enthusiasts. The only result is to discredit taxonomy.

But equally unfortunate is the attitude that would prohibit the recognition of natural hybridization except in hybrids actually bred genetically. The argument that a supposed hybrid, unbred, is merely a matter of personal opinion, and therefore should not be recognized, is invalid, for so are species, genera and families matters of personal opinion. In any taxonomic manual the groups there presented represent the author's interpretation of nature and nothing more. These groups are based likewise on circumstantial evidence, since he has not seen these species arise in nature, and represent only the conclusions reached from careful study of existing material. Why, then, should the taxonomist be adverse to the recognition of hybrids, who is perfectly willing to accept innumerable new species on much less reasonable ground? Taxonomists who do not recognize hybrids are often forced to treat such suspected forms as species. They are willing to assume them to be species until some one proves them to be something else, thus placing the burden of proof on the other fellow. I, personally, belong to that benighted group of taxonomists, who believe that new species should not be proposed as such until the author can not reach any other conclusion. Is it not our duty to science, to workers in other fields and to our fellow taxonomists not to clutter up our subject with endless names and half-baked concepts which seem only to confuse and to cause resentment and to pass the buck? The science of taxonomy stands too low now in the estimation of general workers.

How, then, should hybrids be named? The recognition of supposed hybrids as hybrids should not necessarily increase the number of names which we all deplore. It is my preference to designate ordinarily such a plant by the expression *Quercus bicolor* × *macrocarpa* and not by a new name. The only exceptions would be a few hybrids that in horticulture have acquired well-known specific names. Into this condensed designation "*Quercus bicolor* × *macrocarpa*" we would always read the expression, "A probable hybrid of *Quercus bicolor* and *Q. macrocarpa*, according to my interpretation" unless the hybrid had been actually demonstrated by breeding or synthesis. But so do we also read into the designation "*Q. macrocarpa* Michx." as a species, the statement, "A species, *Q. macrocarpa* in the sense of Michaux as interpreted by me." I can not help but see at least a practical difference between the causal more or less evanescent and temporary hybrid and the fundamental established species reaching back perhaps to the glacial epoch or beyond. The six fundamental species in the Amelanchier study did not seem in the same category with the hybrids, which, when eliminated, revealed them. I prefer to restrict the binomial to these older fundamental units for clearness and also on sentimental grounds.

In conclusion it may be asked again what relation then this experience with hybrids in the wild bears to the problem of the origin of species. As already mentioned, Lotsy was of the belief that hybridity is very likely the sole cause of the origin of new forms. Most biologists, I believe, are not ready to take a stand so extreme, but many students of genetics feel that the stable hybrids produced in their experiments represent at least one way by which new species may arise. While I would not really question this last statement, the point interesting to me is the almost[3] total lack of support for this view in our experience with the wild hybrids. As pointed out, the mass of hybrids in the cases under observation seem wholly casual and in no way to affect the fundamental species, which presumably have existed almost unchanged since the glacial period or before. It is still possible that these fundamental species came about by hybridity, and that others will also, if sufficient geological time is allowed. It would seem, however, that the factors noted by the geneticist ought to produce stable forms much sooner than that, even at the longest. Still another question, of course, is whether hybridity, which combines the genes of two parents, could produce new characters often enough and of sufficient magnitude to account for the great morphological diversity in plants. It is clear, I think, that we have not yet solved the problem of the origin of species.

The observations and view-points expressed in this paper are of course those of one person only, and are presented for what they may be worth. However, the angle from which they are presented is not quite the conventional one, and this may be an excuse for afflicting you with them.

OBITUARY

ROLAND BURRAGE DIXON

ROLAND BURRAGE DIXON, the senior member of the division of anthropology of Harvard University, died on December 19, 1934. He was the greatest ethnographer whom this country has produced. Dixon was born at Worcester, Massachusetts, on November 6, 1875. He took his A.B. degree at Harvard in 1897 and his Ph.D. in anthropology in 1900. From the year of his first degree until his death he was continuously

[3] A few cases: Huskins (*Genetics*, 12: 531, 1931) claims a hybrid origin for *Spartina Townsendii* and Müntzing (*Hereditas*, 14: 153, 1930) describes a hybrid in his cultures indistinguishable from *Galeopsis Tetrahit* L. both morphologically and in chromosome number—a synthetic *G. Tetrahit*. Should be further studied.

in the service of Harvard University, passing through the various academic grades until he was made full professor of anthropology in 1915. In 1904 he became librarian of the Peabody Museum, in 1908 secretary and in 1912 curator of ethnology. At the time of his death he held all these positions.

In the earlier years of his professional career, Dixon did extensive field work in anthropology. He carried on archeological excavations in Ohio, made ethnological researches among the Indians of British Columbia and Alaska, and spent no less than six seasons of work among the California Indians. His subsequent travel and investigation took him to New Zealand, Tasmania, Australia, Fiji and various parts of Asia. Nevertheless, Professor Dixon was primarily a student of anthropological literature rather than a field worker. He acquired a reading knowledge of numerous foreign languages, which, with his indefatigable industry, enabled him to master existing knowledge of the anthropology of four great continental areas: North and South America, Oceania and Asia. He classified and digested this prodigious mass of information, put it in card catalogues, and made it the basis of ethnographic lecture courses which were a model of organization and were exhaustive yet stimulating. From no other anthropologist in the world could students acquire a similar mastery of anthropological fact. Dixon carried an incredible store of this knowledge in his head and could produce instantaneously a detailed and sometimes complete bibliography of any subject within his chosen areas. He even succeeded in keeping up to date with the literature of his subject, and, so far as possible, read all of it.

In the Peabody Museum library Dixon established a catalogue system whereby books and articles in anthropological periodicals were classified not only by author but also by subject and by area. His unflagging energy in pushing forward this formidable task has made the anthropological library of the Peabody Museum the most easily utilizable and the best organized for research of any collection in the world.

Dixon's particular anthropological interest was in material culture and its diffusion. He wrote many articles on this subject—all notable because of his scholarship and his refusal to be lured from the path of scientific truth by romantic theories. His larger works, apart from technical monographs, include a book on the mythology of Oceania, a volume entitled "The Building of Cultures" and his widely discussed "Racial History of Mankind." The last-named was an adventurous foray into the field of physical anthropology, whereby the peoples of the world were classified according to the tripartite categories of three cranial indices as combined in individuals. This work was based upon a complete study of existing anthropometric material, and was a pioneer effort to establish the principle that racial classification should be based upon individual combinations rather than upon isolated group means. In spite of the vulnerability of Dixon's method in several of its processes, he succeeded in establishing a considerable number of important new points concerning human distribution and migration. Many of these have been confirmed subsequently by independent investigations of other scholars employing more elaborate methods than his widely condemned "short cut." Dixon was accustomed to refer to this book jocosely as "my crime," but, in the opinion of the present writer (who disagrees profoundly with many of Dixon's results and with most of his methods), "The Racial History of Mankind" is the most provocative and brilliant book of his anthropological generation. It will be perused when many safe and sane anthropological works have been forgotten.

Dixon was a solitary bachelor who lived contentedly in a beautiful country home, intentionally selected for its remoteness from Cambridge. Three times a week he emerged from his seclusion to empty upon his students his capacious vials of knowledge. Upon graduate students, engaged in research, he lavished his time and his inexhaustible supply of knowledge. As a director and critic of research Dixon was superb. In examinations he was formidably exacting, unsympathetic, but just. He commanded the fear, admiration and respect of his students, and the complete confidence of his colleagues. He labored incessantly and effectively to develop at Harvard a well-rounded anthropological curriculum based upon sound and conservative scholarship and thorough factual knowledge.

Professor Dixon was encrusted with an almost impenetrable reserve, topped with a high gloss of genial courtesy. Almost no one had access to the arcana of his personality. He was, underneath, a sensitive and kindly man, who led his life according to his own private rules and measured up to his own lofty ideals of conduct and performance. Throughout a protracted and wasting illness, he fought indomitably and stubbornly to continue in the discharge of his duties, never admitting to his colleagues (if indeed to himself) the inevitability of his defeat. He fully merited the Horatian encomium, "iustum et tenacem propositi virum."

E. A. HOOTON

RECENT DEATHS

DR. DAVID WHITE, senior geologist in the U. S. Geological Survey and recipient of the Wolcott award from the National Academy of Sciences, died on February 7 at the age of seventy-two years.