history of exploration during the last century and on certain other aspects of geography. There will be a reception on October 22 and a centenary dinner on October 23.

THE London *Times* reports that on the eve of the departure of Professor Nicholas Roerich, the Russian painter and archeologist, for Central Asia it was announced that there had been founded a Himalayan Research Institute, or Roerich Museum, with headquarters in the Kulu Valley, Western Himalaya. The institute, which will cooperate with the American Archeological Institute, is an outgrowth of five years' work in Central Asia of the expedition led by Professor Roerich. Among its honorary advisers are Dr. Ralph V. D. Magoffin, president of the Archeological Institute, Mr. Roy Chapman Andrews, Professors R. A. Millikan, Albert A. Michelson and Alexander Klemin, Professor Jacques Bacot, of Paris, Sir Jagadis Bose, Dr. Sven Hedin, Professor Albert Einstein and Professor A. Geoffré de la Pradelle.

DISCUSSION

A PLEA FOR SANITY IN NOMENCLATURE

THE botanists of six continents are again preparing for another spasmodic effort to settle for all time their troubles with nomenclature. Preliminary discussions have been held, recommendations of all sorts made, committees appointed, excitement raised until the safety-valve is about ready to function, and next August all will be settled.

But will it? If history repeats itself, the trouble will be just as great five years from now as at present. Several codes of nomenclature have already been devised, beginning, if I am not mistaken, with that of Paris, in 1867, and all of them have been intended to produce stability in nomenclature. Instead of stability, every code has caused the change of many names and has induced stability only for those species about which there was no question in the first place. Botanists were content for a century with the good old name *Sassafras officinale* for our well-known sassafras, but the American Code compelled changing it to *Sassafras Sassafras,* and the latest, or International Code, turns it again into *Sassafras variifolium.* The red oak was known universally for more than a century as *Quercus rubra;* now that name is applied to a different species of oak and we are asked to call the red oak *Quercus americana.* Is that stability? No, it is merely legalized confusion. When one reads now about *Quercus rubra* he has no idea except from the context whether the southern Spanish oak or the northern red oak is meant. Codes of nomenclature have simply failed to give stability, and there is no prospect that any future code will be more successful in its aim.

The leading feature of every code is and has been priority. The oldest name shall be used, and the code merely decides how priority shall be judged, interpreted and applied in any specific case. In the last analysis, however, an adherence to the oldest name is not necessarily what we want; stability is the one desideratum, whether the accepted name is old or relatively new. Priority has been adopted as a means toward stability, but it has not given us the desired result. Under a rule of priority not one well-known specific name is safe against the possible discovery of an older one, except the names in the first edition of the "Species Plantarum," and even there we may find that we have applied Linnaeus's names wrongly, as was the case with the red oak, and feel compelled to displace them.

A scientific name consists of two parts, neither of which alone is the name of the plant. Yet we are required by all contemporary codes to bring together isolated halves of names and to construct new combinations which are to be valid, even though long antedated by other perfectly good names. It is undoubtedly proper, in combining two genera for taxonomic (not nomenclatural) reasons to bring the specific name into the genus, on the principle that half a loaf is better than none, but the displacement of an established name for this reason is folly. "Hold fast that which is good," as the apostle said. Yet the botanists of the world enthusiastically discarded the old Kew rule of considering only specific names proposed within the genus, which would have done away with all nomenclatural transfers and double citations, just as they also refused to accept the so-called Berlin rule that names not used for fifty years should have no nomenclatural standing. There are two principles, either of which would have tended toward stability, which botanists rejected. It appears sometimes that botanists do not want stability!

One of the greatest sources of confusion in names is caused by segregation of genera. Separation of a genus into smaller genera implies, naturally, that the segregator knows just what a genus is. On the contrary, it proves that a genus is not a biological entity, but merely a group of species, and that the prevailing fashion tends toward smaller genera, just as the prevailing fashion in dress, at least until this year, tended toward smaller skirts. But think of the trouble caused to the users of botanical literature by the segregation of many well-known genera! It is hard enough for the taxonomist to keep even with the so-called progress, and it must be impossible for the forester, the horticulturist, the pathologist and others who use plant names instead of merely playing with

them. If rules are to be adopted, and in this law-making age it seems impossible to avoid them, we should have a list, not merely of names to be conserved, but also of generic concepts to be conserved, so that such well-known economic genera as *Pinus*, for example, should not be segregated.

The Vienna Code has one good point (or bad point, if you prefer) of giving a long list of *genera conservanda*, names which shall be used in spite of the existence of other prior names for the same genera. That is a step forward toward stability, and the list should be greatly extended and an additional list of *species conservandae* added to it, but it has the weakness of not stating for what the names should be conserved. I know one such genus of six hundred species which must be so conserved; unfortunately the name rightly belongs to another genus of about forty species, so that the entire six hundred should, under the rules, get a new generic name. Don't be alarmed, brother botanist, I shall never make these six hundred new combinations, and I believe that any one who does should be shot without waiting for sunrise. In fact, I have suggested a standard species for this genus (privately, so that no name-maker's attention will be called to the wonderful possibility) which, if the principle of standard species is adopted, will conserve the name in its present usage.

All systems of rules call for priority in the choice of specific names, and if I find a long-forgotten or never-used name in some obscure book, I am supposed to drag it out, dust it off and introduce it to the long-suffering botanical public. If we are going to have rules, why not have also a list of conserved publications to serve as the sole basis of available names? The only real hardship entailed by such a system would be that some botanists would be barred from their favorite indoor sport.

Another interesting point is the use of Latin in describing species, as required by the International Code. Most botanists wish to publish their knowledge and make it available to the world at large and accordingly choose a language which will be intelligible to many readers. So the Czechs and Poles almost always publish articles of extranational importance in another language and the Russians generally add a résumé in French or German. There is a suspicion extant, however, that the taxonomists of one country, which shall be nameless here, wish a monopoly of the knowledge of their own flora, and will begin publication of new species in their own language unless deterred by rules. Maybe we had better insist, after all, on the use of Latin for specific diagnoses.

Laws are generally of no avail unless the law-making body has the power to enforce them. A weakness of all codes of nomenclature has been the lack of this power. As an example, nearly ten thousand new names have been published without Latin diagnoses since the promulgation of the Vienna Code. One eminent botanist once told me that he could enforce on the botanical public any code of names he wished, if he could publish a better flora than any other then available.

All these points go to show the weakness of nomenclatural rules in securing stability of names. Why then have any rules at all? Instead of a complicated code, always subject to change, always subject to disregard in one or more provisions by some botanists, always subject to differences of interpretation, let each botanist adopt the following principles (not rules) for his own work and follow them conscientiously, and stability of nomenclature will be as nearly assured as it ever can be: first, I shall not reintroduce, or insist on the use of, forgotten or nearly forgotten names; and second, I shall not change the scope of any group of plants unless I firmly believe that I am actually adding to our knowledge of plants thereby.

H. A. GLEASON

NEW YORK BOTANICAL GARDEN

TEST OF THE WEGENER HYPOTHESIS BY MEANS OF GEODETIC DATA IN INDIA

IN Vol. III of the Geodetic Report of the Survey of India for 1927 (published in 1929), are data which are of interest in connection with the Wegener hypothesis. Under the heading "International Longitude Project" is given a somewhat detailed account of the work done at Dehra Dun in 1926 in connection with the world longitude campaign. The statement is made that "the mean of these gives the final value of the arc Dehra Dun—Greenwich to be $5^h\ 12^m\ 11.^s794$. . . . The above figure may be compared with the old value of $5^h\ 12^m\ 11.^s770$, derived from the Indo-European telegraphic arc of 1894–96."

Under the heading "Computations and Publication of Data" is a statement regarding the variation of latitude which reads as follows:

> As a result of an enquiry from Professor Wegener, the values of astronomical latitudes, found at stations in India at which observations had been taken at more than one time, separated by considerable periods, were scrutinized to see whether they afforded any evidence of earth movement. The results are given in Table 2. They have not been cleared of polar variation.

A table showing the variation of latitude at a number of old stations is then given, after which occurs the following statement:

> Five sets of observations, at Mussoorie, Sangatpur, Harnāsa and Kundgol, cover intervals of less than a year each, in spite of which they show changes not much smaller than those of the others. Of the remaining stations, four out of five show increases in latitude between