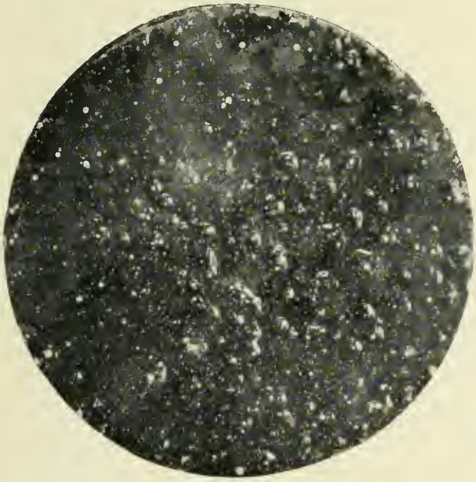
FIG. 12—GERM CONTENT OF BARN AIR DURING BEDDING, CLEANING UP, FEEDING, ETC.

Gelatine plate exposed to the deposition of germs for one minute in a stable when some of the above operations were in progress.

FIG. 13—GERM CONTENT OF BARN AIR DURING BEDDING, CLEANING UP, FEEDING, ETC.

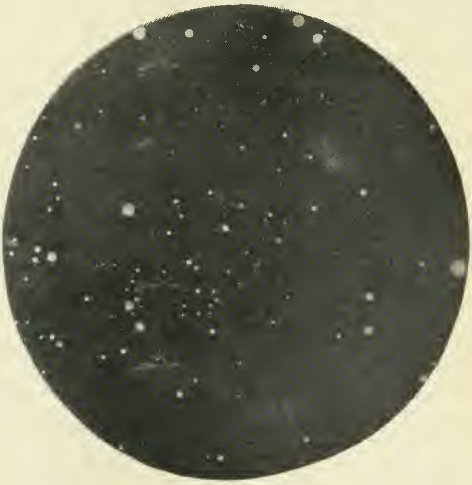
Gelatine plate exposed to the deposition of germs for one minute in a stable when all the above operations had been completed.

FIG. 14—CONTAMINATION FROM THE USE OF IMPROPERLY CLEANED DAIRY UTENSILS.



After a can had been washed with warm water, and thoroughly drained, a quantity of sterile water was added and the can well rinsed. This gelatine plate was made from $\frac{1}{100}$ c. c. (a very tiny drop) of this water.

FIG. 15—CONTAMINATION FROM THE USE OF PROPERLY CLEANED DAIRY UTENSILS.



After a can had been washed, scalded and steamed for five minutes, and thoroughly drained, a quantity of sterile water was poured in and the can well rinsed. This gelatine plate was then made from $\frac{1}{5}$ c. c. (about six drops) of this water.

Fig. 16—A Sanitary Dairy. Filling Bottles.

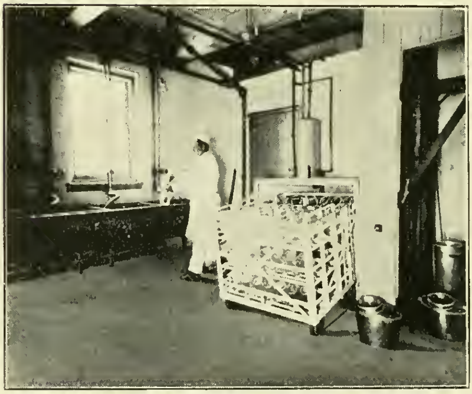


Fig. 17—A Sanitary Dairy. Bottle Washing and Bottle Steam Sterilizer.

THE CHEMISTRY OF WHEAT GLUTEN.

BY GEO. G. NASMITH, B.A.

(Read 26th April, 1902.)

I.—HISTORICAL.

THE first preparation of gluten from wheat flour by washing away the starch from dough seems to have been made by Becari,[1] but Einhof[2] was the first to give special attention to its composition. He extracted wheat gluten with dilute alcohol, and he found that the substance which precipitated on cooling, diluting or concentrating the solution was practically identical with gluten itself.

Taddei[3] named the portion soluble in alcohol gliadin, the residue zymom.

Berzelius[4] thought that he found a second constituent in the part of the gluten soluble in alcohol, which he called mucin, and which was precipitated by acetic acid. He[5] regarded Taddei's gliadin as identical with the substance obtained by Einhof from wheat, barley and rye. The insoluble residue Berzelius called plant albumin, from its great similarity to animal albumin.

De Saussure[6] found that wheat gluten contained about 20 per cent. plant gelatin, or glutin, as he proposed to call it, 72 per cent. insoluble plant albumin, and 1 per cent. mucin ; the latter, although differently prepared, he considered to be similar to the mucin of Berzelius, and it had, as he thought, the power of transforming starch into sugar.

Boussingault,[7] like Einhof, considered that part of the gluten soluble in alcohol to be identical with the entire gluten proteid.

Liebig[8] named the portion of the gluten insoluble in alcohol plant fibrin; he rejected the term zymom given by Taddei, and also that of plant albumin of Berzelius, in the latter case because solubility in water is a characteristic of albumins. The portion soluble in alcohol he called plant gelatin, and considered it to be a casein-like compound of a proteid with an undetermined organic acid.

Bouchardat[9] found in gluten a substance soluble in extremely dilute acid, which he named albumin, since he regarded it as forming the chief constituent of egg albumin, blood fibrin, casein and gluten.

Dumas and Cahours[10] found four proteids in flour, namely, an albumin which was obtained from the water used in washing out the gluten; plant fibrin left as a residue on extracting gluten with alcohol; a proteid from this alcohol which separated on cooling, and finally a second proteid which precipitates from the same alcohol on concentration and cooling. This latter he called glutin.

Mulder[11] prepared plant gelatin by extracting gluten with alcohol, filtering hot, allowing to cool and redissolving the white precipitate which settled out twice. This he considered to be a compound of sulphur with protein, and he found that it did not contain phosphorus.

Von Bibra[12] stated that on exhausting gluten with hot alcohol insoluble plant fibrin remained behind, while plant gelatin and plant casein dissolved; the plant casein separated on cooling. These bodies he thought had the same elementary composition, and were in fact isomers.

Günsberg[13] held that gluten was composed of three proteids, gliadin being a mixture of two. These were, (a) gluten fibrin, soluble neither in alcohol nor warm water; (b) gluten casein, insoluble in hot water but soluble in alcohol; (c) gluten gelatin, soluble in alcohol and hot water.

Ritthausen[14] found four proteids in gluten, namely, gluten casein, gluten fibrin, plant gelatin or gliadin, and mucedin, of which the last three are soluble in dilute alcohol. His casein was prepared by extracting gluten with boiling alcohol, cooling, exhausting the casein which settled out with absolute alcohol, then with acetic acid, and finally neutralizing the clear filtrate from this with ammonia. The decanted alcoholic fluid from the casein contained the gelatin, which separated on evaporation.

Scherer[15] digested gluten with artificial gastric juice and observed that the greater part went into solution in about fourteen hours.

Martin[16] found that only one proteid was extracted from gluten by dilute alcohol or hot water, which gave the reddish violet reaction of proteoses and peptones. Because of this reaction and its comparative insolubility he called it insoluble phytalbumose. The residue was coagulated by boiling water, and was soluble only in acids and alkalies. He claimed that dilute alcohol extracted only fat from dry flour, and came to the conclusion that insoluble phytalbumose was produced from a soluble albumose, and gluten fibrin from a globulin by pre-existing ferments.

Chittenden and Smith[17] made preparations of gluten casein according to Ritthausen's method, which averaged 15.86 per cent. of nitrogen.

Osborne and Voorhees[18] in an exhaustive research brought many opposing views into harmony. Like Martin they found only one proteid in gluten that was soluble in alcohol, and considered that the various proteids claimed by previous investigators to have been soluble in alcohol were impure preparations, perhaps mixtures with fat. Martin's gluten fibrin they termed glutenin, and found its composition to be practically identical with that of gliadin, a conclusion that had not hitherto been suggested. The high percentage of nitrogen they thought due to their improved method of preparation by which all starch, etc., had been removed. Contrary to Martin's experience they found that dilute alcohol extracted gliadin directly from flour.

Osborne and Voorhees further arrived at the conclusion that gluten is made up of two forms of the same proteid, one being soluble in cold dilute alcohol and the other not. They found that flour exhausted with sodium chloride solution yielded the same amount of gliadin as was obtained from the gluten made from an equal quantity of flour, or by direct extraction of the flour with 70 per cent. alcohol. They, therefore, held that gliadin exists as such in the seed.

Teller[19] noted again the fact that gliadin possessed proteose-like characters, as previously stated by Martin. Gliadin he found to be slightly soluble in dilute salt solution, and he regarded it as identical with that body classified by Osborne and Voorhees as proteose.

O'Brien[20] found himself in agreement with Osborne and Voorhees in considering that gluten pre-existed as such in flour in the same proportions as in gluten, and that there was but one mother substance in flour which gave rise by a process of hydration to gluten. His conclusions were, (*a*) that the differently described derivatives of gluten soluble in alcohol merge into one another; (*b*) that the portion soluble in alcohol may be made to pass into the insoluble stage; (*c*) that a proteose is readily formed as a secondary product from gluten.

Fleurent,[21] whose work, like that of O'Brien, was unfinished when Osborne and Voorhees' results were published, found himself also in agreement with the latter in concluding that only one body soluble in alcohol was present in gluten. Besides gliadin and glutenin he found a very small quantity of a third body which he called conglutin.

Morishima[22] found only one proteid in gluten (that he named artolin) which he prepared by kneading gluten with dilute alkali and treating the decanted fluid with hydrochloric acid until it contained one per cent. of the acid in excess. The precipitate washed with one per cent. hydrochloric acid and made up with alcohol to a solution containing 70-80 per cent. of alcohol, was filtered and saturated with absolute alcohol and ether.

Its composition he found to be:

ARTOLIN (MORISHIMA).

C	52.29
H	7.02
N	16.51
S	.74
O	.....
Cl	1.57

Mayer[23] held that the old name of plant gelatin was inappropriate, since it was chemically unlike gelatin: the latter does not contain sulphur, and is insoluble in dilute alcohol.

Ritthausen[24] commented on Fleurent's and Osborne and Voorhees' careful work, but, nevertheless, clung to the views propounded in his early paper. In criticizing the work of Morishima, he pointed out the fact that proteids are soluble in dilute acids, and concluded that artolin was glutenin linked with hydrochloric acid.

The subjoined analyses, taken from the paper of Osborne and Voorhees were found by reference to the original papers to be accurate:

	BOUSSINGAULT.	JONES.	DUMAS and CAHOURS.		MULDER.	VON BIBRA.
	Gliadin.	Plant. Gelatin.	Casein.	Glutin.	Plant. Gelatin.	Plant. Gelatin.
C	52.30	54.44	53.46	53.27	54.85	53.57
H	6.50	7.42	7.13	7.17	7.05	7.12
N	18.90	15.98	16.04	15.94	15.71	15.57
S	....	....	....	....	.60	.88
O	22.30	22.16	23.37	23.62	21.79	22.86
	100.00	100.00	100.00	100.00	100.00	100.00

	Gunsberg. Plant. Gelatin.	Ritthausen Gluten fibrin.	Ritthausen Plant. Gelatin.	Ritthausen Mucedin.	Osborne and Voorhees. Gliadin.
C	52.68—52.65	54.31	52.76	54.11	52.72
H	6.77— 6.88	7.18	7.10	6.90	6.86
S	....	1.01	.85	.88	1.14
N	17.76—17.45	16.89	18.01	16.83	17.66
O	22.79—23.02	20.61	21.08	21.48	21.62
	100.00-100.00	100.00	100.00	100.00	100.00

GLUTENIN.

	Jones.	Scherer.	Dumas and Cahours.	Von Bibra.	Ritthauseen.
C	52.79	54.60—52.34	53.37—53.23	55.57	52.94
H	7.02	7.45— 7.13	7.02— 7.01	6.95	7.04
N	15.59	15.81—15.36	16.00—16.41	15.70	17.14
S	....	....	.. ....	1.02	.96
O	24.62	22.14—25.17	23.64—23.35	22.76	21.92
	100.00	100.00-100.00	100.00-100.00	100.00	100.00

	Chittenden and Smith.	Osborne and Voorhees.
C	52.87	52.34
H	6.99	6.83
N	15.86	17.49
S	1.17	1.08
O	23.11	22.26
	100.00	100.00

II.—OBSERVATIONS.

While working at the composition of wheat flour, Professor Macallum suggested that I should trace to its source the phosphorus which he found to be present in the cellular elements of the wheat grain. It was sought for and found in gluten, no matter how carefully prepared or how long it had been washed in tap or distilled water. Gluten was prepared in the usual manner by kneading dough in a stream of water until free from starch, dried at 110 C. until the weight was constant and the phosphorus estimated according to Neumann's[25] method, which was found by experiment on known solutions of phosphoric acid to be perfectly accurate. Two quantities of gluten yielded 0.11 and 0.12 per cent. of phosphorus respectively.

In order to determine next which of the constituents of gluten (*i.e.*, gliadin or glutenin) contained phosphorus, gliadin was prepared by extracting starch-free gluten with 70 per cent. alcohol, filtering the solution repeatedly, and afterwards evaporating completely to dryness.

Average of five estimations 0.83 per cent. ash.
" two " 0.29 " phosphorus.

Gliadin was prepared by extracting gluten with 70 per cent. alcohol, filtering and diluting with twice its volume of 1 per cent. sodium chloride solution ; the white precipitate, separating out, was collected, washed with distilled water, till free from chlorine, and dried at 110 C. The analyses gave :—

	I.	II.	III.	IV.
Phosphorus........	0.19	0.19	0.18	......
Ash................	0.205	0.201	......	......
Nitrogen	17.705	17.435	17.64	17.555

The ash from these was dissolved with hydrochloric acid ; the solution evaporated almost to dryness in a platinum crucible, was diluted with distilled water, and treated with a quantity of dilute hydrochloric acid containing also potassium ferrocyanide. A blue colouration immediately indicated the presence of iron ; repeated trials invariably yielded the same result.

In order to determine whether the iron was organic or inorganic, a solution of gliadin in ammonia-free distilled water was added to a solution of hæmatoxylin. No darkening whatever occurred, showing that the iron must be organically combined. Inorganic iron salts with hæmatoxylin give an intense dark blue colour. Pieces of freshly-prepared gliadin, suspended in hæmatoxylin, gave no reaction in thirty hours. The iron, like the phosphorus, must be in organic combination.

Previously to this I had found that on digesting gluten with artificial gastric juice, and repeatedly renewing the fluid, a part remained insoluble even after two months. This residue, after extracting with absolute alcohol and ether, was dissolved in 0.2 per cent. sodium hydrate, and precipitated by 0.2 per cent. hydrochloric acid, the precipitate being insoluble in excess of the acid. Evidently this was a nuclein, and must have come from the gliadin or glutenin of the gluten.

A gram of gliadin, purified by precipitating, dissolving, reprecipitating, and extracting with absolute alcohol and ether, was digested with artificial gastric juice at 38°C. A residue remained which gave all the reactions for nuclein, and undoubted reactions also for organic iron and phosphorus.

A large amount of gliadin was now prepared by extracting gluten with 70 per cent. alcohol, filtering, concentrating to a small quantity, precipitating with 95 per cent. alcohol, extracting in the Soxhlet apparatus for sixteen hours with absolute alcohol to remove fat and lecithin, and finally drying for three hours at 110°C.

Analyses gave the following :—

GLIADIN.

C	52.39	Av. 6.
H	6.84	Av. 6.
N	17.46	Av. 2.
S	1.12	Av. 2.
O	21.89	
P	0.267	Av. 2.
Fe	0.034	Av. 2.
	100.00	

The iron was determined gravimetrically since the amount was so small that only a few drops of 1/40 normal solution of potassium permanganate were necessary by the volumetric method, and the exact end point was consequently difficult to determine. Taking all necessary precautions to eliminate aluminium and calcium, results were obtained by extracting the iron from the ash, which were concordant with those obtained from the filtrate after precipitating the phosphorus as ammonium phospho-molybdate. The weight of ferric oxide seldom exceeded 0.6 milligram. The analyses in other respects agree very well with those of Osborne and Voorhees, except that the carbon and nitrogen contents are slightly lower, and that they obtained no phosphorus.

A large quantity of gliadin was prepared and digested at 38°C. with artificial gastric juice in litre flasks. Digestion was continued for three weeks, the flasks being frequently shaken, and the clear supernatant fluid renewed several times. The considerable residue was collected on filters, washed free from proteoses and peptones with water, then with 70–95 per cent. alcohol which removed some fat. The residue dissolved in 0.2 per cent. sodium hydrate solution, was filtered, and the solution precipitated with excess of dilute hydrochloric acid, the process of solution and precipitation being repeated several times; the precipitate was then collected on "hardened" filters and washed with distilled water till free from chlorides. Extracted with absolute alcohol in the Soxhlet apparatus for sixteen hours, dried at 110°C., and analyzed, the residue yielded the following results :—

GLIADIN NUCLEIN.

C	49.47	per cent.	Av. 2.
H	6.98	"	Av. 2.
N	16.60	"	Av. 2.
S	0.80	"	One.
P	0.29	"	Av. 2.
Fe	0.04	"	Av. 2.
O	25.82		
	100.00		
Ash	0.24	per cent.	Av. 2.

The amount of phosphorus was very small, practically the same, in fact, as the gliadin from which it was prepared. The chemicals used were carefully tested in blank experiments, but no trace of phosphorus was found in any of them. Possibly the prolonged digestion with frequent renewals of hydrochloric acid solution had removed some of the phosphorus. The result was, however, quite unsatisfactory, since it was to be expected that the amount of phosphorus and iron would be much greater than in the substance from which it was derived. The analyses of the gliadin and the nuclein, derived from it, may be compared side by side :—

	Gliadin.	Gliadin Nuclein.
C............	52.39	49.47
H	6.84	6.98
N	17.47	16.60
S	1.12	0.80
O	21.89	25.82
P.......... .	0.267	0.29
Fe	0.034	0.04
	100.00	100.00

From this it may be gathered that the two compounds are quite distinct chemically as well as physically.

Glutenin was prepared, as recommended by Osborne and Voorhees[18] by extracting all the gliadin from gluten by dilute alcohol, dissolving the residue in 0.2 per cent. potassic hydrate, and precipitating by exactly neutralizing with 0.2 per cent. hydrochloric acid ; the precipitate washed with 70-95 per cent. alcohol, was again dissolved in 0.2 per cent. potassic hydrate and filtered perfectly clear through heavy filter paper in an ice chest. Precipitated from the solution by exact neutralization with 0.2 per cent. hydrochloric acid, washed with distilled water till free from chlorides, then with 70-95 per cent. alcohol, extracted in the Soxhlet apparatus for ten hours with absolute alcohol, and dried for three hours at 110 °C., the glutenin so prepared gave on analysis the following:—

GLUTENIN.

	Nasmith.	Osborne.
C	52.75	52.34
H	7.22	6.83
N	16.15	17.49
S	1.06	1.08
O	22.58	22.26
P	0.215	....
Fe	0.026	....
	100.00	100.00

Ash 0.188 per cent.

Another preparation by Fleurent's method[21] which was also carefully filtered, yielded 16.55 per cent. nitrogen. The figures are not at all in agreement with those of Osborne and Voorhees for this compound. Mine are considerably higher in carbon and hydrogen, and much lower in nitrogen, a result which might be accounted for by carbohydrate impurity. Since, however, it was prepared exactly as described by him this seems unlikely. The fact that the amount of iron and phosphorus is practically the same as in gliadin at once suggested the possibility of these elements being derived from a certain amount of nuclein mechanically carried along with these compounds in the attempted purification process.

Impure glutenin was digested with pepsin and hydrochloric acid, but the insoluble residue was so difficult to separate from soluble starch, and was so evidently impure that the complete analysis was not made, though the presence of iron and phosphorus in it was demonstrated.

In repeating the work of Morishima[22] a copious precipitate as usual occurred at the neutral point, but when more acid was added nearly all went into solution; after twenty-four hours only a trace of precipitate settled out. Glutenin has again and again been shown to be soluble in dilute acids. Artolin, as I found, is derived from another source than Morishima supposed. A 0.4 per cent. hydrochloric acid extract of flour was made, filtered perfectly clear and potassic hydrate added until neutral, when a precipitate was thrown down, which proved to be nearly all gliadin. If to this 0.4 per cent. hydrochloric acid extract more acid was added, a precipitate began to appear which increased with the acidity. This in large part separated on heating, and it proved entirely soluble in 70-80 per cent. alcohol, the result showing it to be gliadin.

This property of gliadin, of being precipitated with excess of acid, has not, I think, been hitherto noted. Since the compound of Morishima was prepared in practically the same way artolin is evidently gliadin in acid combination. Glutenin remains in solution. The body obtained under these circumstances by Morishima would perhaps correspond to a proteid salt,[35] *e.g.*, a chloride of gliadin. I obtained the substance called conglutin by Fleurent[21] but in quantity insufficient for analysis.

In order to decide whether the iron and phosphorus in gliadin and glutenin were actually in molecular combination in these compounds, resource was had to the microscope. Grains of Manitoba hard wheat were imbedded in celloidin and sectioned. Macallum's methods for determination of iron[26,27] and phosphorus[28] were used.

For iron the celloidin was removed by equal parts of alcohol and ether, the sections passed through absolute alcohol and inorganic iron salts removed by 2.5 per cent. hydrochloric acid in 95 per cent. alcohol. Sections so treated showed no trace of colour with pure hæmatoxylin in aqueous solution (0.5 per cent.) after the lapse of thirty minutes. The sections now placed in sulphuric acid alcohol (4 vols. acid, 100 alcohol) at 40° C. were removed at intervals of half hours ; on washing out the acid, and placing in hæmatoxylin, the sections gave a marked reaction for iron, the organic iron combination having been broken up and the inorganic iron salt formed retained in sitû.

Sections unextracted by hydrochloric acid showed much inorganic iron in the aleuron layer and germ. When this had been removed by hydrochloric acid no colour whatever appeared after standing for twenty minutes in hæmatoxylin solution. After treatment with sulphuric acid alcohol the nuclei of the aleuron and large parenchymatous endosperm cells were stained with hæmatoxylin purplish blue-black. The aleuron cell contents gave no reaction, nor did the proteid matter of the endosperm, which constitutes gluten. Gliadin and glutenin, therefore, do not contain iron in their molecules, and that present must have been derived from the nuclei of the cells of the endosperm and aleuron layer, and possibly in small amounts from embryo cells.

The distribution of iron in the embryo, or germ, is a point of interest. The closely packed cells of the embryo each contained a large nucleus coloured with hæmatoxylin almost black. In the rapidly dividing cells of the radicle and plumule a diffused purplish blue-black reaction occurred, which under the highest power could not be identified with any definite granules or structures. Some of the cells, other than those in a rapid state of division, gave a faint purplish reaction, perhaps from iron derived by diffusion from the nucleus.

In order to show the distribution of organic phosphorus the inorganic phosphates were first removed by soaking for half an hour in acetic acid alcohol. Sections removed at the end of this time, placed for a few minutes in the nitric-molybdate solution, and then in one per cent. solution of phenylhydrazine hydrochloride showed no trace of green colouration, this fact indicating that all inorganic phosphates had been removed.

Such extracted sections were now placed in nitric-molybdate solution at 35° C. and removed in series at intervals of half an hour. When placed in a solution of phenylhydrazine hydrochloride for a few minutes, they showed a green colour, which increased in depth with the times during which the section remained in the molybdate solution. In twenty hours the aleuron layer and embryo were stained a bright green. Sections which had had the celloidin removed by alcohol and ether, and which were subsequently extracted with absolute alcohol in the Soxhlet apparatus for several hours, gave exactly the same reactions as those unextracted. Consequently lecithin could not have been present. The aleuron cells in such preparations showed a large nucleus of a much deeper green than the rest of the cells, and under the high power the colour was seen to be confined to the spaces between the aleuron grains, the coloured parts appearing in the form of a network. The network had a more or less punctated appearance, the grains themselves were perfectly colourless.

In the endosperm of such preparations the nuclei alone were coloured, though sometimes, after twenty-four hours, the proteid matter packed between the starch grains, and even the cellulose gave the phosphorus reaction. Possibly phosphorus had diffused from the nuclei. The manner in which the phosphorus is distributed in the different types of embryo cells is quite varied. The palisade-like absorption cells between the endosperm and embryo appeared finely granular and of a uniformly dark green tint. The cytoplasm of the radicle and plumule cells were of a finely granular character, and gave the phosphorus reaction. Around these tightly packed cells of the radicle and plumule were other cells much more loosely connected, whose contents appeared vesiculated. The intercellular material gave a faint phosphorus reaction, while the large granular nucleus was much darker and very prominent.

Between these vesiculated cells and the absorption tissue of the embryo were large cells loosely bound together. These cells, even under the low power, were very different from the others, containing large, well-separated granules, coloured a bright green. Under the high power these granules appeared round, angular, or often crescent-shaped. In very thin sections they were quite separated from one another and very brilliantly coloured; in thin sections the nucleus often was not apparent. In thicker sections these granules were seen to be connected, forming a loose kind of meshwork, the spaces between being filled with a finely granular substance, giving a faint but distinct phosphorus reaction. When treated for the iron reaction a very faint violet tinge appears in these cells, but only between the bodies which stain so brightly for phosphorus.

From this it seems that, with the exception of the rapidly dividing cells such as those of the radicle and plumule, iron is found in the nuclei only of the various cells of the wheat grain.

Phosphorus is more widely distributed, appearing between the aleuron grains; in fine grains in the radicle and plumule cells; in the foam-like mesh work of another type of embryo cell; in the very distinct large granules just described, and in the nuclei of all these cells. From the various ways in which these different cells stain, and the several methods of phosphorus distribution in them, one may conclude that there are probably several nucleins present.

Osborne and Campbell[29] extracted wheat germ with petroleum naphtha, ground the residue to a fine flour, extracted this with water, saturated the clear filtrate with sodium chloride, and subjected the resulting precipitate to a vigorous peptic digestion. The nuclein so prepared, they conclude, "is not an original constituent of the extract nor of the cells of the embryo, but results through several molecules of nucleic acid with one of Protein." To this nuclein, washed with water and dissolved in dilute potassic hydrate solution, was added hydrochloric acid until a precipitate formed, which readily separated. When this was filtered off a considerable excess of hydrochloric acid was further added to the filtrate, whereupon a precipitate of nucleic acid separated out which became so dense and brittle that it could be ground under water.

This operation, as described, I repeated, but a small quantity only of nucleic acid was obtained, which, however, did not become brittle under water. As I expected, the ash of this nucleic acid and of the nuclein, also prepared, gave distinct reactions for iron, even after standing for several weeks under dilute hydrochloric acid, a fact unnoticed by Osborne, and showing that part at least of his nuclein had come from the nuclei of the cells. If this nuclein had been derived from the nuclei of the embryo cells, it must have contained iron, since, as above demonstrated, its presence is invariable in the nucleus. Probably his nuclein was derived both from nuclei and ground substance of the cells.

It may then probably be admitted that the phosphorus and iron invariably found in gliadin and glutenin, no matter how carefully they have been prepared, are present in the form of nuclein or nucleic acid, which have been derived from the nuclei of the parenchymatous endosperm cells chiefly, and carried with them in the purification process. Perhaps aleuron and embryo cells imperfectly separated in the milling process contribute part of them.

III.—PROPERTIES OF GLIADIN.

Gliadin extracted directly from raw flour by dilute alcohol is always contaminated with fat, which gives to its solution a yellow tinge. On diluting this solution with an equal volume of sodium chloride solution, a snow-white precipitate separates, which, if the dilution is sufficient, collects into brownish flocculent masses, and either rises or sinks, according to the strength of the salt solutions. Prepared in this way gliadin is exceedingly viscid, adhering to everything with which it comes in contact. When precipitated by water alone, gliadin will not readily separate. Evaporation of the alcoholic solution and cooling cause a considerable gummy mass of gliadin to separate, while a few drops of sulphuric acid to the supernatant fluid throw down almost all of the gliadin left in solution.

A solution of gliadin evaporated to dryness forms a glue-like brittle, opalescent, yellow mass ; hydrated gliadin, exhausted with absolute alcohol and ether, and dried over sulphuric acid, forms a pure white friable mass. Either variety will almost wholly go into solution on warming in dilute alcohol. Gliadin is slightly soluble in distilled water, and then gives the pink biuret reaction ; it is not entirely insoluble in dilute salt solutions, as stated by Osborne and Voorhees. In dilute alkalis it readily dissolves, and the greater part of that dissolved separates on neutralizing. Its action with hydrochloric acid is peculiar ; it may be extracted directly from flour by dilute acids, filtered perfectly clear, and yet an additional drop of acid throws down a cloudy precipitate which increases in quantity with further addition of acid, but separates completely only on heating; as it cools, however, more or less of the precipitate goes back into solution. A drop of alkali to the acid solution only produces a faint opalescence, which does not increase with additional alkali until the neutral point is reached, when a sudden clouding occurs, and a precipitate settles out on heating.

A cold alcoholic solution of gliadin filtered clear, clouds slightly in twenty-four hours, depositing a small precipitate which increases in quantity with the length of time under alcohol. It is much more soluble in boiling than in cold alcohol, a saturated solution of the former depositing a heavy precipitate on cooling. Heating to 130° C. in the autoclave renders gliadin insoluble in alcohol. In artificial gastric juice at 38° C. it rapidly dissolves, depositing a small amount of nuclein, and yielding a considerable amount of true peptone, as evidenced by the deep red colouration with potassic hydrate and cupric sulphate in the filtrate after removal of proteoses by saturation with ammonium sulphate. It is a unique proteid, in that it gives this red biuret reaction before as well as after digestion. In this particular the name "insoluble phytalbumose" applied to it by Martin[16] does not appear appropriate. The proteid is entirely insoluble in absolute alcohol, and is precipitated by strong alcohol from solutions in weak. Addition of salt to a solution of gliadin in 70 per cent. alcohol does not produce precipitation until water is added. Millon's reagent, and nitric acid give the usual proteid reactions.

Gliadin is distributed throughout the endosperm, especially toward the periphery, where the small proteid granules are much thicker and the starch granules they enclose smaller. It is also contained in bran, and probably in aleuron cells as part of the packing between the aleuron grains, for both bran and shorts yield gliadin to dilute alcohol.

IV.—PROPERTIES OF GLUTENIN.

Glutenin is almost completely insoluble in salt solutions, water, and alcohol; readily soluble in dilute acids and alkalis, from which solution the proteid is precipitated unaltered when the solution is rendered neutral to litmus. It has a definite coagulation temperature which lies about 70° C. Gluten dehydrated with absolute alcohol and ether, is very slowly soluble in dilute acids and alkalis, more or less remaining undissolved. Experimental evidence seems to show that glutenin exists as such in the wheat grain. Its composition, according to Osborne, is practically identical with that of gliadin, results differing greatly from those of previous investigators, who had only in one instance obtained from glutenin as much as 17 per cent. of nitrogen.

Osborne considered it an altered form of gliadin, but the fact that it has a definite coagulating point, while gliadin has none, would indicate that it is improbable. No one has yet succeeded in making gliadin assume a form at all resembling glutenin. In my opinion the two proteids are entirely distinct in origin as well as in properties. Osborne states that glutenin is slightly soluble in cold but much more in hot dilute alcohol, the dissolved proteid separating on cooling. Since glutenin is coagulated at about 70° C. the proteid dissolved must have either been due to gliadin imperfectly separated from the glutenin, or to part of the latter split off by heat. The trace soluble in cold alcohol, as Osborne himself hints, may have been gliadin, which is exceedingly difficult to separate from glutenin.

V.—THE FERMENT THEORY OF GLUTEN FORMATION.

The question whether gluten exists as such in flour, or whether it results by the activity of a ferment, is one on which there are considerable differences of opinion. Weyl and Bischoff[30] considered gluten to be formed from pre-existing globulins by a pre-existing ferment in flour. They held that flour extracted by 15 per cent. solution of sodium chloride, and heated to the coagulation point of globulin, gave no gluten. They were, however, unable to isolate the ferment.

Martin[6] thought that gluten did not pre-exist in flour as such, but that his gluten fibrin was derived from a precursor globulin, and his insoluble phytalbumose or gliadin, from a soluble albumose. He stated that gliadin was not extracted directly from flour by 70 per cent. alcohol.

Johannsen[31] advanced arguments against the ferment theory, and thought gluten existed as such in a finely divided state in the wheat grain. He stated that a temperature of 60°C. did not injure the gluten-forming power of flour, and that flour made by mixing dry starch and finely-powdered gluten behaved like ordinary flour.

Ballard[32] maintained that gluten pre-existed as such in flour. Osborne[29] arrived at the same conclusion. O'Brien[20] found that flour heated to 100° C. for thirteen hours gave practically the usual amounts of gluten ; also that a paste made with boiling water yielded gluten in apparently normal quantities : that flour left twenty-four hours under absolute alcohol and ether, yielded gluten when these evaporated. He concluded that there is but one compound soluble in alcohol, that the portion soluble in alcohol may be made to pass over into the insoluble stage, and that there exists but one mother substance of gluten in flour.

None of the proofs as to the existence or non-existence of a ferment appear at all conclusive. Dry heat at 100° C. or even 110° C. for several hours does not kill ferments, neither does alcohol for a short period. To prove the non-existence of a ferment presents in this case peculiar and apparently unsurmountable difficulties, but a few facts bearing on the point may be given here.

Seventy per cent. alcohol, cold or hot, applied directly, extracts gliadin from dry flour ; warm 95 per cent. alcohol does the same ; flour moistened with 95 per cent. alcohol and heated to 80° C. yields abundant gliadin, as does flour stirred into boiling water and then extracted with alcohol. When flour, however, is slowly sifted into boiling water, so that every particle comes into instant contact with water or steam at 100° C. it yields no gliadin to dilute alcohol.

Dough made from flour and boiling water does yield gluten on washing, as stated by O'Brien, but it is smaller in amount and is of irregular consistency. The temperature of the dough when mixed was found to be only 52.5° C. Now glutenin has a definite coagulation point. Martin[16] stated that the residue after extracting gluten with dilute alcohol was coagulated by boiling water. Before noticing his work I had found the coagulation point of glutenin to be about 70° C. When, therefore, a dough was made with boiling water, and only reached the temperature of 52° C. only a comparatively small amount of the flour must have been heated to 70° C., a temperature which coagulates glutenin. Consequently a quantity of gluten would be formed from the portion of the flour not heated to that point. A dough made in this way and gradually heated till it reached a temperature of 80° C. yielded no gluten, proving that its formation depended upon the glutenin not being coagulated.

A dry heat of 110° C. for ten hours does not coagulate proteid, and flour heated to this point still yields gluten ; but if flour is heated to 120° C., or even 100° C., for half an hour in the autoclave a dough of little coherence results, and no gluten is obtainable on washing even over silk. The glutenin had been coagulated. In other words any temperature or manipulation that would kill a ferment which might be present would coagulate the glutenin and therefore gluten could not be obtained. The fact that gluten has a definite coagulation point would seem to indicate that it is not derived from the same substance as gliadin. I have never been able to transform one of these compounds into anything at all like the other. With the idea of finding out whether gluten changed into gliadin, I extracted all the latter from flour, let one half stand over night under water and the other under alcohol for twenty-four hours, but neither yielded anything to dilute alcohol.

The fact that ground, dried glutin mixed with starch yielded dough of normal properties, as stated by Johannsen[31] is no proof as to the non-existence of ferment action, since if ferment action were present the dried gluten itself would have been the resultant product of the ferment action.

Flour was slightly moistened with absolute alcohol and heated on a warm bath to 70° C., being stirred all the while with a stout thermometer in order to heat the mixture evenly throughout. Alcohol was used to prevent any possibility of ferment action. After drying in the air, one half was taken and made into a dough, from which, as I expected, gluten could not be obtained. A small quantity of raw flour was intimately mixed with the other half and this was also made into a dough. In this case also no gluten could be obtained. This proved that the formation of gluten depended altogether on whether glutenin was coagulated or not, since the ferment if existing should have been present in the added raw flour.

Now ground air-dried gluten mixed with starch and made into dough yields gluten of normal properties. Such a dough of ground gluten and starch warmed above 70° C. does not yield gluten since the glutenin has been coagulated. Therefore when glutenin which had been already made, as in the the second case, or glutenin, or even its predecessor in the raw flour in the first case, was coagulated, a similar result obtained. The probability, therefore, seems to be strong that glutenin is present in flour as such. And since gliadin is extracted directly from flour or bran with 70–95 per cent. alcohol, cold or boiling, and also by dilute acids or alkalis, it also apparently is present as such in flour, and not derived, as O'Brien[20] holds, from the same parent substance as gluten.

VI.—The Aleuron Layer of Wheat.

The outer endosperm layer of wheat was stated by Sachs[33] in 1862 to be rich in oil and nitrogenous compounds. Ten years later Pfeffer[34] pointed out the fact that gluten was not derived from the aleuron layer as was commonly believed. He maintained that the high nitrogenous value of the latter was due to substance not proteid in nature, and to adhering endosperm rich in gluten.

Johannsen[31] in 1888 again emphasized the fact that aleuron cells do not contain gluten; he stated that these cells contained nitrogenous granules imbedded in a soft protoplasmic mass, rich in fatty matter.

According to O'Brien[20] the protoplasm of an aleuron cell is continuous with that of adjacent cells, aleuron as well as endosperm. He found oil present in considerable quantities. The individual aleuron grains on addition of water appeared to consist of a central core which was more or less soluble in water, salt solutions, dilute acids and alkalis, and not readily stainable. The layer surrounding this core he found to stain readily with iodine, hæmatoxylin and aniline stains, and to be insoluble in any of the above mentioned reagents.

From an aqueous extract of bran he obtained a coagulable proteid, probably a globulin, and proteose which, when evaporated to dryness, yielded a gelatinous semi-transparent substance, partly separating in small round spherules, regarded by him as artificial aleuron grains, since they gave all the reactions of those imbedded in cell protoplasm.

He also extracted from bran by means of dilute alcohol a proteid which corresponded to gliadin.

Dilute alcohol, I found, extracted gliadin from both bran and shorts. Aqueous extracts of bran gave a globulin coagulable by heat, and also a proteose-like body which was not gliadin. On evaporation of this proteose extract no granule corresponding to O'Brien's artificial aleuron grains could be obtained, although a granular material did separate; the solution at the same time exerted a very strongly reducing action upon Fehling's fluid. I was unable to make out a double coat to the aleuron grains. The substance between the aleuron grains seems to be chiefly gliadin, and contains inorganic iron, calcium salts and phosphorus-holding compounds.

VII.—CONCLUSIONS.

Gliadin and glutenin do not come from the same parent substance, nor are they of the same composition. Gliadin has not a definite coagulation point, while glutenin has. Gliadin is obtained from rye, barley, and maize, and from the bran and shorts of wheat, while glutenin cannot be obtained from these. By chemical or other means one has as yet not been transformed into anything at all resembling the other.

Both gliadin and glutenin invariably give the reactions for organic iron and phosphorus, but are not nucleo-proteids. Under the microscope the gluten matrix in thin sections of wheat does not show any indication of iron or phosphorus, and it must, therefore, be concluded that the organic iron and phosphorus found in gluten are due to nucleins or nucleic acid derived from the nuclei of the large endosperm cells. Probably part is derived from nuclei of the aleuron cells, or of the embryo cells, or from the nucleins present in the cytoplasm of the embryo cells.

Gliadin exists as such in the wheat grain, and the theory of its formation by means of ferment action is not justifiable. Strong alcohol mixed with flour and then diluted with water to a 70 per cent. solution extracts gliadin from it ; boiling alcohol also extracts gliadin from flour or bran.

Glutenin exists as such in the wheat grain ; any manipulation that will destroy the hypothetical ferment will coagulate glutenin, thus making gluten formation impossible.

Gluten formation is not merely a mechanical mixture of gliadin with glutenin, but a definite physical state of the two mixing substances is necessary. Coagulated glutenin with gliadin does not form gluten.

There are probably several nucleins or nucleo-proteids in wheat, as shown in the various ways phosphorus is distributed in the different types of embryo cells. Organic iron is found only in the nuclei of the endosperm, aleuron, and embryo cells, and in the cytoplasm of the absorption layer, plumule and radicle cells. The proteid between the aleuron grains shows the presence of organic phosphorus only.

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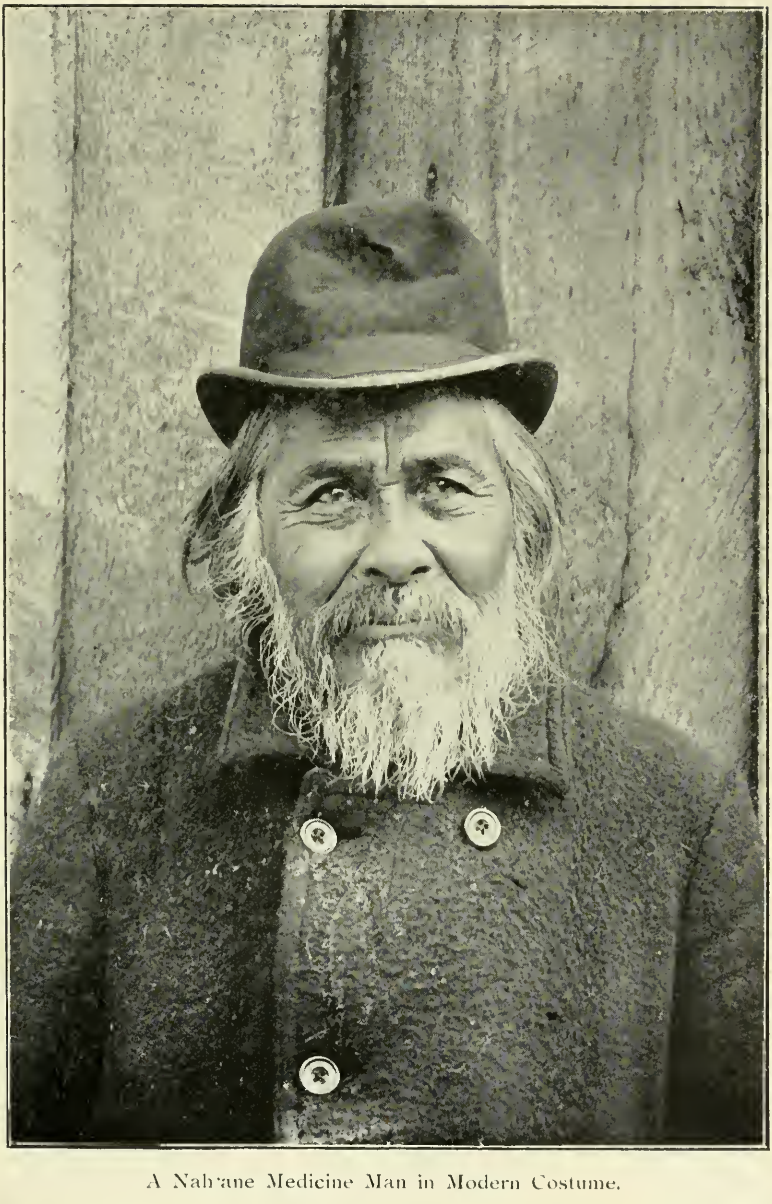

A Nah'ane Medicine Man in Modern Costume.

THE NAH·ANE AND THEIR LANGUAGE.

BY THE REV. FATHER A. G. MORICE, O.M.I.

(*Read 4th April, 1903.*)

OF the twenty odd tribes which compose the great Déné family, few, if any, are so little known as the Nah·ane.

Many are the travellers who had passing references to them in the course of their writings, but exceedingly few are those who had as much as seen one of them. In fact, Dr. G. M. Dawson is the only author who can be said to have introduced them to us, and his information, fragmentary, and at times inexact as it is, is confined to the limits of a few pages.

Writers are not even agreed as to their very name as a tribe. Thus while Pilling in his valuable Bibliography of the Athapaskan Languages has adopted the spelling Nehawni, Kenticott calls them Nahawney; Ross writes their name Nehawney; Richardson changes this into Noh'hanne; MacKenzie dubs those he met Nathannas; Campbell and Dawson alternate between Nahanie and Nahaunie; others prefer Nahawnie, and Petitot himself never speaks of them but as the Na"anne, his " being the equivalent of my upper dot, which stands for the hiatus.

He derives that appellation from Nari'an-o'tine, "people of the West," but does not state from which dialect the word is borrowed. All the western Déné who know of that tribe, as well as its members themselves, pronounce it Nah·ane, and there can be no doubt that Petitot is correct in the meaning he ascribes to that term, whatever may be said of its derivation. For sunset or occident, the Tsilkotin say *nare·i*N, the Carriers *naanai*, the Tsé'kéhne *naren·o*N, and the Nah·ane themselves *naean*. The final e is expressive of personality and sometimes of plurality or collectivity.

On the other hand, Mr. J. W. MacKay[1] repeatedly calls the tribe Ku-na-na, the name given it by the Tlhinket, its neighbours in the south-west. But that he is somewhat mixed as to the ethnographical status of those Indians is shown by his remark that "the Ku-na-nas of the Stickine valley are closely allied to the Tlinkeets of that section,

1 B.A.A.S. Tenth Report on the North-Western Tribes of Canada, p.p., 38-39.

i.e., the Skat-kwan."[1] As a matter of fact, the latter are just as pure non-Déné as the former are undoubted Déné.

In common with all the Déné and many other aboriginal families, the Nah·ane recognize as their property no other vocable than Déné, "men," though the branch of that tribe best known to me, the Thalhthan, will occasionally call themselves Tcitco'tinneh or "stick-people," whereby they simply translate the name given them by outsiders, since, according to Dawson, and as I have myself ascertained, "the interior Indians are collectively known on the coast as 'stick Indians.'"[2]

So much for the name of the tribe. Now as to its ethnographical status. This seems even more of a mystery to the few writers who have ever referred to it.

It is now over nine years since I stated myself that the Nah·ane "hunting grounds lie to the north of those of the Tsé'kéhne. But I am not familiar enough with their tribal divisions to state them with any degree of certainty, nor do I sufficiently possess their technology to speak authoritatively of it."[3]

I am glad to be now in a position to say that, in the course of the present year, I have taken a trip to their chief village Thalhthan,[4] in order to add as much as possible to my knowledge of that tribe and its language. I have succeeded in gathering besides the material for a grammatical compendium, quite a goodly little dictionary, and not a few texts in its dialect which I intend shortly to publish. Yet I must confess that we must still fall back, for the details of their frontiers and some other particulars, on what the late Dr. G. M. Dawson wrote of them in 1887—Notes on the Indian tribes of the . . . northern portion of British Columbia.[5] Inaccurate as it is from a philological standpoint, his is the only account of the Western Nah·ane worth referring to.

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1 Notes on the Indian Tribes of the Yukon District, etc., p. 2.

2 Tenth Report, p. 39, note.

3 Notes on the Western Dénés, Transactions Canadian Institute, Vol. IV., p. 31.

4 Most writers spell this word Tahltan, when they do not have it simply Taltan, and Dr. Boas corrects them by changing it into Ta'tltan. All sin through ignorance of the Déné phonetics and of the meaning of that word, which is a contraction of *Tha-sœlhthan*, *tha*, the usual alteration of *thû*, water n compounds, and *sœlhthan*, a verb which has reference to some heavy object lying therein.

5 Annual Report of Geological Survey of Canada.

I.

Broadly speaking the tribe consists of four main divisions. To my certain knowledge, its principal seat in the west is Thalhthan, a salmon fishery at the confluence with the Stickeen of a river of the same name, by about 58° 2′ of latitude north. From the new village in the immediate vicinity of that place, these aborigines radiate as far south as the Iskoot River, taking in all its tributaries and some of the northern sources of the Nass, and in the east to Dease Lake and part of the Dease River, extending also to all the northern tributaries of the Stickeen. Further north, we meet the Taku branch of the tribe, which claims " the whole drainage basin of the Taku River, together with the upper portions of the streams which flow northward to the Lewes, while on the east their hunting grounds extend to the Upper Liard River and include the valleys of the tributary streams which join that river from the westward."[1]

The third division of the Nah·ane is the so-called Kaska, about whom much misapprehension seems to exist among the whites I met in the course of my journey, a misapprehension of which Dr. Dawson constituted himself the echo when he wrote: "The name Kaska is applied collectively to two tribes or bands occupying the country to the eastward of the Tahl-tan. I was unable to learn that this name is recognized by these Indians themselves, and it may be, as is often the case with names adopted by the whites, merely that by which they are known to some adjacent tribe. It is, however, a convenient designation for the group having a common dialect. This dialect is different from that of the Tahl-tan, but the two peoples are mutually intelligible, and to some extent intermarried."[2]

In the first place I must remark that Kaska is the name of no tribe or sub-tribe, but McDane Creek is called by the Nah·ane KasHa—the H representing a peculiar gutturalo-sibilant aspiration—and this is the real word which, corrupted into Cassiar by the whites, has, since a score of years or more, served to designate the whole mining region from the Coast Range to the Rocky Mountains, along, and particularly to the north of the Stickeen River.

All the whites who mentioned the subject to me concurred in Dawson's opinion that the so-called Kaskas form quite a different tribe, and in a footnote to the latter's essay, a Mr. Campbell goes even so far

1 Notes on the . . . northern portion of British Columbia, p. 3.

2 *Ibid.*, p. 9.

as to state that the " Nahanies of the mountains (who correspond to a subdivision of the Kaskas), are quite a different race from the Nahanies of the Stickeen (Tahl-tan)"[1] Now the Thalhthan Indians I questioned on the subject unanimously declared that those pretended foreigners spoke exactly the same language as themselves, with, of course, some local peculiarities. From a Kaska boy, with whom I travelled for a number of days, I ascertained that even such non-Déné words as *'kûk*, paper, *khukh*, box, *'kunts*, potatoes, which I thought proper to the Thalhthan Indians, who borrowed them from the coast, were the only ones current among his people to designate those objects.

The physique of the Kaska is somewhat different from that of the Thalhthan aborigines, inasmuch as I recognized in the former the thin lips and small, deeply sunk eyes of the Tsé'kéhne, while the latter resemble more the Carriers of the Coast Tlhinket, with whom from time immemorial they have more or less intermarried.

The sociology of the two divisions of the Nah·ane is as widely different, and their respective mode of life aud social organization confirm my previous assertion in former papers that, to all practical purposes, the western Nah·ane are Carriers, while their eastern brethren are Tsekehne.

Another circumstance which has contributed not a little to the estrangement of the two tribal divisions, is the long-standing feuds arising out of difficulties concerning the hunting grounds, the making of slaves, and other causes. Even to this day the Kaska resent the Thalhthan's assumed or real superiority, and will not be confounded with them as co-members of the same tribe. Hence their declarations to the whites and the travellers' and traders' printed statements.

According to Dr. Dawson, the so-called Kaskas are sub-divided into the " Saze-oo-ti-na " and the " Ti-tsho-ti-na " and their habitat is in the neighbourhood of the Dease, Upper Liard and Black Rivers. His " Saz-oo-ti-na " may be Sas-otine or " Bear-People," while his Ti-tsho-ti-na's real name is no doubt Tihtco'tinne, or Grouse-People, an appellation which would seem to leave it open to discussion whether we have not in them rather the names of two different phratries or gentes than those of two genuine ethnical subdivisions of a tribe.

" Eastward they claim the country down the Liard to the site of old Fort Halkett, and northward roam to the head of a long river (probably

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1 Notes, etc., p. 10.

Smith River) which falls into the Liard near this place, also up the Upper Liard as far as Francess Lake."[1]

This statement would seem to dispose of Petitot's Bad-People or *Mauvais-Monde*, a "very little known tribe," he says, "which used to trade at the now abandoned Fort Halkett to the number of 300 or 400 souls."[2]

Father Petitot furnishes us with our fourth division of the Nah'ane when he states that "a little band of 300 Na"annes (Déné) roam over the mountains of the MacKenzie. They are the Nathannas of Sir A. Mackenzie. We can add thereto the Etaottines of the Good Hope mountains, and the Espa-t'a-ottines of Fort des Liards in equal number."[3]

To the above certain divisions of the Nah'ane tribe, we should perhaps add the Ts'Ets'aut, an offshoot of some inland Déné, whom Dr. F. Boas discovered some years ago on Portland Inlet, on the Pacific Coast, somewhat to the southwest of the Nah'ane proper. That Dr. Boas would himself connect them with the Nah'ane tribe is apparent from the statement that "Levi (his informant) named three closely related tribes whose languages are different, though mutually intelligible; the Tahltan (Ta-tltan) of Stickeen and Iskoot Rivers, the Laq'uyip or Naqkyina, of the headwaters of the Stickeen, and the Ts'Ets'aut."[4]

This surmise is fully confirmed by Mr. MacKay, his annotator, who states that those Indians "belong to the Kunâna, a tribe which inhabits the lower Stickine valley and whose headquarters are at Tahltan."[5]

But here *scinduntur doctores*. According to Dr. Boas this handful of natives, which now consists of a mere dozen individuals, would have numbered about 500 souls sixty years ago, while Mr. MacKay has quite a different story to account for their separate existence as a tribe. He relates that, not more than forty years ago,[6] three or four families hailing from Thalhthan in the course of their wanderings made for Chunah, on the sea coast, but took a wrong direction and struck on the west shore of Portland Channel, where they were practically forced to remain in a

1 Notes, etc., p. 10.

2 *Mémoire abrégé sur la Géogrphie de l' Athabaskaw–MacKenzie*, p. 46.

3 *Ibid. ibid.*

4 Tenth Report, B.A.A.S., p. 34.

5 *Ibid.* p. 38.

6 It is now eight years since both statements were published.

subject condition by the Tsimpsians, among whom they had unwittingly tumbled.[1]

Be this as it may, the language of the Ts'Ets'aut such as recorded by Dr. Boas himself, while it shows here and there undeniable traces of a Déné origin, has become so corrupt by the admixture of foreign terms and the alteration of its original lexicon, that the propriety of their being classified as Nah'ane is now quite problematical.

The population of the whole Nah'ane tribe must remain little more than a matter of guess. From the Iskoot, close to the Pacific, to the Mackenzie, across the Rocky Mountains, is indeed a broad stretch of land, and the very fact that it is so sparsely peopled renders it so much the more difficult to obtain anything like an exact computation of the tribesmen. I myself took some years ago a census of the Thalhthan village, and my figures were in the close vicinity of 190 souls. The population has since decreased, so let us call it 175.

From native sources I ascertained that the "Kaska were more numerous, perhaps 200. Petitot puts at 600 the number of the trans-montane Nah'ane and allied subtribes. Allowing for the probable decrease and possible exaggeration, let us say 500. There remain the Taku, of whom I have no means of ascertaining the exact numbers. Probably 150 would be a conservative figure.

We thus obtain a total of 1,025, or in round numbers, 1,000 souls for the whole tribe, and I believe this is as fair an estimate of its population as could possibly be had at the present time.

As already stated, the eastern Nah'ane somewhat differ in physique from their western congeners, the only portion of the tribe with which I am familiar enough to describe it *de visu*. Their stature would be rather below than above the average, the maximum height being five feet eight inches. Their feet and hands are small and well shaped, and their head is round and not so large as that of the neighbouring

1 In the course of his account of that adventure and the circumstances which lead to his getting acquainted therewith, Mr. McKay takes occasion to speak of an invasion by the Tsimpsian of the territory which is now the Tsimpsian peninsula, whereupon Dr. Boas remarks that "there is no traditional evidence of the invasion of the Tsimshian tribe to which Mr. McKay refers," adding that "it is probable that the Tsimshian were originally an inland people," two statements which, apparently difficult to reconcile as they at first appear, nevertheless are in no way conflicting. There may be no tradition of such an invasion among the Tsimpsian, but their very name betrays their origin. The Skeena River is known to them as the *Ksièn*, and they call themselves *T'sæm-sièn*, people from the Skeena, or the river. To this day, anybody can see, two miles from Hazelton, on the Upper Skeena, a prairie or ancient townsite, where one can distinguish the cavities over which were built their winter subterranean houses. Now the name of the locality is *Tamlarh-am*, the beautiful place, in Tsimpsian, and those two words are still used in that connection by the inland Kitkson to the exclusion of any name in their own dialect.

Tlhinket. With them the nose, without being of the regular aquiline type, is not so squatty as among the Tsilhkoh'tin and other tribes. The lips are full, the eyes dark and not quite as large as is common with the Carriers. The forehead is low, broad and bulging immediately above the eyes. The hair is invariably black, coarse and straight.

Their beard is scanty, though a few, especially such as have taken to shaving—they are very progressive and great imitators—disport a fair quantity of dark, bristly facial hair.

As to their complexion, it varies considerably according to the individuals. Contrary to what I have noticed in other tribes, some of the eastern Nah·ane women have cheeks of a tinge which might almost be characterized as rosy, though the facies of others is quite swarthy.

All the adults above forty have the septum pendent and pierced through with a hole which held formerly a large silver ring, perhaps two inches in diameter. The leading men or notables wear likewise silver rings hanging from the lobes of the ear, and these are the only present remnants of the many ornaments which the helix was originally made to support.

Neither in blood, customs nor language are the western Nah·ane pure Déné. They are indebted to no small extent to the Tlhinket of Fort Wrangell for their present make-up. To them also they undoubtedly owe that lack of moral strength and force of character which has left them such an easy prey to the vices of unscrupulous white men. Very few are to-day the western Nah·ane who can be represented as bodily sane. Syphilis, a disease hardly known among the other Déné, is but too prevalent among them. Liquor is also slowly but surely killing them out.

I am bound to add, however, that adverse circumstances are a great deal to blame for the development of such pitiful results. Had missionaries established themselves among them before the rush of strangers to the Cassiar mines, the natives would not, in all probability, be the degraded beings they have become. Since the last few years, a representative of the Anglican Church has struck his tent on the arid hill of Thalhthan. But I am sure he could not well himself take exception to my statement that his influence has not been in the interest of temperance.

Though no other Déné that I know of have had to undergo the test of being left alone to wage their war against such a degraded foe as a majority of the Cassiar miners have shown themselves to be, it is difficult for me to imagine for a moment, for instance, the Tsè'kéhne tribe sunk to the low moral level of the present Nah·ane whom I have met or have been told about.

While the eastern Nah·ane lead the simple patriarchal life of the Tsè'kéhne, with hardly any sign of a social organization, their western congeners, with the remarkable adaptiveness proper to the Déné race, have adopted practically all the customs and some of the mythology of their heterogeneous neighbours on the sea coast. Thus it is that matriarchate or mother-right is their fundamental law governing and regulating all inheritances to rank or property.

Though they have no totem poles, they know of the gentes, which at Thalthan are those of the Birds and of the Bears. Each of these have several headmen or *téné-thîe* (the equivalent of the Carrier *tœneza*), who alone own the hunting grounds, and on festival occasions, such as dances or potlatching, are granted special consideration. These ceremonial banquets are much in vogue, and as a result, almost every house in Thalhthan is now crowded with a quantity of trunks containing goods publicly received or to be likewise given away.

Those houses are now of rough unhewn logs, with stoves instead of fire-places. But the tribe's residences were originally much less elaborate, and consisted of brush shelters, sometimes with low walls made of long, slender poles. Therein they dwell, generally several related families together.

Marriage was never accompanied with any ceremony or formality. It seems to have been based principally on the bestowing of furs or other goods on the parents of the prospective bride.

Polygamy was known everywhere, but it is now practically abolished, the only exceptions being a very few cases among the present Kaska. As to divorce, it is obtained without any formality, and is often enough resorted to.

Shamanism was originally the only form of worship common to the whole tribe, and in the east witchcraft, and the social disturbances it entails seem even now quite prevalent. The Kaska boy I have already mentioned as a companion on part of my trip from Thalhthan was just being taken away from revengeful fellow-tribesmen who had already done to death two of his brothers under the plea that their parents were responsible for the sickness and ultimate death of some Indian or

Indians against whom they were believed to have exercised their black art.

As among the other Déné, such deaths were the cause of family feuds of long duration and bitter hatred, when they did not lead to reprisals and a series of murders. Thus would originate their internecine wars, which consisted merely in ambuscades, surprises and massacres, accompanied sometimes with the enslaving of the women and the children.

But their "wars" were more frequently directed against foreigners, such as the Tsimpsian of the upper Skeena, or against the Tlhinket of the coast. They had no war chiefs, or indeed any chief at all in our sense of the word.

In times of peace, their special avocation and means of subsistence are hunting and fishing, to which a few of the younger men add packing for the miners and the Hudson's Bay Company. As their territory is so extensive, it still abounds in fur-bearing animals and game of almost all descriptions. I found moose especially plentiful all over the country. The mountains are also rich in sheep and goats.

No wonder then, if the Nah'ane are well-to-do. In fact I consider that the western part of the tribe is at present dying on a golden bed. In the house of my hosts at the time of my visit were to be seen, besides gilt bronze bedsteads and laces of all kinds, two sewing machines, two large accordeons, and, will the reader believe it?—a phonograph! All this in the forests of British Columbia, north of the 58th degree of latitude!

Since I have mentioned death, I may remark that cremation was, until recently, the mode adopted by the western Nah'ane to dispose of their dead. And, in this connection, we have a ludicrous admixture of the new order of things with the olden ways, in the small travelling trunks bought from the whites, which are to be seen planted on two posts, in several places along the trails, and which contain some of the bones of the dead picked up from among the ashes of the funeral pile.

II.

As to the language of the Nah'ane, much might be said. I shall point out in the following pages only those particularities which are its exclusive property, and leave out most of the general features which are common to all the Déné dialects, and which the reader will find detailed in my paper on "the Déné Languages,"[1] and in my forthcoming complete grammar of the Carrier language. Furthermore, all the following remarks shall apply more particularly to the idiom of the western Nah·ane, the only one I have ever studied.

Neglected by the ethnographers as the Nah·ane have remained to this day, their dialect has still been more of a *terra incognita* to the philologists. With not even the least grammatical note has it been honoured so far in all the linguistic literature at my command, and the only vocabulary by which it has ever been represented in scientific publications consists of the four columns of Thalhthan words printed by the late Dr. Dawson.[2]

And here I may be allowed to state that, after a careful study of their language, I have had the satisfaction of ascertaining that of all the corrections in the latter's vocabulary which I lately declared[3] were demanded by the general rules of Déné phonetics and suggested by my knowledge of the other related dialects, not one have I found to be unwarranted.

Before going further I must also correct the one statement Dr. Dawson makes concerning their language. Speaking of the Thalhthan and Taku Nah·ane, he writes: "These Indians speak a language very similar to that of the Al-ta-tin, if not nearly identical with it, and so far as I have been able to learn, might almost be regarded as forming an extension of the same division. They appear to be less closely allied by language to the Kaska, with which people they are contiguous to the eastward."[4]

I have already done justice to the latter assertion. By Al-ta-tin, Dr. Dawson means the Lh'ta'tin, or "People of the beaver dams," as the Tsé'kéhne are called by the Carriers. His notion about the similarity of the two dialects I have found prevalent in other quarters. To prove its utter groundlessness, I need but reproduce here the Nah·ane and the Tsé'kéhne versions, for instance, of the doxology. Was the Chippewayan version available, I have no doubt that it would be found more alike to that of the Tsé'kéhne than to that of the Nah·ane. Grammatically speaking, there is more affinity between the Tsé'kéhne and the Chippewayan—two very distinct tribes—than between the Tsé'kéhne and the Nah·ane.

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1 Transactions Canadian Institute, Vol. I.

2 Notes on the northern portion of British Columbia, p. 19. *et seq.*

3 Transactions Canadian Institute, Vol. VI., pp. 99-100.

4 Notes, etc., p. 2.

THE DOXOLOGY.

In Nah·ane.

Séesôga Œtha· 'ka'tcéh, Œtcimé· ka'tcéh, Ahtige-Tî 'ka'tcéh hut'sihkaîhtîn.

Lhann kastséh tûda ahîh'té la, tû'gu 'ka'tcéh, ue'té 'katcéh, ét'tha ta'da œtû wotôzite a'téh éyéne 'ka'tcéh hu'karo'té ni.

In Tsé'kéhne.

Utqon Œtha· qûh, Œtcwinh qûh, Yétqire-Inqî qûh ut'sœrhautœz.

Sé rhasséh tarhît'qé ille a, qû qûh, awuz·on qûh înt'lhon qé ta ussé utœtûzit e'tah éyétœ qûh hahut'qé.

To start with the sounds as such, I will remark that the following desinential letters or groups of letters are never found in Carrier or Chilcotin, but are quite common in Nah·ane: *c*, *ts*, *tc*, *tlh*, *klh*, to which we must add the medial *-slh-*, as in *aslhé*, I make, and *-srh-*, as in *etisrhuh*, I snore. Final *ts* occurs often enough in Babine, and final *tc* is as frequent in Tsé'kéhne, but the other compounds are never found even in those idioms.

On the other hand the letter *m*, which sometimes terminates a word in Carrier, never occupies that position in Nah·ane. We should not forget either to notice that the double letter *tj* or *dj*, which is so frequent in Kut·chin appears also in Nah·ane to the exclusion of all the other Dénè dialects.

Some Carrier letters have their fixed equivalents in Nah·ane. Thus the Carrier initial *n* is often replaced by *t* in Nah·ane. Ex.: *ni*, mind, Nah·ane, *ti*: *na*, eye, Nah·ane, *ta*: *aunilh*, purposedly, Nah·ane, *atilh*; *dûni*, he will say, Nah·ane, *dûti*. The initial *p* of many Carrier words becomes *m* in Nah·ane (as well as in Tsé'kéhne), and we have *pœn*, lake, in Carrier, but *men'* in Nah·ane; *thapa*, shore, in Carrier, *thama* in Nah·ane; *pœ-*, his, in Carrier and *me-* in Nahane.

A Nah·ane sound, which I have found in no other Déné idiom is that which I render by H. It is a kind of a guttural aspiration, much more pronounced than that of the common *n*. Its equivalent in the other dialects is *rh*, or the Greek *rho*, and in the possessive case, it is inflected into a soft *r*. Ex.: *His*, pus; possessive, *me-rize*, his pus.

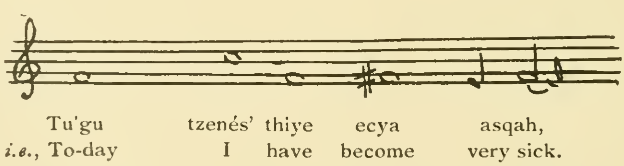
The first particularity which strikes a Déné scholar in his study of the Western Nah·ane, is the presence therein of a regular accent, something quite unknown in all the northern Déné dialects. I have no doubt that the intercourse of that subtribe with the Tlhinket of Fort Wrangell is responsible for that feature of its language. This accent has for effect, not only to lengthen the syllable it affects, but even to raise the pitch of the voice when the accented syllable is pronounced. Thus it often falls on monosyllables. Gun is *û'na* (a Tlhinket word) in Nah·ane; *kussa'* means, " I do not know " in the same dialect. Much stress must be laid on the *u* of the first word and on the *sa* of the last, otherwise neither would be understood.

On the other hand the voice must also be raised with a sort of constrained effort when one pronounces the words *'khon'*, fire, *nehn'*, land, *tzé*, gum, etc., though many other monosyllables lack this distinguishing feature.

In this connection I must not fail to record what, to a student of the Carrier idiom, seems something of an anomaly. In my " Notes on the Western Dénés,"[1] I wrote some years ago : " In these nouns there is generally one syllable which is more important and contains, as it were, the quintessence of the word. Thus it is with the *ne* of *tæne*. . . . In composite words such syllables only are retained.

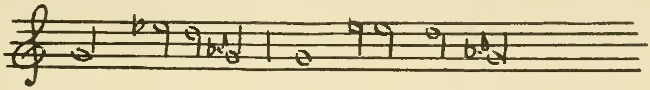
Now it happens that in Nah·ane the accent falls precisely on the first syllable of that word (which means " man " in all the dialects), and not on the second, which is hardly audible when pronounced by a native. In the same way, instead of using only the second half of the word, as is usual with the Carriers and the Chilcotin when they refer to the human body or to any part thereof, as in *ne-yœs'te*, human body ; *ne-na*, human eyes ; *ne-t'sîltcœn,* human neck, etc., a Nah·ane will always utter the whole word, giving particular prominence to its first part, and say, for the same objects, *tèn'e-ri*, *tèn'e-ta*, *tèn'e-'kwos*, which the careless listener will most probably take for *tèn'ri*, *tèn'ta*, etc.

Beside their accent, the Nah·ane have, when speaking, a particularly marked intonation. This is so pronounced that it could almost be compared to a song. In fact, I have noticed the following modulation as being of very frequent occurrence. Its finale especially is hardly ever omitted.



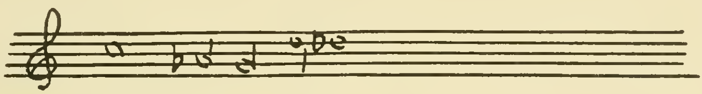
Students of native languages must have noticed that most tribes or portions of tribes have their own peculiar way of singing out, as it were, the sentences of their respective idioms. When there is nothing in their elocution which can be compared to a song, the finale, at least, is almost certain to stray out of the *recto tono*. So the ending of each Shushwap sentence is infallibly from G to upper C, while the Coast Salish, or at least the Sicalh, content themselves with raising the voice from G to A, or one full tone.

The intonation of the Carriers varies too much according to the different groups of villages to be recorded here. I will choose but one, which is characteristic of the Hwozahne, or people of Stony Creek.



Ntœn lheœtni, au t'sœtœst'sœk ;
i.e., What does he say, we don't understand him.

The elocution of the Chilcotin and of the Tsé'kéhne is more uniform. Any member of the former tribe would, I think, easily recognize the following sentence, which they are ever ready to utter when anything is asked of them which they are not disposed to grant.



Tla kan'te kule ; *i.e.*, There is none.

But to return to our Nah·ane dialect. From a terminological point of view, it has all the appearances of an eclectic language. Indeed, I would fain compare it to English, as it occupies to some extent, with regard to the other Déné dialects, the position held by that language relatively to the European idioms. Its vocabulary furnishes us, besides fully forty nouns,[1] which are more or less Tlhinket, several terms which duly belong respectively to the Kutchin, the Hare and the Chippewayan dialects. Here are a few examples. Kutchin : *djugu*, now, Nah·ane, *tugu* ; *lhaon*, quite, Nah·ane, *lhan* ; *œkwœt*, knee, Nah·ane, *ekwœt* ; Hare *due*, no, Nah·ane, *tueh* ; *guntie*, elder brother, Nah·ane, *etiye* ; Chippewayan : *sorha*, well ; Nahane, *sôga* ; *esdan*, I drink, for which the Nah·ane have an absolutely homonymous synonym.

Even the Tsimpsian has lent them one word, *lelk*, to designate the snake, a reptile which is not found within Nah·ane territory. A few words of English have also crept into the Nah·ane vocabulary, and it is worthy of remark that whenever an *l* occurs in them, the Nah·ane have

1 I may here draw the reader's attention to the fact that a people of a low mental standard, a nation of uncultivated intellects, may borrow many unchangeable words from the vocabulary of its heterogeneous neighbours, but will never attempt to appropriate verbs. The former they will leave in their original form, or allow them through neglect or ignorance to slowly degenerate into more or less different terms ; but when it is a question of verbs, the low type intellect is not up to the task of adapting them to the exigencies of its grammar. In other words it cannot digest and assimilate them.

altered it into an *n*. Thus for gold, they say *gon*; for silk, *sink*; for dollar, *dana*. The word *kas* for barrel they owe to the Tlhinket, who had themselves borrowed it from the English speaking skippers and traders (*kas* = cask).

Chinook has contributed *masmas* (a corruption of *musmus*), cattle, and probably *kimdan* (for *kiutan*), horse. Following the example of the coast Indians, the Nah·ane have likewise changed the Chinook for cat, *pus*, into *tuc*.

At times this propensity for appropriating foreign terms leads to curiously hybrid compound words. For instance, the Nah·ane equivalent of organ is half Tlhinket and half Déné. All the Déné call that instrument a "paper that sings." As the Nah·ane had already borrowed the Tlhinket work *'kûk* for paper, and on the other hand, as they did not know or could not use the Tlhinket synonym for "sings," they unscrupulously retained the first vocable to which they added their own equivalent for the verb and said *'kûk-etqine*.

The dictionary may be regarded as a thermometer which faithfully registers a people's status and chief avocation. Its readings are seldom at variance with fact, and when it records, for instance, a multitude of fish names or, still better, when it possesses several names for the same fish according to its age or condition, it will inevitably denote a nation of fishermen. In like manner the sociological status of our Nah·ane is betrayed by their vocabulary, which abounds in fine distinctions for the names of the larger animals on which they mainly subsist.

I will take but one example to illustrate my meaning. With them the generic name of the marmot is *tœtiyé*, and the female is called *hosthelh*, while the name is known as *oet'qetha*. A little marmot in general is named *oe'kane*, or *usthe-tsetle*. But if it is only one year old it goes as *usaze*; the next year it will be known as *oekhutze*, and when in its third year, it will be called *tœtiyé-tucitze*. And note that all of those eight words apply to only one kind of animal, since there is another term to denote the smaller variety of marmot (*arctomys monax*).

We have therefore our Nah·ane stamped by their very vocabulary as a people of trappers and huntsmen, and the abundance of their terms for a mountain animal furthermore sheds a good ray of light on the topography of their country.

Another reliable indicator of a primitive people's main occupations, to which it adds a valuable hint at the nature and climate of its land, is the calendar. Subjoined is that in use among the western Nah·ane, and the careful student of Americana will perhaps find it worth his while to compare it with those of the Carriers and of the Tsé'kéhne published in my "Notes on the Western Déné."[1] Of course, all the months therein recorded are lunar months, and coincide but imperfectly with our own artificial divisions of the year.

January, *sa-t'séslhie*, moon of the middle (of the year).
February, *tœnon-thene*, the snow is a little frozen over.
March, *iht'si-sa*, moon of the wind.
April, *tlhi-pœnetsé-e*, the dog uses to bark.
May, *ih·aze-sa*, moon when all the animals leave their winter retreats.
June, *œyaz-e-sa*, moon of the little ones (when animals have their young.
July, *œtcitc-e-sa*, moon when they moult.
August, *ti'ka-e-sa*, moon when they fatten.
September, *hosthelh-e-sa*, moon of the female marmot.
October, *mœn-then-tsetle*, moon of the small ice.
November, *mœn-then-tco*, moon of the big ice.
December, *kœrh-urwoesse*, the rabbit gnaws.

We have tarried so long over the sounds and substantives of the Nah·ane language that our remarks on the other parts of speech must necessarily be brief.

In its numerals we find a confirmation of what I said some time ago when I wrote, speaking of the roots of languages in general: "The numerals and the pronouns . . . generally have a kind of family air in cognate dialects. As to the pronouns, I think that hardly any qualificative reservation is necessary, but it is not so with all the numerals."[2] Of the ten Nah·ane numbers, only three (one, *lhige*, Carrier, *ilho*; three, *thadè'téh*, Carrier, *tha*; and five, *lholla*, Carrier, *kwollai*) have any affinity with the Carrier, Babine, Chilcotin or even Tsé'kéhne numerals. The other seven have not the faintest resemblance thereto.

A peculiarity worth recording in this connection is the fact that the numbers two, three and four are in Nah·ane perfectly regular verbs which are conjugated with persons—plural, of course—and tenses. Let us take, for instance, the number three, *thadè'téh*. We have at our disposal any of the words of the following conjugation:

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1 Transactions Canadian Institute, Vol. IV., p. 106.

2 The Use and Abuse of Philology, Transactions Canadian Institute, Vol. VI., p. 92.

PRESENT.	PROXIMATE FUTURE.
tha-desî'téh, we are three	*tha-dî'tilh*, we are going to be three
tha-dah'téh, you are three	*tha-dah'tilh*, you are going to be three
tha-hidè'téh, they are three	*tha-hœda'tilh*, they are going to be three
PAST.	**EVENTUAL FUTURE.**
tha-desî'tée, we were three	*tha-dû'tée sa*, we will be three
tha-dah'tée, you were three	*tha-dah'tée sa*, you will be three
tha-hidè'tée, they were three	*tha-hœdû'tée sa*, they will be three

In all these words the main root for three is, of course, *tha*. Yet *thadesî'téh*, etc., are single words whose neither first nor last component parts can be used separately.

The only approach to these conjugable numerals I know of is to be found in the speech of a small portion of the Carrier tribe. It is restricted to the number two, *natne*, which becomes *nat'soetne*, we are two (persons), *nahtne*, you are two, etc. I should not forget, however, a peculiar set of numerals for which I find no more appropriate qualificative than the epithet "inclusive." These not only have in Carrier all the persons and tenses of the above, but they are even modified so as to form a separate class of adverbal numerals. Here are a few examples: *na-t'sœl'torh*, both of us;[1] *na-nel'torh*, both of us; *na-nœlh'torh*, both of you; *na-rhœl'torh*, both of them.

The following are impersonal verbs: *na-hwul'torh*, both times; *na-hwothil'torh*, it is going to be both times, etc.; *tha-hwul'torh*, all of the three times; *tî-hwul'torh*, all of the four times, etc.

All these forms, tenses or persons can be applied in Carrier to all the numerals of that class, except the first, the ninth and the tenth, and in this respect, as in so many others, that language surpasses in richness all the other Déné dialects.

The Nah·ane lacks an equivalent for the personal plural particle *ne*, which the Carriers suffix to the verb when in English we make use of the demonstrative and relative pronouns "those who," as in *hwot'sit-ne*, "those who lie," the liars. Instead of this, the Nah·ane will say, by a curiously abnormal commingling of a plural pronoun with the corresponding singular verb: "he-lies they," *tset'sit oekhune*. This renders speech unnecessarily long and rather unwieldy.

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1 With an idea of impersonality, which it is impossible to express in English, and which is absent in *nanel'torh*.

Nah·ane Women in Dancing Costume.

A feature of the possessive pronouns which the Nah·ane shares with some related idioms is the absence of a term for the second person of the plural. Most of the eastern Déné dialects even lack altogether the same person of the personal pronouns, but the Nah·ane are not so verbally destitute. In their minds, however, there lurks some vague confusion about the difference between the first and the second person plural of those pronouns which, at times, does not seem to be fully grasped.

In common with those of the other Déné dialects, the Nah·ane verbs are rich in persons, some, like the verbs of station and the verbs of locomotion, having as many as eighteen for each tense, as against the twenty-one their Carrier equivalents boast of. In the face of that relative richness it is somewhat of a surprise to find that the regular or common verbs have not even a single person representing the dual, which is rendered, as with us, by the plural, while even the Carrier, which is rather deficient in that respect, possesses, at least, the first person dual for all the verbs.

A point of resemblance with the eastern dialects is the plural of some Nah·ane verbs, which is formed by the incorporation of the particle *da*, without any alteration of the desinential syllable. Thus, until we come to the plural, the conjugation of the verb *t'sé-mészit*, I wake up, is practically that of its Carrier equivalent. But after this, the similarity is confined to the main or initial root, which, through all tenses and with any person, remains invariable in all the dialects. The following partial conjugation of the present of the above mentioned verb will illustrate my remark :

	CARRIER.	NAH·ANE.
Dual.—	*t'se-nîtzit*, we wake up, both of us.	Dual.—*t'se-nîtzit.*
	t'sé-nœhzit, you wake up, both of you.	*t'se-nahzit.*
	t'sé-rhœnzit, they wake up, both of them.	*t'se-hœnîzit.*
Plural.—	*t'se-t'sœntilh*, we wake up.	Plural.—*t'sé-dasîtzit.*
	t'sé-nœhtilh, you wake up.	*t'sé-dahzit.*
	t'sé-rhœntilh, they wake up.	*t'sé-dahezit.*

Another most important point of resemblance of the Nah·ane with the eastern Déné dialects, is the utter absence in the former of any special negative form. This particularity may be said to constitute its fundamental difference from the Carrier, Babine and Chilcotin idioms, the verbs of which are distinguished by at least one, and frequently two or even three syllabic inflections in addition to the negative particle.

Instead of this the Nah·ane set that particle before the verb, which remains under its affirmative or normal form.

To sum up. The Nah·ane language is much less complicated and verbally poorer than the Carrier. It is also less pure in its lexicon, more embarrassed in its phraseology, and owing to its accent, even more delicate in its phonetics.

THE PALÆOCHEMISTRY OF THE OCEAN IN RELATION TO ANIMAL AND VEGETABLE PROTOPLASM.

BY A. B. MACALLUM, M.A., M.B., PH.D.

Read 17th January, 1903.

CONTENTS.

I.—INTRODUCTION.

THE history of the composition of ocean water is a question of very great interest to the geologist, the physiographer and the biologist. To the geologist and physiographer its importance lies chiefly in the fact that it is associated with the history, on the one hand, of erosion and denudation of land surfaces of the globe, and, on the other, of the formation of all the sedimentary strata. The ocean, ever since the first condensation of water on the rockcrust of the earth, has acted as a gigantic solvent, and the salts it now holds in solution represent what it has retained after its action for millions of years as a leaching and filtering agent. The sedimentary rocks are thus but a vast precipitate from the ocean of what had been partly suspended and partly dissolved matter in it during all the geological periods. The history of the composition of the ocean is, on this view, the complement of the history of all the terrigenous changes necessary to fill out all the pages of the record of events that have transformed the surface of the earth.

To the biologist the value of the question obtains from a different point of view. The sea is the original home of all life on the globe, and it was in the sea that the differentiation between animal and vegetable life, as well as the evolution of the great divisions of the animal kingdom were effected. Indeed the great events in the evolution of animal forms have been rendered possible by changes which have taken place in the composition of ocean water. These changes have modified organisms, and have created conditions which have served as factors in directing the course of development. This may be specially illustrated by reference to the case of the calcium salts in sea water. That the earlier Archæan seas contained comparatively small quantities of calcium compounds seems to be clearly indicated by the fact that in pre-Cambrian strata the limestone deposits are very limited, not more than two per cent. of the thickness of the beds, the Huronian portions of which, now generally recognized as of sedimentary origin, are, according to Lawson,* over 50,000 feet in thickness. The small amount of limestone deposits could not have been due to the absence of living organisms, for the oldest Cambrian beds contain Trilobites and Brachiopods, and such highly specialized forms postulate a long course of pre-Cambrian life. The very fact that the Brachiopods of the early Cambrian were largely those provided with a horny or chitinous shell, indicates that all the animal forms of the preceding period had imperfectly acquired the lime "habit," which, one may reasonably believe, would have earlier made its appearance had calcium salts been present in considerable quantities in ocean water from the first. It is perhaps due to the absence of this lime "habit" that fossils do not obtain in pre-Cambrian strata.

Once, however, the lime "habit" was acquired, through adaptation of the animal cell to its environment, the course of development became accelerated, and the evolution of the higher types of Invertebrate life, as well as all the forms of Vertebrata, became possible. The Vertebrate skeleton, and all that it implies in evolution, is, therefore, a result of the gradual increase in the quantity of calcium in the oceans of the pre-Cambrian period.

To both the geologist and the biologist the history of the chemistry of the ocean has recently acquired an additional interest from the attempt made by Joly† to determine the age of the earth, who uses for that purpose as factors the amount of sodium now in the ocean, and that

* Geol. Survey of Canada, 1887, pp. 101 and 102, F.

† An Estimate of the Geological Age of the Earth. Trans. Roy. Dublin Soc., Vol. 7. (Ser. 2), 1899, p. 23.

estimated to be in the annual river discharge of the globe. Joly took for these the results of Murray,* who, basing his calculations on the discharge of nineteen of the principal rivers of the world, estimated the total amount of the sodium and other salts annually put into the sea by river water. Joly finds from Murray's tables that the sodium annually discharged is 157,270,000 tons, and the quantity in the sea is 14,151,-000,000,000,000 tons. Dividing the latter by the former he gets as quotient, approximately, 90,000,000, which, expressed as years, would be the age of the earth, or, rather, the period of time which has elapsed since the first condensation of water vapour took place on the globe. Joly admits that the ocean at first contained a considerable quantity of sodium as sodium chloride, and this he puts at about 14 per cent. of the present amount in the sea. This would make the amount discharged into the sea by river water less than that stated above, but, on the other hand, the volume of the ocean may, as a result of more recent estimations, be given a higher value, and in consequence the mass of sodium in it would be 15,627,000,000,000,000 tons. Further, of the sodium annually put into the ocean, Joly allows as much as 10 per cent. for that which is taken from the ocean by the rain and returned again in river water, and this estimate would make the amount of river sodium, which is annually leached out of the rocks and strata, as 97,800,000 tons. With these values Joly finds that the corrected figures for the age of the earth is 89,300,000 years.

In support of his contention Joly shows that as compared with the igneous rocks there is in the sedimentary rocks, which are derived from them, a deficiency of sodium, and that the sodium now in the sea would approximately account for the difference. The bearing of this fact is that all the sodium now in the ocean was derived from the original rock crust by processes which to-day are in operation in decomposing rock material and removing the sodium therefrom. In other words, the discharge of sodium into the sea has been in the past a uniform one, or at least subject to no great variations that would constitute a factor against determining the age of the earth by this method.

This estimate has been ably criticized by the eminent geologist, the Rev. Osmond Fisher,† who points out that the sodium which is derived from the decomposition of crystalline or igneous rocks is in the form of carbonate rather than chloride; and he asks whether it is not possible that the chloride of river water is derived, not from crystalline,

* On the Total Annual Rainfall on the Land of the Globe, and the Relation of Rainfall to the Annual Discharge of Rivers. The Scottish Geogr. Mag., Vol. 3, 1887, p. 65.

† Geol. Mag., New Ser., Vol. 7, p. 124, 1900.

but from sedimentary rocks, or from what Sterry Hunt calls "fossil sea water, still to be found imprisoned in the pores of the older stratified rocks, and presumably in the younger as well." To answer this affirmatively would be of necessity to assert that the sodium which now goes to the sea as sodium chloride comes from the supply derived from and deposited by the sea in ancient geological strata—that is, what was at one time in the sea is being returned to it again. Fisher also points out that the strata which are now in the process of formation, imprison sodium chloride in their mass, taking it from the sea. There would thus be a constant circulation of sodium chloride from the ocean to the stratified rocks and back again to the ocean. That would also postulate that the sea was almost as rich in sodium chloride in Silurian times as it is now, and it would go far to support the view that "the sea was salt from the first;" but if we assume that the sodium of the sea is derived from those sodium compounds supplied by rivers other than the chloride, the estimate of the age of the earth, as given by Joly, would have to be multiplied several times in order to get the approximate length of the period which has elapsed since the oceans of the globe were first formed.

Another criticism of Joly's view, made along the lines followed by Fisher, is that advanced by Dubois,* who, from a comparison of the amounts of sodium and chlorine supplied to the sea by a large number of rivers, concluded that only a small portion, if any at all, of the sodium derived from denudation appears in river water as sodium chloride; that the sodium chloride discharged into the sea annually is derived from the rainfall, and the salt deposited in the older strata by the sea.

As Fisher has already pointed out, it is the sodium compounds other than the chloride that ought to be considered as being primarily derived from the disintegration of rock mass, and, therefore, primarily added to the sea. What the total amount of this sodium is cannot be determined with approximate certainty, but Dubois is inclined to regard it as about one quarter of the total discharge of sodium into the sea as given in Murray's tables, and, consequently, Joly's estimate of the length of the period which has elapsed since water first condensed on the earth's surface would have to be multiplied by four, the product being approximately 400 million years.

* On the Supply of Sodium and Chlorine by the Rivers to the Sea. Kon. Akad. v. Wetensch., Amsterdam, Proceedings of the Section of Sciences, Vol. 4, p. 388, 1902.

II.—THE ORIGIN OF THE PHYSIOLOGICAL RELATION OF THE CHEMICAL ELEMENT IN BLOOD PLASMA.

I have thus dealt at some length upon the importance of the history of sea water, and with Joly's views and those of his critics, because all this leads up to a question which is of very great importance to the physiologist. The life of the globe in the earlier geological ages, so far as the strata reveal to us the past history of the earth, as already pointed out, was closely associated with the sea. It is indeed almost universally assumed that life began in the ocean and continued in association with it alone till the close of the Cambrian period, although the presence of graphite in Cambrian and older rocks seems to indicate that vegetable organisms were accommodating themselves to a land life. Even this may not be an exception, for these rocks must have been laid down under water, and therefore their organic remains would be those of the sea. If accordingly we could know what the composition of the sea water in the Cambrian and pre-Cambrian periods was, we would, in all probability, be able to determine some of the chemical and physical forces to which living matter was then subjected and thus explain the relations which obtain to-day in living matter between it and its salts. In a recent paper* I have pointed out that the relative proportions of the elements, sodium, potassium, and calcium in the plasma of the blood are surprisingly very like those which are found in the ocean water of to-day, and that the differences which obtain between the two series of proportions of these elements may be explained on the ground that such proportions in the blood plasma are those that obtained in ancient sea water when the ancestral form of Vertebrates, in which sea water was the circulatory fluid, as it is in many marine forms to-day, acquired a closed circulatory system. That the ancient proportions are reproduced to-day in all forms, which have a closed circulation, I attribute to the influence of heredity, the cells of the organisms having for ages been associated with the sodium, potassium, and calcium in certain proportions, and having been accommodated to them, the relations ultimately became so fixed that living matter reproduces the ancient proportions in the fluids which bathe itself. There is one point in which the proportions in the circulatory fluid and those in sea water differ, and that is in respect to the magnesium. In the sea water of to-day there are 11.99 parts of magnesium for every 100 of sodium, while in plasma there are 0.8 parts of magnesium to 100 of sodium. This is

* On the Inorganic Composition of the Medusæ, Aurelia flavidula and Cyanea Arctica. Journ. of Physiol., Vol. 29, p. 213, 1903.

a striking difference but it is easy of explanation. The proportion of magnesium in sea water is now slowly growing. In the pre-Cambrian oceans it must, therefore, have been very small, not perhaps as low as it is in blood plasma, for in the latter the magnesium would only represent the proportion of an earlier period than that in which the circulation became closed, as the tissues would only reproduce the proportion which had by long accommodation become fixed in them. Even the organisms which live in the sea to-day, whose ancestral forms have lived in the sea since the Cambrian, do not take up the magnesium from the sea water in the full proportion which it has in the latter.

III.—THE ORIGIN OF THE RELATION OF THE CHEMICAL ELEMENTS WITHIN PROTOPLASM ITSELF.

There is, therefore, so far as the circulatory fluid of Vertebrates is concerned, a reproduction of the proportions of the sodium, calcium, and potassium of the pre-Cambrian oceans. The problem which now arises is one whose solution involves greater difficulties. If organisms should reproduce in their own circulatory fluids the proportions of the elements in the early geological periods, what contributed to those remarkable proportions which obtain, not in the circulatory fluids, but in the living matter itself? These proportions are widely different from those found in the circulatory fluids, and one cannot bring oneself to regard the former as derived from the latter. In vegetable organisms the potassium and the calcium much exceed the sodium, and even the magnesium may be greater in amount than that of the latter. In animal organisms the proportions are difficult to ascertain owing to the presence of skeletal and other structures in which the calcium and sodium greatly preponderate, but even in these the potassium is nearly equal to the sodium, and in muscle it is greatly in excess, while the calcium and the magnesium are much less than the sodium. Thus, in the muscle of the dog the relative values for each are* :—

Na.	*K.*	*Ca.*	*Mg.*
100	354	7.26	25.1

These proportions may or may not represent approximately those found in unicellular organisms like an Amœba, or even a white blood corpuscle, but do they represent to any degree the proportions which obtained in the early pre-Cambrian seas when life was represented by unicellular organisms only; which accommodated themselves to the sodium, potassium, calcium, and magnesium in their habitat, just as the

* Julius Katz, Pflüger's Arch., Vol. 53, p. 1, 1896.

marine unicellular organisms of to-day have accommodated themselves to these elements in the sea water? If the blood plasma of Vertebrates, because of the forces of heredity, reproduce the proportions which obtained in pre-Cambrian oceans, why should not the cells of the tissues because of the same forces, reproduce in themselves the proportions which obtained in sea water of a much earlier geological period? In other words, if the proportions in the plasma are inherited, why should not those found in the living matter be considered as inherited also? An affirmative answer to this question would postulate that the proportions of the four elements in early pre-Cambrian seas were very greatly different from what they are now in the ocean—as different almost as the proportions of the four elements in muscle are from those found in the blood plasma.

The question is one of great importance in physiology, and, though its solution presents great difficulties, its very interest compels a consideration of it. We know that the unit of living matter, the cell, whether of animal or vegetable kingdom, presents, on the whole, the same type of structure, and it goes through the same morphological changes. Some of these are grouped under the process of division, and its characteristic details are the same in both animal and vegetable forms. Now, the animal and vegetable cells are derived from a single type which must have existed at the very dawn of life on the globe. The whole process of division, with its peculiar morphological features, was elaborated in this single-celled organism, which transmitted it to its descendants. Since, as already stated, the process of division is the same in both kingdoms, it is obvious that it has continued almost unchanged through an infinity of generations, animal and vegetable, and for many millions of years, and that this preservation of the original type is due to heredity. If, now, heredity is so powerful in regard to structure, is it a negligible force in regard to chemical composition? Is living matter fixed in structure almost beyond change, however widely the conditions under which it lives may vary, but unfixed and changeable in its relations to the chemical elements? As structure depends so largely on composition, it would be difficult to explain how living matter could so widely vary its relations to the elements and at the same time retain its structure.

We are, therefore, forced to a choice of hypotheses of which one postulates that all of the relations of living matter to sodium, potassium, calcium and magnesium are a result of inherited forces, while the other concedes that in regard to the circulatory fluids the proportions are determined by heredity, but the relations of these elements in living matter itself are due to quite different forces in which heredity is a small factor or no factor at all. The acceptance or rejection of either hypothesis depends on the evidence which we can bring as to the composition of the ocean in the very earliest geological periods.

The conclusions which we can formulate on this point depend on what we accept as the composition of the original crust of the lithosphere, and in our knowledge of the character and composition of the sedimentary rocks, and they must also be based on the changes which are admitted to have taken place in the composition of the ocean during all the periods. These conclusions I propose to deal with here in a general way only, for a full consideration of all the facts which have a bearing on them would demand a detailed treatment which would far exceed the limits set for this paper.

IV.—THE COMPOSITION OF THE PRIMEVAL OCEAN.

The original condition of the earth was a molten mass in which the temperature was so high that many of the elements now in the rock crust were in a gaseous condition, and dissociated, just as they are at present, in the solar atmosphere. As the dissipation of heat went on some of these must have condensed at degrees of temperature which approximated their present respective volatilization points, while the remainder, oxygen, hydrogen, chlorine, sulphur and carbon would combine to form water, hydrochloric, sulphuric and carbonic acids. The elements, sodium, potassium, calcium, magnesium, and aluminum would also before condensation take out of the original atmosphere chlorine, sulphuric acid, oxygen, and perhaps, carbonic acid, to form the chlorides, sulphates, oxides, and carbonates of these elements, but whether these compounds obtained after condensation depended on whether the temperature of the heated rock surface was still as high as their respective dissociation points. When the molten magma had cooled down to a degree below the lowest dissociation point, all the compounds referred to would be either deposited on the hot rock surface or in the form of vapour in the then atmosphere. When the temperature of the latter had fallen to about 1000°C, all these compounds were removed by condensation, for although, under the atmospheric pressure which now obtains, the temperature of condensation is for nearly all these compounds about 200° lower, the very great atmospheric pressure of the pre-oceanic period must have rendered the combination of the dissociated elements and the condensation of the compounds formed from them possible at a much higher temperature.*

At such a temperature the previously molten rock had become rigid, and of course the condensed compounds would be deposited on its surface, and when refusion of the rockcrust occurred, as it must have done over large areas, large quantities of the deposited compounds would be diffused through the superficial crust. When the cooling of the atmosphere and globe progressed until the temperature of the former was 370°C, the first condensation of water took place on the rock surface. The atmospheric pressure, according to Joly,† must have been about 270 times what it is now. According to Clarke's ‡ estimate of the relative values of water and carbon dioxide to that of the solid portion of the globe, the atmospheric pressure before the first condensation took place, was about 247 times what it is at present. Joly affirms that at 370°C a pressure of 190 atmospheres would produce a condensation of water, and, as the pressure was much higher, condensation would go on till the pressure fell below 190 atmospheres. This would entail rapid evaporation, for at many points the temperature of the rock surface would be so high that the water would condense only to boil away immediately. This would collect the salts deposited on the surface in masses, and it would, as in the case of the chlorides of magnesium, iron and aluminium, convert these into oxides of these metals and free chlorine, which, uniting with hydrogen, would form free hydrochloric acid. The other chlorides, namely, those of sodium, potassium and calcium would be unaffected. The ferric chloride would in some cases be volatilized but to be recondensed.

This condensation of the water vapour, and the re-evaporation would occur a countless number of times before there would obtain a permanent body of water on the globe. Where such first occurred there would be a lower temperature than elsewhere, and in consequence further condensation of water vapour would occur there also. The result would be the first ocean basin, the weight of the body of water acting on the

* The volatilization points of potassium, sodium and magesium are 667°C, 742°C, and 1100°C respectively. The melting points of calcium and aluminium are unknown. The melting points of certain sodium and potassium compounds are, according to V. Meyer & Riddle (Ber. d. d. Chem. Gesell. Vol. 27, p. 2,443,) as follows:

Na Cl	851°C.	K Cl	766°C.
Na Br	727°C.	K Br	715°C.
Na I	650°C.	K I	623°C.
Na_2CO_3	1098°C.	K_2CO_3	1045°C.
Na_2SO_4	843°C.	K_2SO_4	1073°C.

† *Op. cit.*

‡ F. W. Clarke, The Relative Abundance of the Chemical Elements. Bulletin U. S. Geol. Survey No. 78, 1891.

thin crust and easily affecting the depression. These phenomena would be repeated at other points as the temperature of the crust and the atmosphere gradually lowered, until at a point below 100°C. nearly all of the water originally present in the atmosphere had condensed to form the oceans of the globe.

The composition of the ocean would follow from the occurrence of the soluble chlorides, sulphates and carbonates of the metals which came in contact with the first condensations. As pointed out, the condensation of superheated water would convert the chlorides of magnesium, iron and aluminium into magnesia (Mg O) oxide of iron ($Fe_2 O_3$) and alumina ($Al_2 O_3$), the first of which is soluble only in 55368 parts of hot or cold water,* while the two latter are practically insoluble, even in dilute acids. The magnesia, of course, would dissolve in water which contained either hydrochloric or carbonic acids, but the amount dissolved would, on account of the slight quantity of these acids in the water, be very small. The other chlorides, namely, those of sodium, potassium and calcium, although equally abundant, would not be leached out of the rock surface in equal amounts. The solubilities of these salts differ. For example, 100 parts of water dissolve at 99°C 154 parts of calcium chloride, 56.3 parts of potassium chloride, but only 39.7 parts of sodium chloride. In consequence there would be different quantities of each chloride dissolved, and the calcium chloride would by far predominate, while the potassium chloride would be more abundant than the corresponding sodium compound. There would, as already pointed out, be very little ferric chloride and what would be dissolved would gradually all be converted, first into the colloidal ferric hydrate, and eventually into the insoluble oxide of iron.

It does not follow that the ocean would contain, even after a long period of action on the rockcrust, the whole of the chlorides of calcium, potassium and sodium originally disposed over and diffused through the now more or less rigid rockcrust. The constant washing out of the land areas would no doubt tend to remove these salts from the rocks until there would be little left in the latter and at the same time they would become correspondingly more abundant in the sea water. But other salts would begin to appear there also. The magnesia derived from the chloride of magnesium, through the action of superheated water, would, under the action of carbonic acid in the rain water, go into solution as carbonate, but the amount so dissolved, would, on account of its low degree of solubility, be very small and it would only

*Fresenius, Liebig's Annalen, Vol. 59, p. 123.

after a long period of time become appreciable in the ocean. The carbonic acid in the rain water must have acted, as it does now, on the silicates of sodium, potassium and calcium in the rocks and produced free silica and carbonates of these elements, these latter going into solution and thus reaching the ocean, where, acting on the chloride of calcium, carbonate of lime and chloride of sodium and potassium would be formed. The calcium carbonates would be removed by deposition and thus constitute the origin of the limestone beds of the pre-Cambrian age, but the chlorides remaining in solution, thus contributed to an increase in the amount of sodium and potassium in the sea water.*

The sulphates in the rock crust disintegrated or affected would also be carried to the sea, but, as these would be small in quantity, they need not be specially considered here.

Thus the history of the sea must have begun and continued for a period of unknown length. The only change came from the discharge into the sea of the carbonates, the consequent removal of the lime and the slow increase in amount of magnesium, sulphuric acid, and of potassium and sodium. The two latter elements were not removed from the sea except through the rainfall. As I shall presently point out, the potassium compounds are to-day removed from the ocean apparently as rapidly as they are added by river water, and, in consequence, the amount in sea water now appears to be stationary. In the earliest geological period the conditions which now contribute to this result did not exist, and the ocean retained all the potassium it held or received through river discharge. In all probability the potassium equalled, and even exceeded, the sodium in amount.† When sediments began to form, and, when soils made their appearance, then, and then only began the elimination of the potassium from the ocean. It has been long established that potassium manifests a marked capacity to unite with silicates of alumina to form firm compounds, and these obtain whenever potassium salts in solution come in contact with argillaceous material, sedimentary or otherwise,‡ while the sodium, magnesium, and calcium are unaffected.

* Sterry Hunt (Chemical and Geological Essays, Boston, 1875) held the view that the most abundant constituent in primeval sea water was calcium chloride, and that with the gradual addition of sodium carbonate calcium was removed as carbonate and sodium chloride consequently took its place.

† Joly (*loc. cit.*) assumes that the greater part of the chlorine now in the ocean was originally united with the iron, calcium, magnesium, potassium, and sodium, these elements entering into combination in proportion parallel to the proportions in the rockcrust as determined by F. W. Clarke (*loc. cit.*) This postulates that 14 per cent. of the chlorine now in the ocean was united with sodium, and consequently the ocean originally contained about one-seventh of the sodium it now holds. As the proportion of sodium to potassium in the rock crust is 100 to 95, on Joly's hypothesis the potassium in the primæval ocean must have really equalled in amount the sodium therein. Joly, however, is in error in supposing that the chlorides of magnesium and iron could have existed, and he should consequently have made a greater allowance for the amounts of chlorine combined with the sodium, potassium, and calcium.

‡ Sterry Hunt (*op. cit.* p. 95.)

The capacity to abstract the potassium is increased if the silicates are mixed with organic matter. Consequently the potassium which rain water may contain is in great part removed when the latter filters through soils, and, therefore, the water discharge from alluvial areas is always richer in sodium than potassium. This capacity of soils to abstract potassium is a matter of direct demonstration, and it "explains the presence of so small an amount of potassium salts in the waters of rivers, lakes, streams, and oceans where the lime and soda have accumulated."* This cause of deficiency acts not only in the case of the potassium leached out of disintegrating rock by rain water, but also on the potassium carried from the sea to the land areas by rain water. The potassium thus carried is not inconsiderable, for, according to M. J. Pierre,† the rain water in the neighbourhood of Caen (France) annually carries to each hectare of land, about 7.9 kilograms of this element, or about 1.23 tons per square mile.

This mode of elimination also operates in the ocean, where, however, the organic matter responsible for the removal, is derived from plankton organisms, which, on dying, fall to the sea bottom and their remains decomposing, the potassium they hold reacts with the argillaceous material on which the deposits rest and forms the mineral known as glauconite, containing as low as 0.95 per cent. of oxide of potassium, but other estimates range from 2.52 to 4.21 per cent. The sodium present is very much less in quantity.‡ This mineral is now being formed, as it has been formed in the past, on the ocean bottom over the areas which fringe the continental coasts and it constitutes as much as, or more than, half of the deposits in shallower waters. Considering the extent of these areas as well as the fact that they cover the sea bottom of those localities into which river discharge takes place, it will be recognized what a very important factor the constant formation of glauconite is in eliminating potassium from sea water and thus preventing an increase in the amount of that element in the ocean. This formation has been going on in the past geological periods, for it is to be found§ in the primary formations of Russia and Sweden, in the sands

* Mendeleef's Chemistry, Vol. 1, p. 547, 1897.

† The reference is given in Dr. Angus Smith's "Air and Rain," which is quoted by Joly (*loc. cit.*)

‡ The analysis of five specimens as given by Murray & Renaud (Challenger Report, Deep Sea Deposits, p. 389) gave:

	I.	*II.*	*III.*	*IV.*	*V.*
Ca O	1.69	1.26	1.27	1.34	1.19
Mg O	2.49	3.13	3.04	2.83	4.62
K_2 O	2.52	4.21	3.86	3.36	0.95
Na_2 O	0.90	0.25	0.25	0.27	0.62

Other analyses quoted by Roth, (Allgemeine and Chemische Geologie, Vol. 1, p. 559, 1879) gave a percentage of potassium (not K_2 O) varying from 2.8 to 7.3.

§ Murray & Renaud, op. *cit.*, p. 384.

and gravels of the Cambrian sandstone of North America, in the Quebec group of Canada and in the coarse Silurian sands of Bohemia. In the Mesozoic period it was more abundantly formed and its deposits are very marked in the strata of the Cretaceous division. It is also found in the Tertiary from the lowest strata to the highest of the series. It is thus shown that the formation of glauconite occurred in all the geological periods from the commencement of the Palæozoic Age to the present time and that thus a very large proportion of the potassium which the ocean would now contain, were it not for the formation of glauconite, has been removed from it.*

In the formation of glauconite, organisms appear to play a very distinct part and amongst these the Foraminifera are the most important. The decomposing organic matter of the dead forms liberates sulphur which combines with the iron in deposits to form sulphide.† This latter is converted into sulphuric acid which, acting on the fine clay sets free colloidal silica and ferric hydrate in a condition which promotes their union and the silicate so formed combines with potassium to form glauconite. It is obvious that organic matter is a very important factor in the process and that in the absence of animal organisms no glauconite would be formed, a view which explains the almost complete absence of this mineral from the deep sea areas, but it also postulates as decidedly, that before the appearance of living forms in the primeval ocean, there was little or no potassium eliminated from it, and this, taken in conjunction with the fact that in earlier pre-Cambrian times there could not have been much or any soil to affect the potassium in the waters discharged from the land areas, makes it quite clear that there was a period during which the potassium content of the ocean must have increased absolutely and that this was succeeded by a period in which the amount of the potassium ceased to increase or remained practically stationary, while decreasing relatively to other constituent elements. The beginning of this latter period coincided with the appearance of living forms in large numbers in the sea.

The history of sodium in the ocean has been one of uniform increase through all the geological ages. The addition that is to-day being made by river discharge is large and must have obtained as abundantly in the past. There have, on the other hand, been no important agencies which have served to eliminate it from the ocean. The great salt deposits, some of which are as old as the Cambrian, are,

* Forchhammer was the first to point out that potassium is being removed from the ocean, (British Association Report, 1844, p. 153.) From his analysis of Fucoids and of the metamorphosed Fucoid schists of Scandinavia he came to the conclusion that Fucoids constitute a very important factor in the process.

† Murray and Renaud, *op. cit.*, p. 389.

as is certainly the case with the Stassfurt beds, the result of the evaporation of land-locked arms of the sea.* and they are constituted of but an infinitesimal fraction of what is contained in the ocean. Sodium chloride, like other constituents of sea water, is carried landward with evaporation and rain clouds, but it appears to be returned to the ocean, without any perceptible loss, through the river discharge. The only method of elimination which at all possibly counts is that in which it is imprisoned mechanically in the sedimentary deposits during their formation. That sodium chloride is removed in this way has been pointed out and emphasized by Osmond Fisher, but there are no data which serve to indicate that this is a considerable factor in diminishing the sodium content of the ocean. All the known facts point in the contrary direction. There is no mineral in the course of formation, which is extensive or abundant in its distribution and which also requires considerable quantities of sodium for its production, and there are, further, no agencies acting in the soils which serve to remove sodium compounds from the percolating water.

In these considerations we find a full explanation for the relative proportions (100 : 3.613) of the sodium and potassium which now obtain in sea water, and also for those which obtain in the river discharge of the globe. According to Murray's estimate for nineteen principal rivers, the proportions would be 100 : 38.6. We may postulate from this that in the early geological periods of the pre-Cambrian period, when soils did not exist, the quantities of each element discharged by rivers or bodies of water derived from the land surface, were nearly equal. Since the primeval ocean, as pointed out above, contained these elements in almost equal quantities, this condition must have continued until long after soils holding organisms and organic matter had appeared, and even for an indeterminable period after organisms had made the ocean their habitat. The change in the relative proportions once begun must have gone on with extreme slowness, and oceanic organisms, at first wholly of the unicellular kind, must have, after acquiring a relation to these elements, just as slowly responded to the changes in the proportions of their medium.

The river discharge of the globe has been from primeval times adding also magnesium and calcium to the sea. According to calculations based on Murray's data, the proportions relative to the sodium shown in these are 134 and 591 respectively to every 100 of the latter. This is, of course, based on approximate estimations, and they may be incorrect,

* See G. P. Merrill's "Treatise on Rock and Rock Weathering and Soils," p. 120, 1897.

as they seem to be, if one scrutinizes the proportions that are found in rivers whose waters have been carefully analysed. There are only two rivers, the Amazon and the St. Lawrence, which give nearly the proportion of magnesium called for by Murray's estimates, while the Ottawa, the Mississippi, and the Nile give quantities much below that of the sodium, and the quantities of the calcium are found to vary very much for the different rivers. If we disregard Murray's estimates and base our observations on the analyses of the various rivers, we can safely conclude that, while the quantity of calcium added, except in the case of the Nile, is always, and sometimes very much, greater than the sodium addition, the latter does not probably exceed the amount of the magnesium discharged. In the ocean, however, the sodium, calcium, and magnesium have the proportions of 100, 3.91 and 12.0.

The comparatively low proportion of magnesium in sea water is explainable. In the first place, as pointed out above, there must have been in the primeval ocean but very little magnesium, owing to the conversion of all the chloride of magnesium into magnesia which is, except in minute quantities, insoluble. The conditions which so affected magnesium chloride left the chlorides of calcium, sodium and potassium unchanged, and in conseqence these went into solution in primeval sea water, and were, therefore, as compared with magnesium, very abundant. Further, the ocean at first must have contained only traces of the latter element and the subsequent addition of it through river discharge would increase the amount in sea water, but not to such an extent as to make it overtake the sodium.

There is another factor which operated in limiting the amount of the magnesium. This is the tendency shown by the chloride to interact with the carbonate of lime when the latter undergoes deposition to form limestone, and, in consequence, this always contains carbonate of magnesia. When the latter exceeds 10 per cent. the mixture of the carbonates is given the conventional name of dolomite, and in some formations of this kind the magnesia is found greatly to exceed the lime. Dolomites are found in all the periods down to and including the Cambrian and even in the pre-Cambrian, it is associated with the crystalline schists.* An exact estimation of the magnesium so localized is impossible, but on the average it cannot be more than 10 per cent. of the quantity of the calcium due to deposition, so that the amount of magnesium removed annually from sea water must fall far behind that of the calcium. It follows from this that whatever were

* Zirkel, Lehrbuch der Petrographie, 1894, Bd. 4, p. 499.

the proportions of these two elements in primeval sea water, the proportions must have slowly changed, and as a consequence the magnesium must have gradually increased while the calcium practically remained stationary.

It must of course be admitted that magnesium is withdrawn from the ocean by organisms, but the amount thus removed is very small, and in no case is it an important method of eliminating the element from sea water. In the hard part of corals it is as a rule under one* per cent. and in the coralreefs it is less than that in amount, while the calcium constitutes nearly 40 per cent. Forchhammer's† analyses of the ash of sea weeds reveal a quantity of magnesium which he regarded as important, and he held that the Fucoids thus remove quantities of this element and deposit them in the beds which contain the solid substances of sea weeds as far as they are insoluble in water.‡ According to the analyses of Gödechens,§ the ash of Fucoids contains from 4 to 7 per cent. of magnesium. That the element is eliminated from sea water by these forms may be conceded, but it is doubtful if the quantity removed in this way is sufficient to affect materially in time the total amount retained in the ocean.

We may conclude, therefore, that in the formation of dolomites, of magnesia-holding limestones and chalk deposits, and, to a minor degree, in the activities of animals and plants, elimination of magnesium from sea water has always obtained; and, further, that the amount eliminated annually does not equal the amount of magnesium added to the sea by river discharge. This postulates a constant increase in the amount of magnesium in the sea; and in this respect it must be ranged with sodium, which increases in amount at a greater rate, since, so far as is known, there are for it no agencies of elimination in operation which compare with those affecting the potassium, the calcium, and even the magnesium. The sodium, therefore, though it is not added in greater amount than in the case of the latter, is increasing at a greater rate, and thus the proportion of sodium to magnesium in sea water is slowly altering. As pointed out above, the primeval ocean must have contained but an exceeding small quantity of magnesium, and the amount of the latter now in it is practically wholly derived from the leaching out of the land surfaces during the intervening ages.

As regards the calcium in sea water there is less uncertainty. The

* According to Forchhammer the corals, *Isis nobilis* and *Corallium nobile*, contain 6.36 and 2.1 per cent. respectively of magnesium carbonate.

† Roth, *op. cit.*, p. 616, where the results of analyses of a number of forms are given.

‡ *Op. cit.*, p. 159.

§ Ann. d Chem. und Pharm., Vol. 54, p. 351, 1854.

calcium of river discharge greatly exceeds in amount that of the three other elements, and yet it is less abundant than either in the ocean. If there were no elimination of calcium from sea water, the salts of the latter element would long have reached the point of saturation in the ocean. The present condition is easy of explanation. On the one hand, calcium separates from sea water through the formation of sulphate and carbonate of lime, which are to a high degree insoluble. This constitutes in part the origin of the gypsum beds and of the limestones of sedimentary origin. On the other hand the myriads of organisms that have their habitat in the sea have the lime "habit," and they consequently remove from solution enormous quantities of calcium. This is the case not only with all forms provided with exoskeleta and endoskeleta, into the composition of which lime largely enters, but also with those which exercise the precipitating effect on the calcium salts they absorb from sea water, the precipitation rarely going so far as to form a distinct deposit in the cells or tissues of the organism. This power to precipitate is universal, as shown by the fact that the capacity to form calcareous skeleta is almost universal, and this capacity is merely an enhancement of the power to precipitate. The latter, therefore, operating so largely, separates calcium from sea water, and on the death and disintegration of the organisms, the element is deposited on the sea bottom either as phosphate or carbonate of calcium.* These deposits, owing to the fact that they contain few calciferous fossils, are regarded as due to chemical reactions alone; but if they are, sedimentary limestones should be of a more uniform distribution, whereas we find them more or less localized. The explanation that they are due to protoplasmic "secretion" and not to either chemical reaction or skeletal deposition in living forms accounts for much, and indicates what a factor living protoplasm, animal and vegetable, is in the separation of calcium from sea water.

Sterry Hunt † advanced the view that in the primeval ocean the chief salts were chlorides of calcium and magnesium, and that the constant, large output by river water of carbonates of sodium and potassium, and particularly of the former, affected a conversion of these into carbonate of lime and chlorides of sodium and potassium which were retained in solution, while the carbonate was deposited. The objection to this view is that, if it is correct, the conversion ought to have taken place in the pre-Cambrian period, and, therefore, there ought to be extensive limestone deposits in the rocks attributed to that period.

* Sterry Hunt, *op. cit.*, pp. 82 and 311.

† *Op. cit.*, pp. 2 and 41.

There is, indeed, in these rocks only a small amount of crystalline or other limestone, and there appears to be still less in the divisions of the Huronian, which, as pointed out, have a thickness in the Lake of the Woods and Rainy Lake districts, according to Lawson,* aggregating 50,000 feet, all representing sedimentary formations. What limestone and gypsum are present in pre-Cambrian rocks can very well be attributed to the occurrence of calcium salts in the oceans of the Archæan in quantities, however, which could not have very greatly exceeded those which obtain to-day in sea water.

V.—THE RELATION OF THE SALTS IN THE OCEAN TO PROTOPLASM.

From the considerations advanced in the preceding section of this paper, it follows that the ocean has been, and is now, slowly changing, not in its composition, but in the proportions in it of the various elements to each other, and that, as a consequence, it is now in this respect greatly different from what the primeval ocean was in the period following the first condensations of water vapour on the rock crust of the globe. It may again be noted that in all these changes there are two distinct periods. In the first, or older, life was not represented except towards its close, and therefore, the only factors engaged in eliminating any of the elements from the sea were purely chemical ones such as are illustrated in the precipitation of lime as carbonate and sulphate and of magnesia as carbonate. In this period the elements must have differed in amounts from each other less markedly than they do to-day, and the constant addition to these from the discharge from the land surfaces did not tend to alter, even after a very long interval, the proportions which first obtained. This period must have terminated some considerable time after the appearance of living forms on the globe, and especially only after the adaptation of vegetable forms to a land life, and the consequent production of soils. The second period could not have begun at once after the appearance of living forms, for these must first have acquired a relation to the elements and then have developed the habit of disposing of the various salts which they took out of the sea water. This period may well be supposed to have begun when there had developed not only a considerable diversity of forms in the sea, but also the organisms which contribute to the production of organic matter in soils. In this period the removal of potassium from the land surfaces decreased,

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* *Loc cit.*

and the combination of this element with argillaceous matter at the bottom of shallow portions of the sea began. As a result the amount of potassium in sea water became stationary. At the same time the removal of calcium on a larger scale than obtained in the preceding period commenced and this checked the increase of calcium salts.

The first forms of life in the primeval ocean were undoubtedly unicellular, and they were probably also organisms which presented features intermediate between those of the vegetable kingdom on the one hand, and those of the animal kingdom on the other. These forms must have persisted for a period of unknown but very great duration, for in them developed not only a nucleus but also the capacity on the part of the latter to divide in the remarkable and complicated manner illustrative of karyokinesis, and which is characteristic now of the cells of both kingdoms. This process of division, so alike in its main features in animal and vegetable cells, must have become fixed before specialization had gone so far as to evolve both animal and vegetable types, for, had it been otherwise, there would have been greater differences in the process in animal and vegetable forms. That the process has continued practically unchanged in all the intervening millions of years shows how deeply fixed in the organism this morphological habit has become, and, therefore, the act of fixation must have taken an incredibly long period of time during which the ocean was changing, not in the relative proportions to each of the elements it contained, but in the absolute amounts of these.

During this long period, these organisms, neither distinctly animal nor distinctly vegetable, exposed as they were to action of these same elements, must have acquired a relation to them as fixed as the karyokinetic process was becoming. Their protoplasm had established all its normal processes in the presence of potassium, sodium, calcium, and magnesium in certain proportions in sea water, and, after the lapse of the long period of time required for the elaboration of the karyokinetic method of division, these processes became unalterably dependent on the presence of the elements in the proportions which then prevailed. Without this fixed relation life could not continue, and when specialization into animal and vegetable forms occurred this fixed relation was transmitted to the forms of both kingdoms. How long these latter forms remained unicellular cannot, of course, be surmised, for there are no means of determining the length in time of this or any part of the pre-Cambrian age, but that it was of very great duration can hardly be questioned, and it must have strengthened the relation which obtained between protoplasm on the one hand and the elements in certain proportions, on the other.* In consequence, their descendant forms inherited this relation, and transmitted it to the forms and species which arose through variation and other causes. When multicellular forms arose these were endowed with the same relation.

The proportions of the elements in the early pre-Cambrian ocean with their long action on protoplasm must then have conferred a more or less fixed property on the latter and, in consequence, living matter, whether animal or vegetable, now shows in its ash proportions of the elements greatly different from those found in the media in which it lives or in the circulatory fluid which bathes it. This relation or property resists change even after exposure to altered conditions for a very long period of time. Before the circulatory fluid (blood plasma) was established in multicellular animals, a great change must have occurred in the proportions of the elements in the ocean, a change which would account for the wide differences between the proportions in the protoplasm or tissue on the one hand, and those in the blood on the other.

The proportions of the elements in living matter are due then to conditions which obtained in the ocean far back in the pre-Cambrian age, while those in the blood or plasma are due to conditions which occurred in the ocean long after this and yet before the beginning of the Cambrian period. The proportion of potassium to sodium in blood plasma is nearly† double what it is in the ocean and therefore that difference must have resulted in the period that has elapsed since the rudiments of a circulatory system were developed in those Metazoan animals which gave rise to Vertebrates.

As pointed out above, it is difficult to obtain the exact proportions of the sodium, potassium, calcium and magnesium in living matter, for, except in muscle fibre, protoplasmic structures cannot in sufficient quantities be freed from adherent material which carries these elements in very different proportions. Calcium exists in tissues apart from the protoplasm and as precipitates or deposits, and according to recent observations which I have made, this is true in a very large degree of

* Geologists concede a very long time to the pre-Cambrian, a duration which, according to the different estimates, ranges from one-third to four-fifths, and even nine-tenths, of the whole geological period. The very fact that all the chief types of animal life, and perhaps also of vegetable life as well, appeared before the close of the pre-Cambrian age, indicated that the latter was of inconceivably long duration.

† Amongst the oldest and highly specialized forms are *Olenellus* and the Brachiopods of the Cambrian. The oldest Vertebrate remains are in the Trenton division of the Silurian, more recent than the Cambrian, but these are "ganoid" in character and this fact postulates a long preceding period of development out of Protovertebrate forms which therefore could not have first appeared much later than the beginning of the Cambrian. The circulatory system of Vertebrates accordingly has a history which began in the pre-Cambrian age.

potassium. With regard to this element it may be said that active living matter has the power of absorbing it in large quantities and disposing of it in an inert form by precipitating it at the peripheries of cells or in inert organic masses within them, and, as a consequence, the ash of animal and vegetable cells shows a larger quantity of potassium than the protoplasm of the cells required. This illustrates how difficult it is to determine the primitive and fixed proportions of potassium and calcium, and further, how little we should depend, even in the case of muscle, on the analyses of the ash of organs or organisms, for this purpose. If a sufficient quantity of Amœbæ* could be obtained for analysis it might yield results of value but until that is done, the exact proportions of sodium, potassium, calcium and magnesium must be a matter for conjecture. It can scarcely be that the proportions found in muscle represent even approximately those which should obtain in undifferentiated propoplasm or cells.†

VI.—Evidence from the Lakes and Rivers of the Present Period.

It may be pointed out that in the composition of the rivers, large lakes, and seas of the world, there is evidence confirmatory of the view that potassium and calcium predominated in the pre-Cambrian seas. The conditions, of course, which contribute to the composition of the lakes of to-day are not the same as those which existed when the oceans of the globe were formed. There are but infinitesimal traces of the chlorides of calcium and potassium in the rock crust or sedimentary strata, and, further, there are, apart from the deposits of salt, and that amount of it due to rainfall, but small quantities of sodium chloride which can to-day come under the leaching action of water. There are also soils to alter the proportions of the chemical elements derived from them.

Nevertheless it happens that in lakes surrounded, either wholly or

* Fresh water, unicellular animal organisms are apparently free from excess of the elements. They are, as a rule, free from potassium, at least in such quantities as are found in other organisms.

† According to J. Katz (Pfluger's Arch., vol. 63, p. 1), the proportions in muscle from different animals are :

	Na.	*K.*	*Ca.*	*Mg.*
Man	100	400	9.3	26.4
Dog	100	354	7.26	25.1
Rabbit	100	870	40.0	60.5
Pike	100	1415	145.0	105.0

These and other results of the same observer are open to the objection that no effort was made to get muscle fibre free from all adherent tissues. Visible blood vessels, tendons, nerves and fat were indeed removed but these constitute only a part of the non-muscle portions of the tissue and they may be the cause of the variations shown in the results.

partially, by regions in which pre-Cambrian formations occur, or are the only apparent rocks, the potassium predominates over the sodium. For example, in the water from Reindeer Lake, which is situated 400 miles directly north of Lake Winnipeg, Professor Adams found the potassium to exceed very greatly the sodium. In the water from the Churchill River, as well as in the water from the Saskatchewan River above the junction of the Big Stone River, the potassium is much richer than the sodium.* These rivers drain rocky areas chiefly of the pre-Cambrian type. Rocks of the primitive kind, therefore, contrary to the prevailing opinion,† supply to the water which comes in contact with them more potassium than sodium.

Even in the case of Lake Superior which draws its supply not only from the primitive rock region on its northern side, but also from the areas covered with soils of alluvial and drift origin on the south, the potassium is about equivalent to the sodium. In the lakes of the Bavarian Highlands, Rachel See, Würm See and Ronig See, the potassium is twice in amount that of the sodium. In Lake Zurich the potassium exceeds the sodium. In Lake Geneva, in Pyrenean and Vosgean Lakes and in those of Russia, Armenia and Central Asia the potassium is approximately two-thirds of the sodium. It is probable that if proper methods for estimating potassium had been current in his day, C. Schmidt would have found for the lakes of Russia, Armenia and Central Asia a higher potassium value than he obtained, for the methods then in vogue for the determination of the element in the presence of sodium were very faulty and gave very low results. It is probable also that this may explain the low value found by Sterry Hunt for the potassium of the Ottawa River, whose waters, as well known, are derived largely from Archæan regions.

The Tables A and B show further that, in nearly all cases, the calcium is very abundant. In the Nile only, amongst the rivers, is it less than the sodium, while it very greatly exceeds it in the rest. In the lakes it is very abundant relatively, with the exception of the Rachel See and Lake Onega. In the Bavarian lakes, Lake Geneva, Lake Zurich, and some others, it is exceedingly abundant relatively.

The magnesium is always less than the calcium, and the relative difference is sometimes very great. It may fall below the sodium, but, as a rule, it is greater in amount.

These proportions, one can readily understand, must have been

* F. D. Adams Geo. and Nat. Hist. Survey of Canada, 1880-2, p. 6, 4.

† This opinion is based largely on the fact that the potash feldspars are difficult to decompose while the soda feldspars readily undergo decomposition.

uniformly maintained for an indefinitely long period. It may even be claimed that, in the case of Lake Baikal, of Reindeer Lake, and other lakes supplied from Archæan areas the proportions have obtained from pre-Cambrian times, and further, that the river discharge of that period, coming as it did from pre-Cambrian rock areas wholly, would contain the four elements in these or similar proportions. That would postulate that the primeval ocean was merely a gigantic body of fresh water, in which the sodium, potassium, calcium, and magnesium obtained in quantities and proportions as they now obtain in a lake situated in Archæan area. As already pointed out, these proportions gave place to others, and to-day, as in the past, the relative amounts of each element are changing, so that in a few million years hence the composition of ocean water will be appreciably different from what it is now.

One can, indeed, illustrate what changes have taken place in the ocean by reference to such a large body of fresh water as Lake Superior. If the latter were to lose its outlet no doubt its area would be larger than it is now, but when that had attained a certain extent the evaporation would balance the inflow as in the case of the Caspian Sea, and in consequence the salts held in solution would constantly increase in amount, but each at different rates up to a certain point, when the proportions would begin to approximate those in ocean water. One cannot of course say that this is what has happened in the case of either the Caspian or the Sea of Aral, for these bodies of water were connected with the ocean as late as the beginning of the Tertiary age, but it may be pointed out that if their composition was, to start with, the same as that of the ocean in Tertiary times, their present composition is strong evidence of the effect that the salts derived from leaching of the land areas have in modifying the proportions, for in that respect either is markedly different from the other and from the ocean.

The Great Salt Lake of Utah may be adduced as an instance of the change of a body of fresh water into one which presents a high degree of salinity and which in the proportions of its salts is remarkably not unlike the ocean. This lake, which is in part of the area covered by the glacial Lake Bonneville, is considered by G. K. Gilbert to have been a body of fresh water about 25,000 years ago. He arrived at this result by determining the discharge of chlorine into the lake by river water and comparing it with the quantity at present obtaining in the lake. Lake Bonneville had* an outlet delivering its waters into a tributary of the Columbia River and thus the lake was kept fresh. When, however, this outlet was lost, changes climatic and physical operated to reduce

* Monographs of the U. S. Geol. Survey, Vol. 1, Lake Bonneville, 1890, p. 254.

the volume of water, and, evaporation keeping pace with the inflow, a concentration of the salts held in solution took place. An examination of the present sources of inflow shows that these do not contain the sodium, potassium, calcium and magnesium in the relative proportions which are found in the lake. Gilbert estimates that it would take only eighteen years to give the lake through its fresh water inflow, all the calcium it now contains and that 850 years would to this end be required for magnesium. He does not deal with the case of the potassium of which the analyses he reports show only traces in the inflow water, but this also may have been due to faulty methods of determining that element. These latter seem to be the only explanation for the great discrepancy between the amounts of potassium found by Talmage* in 1889 and Bassett† in 1873‡.

Short as is the extreme period required by Gilbert's calculations to affect all the changes in the composition, it has epitomized the history of the ocean. Even if we postulate that the primitive rock crust of the globe in pre-Cambrian times contained more sodium chloride than what is found now in Archæan formations, there is also more of this salt in the strata of later geological periods which cover the drainage area of Utah Salt Lake. Of course there is not a complete parallel between the latter and the ocean, for the relative proportions are not exactly the same, but their approximate similarity is striking, and, it may be added, very convincing as to the extreme probability of the thesis maintained above.§

TABLE A.

RIVERS.

	Na.	*K.*	*Ca.*	*Mg.*	*SO3.*	*Cl.*	*Si.*	*Fe.*
1. St. Lawrence ...	100	22.9	638.0	143.4	136.0	223.0	343.0	....
2. Ottawa	100	64.2	416.7	82.5	67.3	224.3	402.0	....
3. Mississippi	100	35.5	462.0	82.0	17.1	8.4	86.4	17.0
4. Amazons........	100	72.6	1,089.0	135.6	36.0	90.0	....	....
5. Nile	100	22.2	75.1	41.5	18.5	16.0	44.4	....
6. Assinaboine.....	100	10.5	122.0	69.4	127.9	50.0	....	....
7. Red River	100	12.3	133.3	83.2	190.0	91.4	....	....
8. Nineteen Rivers (Murray)	100	38.6	590.9	134.2	197.6	53.5	145.3	37.9

*Science, Vol. 14, 1889, p. 445.

†Chemical News, Vol. 28, 1873, p. 236.

‡See Table B, Utah Salt Lake, 26 and 27.

§ In Lake Shirwa, according to J. E. S. Moore, ("The Tanganyika Problem," 1902, p. 22,) we have a lake which was once fresh, but has become salt through the loss of its outlet. So far as I know no analyses have been made of its waters.

TABLE B.

LAKES AND SEAS.

	Na.	*K.*	*Ca.*	*Mg.*	*SO_3.*	*Cl.*	*Si.*	*Fe.*
9. Superior	100	97.2	1,015.0	208.0	27.5	103.2	19.3	....
10. Rachel See	100	199.4	13.0	....	....	17.5	....	1.6
11. Würm See	100	223.9	2,445.0	809.0	....	....	....	....
12. Walchen See....	100	117.7	2,557.0	808.5	....	....	....	....
13. Ronig See	100	207.9	5,812.0	588.8	....	....	....	....
14. Schlier See......	100	96.4	3,141.0	661.0	....	....	....	....
15. Lac Gaube (Pyrenean).....	100	66.3	421.0	17.9	....	....	....	....
16. Lac Gerardmer (Vosges).......	100	70.4	117.6	29.8	....	....	....	....
17. Lake Geneva....	100	74.5	2,345.0	341.3	....	55.0	1,806.0	....
18. Lake Zurich.....	100	111.7	1,843.9	272.0	441.0	36.0	60.8	....
19. Lake Peipus.....	100	75.2	929.5	159.9	18.5	134.0	13.3	3.4
20. Lake Onega	100	71.4	67.3	51.4	33.4	104.7	36.9	2.2
21. Lake Tschaldyr (Armenian Highlands)	100	59.3	219.3	45.7	88.2	65.3	172.1	5.5
22. Lake Baikal.....	100	58.9	399.6	60.6	98.5	41.7	16.1	17.3
23. Sea of Aral	100	2.38	18.6	24.2	113.0	156.0	....	....
24. Caspian Sea.....	100	3.35	9.4	22.7	80.0	163.7	....	....
25. Dead Sea	100	28.4	40.3	1.27	2.0	556.1	....	....
26. Utah Salt Lake..	100	3.22	1.22	7.81	14.9	169.2	....	....
27. " " ..	100	25.8	1.56	7.82	19.1	192.2	....	....
28. Ocean	100	3.613	3.91	12.0	20.9	180.9	.. .	....

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SUMMARY.

The points discussed in the preceding pages may be summarized as follows :—

1. The composition of the ocean represents the result, on the one hand, of the leaching action of water on the land surfaces of the globe continued throughout all the geological periods, and, on the other, of the chemical and other agencies modifying or enhancing the power of sea water to retain in solution the mineral constituents derived from the land surfaces through river water since the beginning of the primeval period.

2. The relative proportions of the elements, and especially of sodium, potassium, calcium, and magnesium, in river discharge are not parallel to those of the same elements found in the sea. In river water, the calcium is always more, and the potassium less, abundant than the sodium, while the magnesium appears to approximate in amount the latter. In the sea, on the other hand, the sodium is much more abundant than the other three elements, and this is due to the continuous precipitation of a very great portion of the calcium added by rivers as carbonate, to the subsequent fixation in the limestone so formed of the magnesium as carbonate, and to the removal, continually taking place, of potassium, which is affected through animal and vegetable forms, and its consequent fixation in submarine deposits as glauconite and other potassium-holding minerals. The calcium and potassium appear to be stationary in amount, while the magnesium added by river water appears to exceed in amount that removed from the sea, and, in consequence, is slowly on the increase in the ocean, but its rate of increase is far behind that of the sodium.

3. The relative proportions of the elements in the ocean have, therefore, always been changing, and these proportions must have been, in the earlier geological periods, very different from what they are now. In the ocean of the earliest period the relative proportions of the elements approximated those found in river discharge, or rather those found in fresh water shed from areas covered with Archæan rocks. In this the potassium approaches the sodium in amount while the magnesium exceeds the latter, and the calcium is relatively very abundant.

4. This condition must have continued until living forms made

their appearance in the ocean when the gradual elimination of the magnesium, and particularly of the potassium and calcium, began. The forms were in all probability unicellular, and as the period must have been of great duration, the organisms and their protoplasm acquired a fixed relation to the four elements.

5. With the appearance of vegetable land forms and the formation of soils the removal of potassium from the land to the sea by river water diminished, and this, in conjunction with the elimination of the element from sea water by organisms, made the amount in the sea stationary. Through the action of living forms the calcium also in sea water has been kept stationary since that remote period.

6. In the transition from the ocean of the more ancient composition to that of the present, the unicellular forms became multicellular, and developed circulatory systems, the vascular fluids of which were at first simply modified sea water. In the blood plasma of Vertebrates, the three elements, sodium, potassium, and calcium are in relative proportions strikingly like those which now obtain in sea water. The magnesium only is considerably less than it is in sea water. The whole is due to heredity, the proportions of the saline constituents of the plasma being a reproduction of the proportions which obtained in sea water when circulatory plasmata were developed.

7. The proportions of the four elements which obtain in living protoplasm are as yet unknown, for the latter has the power of precipitating the potassium, calcium and probably the sodium and magnesium as inert compounds in itself or in its adventitious structures, and thus analyses would comprehend the inert material as well as the quantities of these elements which are actively participating in the processes of the living substance. If we could determine the latter quantities alone we could regard them as a representation of the proportions obtaining in primeval sea water to which the protoplasm of unicellular organisms had established a fixed relation.

8. That such a relation could be inherited may be inferred from the fact that the karyokinetic process, being practically the same in the animal and vegetable cell, has continued unchanged in both from the primeval period when the karyokinetic process first developed in a parent unicellular organism neither distinctly animal nor distinctly vegetable. This indicates how marked an influence heredity wields.

9. Briefly, animal as well as vegetable protoplasm owes its relations

to the elements sodium, potassium, calcium and magnesium to the composition of sea water which obtained when all forms were unicellular, just as the blood plasma owes its relations to the same four elements to the composition of sea water which prevailed when circulatory fluids were established. In other words the relation of protoplasm to salts is due to the action for ages of sea water, for incalculably long periods of time, on the living matter of unicellular organisms.

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