knew of no proposal to clear away the ornamental water or to interfere with the general appearance of the gardens. Yesterday Mr. Lansbury said that the beauty of the gardens would not be destroyed when the lease expired. The gardens would be added to Regent's Park and the public would be able to enjoy them. He added that no doubt provision would be made for carrying on horticultural research work.

DISCUSSION

ARE PLANETS RARE?

IN the August 15 number of SCIENCE Professor Jermain G. Porter challenges a statement of mine that "a planet is a very rare occurrence."

Permit me to quote as authority for this statement Sir James Jeans, who in his "Astronomy and Cosmogony" (1928) follows Chamberlain and Moulton in ascribing the birth of the solar system to the near approach of another star, which is necessarily a rare event. After developing the theory in detail, he concludes (p. 401):

> . . . only about one star in 100,000 is at present surrounded by planets. Planetary systems must then be of the nature of "freak-formations"; they do not appear in the normal evolutionary course of a normal star.

Also Professor A. S. Eddington, in his "The Nature of the Physical World" (1929), p. 177, says:

> The data are too vague to give any definite estimate of the odds against this occurrence, but I should judge that perhaps not one in a hundred millions of stars can have undergone this experience in the right stage and conditions to result in the formation of a system of planets.

To a humble physicist it would seem that Mr. Porter is hardly fair to his fellow astronomers when he says:

> That double stars have planetary systems may be doubtful, but there is absolutely no reason for the assumption that the formation of families of attendant worlds may not be the ordinary course of evolution for the single stars.

Rather than referring to a second-hand account of a press interview with me, in which obviously no arguments or authorities could be presented, would it not have been wiser for Professor Porter to present his case for frequent planets in the astronomical literature for the consideration of Messrs. Jeans and Eddington and others of like mind?

ARTHUR H. COMPTON
UNIVERSITY OF CHICAGO

CURIOSITIES OF SCIENTIFIC NAMES

UNDER the above title, Dr. Gifford in a recent number of SCIENCE adds certain instances of "errors in nomenclature," particularly in the coining of new names, and implies that care should be used in seeing that these are bestowed with due regard to classical usage. That this is an excellent principle no one will deny, yet a book full of "odd stories about scientific names" will some day make good reading. The birth of a new scientific name is, with Dr. Gifford, a "serious business," but with those who have much to do with this matter of names the solemnity of the occasion eventually loses somewhat of its glamour. It is, of course, well known that many names are merely anagrams that have no classical counterparts, for names, after all, are nothing more than handles by means of which particular objects are designated. So *Daption* for the Pintado petrel is merely an anagram of that word; *Teonoma* is another formed from *Neotoma,* to designate a genus of similar rats; *Delichon* from *Chelidon* is another instance. But the element of subtle humor comes in where a deliberate play upon words, often inobvious to the uninitiated, is made. It was perhaps a doubtful compliment when one zoologist named a new skunk in honor of a colleague, but when another named a bat *carissima* few might see that it was in honor of its discoverer, Mr. Darling. The term *Kogia,* for a genus of strange looking cetaceans, is said to have been coined by J. E. Gray because it was an odd "codger." In like manner the name *clavium* by Barbour and Allen for the Florida Key deer, to which Dr. Gifford refers, was a deliberate pun, for which the authors are entirely unrepentent, while the name *keyensis* that he suggests would be not only an amateurishly and awkwardly coined word, but would obviously refer equally to Key Island near Papua. There are many other names that hide a bit of humor and all of which, no doubt, are a manifestation of that same twist of human nature that prompted the builders of cathedrals in the middle ages to add to the sacred structure in out-of-the-way places the faces or figures of demons or evil spirits as a relief from the seriousness of their undertakings. The Lincoln Imp is a famous instance.

> So they whistled the Devil to make them sport,
> Who knew that sin is vain.

G. M. ALLEN
T. BARBOUR

PRIORITY IN FAMILY, ORDER AND HIGHER GROUP NAMES

THE International Rules of Zoological Nomenclature provide that a family name shall be formed by adding *idae* to the stem of the type genus, and that if the name of the type genus is changed, the family name shall also be changed. It does not specify how

it shall be changed, however, and there does not seem to be general agreement whether the next younger valid name based on another genus of the family and already in family form should be used, or whether the new name of the oldest or the type genus should be given a family suffix. It would seem desirable to introduce some uniformity of procedure. For example, if the generic name *A-us* 1850, the type genus of the family A-idae 1850, is found to be a synonym of *B-us* 1840, should A-idae be replaced by a newly coined family name, B-idae, in preference to an already proposed name, C-idae 1860, founded on *C-us* 1860, if *C-us* is clearly a member of the same family as *A-us*? And if B-idae should be used in this case, if *A-us* 1850 must be discarded as a newly recognized homonyn and is replaced by *B-us* 1930, should B-idae be the family name? It seems simpler and more consistent with the underlying principles of nomenclature to use C-idae in either case.

The one rule specifically applying to names above family rank is that they shall be uninomial. There seems to be general agreement that although it is desirable to use the older of two synonyms, other things being equal, it is not absolutely essential, if usage has established the later name. For example, Rodentia Smith 1827 is generally used in preference to Glires Linné 1758, and Carnivora Latreille 1825 instead of Ferae Linné 1758. There can be no serious ambiguity in the use of a better-known synonym of later date, but the situation is decidedly different if a homonym is used. The International Rules condemn homonyms for generic and specific names, explicitly and unreservedly. It would seem as if the grounds were equally cogent for the larger groups. To give specific examples, the name Cyclostomata Busk 1852 for a bryozoan order is an exact homonym of Cyclostomata Müller 1834 (= Cyclostoma Rafinesque 1815, also Cyclostoma Latreille 1829, preoccupied by Cyclostoma Lamarck 1801), the lampreys and their relatives. Decapoda Leach 1817, as a subdivision of the cephalopod mollusks, is preoccupied by Decapoda Latreille 1806 in the Crustacea. Tardigrada Illiger 1811 for the tree sloths has precedence over Tardigrada for the water-bears, a Latinization of "Tardigrades" Doyère 1840 (from "le tardigrade" of Spallanzani). In some cases the French form was in use earlier than the date given, but in no case could it reverse the technical priority, as not in Latinized form, nor does it reverse the essential priority, unless, by a stretch of the imagination in the case of "le tardigrade," which is used in the singular referring to an individual and not as a group name. In the case of "Cyclostomata" and "Decapoda," the earlier usage is quite certainly more wide-spread than the later homonym; this is probably not the case with "Tardigrada." In any case, the use of the identical name for entirely distinct groups, besides being slovenly, is a source of possible confusion, especially in bibliographic work. It would seem desirable to discontinue the use of the later term, replacing it with the earliest or best-known valid synonym (for example, Tubuliporina Milne Edwards for Cyclostomata Busk), or if none is available, by a new term.

HORACE ELMER WOOD, 2ND
WASHINGTON SQUARE COLLEGE,
NEW YORK UNIVERSITY

THE ORIGIN OF SYMPHORICARPUS

IN a paper entitled "Chromosomes and Phylogeny in Caprifoliaceae," by Karl Sax and D. A. Kribs, published in the *Journal* of the Arnold Arboretum,[1] the authors point out that the genus *Symphoricarpus* is represented in China by only one species, of very limited distribution, whereas the other species are, all of them, natives of North America. Since most of the genera of Caprifoliaceae are most abundant in Asia, and certain genera are found only in China, "it would seem probable," they say, "that the family is of Asiatic origin."

On this assumption they ask the question, "Does this mean that the genus is so old that the original Oriental forms have disappeared and only the newer American species remain?"

Is it necessary to assume that there was ever more than one species of the genus in China? Alternatively may there not have been in North America a species (allied to or even conspecific with the Chinese species and coeval with it) which died out, perhaps through climatic changes? This hypothetical species, now defunct, may first have produced offspring some of which were better adapted to the American climate. By isolation, or otherwise, such species might, conceivably, have given rise to the fifteen (or so) existing American species, which may not all be of equal age.

J. BURTT DAVY
IMPERIAL FORESTRY INSTITUTE,
UNIVERSITY OF OXFORD

ENTROPY AND ORGANIZATION

THE growth of physical concepts depends on the conditions under which they arise. As the context of ideas and experimental facts changes, these concepts also change. From this point of view, it is easy to see how the physical or mathematical probability of an event depends on the assumptions or conventions under which it is calculated. Further,

[1] *Journal* of the Arnold Arboretum, Harvard University, 11 (No. 3): 147–153, July, 1930.