Of Maize and Men
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NEW BIOLOGICAL BOOKS

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OF MAIZE AND MEN

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A Review of

CORN: ITS ORIGIN, EVOLUTION, AND IMPROVEMENT.

By Paul C. Mangelsdorf. The Belknap Press of Harvard University Press, Cambridge. $20.00. xv + 262 p.; ill.; index. 1974.

Twelve of the twenty chapters of this eloquently written and attractively illustrated volume are autobiographically oriented, appropriately so, for Paul Mangelsdorf has had nearly half a century of personal and highly influential association with his subject. This book contributes significantly in many ways to our knowledge of the origin and evolution of maize (*Zea mays* L.), its races, distribution, genetics, cytology, archeology, uses, and improvement. As just one example, without Mangelsdorf's interest, influence, and participation in maize research, it is doubtful if more than a small part of the remarkable 7,000-year archeological record of maize evolution would by now have been discovered, or be so well documented and interpreted.

The origin and development of maize is of special interest in many ways. Of New World origin, it is unique among all major cultivated food plants, in that early taxonomists assigned it to a newly established genus, *Zea*, in which no wild counterpart was then recognized. At the time of Columbus, some two to three hundred races of maize had been developed by Western Hemisphere natives, some of which were adapted as far north as the mouth of the St. Lawrence River and south to what is now mid-Chile. Maize was their single most important food plant. The remarkable Inca, Maya, and Aztec cultural centers were all heavily dependent on maize as one of a few major foods that could readily be dried, transported, and stored for long periods. Today maize is the world's third most important grain crop.

Long before Columbus, natives of parts of Mexico and Guatemala were familiar with a close relative of maize, now widely referred to as teosinte. But it was not until near the mid-nineteenth century that its close genetic relation to maize was recognized by plant taxonomists. Teosinte's mature female inflorescence is so unlike that of an ear of maize that botanists found it most difficult to believe that maize could have been derived from it. But with the discovery that fertile maize-teosinte hybrids appear naturally where the two forms coexist, and that they can be made artificially without difficulty, interest was revived in teosinte as a possible direct ancestor of maize.

In the late twenties and early thirties this reviewer, then a graduate student and research assistant with the late R. A. Emerson, was privileged to participate in a cytogenetics study of maize-teosinte hybrids. We confirmed that meiosis in first-generation hybrids is essentially normal and that nine of the ten chromosome pairs for which we had appropriate genetic markers recombine in frequencies much like those of pure maize. It thus seemed eminently reasonable, even highly probable, that teosinte had been directly transformed into maize during the course of many centuries of human selection. There were and are alternative hypotheses, three of which Mangelsdorf

regards as major: (1) that cultivated maize originated from a wild pod corn now extinct; (2) that maize, teosinte, and species of the genus *Tripsacum* have descended along independent lines; or (3) that from hybrids of the postulated wild pod corn and tripsacum, teosinte arose—the tripartite hypothesis.

Of these, the tripartite hypothesis has been most voluminously and eloquently defended by Mangelsdorf and Reeves, first in their 1939, 315-page monograph, *The Origin of Indian Corn and Its Relatives,* and elsewhere during more than three subsequent decades. Now, as a rather sad anticlimax, we find in Mangelsdorf's newest treatise an admission that the hypothesis is no longer tenable, this in two footnotes, presumable added after the main text of the book was in the final stages of production. This reviewer, who has never been convinced of the tenability of this hypothesis, is not persuaded that the stated basis of withdrawal is sound. It consists in the argument that the pollen wall "spinules" of tripsacum are clustered but in corn are evenly distributed, plus the assumption that if teosinte arose from a hybrid of the two, its pollen spinule pattern should be intermediate. In actual fact, the teosinte spinule pattern is like that of maize. Since there appears to be no a priori basis for assuming that segregants of the postulated hybrid could not inherit a maize-like spinule pattern, it is not at all obvious that on this basis alone Mangelsdorf's conclusion is justified. A far more persuasive argument, to me, is Galinat's finding (*Annual Review of Genetics,* 5: 447–478, 1971) that, unlike maize, teosinte chromosome 4 carries a segment or genetic complex determining the development of its specialized fruit case, but that no such cluster of loci is found in tripsacum from which teosinte could have acquired this critical genetic assemblage. Mangelsdorf is well aware of this objection to the tripartite hypothesis, but strangely fails to refer to it. Neither does he refer to the criticism by de Wet and Harlan of the tripartite hypothesis (*Euphytica,* 21: 271–279, 1972).

In science as elsewhere, strong intellectual commitments are likely to engender emotional counterparts. Thus the reviewer cannot but wonder if Mangelsdorf's thirty-five-year defense of the tripartite hypothesis has not biased his evaluation of the palynological, archeological, and other evidence believed by him to indicate the origin of modern maize from a postulated extinct wild maize almost as different from teosinte as is modern maize. One line of evidence comes from the analysis of fossil pollen. On the basis of size range, fourteen analyzable pollen grains recovered from construction drill cores taken at a depth of 70 meters at the Belles Artes site in Mexico City have been judged by Barghoorn and associates to be those of maize, not teosinte. This would indicate the existence of maize some 80,000 years ago. Is it justified to conclude, as Mangelsdorf does (p. 181) that this ". . . shows beyond a reasonable doubt that the ancestor of cultivated corn was corn, not teosinte or any other of corn's relatives . . ."? Kurtz et al. (*Bulletin of the Torrey Botany Club,* 87: 85–94, 1960) believe not. They point out that ". . . using axis/pore ratios of Barghoorn et al. for *Tripsacum* and teosinte, individual corn pollen grains in the present study (a single maize line grown under a wide range of environmental conditions) could be classified as *Tripsacum,* teosinte or maize. Thus . . . if the plants from which the fossil pollen was derived grew under extreme climatic conditions, the degree of reliability of identification of the pollen would be very poor." Galinat (l.c.) shares this skepticism, and points out that although the Belles Artes pollen grains are found to be larger than those of teosinte, they are also larger than those of Chapalote maize, an ancient race still grown in parts of Mexico. If anything, one would expect both the more primitive maize types represented in the oldest archeological remains and those alleged to have grown 80,000 years ago to have even smaller pollen than that of any living maize. Thus, one cannot but wonder about the possibility of contamination by modern pollen in a drill core taken for construction purposes and thus with no reason to take special precautions against minor contamination.

A second line of evidence believed by Mangelsdorf to support the hypothesis of a wild maize other than teosinte comes from the remarkable archeological record of plant and animal remains preserved in various dry caves, especially those of the Tehuacán Valley of Mexico, in the analysis of which Mangelsdorf has had such an important part. But even with these findings plus a wealth of additional evidence from morphology, anatomy, taxonomy, genetics, cytology, biochemistry, physiology, anthropology, folklore, linguistics, and other disciplines the question remains whether the direct ancestor of maize was teosinte or a wild maize now presumed to be extinct. Mangelsdorf continues to advocate the latter and to assume the earliest archeological maize cobs of Tehuacán to be those of such a wild maize. An obvious alternative assumption is that these earliest specimens are transitional forms between teosinte and later stages in the development of modern maize. This reviewer favors the latter interpretation and would argue that since modern maize and teosinte are cytologically and genetically similar enough freely to produce fully fertile hybrids, their ancestors of preagricultural times could not have long coexisted in the same range or overlapping ones, for teosinte with its superior ecological attributes would surely have rapidly replaced a maize relative with the attributes of the earliest archeological specimens, especially with their apparent lack of any effective seed dispersal mechanism. If, on the other hand, the two forms had existed under conditions of reproductive isolation, it is diffi-

cult to believe they could have remained for so many millenia fully compatible genetically and cytologically.

If I have dealt disproportionately with the sections of Mangelsdorf's book concerned with the origin of maize, it is because these are its most debatable parts. Mangelsdorf's views, interpretations, and conclusions in this regard are at once controversial, challenging, and stimulating to all who are interested in the continuing mystery of how modern maize came to be. As one small example, I am personally grateful for having been stimulated in this way to resume full-time research on the teosinte-maize relation, after more than forty years of diversionary academic activities.

Anyone interested in man's little-heralded but enormously significant revolution, the origin and growth of agriculture, which in 10,000 or so years has made possible a thousandfold increase in human population and freed at least ninety per cent of the world's population for participation in aspects of cultural evolution other than food procurement, cannot fail to be deeply interested in all parts of Mangelsdorf's absorbing, and on the whole authoritative, saga on corn.

DOCTRINAL BIOGEOGRAPHY

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A Review of

MARINE ZOOGEOGRAPHY. *McGraw-Hill Series in Population Biology.*

By John C. Briggs. McGraw-Hill Book Company, New York and other cities. $25.00. xiv + 475 p.; ill.; index. 1974.

If tomorrow the many individual philosophies of animal and plant geographers were openly and conscientiously debated, there would soon exist many intensely polarized and irreconcilable schools of biogeography. But there now exists only an immense tranquility. Except for some notable apostates among phytogeographers, the biogeographers of today follow their own preferred and sometimes bizarre premises without casting more than a troubled glance at the conflicting theories and ideas of their colleagues. Even the dozen or so major works on biogeography of the last decade illustrate a bewildering divergence of methods and purposes. Explanations of past and present distributional patterns are variously referred to local ecological conditions, active dispersal of individual species, their passive dispersal via prevailing winds or moving water, or to natural catastrophe, world climatology, geological change on a local or global scale, or, frequently, some combination of these. Present distribution patterns often are interpreted as wholly or largely of recent origin, attributed to events not older than the Pleistocene or Pliocene.

"Primitive" species of a group are said to be either peripheral or central to the distributions of related "derived" forms; ecological correlations are considered decisive or irrelevant to explain distribution; different degrees of endemism are thought to reflect successive immigrations into a region or different rates of evolution for various members of a stable biota; present distributions are said to be mainly the result of dispersals from remote centers of origin or to reflect in situ events of subdivision. In short, biogeography today, sadly, has become an arena for indiscriminate invention rather than a stage for the disciplined interpretation of data.

Marine Zoogeography by John C. Briggs brings no relief: it is anecdotal, implies the existence of former distributional events that are now opaque to precise understanding and analysis, and invites the reader to be satisfied with a series of approximations and speculations.

The present work is said by the author to be "an up-to-date treatment of marine zoogeography along the lines of Ekman's original 1935 book" (*Zoogeography of the Sea*, Sidgwick and Jackson, London), and in some ways the two works are similar. Both stress the importance of identifying and comparing regions of endemism, and both stress the importance of viewing organic distributions with an historical perspective, but both fail to achieve satisfying conclusions about the methods and premises to be used in interpreting endemism historically. What is the significance of endemism to the history of a biota? We are left guessing. A preoccupation with the question of endemism is typified by the following remarks of Briggs (p. 32): "Since the shelf fauna of Easter Island is still so poorly known, it is almost impossible to estimate the overall extent of endemism. In the fishes, there are obviously many endemics, possibly 30 to 40 percent of the total fish fauna. When the entire marine fauna of Easter Island and Sola y Gomez becomes reasonably well known, it will be most interesting to compare it with that of Hawaii. Will the endemism be greater or less? How many of the