vestigations are those of bacterial poisons, which he made in common with Ludwig Brieger. They led to the discovery of toxalbumin, and to that above mentioned. His other discoveries are those concerning the bacterial contents of ice, the cultivation of bacteria which thrive without air, the occurrence of micro-organisms in the various layers of the soil, etc.

Dr. Kitasato, a Japanese by birth, has lived in Germany for five years, and has occupied himself almost all the time with bacteriological studies in the Hygienic Institute. The biology of the cholera bacillus has been the theme of many of his researches. He has investigated its behavior in milk and in fæces, and its relations to other pathogenic and non-pathogenic bacteria in nutritive solutions. He has also gone deeply into the study of the tetanus germs, and has now published the results of his investigations in his article on immunity. One of his chief discoveries is that of the musk fungus.

Dr. Ernst Behring, who has shown, in conjunction with Dr. Kitasato, how immunity against diphtheria and tetanus is conferred on animals, is an army surgeon, and has been working as an assistant for about a year and a half past in the Hygienic Institute. Among his first studies after he became a surgeon, ten years ago, was the manner in which antiseptic remedies for wounds, especially iodoform, act, and he made a special study of the symptoms of iodoform poisoning. He afterward tested the antiseptic value of silver solutions, creoline, and other chemicals. Cadaverine, the etiology of anthrax, and the immunity of rats, are also among the themes to which he has devoted special attention, but diphtheria has recently been his exclusive study.

HEALTH MATTERS.

Action of Living Blood on Bacteria.

PROFESSOR BONOME has recorded the results of his researches on the following points: whether physiological alterations in the blood play any part in modifying its destructive action on bacteria; whether it is possible to produce alterations in the composition of the blood of such a nature that the normal inimical action against bacteria may be altered; and whether it is possible to derive any reliable data that will throw light on the subject of immunity. As a result of his experiments, he comes to the conclusion that staphylococci introduced directly into the blood are destroyed in from ten to twenty-five minutes, more rapidly in the blood of young rabbits than in older animals of the same species (*British Medical Journal*). He then, by injecting the poison obtained from the pus of an old empyema or a chronic abscess in small quantities into healthy rabbits, proved that the bacteria-destroying activity of the blood is increased, the organisms used being staphylococcus aureus, albus, and citreus. He holds, however, that the introduction of such poison does not appear to exert any influence upon the similar activity of the fixed tissues. Poison from acute pus obtained in a similar manner appears to exert not the slightest influence on the destructive action of the blood; while, owing to its effect upon the tissue-elements, it diminishes their power of destroying such organisms as the staphylococci above mentioned. Similar poison from pyogenic staphylococcus culture does not increase this destructive power of the blood against the above-mentioned organisms; and any immunity that is produced depends, not on the rapidity and certainty with which the blood destroys the organisms introduced into its stream, but rather upon a greater resistance which the tissue-elements exert against the bacteria poison, when they have become accustomed to the action of the poison by remaining in contact with the metabolic products of the same bacteria. He also gives experiments to show that water injected into the veins can diminish this destructive activity of the blood to a certain extent, but never completely; for although the animals so injected, and control animals, died about the same time, those in which water had been injected usually showed small purulent deposits in the kidneys and myocardium, and more or less fatty degeneration of the epithelium of the kidneys: so that he considers, that, in addition to this slight diminution in the destructive activity of the blood, there is some alteration of the protoplasm of the cells, probably due to the absence of salts and the cutting-off of the full oxygen supply by the presence of water, by which their resistance is considerably diminished in certain areas, and owing to which they are more readily attacked by the injected staphylococci.

Amount of Sugar in Blood in Disease.

Dr. N. P. Trinkler recently read, before the Kharkoff Medical Society, a paper on the "Diagnostic Significance of the Quantity of Sugar and Reducing Substances in the Blood," in which he detailed a number of observations he had carried out on patients in Professor Grube's surgical clinic, the majority of whom were suffering from cancer (*The Lancet*). The blood of some, as described in the *Medical Record* of Jan. 3, was taken for examination during an operation, that of the rest being only obtained after death. The examination was in all cases made by means of two processes, — that of Fehling and Soxhlet, and that of Knapp (Knapp's solution consists of cyanide of mercury dissolved in caustic alkali),— the mean of the two results being taken. He found that the blood during life always contains less sugar than after death, and that that of persons suffering from cancer contains a larger proportion of sugar and reducing substances than that of healthy persons, or of persons suffering from other diseases. Affections of internal organs appeared to be accompanied by a greater percentage of sugar in the blood than diseases of the skin or of external parts. The degree of emaciation produced by cancer did not seem to have any direct effect upon the quantity of sugar in the blood. There did not seem to be any real correspondence between the amounts of sugar and other reducing substances: the sugar was much more constant in its amount, the quantity of the other reducing substances being liable to very considerable variations. In the observations made on various diseased conditions, the following were the amounts of sugar found: cancer, 0.1678 per cent to 0.2037 per cent; typhoid-fever, 0.0950 per cent; pneumonia, 0.0943 per cent; dysentery, 0.0838 per cent; organic diseases of the heart, 0.0737 per cent; peritonitis, 0.701 per cent; phthisis, 0.0653 per cent; syphilis, 0.0553 per cent; nephritis, 0.0489 per cent; hæmaturia, 0.0375 per cent.

A Surgical Use for Ants

Ants have very powerful jaws, considering the size of their bodies, and therefore their method of fighting is by biting. They will bite one another, and hold on with a wonderful grip of the jaws, even after their legs have been bitten off by other ants. Sometimes six or eight ants will be clinging with a death-grip to one another, making a peculiar spectacle, some with a leg gone, and some with half the body gone. One singular fact is, as we learn from the *Medical Record*, that the grip of an ant's jaw is retained even after the body has been bitten off and nothing but the head remains. This knowledge is possessed by a certain tribe of Indians in Brazil, who put the ants to a very peculiar use. When an Indian gets a gash cut in his hand, instead of having his hand sewed together, as physicians do in this country, he procures five or six large black ants, and, holding their heads near the gash, they bring their jaws together in biting the flesh, and thus pull the two sides of the gash together. Then the Indian pinches off the bodies of the ants, and leaves their heads clinging to the gash, which is held together until the gash is perfectly healed.

The Cradle of Influenza.

Professor Tessier, of the medical faculty of Lyons, has returned from Russia, whither he was sent last March to take evidence upon the course of influenza there, and the various conditions of its evolution. He found, according to the *Medical Record*, that influenza is a growth of Russian soil, and, when not a raging malady, is a smouldering one. The way the people live in winter, locked up in heated houses; the flatness of the soil, its consequent bad drainage, and universally sodden condition when the April thaw begins; the filthiness of the farm-yards, the village streets, and the rivers, which become suddenly swollen, and on falling leave a putrid mud behind,— all conduce to make influenza endemic. Its microbe is, in fact, to be found in this mud. Dr. Tessier calls it a strepto bacillus. What is peculiar in this dis-

ease is the alliance with this bacillus of pneumococcus, which also lives in Russian marshes, river-mud, and village pools.

Hunger and Infection.

It is a well-known fact, says the *Medical Press*, that hunger predisposes to certain diseases, but it has been reserved to two Turin doctors to demonstrate the increased liability experimentally. Their observations were carried out with the virus of bacillus anthrax on pigeons,—a disease to which these birds are, under ordinary circumstances, refractory. They found, however, that six days' total deprivation of food rendered the birds amenable to the virus, on condition that food was still withheld. If, however, food was given at the same time as the virus, then they still successfully resisted infection. Further, when starvation was continued for two days after the inoculation, and food then given, the development of the disease, though not prevented, ran a slower course. Lastly, the virus proved capable of infecting birds well fed up to the date of inoculation, but starved subsequently. The line of investigation is evidently one which admits of further research, but the moral is obvious.

LETTERS TO THE EDITOR.

*** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

The editor will be glad to publish any queries consonant with the character of the journal.

On request, twenty copies of the number containing his communication will be furnished free to any correspondent.

Cyclones and Areas of High Pressure.

IN his communication to *Science* of Jan. 16, Professor Ferrel speaks of my storming a camp in which he was not to be found. This I cannot consider entirely wasted effort, since it has enabled me to more exactly formulate the position which he does occupy. I, however, do not like the simile, for I am sure I can speak for Professor Davis when I say that we are not enemies trying to knock down, undermine, or even disparage Professor Ferrel's work; neither are we partisans whose duty, as Mr. McAdie appears to think, is to look with special favor upon views promulgated by our own countrymen, and with corresponding disfavor upon views of foreigners. We are merely scientific men, trying, with the best knowledge we can command, to determine the truth about a matter which certainly admits of a difference of opinion. I did not set out with the ambitious task of stating a new theory which was to stand out as a rival to the life-work of Espy and Ferrel, but merely to quote certain facts which to me indicate that the present theory of cyclones as commonly understood needs modification. As a result of my reading and continuous observation of weather-maps, I frequently frame new hypotheses to enable me to more closely follow and anticipate the phenomena that are presented to me. Some of these I stated in my last communication, rather hoping that the criticism of Professor Ferrel's well-stored mind would enable me to gain more light on them.

Had not Ferrel so warmly espoused the condensation theory, I should not have thought this an essential part of his own. Is it not Espy's theory, rather than Ferrel's, that needs reconsideration? Ferrel's work has been in showing the effect of the earth's rotation on atmospheric currents, and, it seems to me, is unassailable. He has shown more convincingly than any other writer the possibility of the existence of dynamic gradients as distinguished from thermic gradients; and we find Teisserenc DeBort calculating by Ferrel's formula how much of each cyclone is to be attributed to thermic and how much to dynamic gradients, and even going so far as to show that cyclones may exist in which there is only a dynamic gradient, the thermic gradient having disappeared. In his last article in *Science*, Professor Ferrel, in speaking of low temperature as a cause of high-pressure areas, says, "While I regard this as adequate to account for it, I have never said or thought that it is the only cause, but simply the principal cause. I think there are other causes, especially in the origin of these high-pressure areas."

In speaking of the case referred to by me of a long trough of low pressure becoming nearly circular by the increase of pressure at both ends, he says, "I do not say that in such a case there would not be a certain very small amount or gyratory movement produced by the air flowing into the trough while it was filling up, as it would be at once if there were no restraining force to keep the air from the high pressure on each side from rushing in."

But Professor Ferrel will say these are only secondary effects, and there must be an originating and sustaining force behind them. This he finds in differences of temperature in adjacent bodies of air, even admitting that cyclones of moderate power may exist without precipitation.

I do not think any one who has entered into this discussion, unless it be Professor Hazen, has doubted that differences of temperature resulting from solar energy is the ultimate power from which all cyclonic and anticyclonic phenomena are derived. I stated as clearly as I could, in my last article, that differences of temperature between pole and equator, ocean and continent, were, in my opinion, the ultimate cause of differences of pressure over large areas, and indirectly the cause of the smaller cyclones and anticyclones of our weather-maps. I have just read my statements over, and do not see how I could have made them any clearer, though Professor Ferrel apparently failed to understand them, and quotes for my benefit the fable of a tortoise standing on nothing and supporting the world.

Loomis believed that areas of high pressure, which he placed as the antecedent phenomena in the development of cyclones, were mainly the result of low temperature. Hann finds in the temperature gradient between equator and pole the force which originates and maintains cyclones.

As I understand it, then, the point at issue is as follows: Ferrel maintains that the essential condition for the development and continuance of a cyclone is a higher temperature within the field of the cyclone than in the surrounding air. Loomis and Hann, while not denying that cyclones may thus originate, conclude, as a result of the study of observational data, that cyclones also exist as secondary whirls resulting from atmospheric motions originating outside the area of the cyclone. The cyclones thus originated probably bear some analogy to the small whirls often seen in the current of a river.

I have little doubt that Ferrel's explanation of the general circulation of the winds is the correct one, and it is possible that the views of cyclone generation advanced by Loomis and Hann will need modification; but I believe that the observational data are sufficient to warrant the conclusion that the condensation theory needs modification.

Professor Ferrel appears to think that it is scarcely justifiable to advance a new hypothesis until it is certain that the older theory is inadequate. I cannot think, however, that this is the method by which science has been advanced. There was a time when the wave theory of light was less probable than the emission theory elaborated by the mathematical genius of Newton; and, if the less probable theory had not been thought over and discussed, the present position of optics could never have been reached. There was a time when the fluid theory of electricity was much more probable than any other; and, had not investigators sought other hypotheses which would explain the phenomena equally as well, or better, progress would have been greatly retarded.

Many other examples might be given, but these will suffice to show why I prefer the method of multiple hypothesis advocated by President Chamberlin to the method of not considering but one hypothesis or theory until it is absolutely certain that it is wrong.

If we only had some method of determining the air temperature at each successive height, it would be possible to calculate in any area of high pressure exactly how much of the high pressure was due to temperature, and how much was due to dynamic or other causes. There are certain limiting values, however, which observation and well-known physical laws render it safe to assume the mean temperature of any air-column will not depart greatly from: 1st, It is improbable that the decrease of temperature with height can ever be much or any greater than the adiabatic rate when the air above would be potentially heavier than the air below; 2d, It is improbable that the mean temperature of the air-column up