

fundamental case mentioned above, and that they introduce a slightly more general form of notation, which is necessary in the last analysis for the solution of certain problems. An adequate account of these chapters can be omitted here with more grace since the writer has shown his appreciation of them in a manner more befitting their real importance in a review of a more technical character in the *Bulletin of the American Mathematical Society* (November, 1905), which will be accessible to a reader whose interest is professional.

It would be misleading to leave an impression that Bolza's book is elementary or that it can be read without special mathematical training. It is highly technical even in comparison with most of the extant *English* works on mathematics. Its standards are not higher than they should be, however; they are unusual in a book printed in English; they are not higher than the average standard of the classical foreign mathematical treatises; they are beyond question a step in the right direction.

This book is not primarily a text-book; it will not share the speedy oblivion of that class; and there are many evidences that this and a few other extremely recent mathematical books published in America are merely the first of a considerable volume of mathematical productions of like grade, which, if the average standards remain equal to those set by this book, will indeed deserve to be called an American mathematical literature.

E. R. HEDRICK.

COLUMBIA, Mo.,
December 1, 1905.

SOCIETIES AND ACADEMIES.

THE WASHINGTON ACADEMY OF SCIENCES.

At a meeting of the academy on November 28, Professor Wilhelm Ostwald, of Leipzig, who is now lecturing at Harvard University, gave an interesting address upon the proposed 'Universal Auxiliary Language.' The substance of his address was as follows:

In our time, when international congresses of all kinds, scientific, commercial, political, etc., are gathering almost every day, the necessity of a general means of mutual understand-

ing is felt more keenly than at any former time. Small wonder therefore, that at one of these congresses, that of philosophy, held at Paris, 1900, the question was earnestly discussed, whether it was possible to overcome this insufferable obstacle to the common work of mankind. As a result, an international committee was elected to consider the matter and to propagate the general idea. This committee came to the conclusion that the selection or creation of a general auxiliary language was necessary, which should not supplant the existing languages, but should be learned by everybody besides his native speech for the purpose of international intercourse. This language must satisfy the following three conditions:

1. It must fulfill the needs of ordinary intercourse of social life, of commercial communications and of scientific and philosophic relations.

2. It must be easily acquired by every person of average elementary education, and especially by persons of European (and American) civilization.

3. It must not be one of the national languages.

Upon this program a widespread and effective propaganda has been developed. About 800 scientists of various countries, all university professors or members of scientific academies, and besides these about 200 societies of the most various kinds: scientific societies, chambers of commerce, touring clubs, etc., representing many thousand members, have joined the general movement. The purpose is, to ask after due time the Association des Academies, which represents the most important academies all over the world and is, therefore, the highest scientific corporation existing in our days, to take over this work of the international auxiliary language as its own, to appoint a working committee for the selection and introduction of such a language and to form a lasting and effective center for its culture and development. In case the association should decline this noble and important task, such a working committee will be selected by the above named international committee, formed by the representatives of

the different societies, etc., which have joined the movement.

Of the three conditions, the first two are self-explaining; only the third needs some discussion. The reason that any national language is considered as to be excluded is twofold. First, because the nationality whose language is chosen would obtain thereby an undue and unjust advantage over all other nations; it can not, therefore, be hoped that any such language will ever become truly international. The other reason is, that every natural or national language is sadly imperfect as compared with the possible ideal of a normal or regular language. The purpose of every language is to give a twofold coordination: one between ideas and sounds, and another between sounds and letters; by this means we can tell and write our ideas and can hear and read the ideas of our fellow men. To make such a coordination effective and consistent, it should be *univocal*; this means, that every idea should be coordinated only to one word, and *vice versa*, and that every sound should be coordinated only to one letter and *vice versa* again. Besides this, the coordination should be as simple as possible.

Of the natural languages none fulfils any of these conditions. Even if we abstract from the coordination between ideas and sounds (or written words) and consider only the coordination between letters and sounds, we never find it univocal. Italian and German are rather near to the ideal, because here the scripture is phonetic, but not yet fully so, while English is known as the worst possible language from this point of view. On the other side, Italian and German are very imperfect as to simplicity; they keep still the entirely useless differences of gender and express the same idea several times in the same sentence, for example, in coordinating to the plural in the subject superfluous plural forms in the adjective, the verb, the article and so on. English has made itself free from most of these superfluities, using only one article and almost no coordinations between the different members of a sentence. But there are still superfluous forms enough; the

most frequent English phrase: 'how do you do?' giving a striking example.

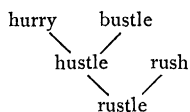
If there is no natural language, which fulfils the claim of simplicity and regularity, the question arises, if an artificial language may be constructed according to these conditions. Generally this question will be answered in the negative, but only on so-called theoretical reasons. To do away at once with probabilities, we will consider only facts. There exists an artificial language, called 'Esperanto' which is spoken and written by almost half a million people, which has about fifty periodicals and a stately collection of other books. A few months ago the Esperantists held their first international meeting at Boulogne, France, where 1,200 persons of twenty different nationalities were assembled, to try if it was possible to understand one another only on the basis of a language learned from books without oral teaching. This first experiment was in every way a grand success. There was not the slightest difficulty in understanding one another; even the English Esperantists pronounced the vowels quite clearly, in spite of their use or misuse in their own language. A play was performed, in which each player was from a different nationality and had learned his part at home, so that they met for the first time on the stage; the play went on without any difficulty for the players or the audience. Indeed, every possible application of a language was tried on this occasion, and all succeeded perfectly.

Space will not permit me to give a closer description of this language; every one, who desires to learn somewhat more about it, is kindly invited to apply to The Esperanto Club of Harvard University, Cambridge, Mass. By the same way information may be obtained as to the Delegation for the Adoption of an Auxiliary International Language, and its work.

Only one question can be answered at this place: will Esperanto be adopted by the delegation? It is impossible at present to say. Personally, I am inclined to believe that, indeed, Esperanto, either unchanged or after some slight modification, will be the auxiliary language of the future, but this is only a

personal judgment. Whatever language is chosen, it will be in any case the most important step mankind has taken since the invention of printing, for its future unification and for the saving of an enormous amount of hitherto wasted energy.

In the discussion which followed Professor Maurice Bloomfield, of the Johns Hopkins University, pointed out that the history of the second half of the nineteenth century in Europe was largely a history of re-nationalization. United Italy and Germany, Hungary and Bohemia are cases in point. Every minute and subjected people in southwestern Europe is engaged in furbishing up anew its national character. Each of them insists upon its own language as the chief sign of its individuality. Under these circumstances the hope that any one of the existing languages may become universal vanishes into thin air. The possibility of an artificial universal language comes to the fore, and the experiment deserves the most cordial support of all enlightened men. Nevertheless, Professor Bloomfield could not refrain from pointing out how much there is in language of organic growth, of mixed expression, which can not be produced artificially—chains of related and mixed words like 'hurry,' 'flurry,' 'scurry,' like



showing some of the mixed expressions which arise in speech through the subconscious creation of the individual and through the subconscious adoption of that creation by the masses. Simple idiomatic expressions like 'three years ago'—'Il y a trois ans,' 'vor drei Jahren'—illustrate how difficult it is to find common ground for even the most commonplace expression. Continuous issue of new words from a bureau of fabrication would exercise an incessant strain upon even the most willing disciples of an artificial language.

Professor Bloomfield was followed by Professor Mitchell Carroll, of the George Washington University, who spoke briefly in favor of Esperanto.

THE BIOLOGICAL SOCIETY OF WASHINGTON.

THE 405th regular meeting of the Biological Society was held on November 11, 1905, with Vice-President Hay in the chair and 36 persons present. Under brief notes and exhibition of specimens, Dr. B. W. Evermann said that while collecting yellow fever mosquitoes in Santo Domingo, Mr. August Busck had incidentally collected six specimens of fish, including four species, three of which proved to be new. Mr. W. H. Osgood exhibited an almost perfect molar tooth of a mastodon from Dawson, Yukon Territory, Canada. He stated that while mastodon remains had been found over a great part of the United States, the northernmost record up to the present time had been from near Lake Winnipeg, Manitoba. The tooth had been compared with specimens in the National Museum and found to agree with those of typical mastodons. Mr. W. L. Hahn showed a photograph of a red maple branch that had taken root in the ground and proceeded to grow. He also reported a specimen of the little brown bat, *Myotis lucifugus*, in the U. S. National Museum collection from Kamchatka, the previously most western record being from Kadiak Island, Alaska. The specimen was collected by Dr. W. H. Dall and proves to be typical *lucifugus*. Dr. C. W. Stiles reported that he had recently had sent to him for determination specimens of rat-tailed larvæ (*Aristilia*) alleged to have been passed by human patients. A case of pseudo-parasitism of this form is reported by Brera in 1809, and Dr. Stiles has recently examined specimens from two or three such cases in the United States. These specimens were also examined by Dr. L. O. Howard. Dr. Theodore Gill spoke of the wolf fishes of the genus *Anarrhichas*, of their distribution in the Atlantic and of their relationship. He finds that the species *donticalatis* and *latifrons* are generically distinct from the rest of the group. Mr. Walter H. Evans spoke of the introduction of minnows into the tero and rice ponds of Honolulu for the purpose of destroying the larvæ of the yellow fever mosquitoes. Dr. B. W. Evermann added that between 450 and 500 of these minnows, *Fundulus* and *Mollienesia*, transferred by Mr. Seale from Galveston,

Texas, to Honolulu were doing well and increasing rapidly. When liberated they immediately began devouring the mosquito larvæ. The *Mollienesia* are viviparous and soon after they were released many young were born. At the latest report the minnows numbered about 2,000.

The first regular paper of the evening was by Dr. E. L. Greene on 'Linnæus as an Evolutionist.' In the 'Philosophia Botanica' it was shown that Linnæus had clearly expressed his views that species were the same as when created in the beginning and as they were to remain for all time. In the later 'Species Plantarum' Dr. Greene was interested to find a query as to whether a certain species were not derived from another related species, and on reading through the 1,600 pages of this work he found numerous queries as to whether one species of plant had not been derived from some other species or acquired certain characters by adaptation to environment—soil or climatic conditions. Thus while in one place expressing an orthodox belief in special creation, his later querying is that of an advanced and pronounced evolutionist.

The second paper on the program was by Mr. Frederick V. Coville, on 'The International Botanical Congress at Vienna.' He said this was one of a series held every five years. It was attended by about 500 delegates, from the United States and most European countries, of which the German-Austrian group was greatly in the majority. The morning sessions of the meetings were devoted to presentation of botanical papers, the afternoon sessions to the adoption of a code of botanical nomenclature. The main questions brought up were: (1) The rejection of the Kew rule by adopting the principle of retaining the oldest specific name in whatever genus published; (2) an exception to the retention of the oldest specific name was made when it was identical with the name of the genus; (3) the American principle of rejecting homonyms was not adopted; (4) the principle of fixing upon a single species as the type of a genus was not accepted; (5) the principle of binomialism as a requisite for the publica-

tion of a genus was not adopted; (6) it was voted that after January 1, 1908, new names should not be valid unless accompanied by Latin diagnoses. Unfortunately, no preliminary meetings of the committee had been held for discussion and perfection of the code, and the proposals of the Americans to refer to committees points to which there was a large opposition was rejected.

Dr. Stiles discussed several points in connection with Mr. Coville's paper. He explained the organization of the international commission on zoological nomenclature and expressed the opinion that such organization was superior to the plan followed by the botanists, as the zoological plan not only provides for any necessary changes, but guards against any radical and unnecessary changes in nomenclatural customs. He thought that when zoological and botanical nomenclature were declared as independent of each other a very unfortunate mistake was committed, since such a rule was made without consideration of the numerous difficulties it would make for men working in the protozoa. However, now that the rule was generally adopted it was not feasible to attempt to rescind the rule. The zoological code provided, however, for certain cases which arise. He was of the opinion that the botanical provision that all new descriptions should be written in Latin was an unfortunate one and not capable of general practical application. He maintained that any man is able to write his own mother tongue more clearly and more concisely than he is able to express himself in a foreign language; further, probably not fifty per cent. of the men actively engaged in zoological and botanical work are able to read Latin with ease and certainly a smaller per cent. are able to write Latin. The rule in question as adopted by the botanists seemed to him not only a step backwards but further it played into a rather important local question in educational matters in certain countries and the rule would doubtless be cited by scientists in those countries in support of certain questions and legislation which were of purely local interest to those countries. It would further result in decreasing the activity of a number

of men in scientific work in case an attempt were made to enforce the rule. He believed, however, that the rule would be ignored by the majority of workers. Regarding the principle of tautonymy, he expressed himself not only as not opposed to it but as highly in favor of it, and he himself has decided to purposely introduce tautonomic names whenever the occasion presents itself. By use of tautonymy, the type species of a genus is shown in its name; without tautonymy, it is necessary to refer to the literature in order to recall the type species.

He did not believe it possible to enforce the rule concerning the list of excepted names. In fact, this rule did not seem to mean very much to him, as the congress had failed to determine the types for the names which were excepted and had apparently failed to provide for cases in which the genera in question might be defined in the future.

He recognized the delicacy of the situation which now faces the American botanists, but it seemed to him that the botanical code contained so many subjective elements that it was impossible for this code to expect to have a very long life. It has been the history of nomenclature since the time of Linnaeus that rules based on subjective ideas were short-lived. A rule in nomenclature must be objective if we expect it to be accepted generally and if we expect it to be permanent. Discussion followed by Dr. Gill.

The last paper on the program was by Dr. Hugh M. Smith on the 'Sargassum Fish,' a tropical species of the southern Atlantic occasionally brought by winds and currents to more northern shores. A large number of the fish were taken at Woods Holl and vicinity. The eggs from these prove to be entirely different from those described by Agassiz and long supposed to have belonged to the sargassum fish. Specimens of the fish were shown and a beautiful painting of it by C. R. Knight, showing its wonderful protective coloration. The paper was discussed by Dr. Gill.

VERNON BAILEY,
Recording Secretary, pro tem.

DISCUSSION AND CORRESPONDENCE.

ONTOGENETIC SPECIES AND OTHER SPECIES.

REFERRING to the admirable article in SCIENCE (November 24, 1905, p. 661) on 'The Evolution of Species through Climatic Conditions,' by Dr. J. A. Allen, I may once more gratefully recognize my own especial indebtedness to Dr. Allen's pioneer investigations of thirty years ago in this particular direction. These studies have been epoch-making in the history of ornithology.

It remains, however, to be determined whether these environmental forms—these species and subspecies produced by the direct influence of heat, cold, humidity and aridity—are 'ontogenetic species' (a term originating, so far as I know, with Professor V. L. Kellogg) or whether they have a real existence outside the lifetime of the individuals actually composing the group or species. We do not know which of the traits induced by direct action of the environment, if any, are actually hereditary and which are not. If we find that the dusky woodpeckers of Vancouver Island retain this shade when reared in Arizona, then humidity would be a real factor in the formation of species. If such birds, transferred in the egg to a new region should develop in the fashion of the local race of this region, and not like their own parents, then the duskiness is not a true specific or subspecific character. The real character of the species would be found in the tendency to develop dark plumage in humid surroundings and pale feathers under other conditions. In such case humidity would be merely a factor modifying individual development but not connected with the origin of species.

It may be that these questions have been already solved by experiment on birds, but if so, the experiments have escaped my attention. Eggs of the woodpeckers, chickadees and other birds showing dusky phases should be hatched in the arid plains. The red-shafted flicker of California should be bred in New England, and the permanence of the difference between large birds of northern range and their smaller southern homologues should be tested.