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 to 5,000 metres will be higher than the temperature observed at the earth's surface.

Taking the average decrease of temperature with height found from the observations on Pike's Peak and Mount Washington, and using the temperature and pressure recorded at stations on the daily weather-chart, I have, by Köppen's method, calculated the pressure at the height of 5,000 metres above a large number of areas of high pressure, and drawn isobars for this height. These show that above the larger number of winter anticyclones on our Western plains the pressure is lower than on the same latitude farther east. Even if we make the extreme assumption that there is no decrease of temperature above these anticyclones up to 5,000 metres, some of the cases will still show a lower pressure at this height than on the same latitude on each side. In these cases there seems no escape from the conclusion that the pressure at the earth's surface is due chiefly or entirely to the low temperature of the air. But there are other cases of anticyclones over these plains in the summer-time, and of anticyclones on our seacoast in winter, in which the temperature is as high as, or higher than, near the earth's surface within the anticyclones as on the same latitude farther west. In these cases it is sometimes difficult to get a lower pressure in the upper air above them, even though we assume the adiabatic rate of cooling. Moreover, I know that these high pressures on rare occasions extend up even to the cirrus region, for I have observed cirrus-clouds moving out from them toward the west in their south-west quadrant as the surface wind does near the earth. I am hence led to believe that there are two classes of anticyclones,— one due chiefly or entirely to low temperature, and the other due chiefly or entirely to dynamic causes. It seems to me probable that the same is true of cyclones.

H. HELM CLAYTON.

Blue Hill Observatory, Jan. 22.

Questions of Nomenclature.

PROFESSOR C. S. SARGENT, author of the "Silva of North America," says, in the first volume of that work, "I have adopted the method which imposes upon a plant the oldest generic name applied to it by Linnæus in the first edition of the 'Genera Plantarum,' published in 1737, or by any subsequent author, and the oldest specific name used by Linnæus in the first edition of 'Species Plantarum,' published in 1753, or by any subsequent author, without regard to the fact that such a specific name may have been associated at first with a generic name improperly employed."

To secure stability in nomenclature, it is obvious that the method adopted by Professor Sargent is the one which should universally be adopted by botanists. Other questions relating to botanical nomenclature are not so well settled as might be desired, and a few of these may be briefly stated, with the writer's present views concerning them.

The first in importance, perhaps, is the use of the names of forms at first described as varieties of other species, and later raised to specific rank, or *vice versa*. It would seem that the varietal name as first used should be adopted for the specific name when raised to specific rank, though many botanists have felt at liberty to rechristen them at pleasure. A varietal or subspecific name would, if this rule were followed, receive precedence over later names. Professor E. L. Greene, in "West American Oaks," has adopted the name *Quercus Palmeri* Engelm. in preference to *Q. Dunnii* Kell., although first published as a species under the latter name, *Q. Palmeri* having first been published as a subspecies by Dr. Engelmann, and later as a species. One is led to infer by Professor Greene's remarks, that, had *Q. Palmeri* been published as a variety instead of as a subspecies, he would have adopted Kellogg's name for the species, though why such a distinction is made is not very evident.

Bentham, in fact, held that the earliest published name, whether applied as a specific or varietal, belonged inalienably to that individual form, whether subsequently redescribed and raised to specific, or degraded to varietal rank.

"Once a synonyme always a synonyme," is a rule which I believe obtains among zoölogists in general, and should, if tenable with them, be adopted by botanists as well. This would necessitate some important changes if adopted; and as an instance may be noted the genus *Washingtonia*, now in use for our Californian fan-palms, a synonyme of *Sequoia*, having been unfortunately applied to our Californian giant before its application by Wendland to our palm.

If the facts permitted, some enterprising botanist might see fit to reinstate the coniferous genus, in which case the genus of palms would of necessity have to be renamed. Still, it seems like creating needless synonymy in this case to rechristen Wendland's genus, though strict adherence to the rule would render it imperative.

Uniformity in the method of citing the authors of species is another desideratum in botanical nomenclature. The most explicit custom is that adopted in general by zoölogists, — the enclosing in parentheses the name of the author of the species or variety, where originally given wrong rank, or referred to a genus incorrectly. While this is often cumbersome, yet it greatly facilitates subsequent work beyond question, and is preferable to the citing of the name of the author who has referred the plant in question to a different genus, or considered it as of different rank. The existing confusion in the manner of citations renders it impossible for a writer to do strict justice to the founders of species, unless he is favored with access to large botanical libraries, and blessed with abundant leisure for consulting original descriptions. The author of the species (or variety), it seems to the writer, is the one to be cited (if the system of double citation is discarded as inconvenient) in preference to the authority for its transferrence from one genus to another.

Another point upon which botanists are not fully agreed is the citation of names adopted in manuscripts or herbaria, and receiving earliest publication by others than their authors. It is the custom in America (and a sensible custom it is) to cite the real author's name, even when first described and published by another author (unless published by that author as of his own authorship). Thus, Nuttall is credited with the authorship of many genera and species first described by Torrey & Gray in the "Synoptical Flora," or by DeCandolle or others elsewhere.

It is now generally conceded that an author, after publishing a name, has no longer any right to substitute another name therefor in subsequent publications, even though the first name he finds to be a misnomer. This right, claimed by many of the older botanists of a past generation, is no longer contended for. It is also an open question as to how far published names may be changed or corrected by their own or subsequent authors.

A common Californian cactus is published by Prince Salm in "Cacteæ Horto Dyckensi," p. 91, as *Mamillaria Goodrichii* Scheer, named in honor of Mr. Goodrich. Professor Sereno Watson informs me that Seemann says in the "Botany of the 'Herald'" that it was a "Mr. J. Goodridge, surgeon," whom the plant was intended to commemorate in its name as its discoverer. The name, therefore, has been written *M. Goodridgii* by many subsequent authors. Gray (*Botanical Gazette*, ix. 53) inadvertently publishes *Antirrhinum Nivenianum*, and repeats this spelling on the following page. This was collected by Rev. J. C. Nevin, and it is obviously proper to write *A. Nevinanum*, as the former spelling was mere inadvertence or a typographical error. But in the instance of *Mamillaria Goodrichii*, as originally written there is less cause for change, since the man may not have been clear in his own mind as to the correct spelling of his name,— like Shakspeare, spelling it differently at different times.

C. R. ORCUTT.

San Diego, Cal., Jan. 20.

BOOK-REVIEWS.

Inorganic Chemistry. By WILLIAM JAGO. London and New York, Longmans. 12°. $1.50.

THIS text-book is intended to meet certain conditions of science-teaching prevalent in Great Britain, due to the work going on under the auspices of the Science and Art Department. It is a more advanced book than the author's "Elementary Text-Book" on the same subject, issued some time ago. The supervision of