method." This is a technique of great beauty and generality which brings great refinement of mathematical analysis to bear on a wide class of unsolved problems in the theory of numbers. The method has been elaborated and improved by other mathematicians, but on its account alone the name of Hardy must for all time rank high among the masters of his subject.

No appreciation of the services of Hardy to the advance of mathematics would be complete which did not attempt to assess the value of his personal influence. Throughout his career he has been the driving force behind a vigorous group of younger research workers. A very considerable proportion of the pure mathematical research now being published in this country is traceable more or less directly to his interest and encouragement, or to the inspiration of his earlier work. His unstinted service during many years to the detailed work of the London Mathematical Society, and the freedom with which his experience and advice are available to all, have established him in a unique position in the regard of British mathematicians.

The Hughes Medal is awarded to Professor Arthur Holly Compton.

Professor Compton has made a number of important contributions to physical science in the field of x-rays and elsewhere. Of late years he has been one of the leaders in the study of cosmic rays.

The experiments of Young and Fresnel early in the nineteenth century proved that light certainly had undulatory properties. But in the present century facts have been emerging, notably in connection with photoelectric action, which are impossible to reconcile with the assumption that light can be described only as an electromagnetic wave of the classical type. These difficulties disappeared if light of frequency ν is assumed to be dynamically equivalent to a collection of particles of energy $h\nu$ (h = Planck's constant).

It occurred to Compton that from this standpoint the interaction between radiation and free electrons is very simple, and in fact is the simplest interaction which radiation can undergo. Associated with the energy $h\nu$, according to the electromagnetic theory, there is momentum $h\nu/c$ (c = velocity of light). The interaction is thus reduced to a very ancient problem, that of the encounter of two infinitesimal billiard balls with known energies and momenta. As the radiation moves with the velocity of light, in most cases the electron can be treated as if it were at rest. It is then obvious that in the collision the electron will acquire energy from the radiation and the conservation of momentum requires that if the electron moves off in a certain direction the radiation will travel in a certain other direction. But reduction of energy of a quantum of radiation means increase in wave-length, and this increase will be a predetermined function of the direction of the "scattered" radiation and of the direction of motion of the "recoil" electron.

He published these conclusions in 1922. In 1923 he established the change in wave-length, first qualitatively by Barkla's absorption coefficient methods and then quantitatively with the x-ray spectrometer. In the succeeding years he investigated the energies of the recoil electrons as a function of their direction of motion and showed that the correlation, predicted by the theory, between the direction and energy of the recoil electrons on the one hand and the direction and change of wave-length of the radiation on the other did in fact occur. This correlation is of fundamental importance in the general theory of the interaction of radiation with matter.

EACH AFTER HIS KIND[1]

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PRACTICAL CONSIDERATIONS

LONG after the binomial system was well established, collections of animals and plants were relatively small and attention was centered on the general type of species rather than on the intergradations between them. As larger series of specimens were studied intensively and critically the tendency naturally was to apply additional specific names to recognizable intermediate groups, until the series became so complete as to be practically continuous. At that stage the whole series might be thrown together again under a single species name, with the various component groups ranked under it as subspecies. Such shifts seem inevitable if nomenclature and classification are to keep up with increasing knowledge, but the consequent instability and constant change have been a perpetual source of annoyance to those biologists who are more concerned with the anatomy, physiology or embryology of a species than with what it is called. The multiplication of species seems to have been particularly irritating and led Cowles to exclaim:[14] "One of the noblest aims of

[1] Concluding part of the address of the retiring vice-president and chairman of the Section for the Zoological Sciences of the American Association for the Advancement of Science, Philadelphia, December 31, 1940.

[14] *Amer. Nat.*, 42: 266, 1908.

ecology is the destruction of many of the 'species' of our manuals." The general impression among this class of biologists has been that taxonomists consist of two groups, the "splitters" and the "lumpers," and that the current manuals and monographs reflect the changing vicissitudes of the struggle that goes on between them. There has been protest particularly against the changing of long-used and widely known generic names. It is a trifle disconcerting, it must be admitted, for the casual student of birds to find that our common American robin, which started out as *Turdus migratorius,* and which he perhaps learned, if he is about my age, as *Merula migratoria,* and which later became *Planesticus migratorius,* is now back where it started at *Turdus.* The only things that have remained constant about it are the specific name *migratorius* and the common name, robin—but it should be noted that it was one given another specific name, *canadensis,* and that so far as the common name is concerned, the bird never was properly a robin in the first place! The laboratory worker and teacher is equally disturbed when he meets such an old friend as *Amphioxus* parading as *Branchiostoma,* though he may be inclined to agree to the appropriateness when *Amoeba* becomes *Chaos!*

In 1897 Merriam published a paper entitled "Suggestions for a New Method of Discriminating between Species and Subspecies,"[15] which embodied the idea already discussed of including in a species those groups which form a continuous series. The following year Davenport and Blankinship[16] made what was presumably the first attempt to put these distinctions on a purely objective basis. They suggested that since "confluent species are usually separated chiefly by one most distinctive character," this "chief differenial" should be carefully measured (or counted if the character differed in discrete units) in the several populations, and the results plotted as a frequency distribution. If the resulting curve was evenly unimodal the whole array was interpreted as a single, uniformly varying species. Two or more modes were to be interpreted as indicating as many species or subspecies, the distinction depending on the distance between the modes and the depth of the valleys separating them. For this purpose they proposed an *Index of Isolation* —the depth of the depression between two maxima (modes), expressed in per cent. of the length of the shorter mode; and an *Index of Divergence*—"the distance between modes, expressed as the ratio of the distance between the modes of the half-range, or thrice the standard deviation of the broader curve." While these definitions and the method do not appear to have been adopted in detail, the general conception, blended with judgment, has been very useful in cases where good quantitative or numerical characters are available, as in the number of fin rays or lateral line scales in fishes. Ginsburg[17] has recently resurrected what is basically the proposal of Davenport and Blankinship, but has carried it to further degrees of refinement in order to differentiate groups below the order of subspecies. He proposes, however, to use only binomial names (which will be applauded by many) and to use a numeral subscript to indicate the relation of any sub-group to the species as a whole. Like Davenport and Blankinship, he bases his classification on a single "principal character." The general scheme will undoubtedly prove useful in some groups, but it is doubtful that the designation of the species and sub-groups by such arbitrary means will have widespread acceptance.

Edgar Anderson[18] has suggested an arithmetic method of evaluating species hybrids, which he and Ownbey[19] later extended for the determination of genetic coefficients to be used for: "(1) The efficient measurement of specific and subspecific divergence; (2) The genetic analysis of differences between species; (3) The determination of phylogenetic patterns." These authors are skeptical of the use of strictly quantitative characters and prefer to measure the similarity or difference with respect to the expression of a considerable number of qualitative characters in the forms being compared. The method loses considerable of its seeming exactness, however, since it is admitted that the characters chosen differ in importance, and judgment again enters in rating their relative value.

From what has preceded it will be seen that thus far attempts to stabilize taxonomy, and hence nomenclature, have by no means been altogether successful for reasons which have been pointed out. Numerous proposals have been made to cut the Gordian knot and separate the naming of species from the implications of relationship. Needham[20] long since proposed that only broad generic names should be used and that species in the genus should be known by numbers which would be applied in the order of their description. Certain supplementary signs would give additional information as to types and synonymy. This shorthand method not finding favor, Needham twenty years later,[21] apparently in despair, cried: "Let the existing system stand for the systematists. Let it grow and flourish. Let the splitters have their revel. The *mihi* itch is such a delightful disease, I would by no means deprive my worthy systematic colleagues of the

[15] SCIENCE, n.s. 5: 753–758, 1897.
[16] *Ibid.,* n.s. 7: 685–695, 1898.
[17] *Copeia,* 3, 1937; *Zoologica,* 23: 253–286, 1938; *Jour. Wash. Acad. Sci.,* 29: 317–330, 1939; *Zoologica,* 25: 15–31, 1940.
[18] *Ann. Missouri Bot. Garden,* 25: 511–525, 1936.
[19] *Ibid.,* 26: 325–348, 1939.
[20] SCIENCE, n.s. 32: 295–300, 1910.
[21] *Ibid.,* n.s. 71: 26–28, 1930.

pleasure they find in scratching. But let us have simpler names for common use."

Another attempt at a shorthand designation of species has recently been put forward by Rabel.[22] Maintaining that, as Needham says, "A name is a name and not a treatise on relationships," she would go the whole way and have the name represented by a number built on a scheme like an artificial key. She would: "Let then the students of phylogenies indulge as they please in their own private theories, let their conjectures fluctuate from year to year, from man to man, they would simply announce that according to their opinion the animals 7139275 . . . and 7132908 . . . are of common descent." I can imagine a modern Audubon's field notes reading something as follows: "Observed a 9657051 dive and catch a 476408; it was in turn pursued by a 9676275, which forced it to drop its prey; this the 9676275 then carried to the stub of a 2626519 and proceeded to devour"—innocently unmindful, I may add, of the indigestion such a meal might engender. Of course, the scheme has the advantage that it might be handled by the punch card system —and would be about as readable as a punch card held to the light.

The foregoing discussion has concerned largely the taxonomic problems in the higher animals and to some extent in plants. As Darlington says:[23] "We feel that we ought to have a 'species concept,'" but, he adds: "In fact, there can be no species concept based on the species of descriptive convenience that will not ensnare its own author so soon as he steps outside the group from which he made the concept." The truth of this is apparent when one considers the special problems in the lower invertebrates, and in fungi, particularly the imperfecti and bacteria. In these, morphological characters are commonly indefinite or lacking, and the situation is further complicated by complex life cycles and various methods of reproduction. As a consequence species are separated largely on the basis of physiological response, which usually can be determined only by extensive controlled cultural experiments, and the same is true of reproductive cycles. Hadley[24] has recently advanced the view that "the bacterial individuum should not be conceived of as a single cell but as a minute plant organism, the bacterial species-microphyte," and that species distinctions must be based on all stages of dissociative variation. Carrying the matter further, Darlington[25] has suggested that, "In the virus the definition of a species is clearly a matter of molecular structure,"—if so the virus taxonomist will either have to be also a physicist or have one at his elbow.

[22] *Discovery* (Cambr. Univ. Press), n.s. 3: 16–24, 1940.
[23] "The New Systematics," 1940, p. 159.
[24] *Jour. Infect. Dis.*, 65: 267–272, 1939.
[25] "The New Systematics," p. 158, 1940.

CONCLUSIONS

In conclusion, we may do well to consider the "new" systematics in relation to the "old." The older viewpoint looked upon systematics as essentially static, or at any rate the taxonomist tried to make it so. The wail of his non-taxonomic colleagues was that he stirred things up so much in the process, and that he did not succeed in his worthy objective of "stabilizing" nomenclature. According to the modern view the reason it can not be stabilized is that it deals with a changing, dynamic process of many currents. There are many kinds of isolating mechanisms and many degrees and kinds of group sterility; consequently there are different sorts of species, as well as of other groups. This means that in order to discover true relationship and phylogenies, as well as the *modus* of speciation, exhaustive investigation must be made of all phases of organisms, including, in addition to their more obvious characters, their range and ecological adaptations, their variability, breeding behavior, chromosome structure and anything else that will serve to characterize them in all stages of their life cycle.

But in our enthusiasm over this exciting new game let us not forget our faithful taxonomic colleagues who are still diligently catching all kinds of animals and sorting and penning them in nomenclatorial cages for us until we can get around to the more intensive study. In the meantime we should accept gratefully what they can give us. For how can they plot curves of variability on some new deep-sea fish, of which perhaps a half dozen specimens have been taken in a century, in order to determine its index of divergence? And how can they determine its relationship to other species by hybridization methods, since it is invariably dead when it reaches the surface? By what means can its chromosomes be observed and their behavior studied? Because these things can not be done, should these specimens be put away in pickle and go nameless, or will greater service be rendered by naming them and assigning them a place in the classification as accurately as judgment will allow? If some change has to be made later as a result of further information, that is scarcely to be avoided. But, as many have pointed out, this is a job for experts, and even they should bear in mind that terminology has a use besides that of expressing the latest individual ideas as to systematic arrangement.

As a help towards the stabilization of names, it has been suggested that strictly binomial terminology—that is, only the names of the genus and species—should be employed for all general purposes. Accordingly the genus should be kept of rather broad definition. Genus splitting seems often to be carried to the extreme of absurdity; for example, consider the case cited by Stone of three East Indian cuckoos which are classed in three separate genera on the basis of differences in the position of the nasal opening, in spite of the fact that "their coloration is so exactly similar that they are with difficulty distinguished in the hand without examining the bill."[26]

There would appear to be a similar advantage in putting as broad an interpretation as possible also on the species, in accordance with the general principles which have been stated. Then for purposes of reference in general use it would not matter so much about changes and rearrangements in other groups. As species became more thoroughly analyzed they would naturally be split into subspecies, which might in turn be divided still further. Let these be carefully described, and if naming will serve any useful purpose, let them receive quadrinomials and quinquenomials, even to the ecotypes and ecads, and to genotypes and karyotypes and cytotypes; yes, even to individuals when necessary. But in general as the divisions are smaller the process of change will be greater, and any grouping is likely to be transitory. We should be careful, therefore, that these micro-groups should not acquire the sanctity that seems to attach to the species. Accuracy and intelligibility should be sought rather than priority.

It has also been suggested that the International Commissions on Nomenclature in both zoology and botany could help more towards stabilization if they were accorded somewhat broader as well as more arbitrary powers to deal with cases that come before them. Certainly there is danger that the rules of nomenclature, like any other legal code, will become inflexible and outmoded unless provision is made for change. Classical taxonomy should on the whole exert a conservative force, but it must nevertheless take cognizance of the advances in knowledge in order to keep abreast of the times.

Appeasement is not popular at the moment, yet I would bespeak an attempt at greater mutual understanding and certainly of tolerance. Much of the heat that has been engendered between and among taxonomists and other biologists has been due to the failure of each to understand the problems of the other. I will not presume to make any prediction of my own, but you may be interested, whether you agree with it or not, in a recent prophecy by Julian Huxley.[27] After saying that micro-evolutionary studies (that is, those dealing with small groups) will become increasingly important in the near future, he concludes as follows:

> As such work proceeds, the New Systematics will gradually come into being. It will in some ways doubtless help classical taxonomy in its practical pigeon-holing functions; it will give a more detailed picture of the actual facts of the diversity of organic nature and its distribution in groups and in character-gradients over the globe; it will reveal many facts and principles of great importance to general biology; and through it taxonomy will become the field of major interest for all those concerned with the study of evolution at work.

As for myself, I am uncertain whether to interpret this last prediction to mean that in that time the taxonomic lion and the genetics lamb will lie down peacefully together, or whether the latter will have turned carnivorous and have devoured his ancient and honorable companion.

OBITUARY

DAYTON C. MILLER AND THE POPULARIZATION OF SCIENCE[1]

ANY record of the accomplishments of the late Dr. Dayton C. Miller, the distinguished American scientist and teacher who died on February 22, would be incomplete without mention of his services in the popularization of science. A skilled worker in the research laboratory and a wise teacher in the classroom, he had broad enough vision to see beyond laboratory and classroom. A deep and sympathetic understanding of humanity impelled him to lend his influence and energies to this end.

In company with some of the greatest figures in the history of science, he possessed the gift of making science clear to laymen and to young people. In this he was one with the great Faraday, with Huxley, with Sir Oliver Lodge and with Sir William Bragg. It is interesting to note that one of the last major activities of his life was a series of children's lectures delivered at Christmas time at the Franklin Institute in Philadelphia in 1937 and expanded into a book two years later under the title of "Sparks, Lightning, Cosmic Rays."

With the reader's permission, I would like to draw upon my own experiences to illustrate Dr. Miller's efforts for the popularization of science.

I saw Dr. Miller for the first time at one of his young people's lectures. The year was 1913 and I was then a junior at Central High School in Cleveland. Our scientific club, named the Faraday Club, had been invited to the Physics Laboratory of Case School of Applied Science to hear Dr. Miller lecture on sound waves.

Had Professor Miller been lecturing to his colleagues in the National Academy of Sciences, he could

[26] *Jour. Acad. Nat. Sci. Phila.*, 15: 314, 1912.

[1] An obituary appreciation of Dr. Miller, by Professor H. W. Mountcastle, of the Case School of Applied Science, was printed in the issue of SCIENCE for March 21.

[27] "The New Systematics," 1939, p. 42.