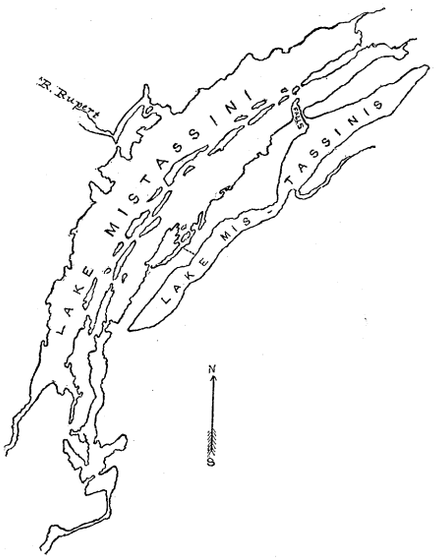
until it surpassed Lake Superior in size. In 1884 Mr. John Bignall of the Geological and Natural History Survey was ordered to complete an unfinished survey of the lake; and his work, essentially finished, appears in the report of Mr. A. P. Low, also of the Geological Survey. A carefully reduced copy of Mr. Bignall's map is herewith presented; some of the details, however, having been omitted for want of space. A casual inspection shows that not only is Lake Mistassini insignificant compared to Lake Superior, but also that it is not comparable even to Lake Ontario in size. In examining the maps of Mr. Bignall and Father Laure side by side, the differences are not so great as one might imagine. The salient features are alike in both, and the one is easily reducible to the other. The foreshortening in the latter probably arose from placing too much reliance on the appearance to the eye. Every topographer who has plotted a similarly shaped object, guided by the eye only, knows that it is extremely difficult to avoid such distortion.

The axis of the lake in Father Laure's map is certainly out of its proper angle; but, if we allow about 30° for variation of the compass, this objection disappears. It is hardly probable that at that early date Father Laure should have any means of estimating the variation of the compass, or that such a factor should enter into his calculations; so that, on the whole, there are but very few discrepancies between the two maps that cannot be reconciled.

Extrait de la Carte du Domaine du Roi en Canada, du Reverand Père Laure, Jésuiste, 1731.



Map of Lake Mistassini reproduced from the official surveys of Mr. A. P. Low, Geological Survey of the Dominion of Canada, 1886.

REDUCED COPIES OF THE MAPS OF MR. BIGNALL AND FATHER LAURE.

Furthermore, except the direction of the axis, there are no differences between the outlines as shown by the two maps that might not have resulted from the natural erosion of the basin and the corrasion of its outlet. "Rivers," as Gilbert aptly remarks, "are the mortal enemies of lakes;" and it is not reasonable to suppose that Rupert River is an exception to the rule. "Le grand percè" of Father Laure's map has been degraded to a narrow gash, and it is by no means improbable that the level of the water has been considerably lowered by drainage. Indeed, the fall between the adjacent lakes renders such an hypothesis highly probable, for a feature of such importance would not likely have passed Father Laure's notice. Lac Dauphin has disappeared, — possibly from having been drained, — and the long chain of islands traversing the centre of the lake bears further testimony to the lowering of the water in recent times. Unfortunately, Father Laure gives no estimate either of the depth or of the area of the lake, beyond the allusion "*d'environ* 300 *lieues de tour;*" so that a comparison of these elements at the two different dates is impossible.

It goes without saying that the lake bears every indication of glacial origin, and the severe winters of the present age cannot fail to leave their traces on the outlines of the lake, even from year to year.

JACQUES W. REDWAY.

Ptomaines and Leucomaines, and their Relation to Disease.

SEEING the article in *Science* of Oct. 18 induces me to send you this. It was published in a local medical journal (*Pacific Medical Journal*, September, 1889), but I should be glad to give it wider circulation.

Some recent notices in regard to the composition of leucomaines, and suggestions as to their probable relation to disease (*American Microscopical Journal*, vii. p. 216, 1888; *Science*, xii. p. 335, 1888; *Revue Scientifique*, xliii. p. 187, 1889), have induced me to embody some reflections of my own on this subject.

There is no longer any doubt that the announcement and general acceptance of the germ theory of disease constitute one of the greatest epochs in the history of medicine. But as in the case of all great truths, so in this, the first ideas on the subject have had to be greatly modified: the first extravagant hopes have been disappointed or deferred, and the first claims of its advocates found to be too sweeping.

At first it was imagined that all the grave symptoms of a germ disease, and the death of the patient, were due directly to the presence and multiplication of a specific microbe in the same sense as the destruction of fruit trees and field-crops is sometimes due to the ravages of insect-pests. The first great modification of this original idea was, that the disease and death in these cases are not due directly to the microbes, but to the accumulation in the blood (or on the mucous surfaces to be absorbed into the blood) of a poisonous chemical substance, a by-product of microbian multiplication. These by-products of albuminoid fermentation (for there are many kinds) have now been isolated from their microbian culture-fluids and analyzed. They may be regarded as alkaloids of albuminoid decompositions, and are called ptomaines. They are most of them deadly poisons. Septic poison, which is the by-product of putrefactive fermentation, i.e., of the multiplication of putrefactive bacillus, is the most familiar example.

The fact of a poisonous by-product of disease-germ multiplication ought to have been anticipated; for every form of fermentation has its peculiar chemical by-product, and many of these are poisonous. The different kinds of alcohol, ethylic, amylic, etc., and different kinds of organic acids, such as lactic, acetic, butyric, etc., are familiar examples. It would be strange indeed if the same were not true of albuminoid fermentations determined by the growth and multiplication of disease germs. As already said, some of these chemical by-products of disease germs have been separated from their generating microbes (as alcohol may be separated from the yeast-plant); and, by the inoculation of these pure chemical products, the corresponding diseases have been produced.

All the symptoms of typhoid-fever and of diphtheria have been thus produced without the presence of any pathogenic microbes. These pure chemical substances have also been successfully used as a vaccine against the corresponding disease, precisely as alcohol is used as a preventive of alcoholic fermentation.

This was indeed a great modification of the original form of the germ theory, but one which only confirmed its truth. We are now probably on the eve of another modification equally important and sweeping. I must explain.

We have seen that ptomaines are alkaloids of albuminoid decomposition generated in the presence and under the guidance of microbian life. Now, there is going on continually in the animal body, as a strictly physiological process, albuminoid decomposition (wasting of the tissues) in the presence and under the guidance of cell life. This also, as might be expected, produces poisonous products. These products also have been isolated and analyzed, and are found to belong to the same class of chemical bodies as the ptomaines. They are alkaloids of albuminoid decomposition, and are therefore in the highest degree poisonous. They are called leucomaines. If they are not also usually deadly to the animal body, it is only because they are continually being eliminated by appropriate organs.

But suppose there should be some change in the process of tissue-waste, and therefore of the composition of the leucomaines, rendering these more poisonous; or suppose, what is still more probable, there be some failure in the function of the organs by which these poisons are normally eliminated: evidently the result would be disease. And not only so, but (mark this) disease similar to those produced by disease germs, except that they would lack the property of contagiousness, because not due to the presence of microbes. Here, then, we would have diseases similar to so-called germ diseases produced without germs. Can we point out any such? Perhaps not yet with any certainty. It is hardly probable that any strongly marked specific and clearly contagious diseases, like small-pox, measles, scarlet-fever, whooping-cough, diphtheria, etc., are ever produced otherwise than by microbes. But it is possible that some of those obscure, sporadic, and apparently non-contagious forms of fever which often run so insensibly into each other, and so puzzle the physician to classify, such as some forms of typhoid, malarial, typho-malarial, continued fever, etc., may be produced in this way. Perhaps, also, countless unclassified, slight fevers and indispositions may come under the same head.

As thus modified, it seems to me that the last remaining objection to the germ theory is removed. But observe: this modification is an abatement of the arrogance of that theory, — is equivalent to an abandonment of its former claims as a universal theory of the cause of disease.

We have said that leucomaines are not usually deadly in their effects on the animal body, only because they are continually eliminated by appropriate organs. What organs? I answer, there may be more than one, but undoubtedly by far the most important is the liver. By careful experiments on animals, Schiff has shown that the liver has the remarkable property of eliminating, or else of decomposing and rendering innocuous to a greater or less degree, all kinds of organic alkaloid poisons, but especially alkaloids of albuminoid decomposition produced by wasting of tissues; i.e., leucomaines. If the vessels of the liver of a dog be ligated so that the venous blood containing these leucomaines cannot pass through that organ, the animal quickly falls into deep lethargy, and in a half-hour dies of blood-poisoning. That death is not the result of mere mutilation, is proved by the fact that a single drop of the blood of a dog dead of ligated liver injected into the veins of a frog will immediately kill the animal if his liver be ligated, but is innocuous if his liver be free (*Archives des Sciences*, lviii. p. 293, 1877).

But the question still remains, "How does the liver eliminate these poisons?" Not directly as such, for they do not appear in the bile. The answer to this weighty question is, I am persuaded, to be found in my interpretation of the glycogenic function of the liver. In my article on this subject, published in 1878 (*American Journal of Science*, xv. p. 99, 1878; also *Western Lancet* for the same year, but I do not remember the number), I maintain that the liver has the power of splitting albuminoids, whether of food or of waste tissue, into glycogen (which is immediately changed into liver sugar and burned) and a nitrogenous incombustible residue, which is eliminated by the kidneys as urea. Thus leucomaines are rendered innocuous, and at the same time utilized as fuel to maintain vital heat and force by the liver.

But if leucomaines, then also probably ptomaines, produced by microbes may also be disposed of by the liver in the same way, and the patient often saved. If this view be true, then the belief in the pre-eminent importance of the functions of the liver, and the practice based thereon, of clearing the bowels and stimulating the action of the liver in the onset or in the early stages of disease, — a practice reached empirically, and often ridiculed as savoring of routine, — receives ample justification.

JOS. LECONTE.

INDUSTRIAL NOTES.

Storage-Battery Litigation.

THE Electrical Accumulator Company of New York have issued a circular under date of Nov. 1, in which they state that the litigation involving a patent monopoly of the secondary-battery industry has been so prolonged, and is so technical, that it is believed a few words of explanation are appropriate, in order to enable the public to have a clear understanding of the situation.

In March, 1887, suit in equity was commenced in New York by the above-mentioned company, owning the Faure patent, against the Julien Electric Company, designed to stop further infringement of that patent, covering improvements in secondary batteries. During the progress of the suit it became evident that the Faure patent would be sustained, and early in 1888 the Julien Company modified their method of applying the active material to the battery-plates. In March, 1889, Judge Coxe rendered his decision sustaining the Faure patent, and holding that it could be construed to cover any secondary battery having the active material applied to a plate or support in the form of a "paint, paste, or cement." The modified method of the Julien Company accordingly came within the scope of the Faure patent. On April 11, 1889, an injunction was issued restraining the defendants from further acts of infringement. In June the Julien Company petitioned the court for a rehearing of the case; and their factory, which had shut down in April after the injunction was issued, again resumed operations, the method of manufacturing the batteries being again slightly modified; which second modification, it was claimed, did not infringe the Faure patent. Apparently becoming alarmed at the probability that this second modification was also an infringement, the Julien Company devised a third form, and subsequently a fourth form was employed.

In August a new suit in equity was brought against the New York and Harlem Railroad Company and the Julien Electric Traction Company as co-defendants. These parties were using large numbers of these so-called new forms of battery. Motion was made for a preliminary injunction, and in October Judge Lacombe rendered his decision, which, as will be seen after careful perusal, virtually gave the Electrical Accumulator Company all that they asked or claimed. An injunction was issued on Oct. 28, operating to stop the use of all of their four modifications as well as the original form. This decision of judge Lacombe has been printed for the information of interested parties. It is concise, accurate, and clearly defines what Brush is said to have done in anticipation of Faure's patent.

Quoting from the decision on this point, "What Brush did was to immerse a plate coated with dry material not only into fluid, but into the very fluid in which it was forthwith, and *without removal therefrom*, put to use as a battery plate." It is to be noted, that, under this decision, the manufacture of secondary batteries in any quantity will, if at all possible, be utterly impracticable without infringing Faure's patent.

It has yet to be demonstrated that such form of battery will work outside of the laboratory. It has never been done, although ten years have elapsed since Brush is said to have made the experiment; while manufacturers, both in this country and Europe, have been studying the problem with the strongest incentives to attain success.