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.

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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

1800 and 1870, and three out of six show increases between 1870 and 1927. The changes at Gogīpatri and Poshkar, which are situated 15 miles apart, are directly contradictory. It can only be concluded that all the changes may be attributed to errors of observation or of star place, and that there is no evidence of continental drift. Nor, on the other hand, is there any disproof of the existence of a drift of the order of fifty feet per century.

The latitude and longitude data for India indicate very clearly that there is no rapid movement of that country in a north and south or an east and west direction. There is no possible way to tell whether or not there is a very slow drift. It will take another century or more, with repeated astronomical determinations of latitude and longitude, to get any clear idea as to the stability or instability of the Indian region. It is interesting, however, to have the valuable evidence contained in the report from India.

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ANTAGONISM BETWEEN ZOOPHARMACOLOGY AND PHYTOPHARMACOLOGY

DR. DAVID I. MACHT has presented an interesting review of cases of the dissimilarity between the zootoxic and the phytotoxic action of various alkaloids and toxins.[1] To quote from his article:

> It has been the experience . . . that poisons produced by plants, or phytogenic poisons, are more toxic for animals than for plants, while poisons elaborated or produced by animals, or zoogenic poisons, are commonly much more toxic for living plant protoplasm than for living animal tissues.

It would be possible to extend this idea of an antagonism between animals and plants to the subject of diseases and their treatment. If we consider the bacterial diseases of animals as diseases in which a plant (the bacterium) is infecting an animal host, we find that these diseases are, as a general rule, virulent. The infecting organism does not show any compatibility with the host, and produces various highly toxic substances (the bacterial toxins) which circulate in the blood stream and very rapidly bring the disease to a crisis from which the animal either dies or recovers. In the case of recovery, we find a very marked protective reaction on the part of the animal host, as indicated by the production of various immunological substances, the antitoxins, bacteriolysins, agglutinins, opsonins, etc. The net result is either that the plant (bacterium) kills the animal, or that the animal kills the plant. We have, therefore, in this case, no compatibility between the two forms, but instead a marked incompatibility.

[1] SCIENCE, 71: 302–306, March 21, 1930.

When we consider the infestations of an animal host with animal parasites, we have a markedly different picture. The host and parasite live together without any marked protective or offensive action on the part of either. When death occurs in these conditions, it is a result of the gradual destructive action of the parasite on some particular tissue of the host. The tapeworm, the liver fluke, the malarial plasmodium, the trypanosome, the filaria worm, the spirochete and the intestinal ameba may be taken as examples of this type of infesting organism. These organisms do not produce any great amount of toxins, and do not stimulate the host to form any great amount of protective substance. The host and parasite are seemingly quite compatible, and live together in what might be called a semi-symbiotic relationship, until gradually the infesting organism produces enough organic damage to the host to interfere with normal function. These diseases are, therefore, of a chronic type as compared with the virulence of the bacterial diseases.

This antagonism between animals and plants is reflected in the treatment of our diseases. If we wish to cure a bacterial disease we either let the patient prepare his own defensive substances (let the patient get well), or we make use of the same defensive substances prepared by another host (the antitoxins). Ordinarily, treatment and medication are valuable only in so far as they make for the physical welfare of the patient. If we wish to cure our infestations with an animal parasite, we must make use of various plant extracts (quinine and emetine are examples), or resort to the preparation of synthetic chemotherapeutic agents. The patient is practically entirely powerless to cure the disease completely, although he may reach an equilibrium with the parasite, in which the disease is to all intents latent, but from which condition the disease may later flare up and the patient suffer a relapse.

We may, therefore, entrust the treatment of bacterial infections to the patient, the physician or the bacteriologist and immunologist, knowing that their efforts will be assisted by the natural antagonism between the host and the infecting organism. In the parasitic infestations, the host, the physician and the parasitologist can do no more than describe the disease. The cure of the disease must be sought for in the growing field of chemotherapy.

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TEREDOLITHUS, A NEW COLLECTIVE GROUP NAME

QUITE a number of fossil ship worms have been described under the generic name Teredo. Most of