Letters

Corn in the Old World

In their article "Domestication of corn" (*1*), Mangelsdorf, MacNeish, and Galinat write that corn (maize) was unknown in the Old World before 1492. This reiterates a statement made by Mangelsdorf and Reeves (*2*) in 1959. There they relied on maize-indented pottery from Yorubaland, Nigeria. Carter of Johns Hopkins University has stated in a letter to me that the depths at which this pottery was found indicate antiquity, not recentness.

Mangelsdorf and Reeves also wrote:

> The confusion which can result from what Enfield has called "idle and unprofitable speculation" is nowhere better illustrated than in Jeffreys' acceptance of that part of the Stonor-Anderson thesis which holds that if maize did not originate in Asia it must have been taken there in prehistoric times (*2*).

I had written (*3*) that Stonor and Anderson

> conclude that ". . . these varieties [of maize] must have arrived there in pre-Columbian times" [*not* ". . . in prehistoric times"]. I am fully in agreement with this conclusion.

They continued:

> So far as Europe and Africa are concerned, the early post-Columbian occurrence of maize is explained by Wright, who showed how the Moors, after being partially expelled from Spain between 1499 and 1502, took maize with them on leaving Spain.

Wright (*4*) wrote:

> It is probable that [the Moors] became acquainted with the maize plant soon after its introduction into Spain and that they took it with them . . . to Tangier and the north African Coast.

Wright is here speculating, yet his *probable* becomes with Mangelsdorf and Reeves "Wright *showed*." From Wright's speculation these two explain how in Europe maize became known as "the grain of Turkey." No explanation is offered, however, of why, if the Moors met maize in Spain, it is in some parts of Spain, according to Muratori (*5*), called *trigo de turkina*; or why, according to Cenival and Monod (*6*), Valentim Fernandes wrote in 1506 that milho zaburro, an early Portuguese name for maize, was exported from Guynee to Sao Tomé till 1501.

Contrast Mangelsdorf's claim with that of the Portuguese. Santa Rosa de Viterbo, a Portuguese historian, wrote in 1798—as cited by Ribeiro (*7*)—that maize was brought from Guynee in Africa to Portugal in the reign of King John II (1481–1495). Ribeiro also quotes Manoel Severim de Faria as writing in the 17th century that *milho grosso de maçaroca* (maize) "comes from Guiné." Lains e Silva (*8*) wrote:

> *Zea mays* . . . has been cultivated in São Tomé for a very long time. Valentim Fernandes calls it *milho zaburro* of which he gave so perfect a description that it can be admitted that it is the *â mays* already known to the African peoples before the Armada of Columbus brought it from America. . . .

Sauer (*9*), checking the early Portuguese writings, "found the Jeffreys theses confirmed that maize in Africa was pre-Columbian. . . ." Mangelsdorf, however, relies on Portères's dating, which I have shown is wrong (*10*).

Recently Hui-Lin Li (*11*) has shown that Arabs were navigating the Atlantic about A.D. 1100 and had encountered maize. This evidence supports the data I had published in 1953 showing the presence of pre-Columbian Negroes in America about 1000 (*12*).

Mangelsdorf might well make a reappraisal of the evidence now available for the pre-Columbian presence of maize in the Old World.

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References

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10. M. D. W. Jeffreys, *Africa* **33**, 115 (1963).
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The reappraisal which I have made, at Jeffreys's suggestion, of the evidence on the pre-Columbian presence of corn in the Old World has included a number of important references not mentioned by him (*1*, *2*) and has reaffirmed my previous conclusion: there is no tangible evidence of any kind of the existence of corn in any part of the Old World before 1492.

The impressions of corn on the Nigerian pottery are suggestive, but until clearly shown to be pre-Columbian they are no more than that. The remaining evidence—historical, traditional, linguistic—is highly involved. It can be and has been interpreted in various ways to reach a variety of conclusions, some of which are diametrically opposed to Jeffreys's (*2*).

I recognize the *possibility* that corn may have reached Africa from America before 1492. The distance between Brazil and Africa is relatively short as ocean distances go—the obvious reason for using this route to deliver our planes to the European theater in World War II. Certainly the possibility that corn reached Africa across the Atlantic is greater than that it reached Asia across the Pacific, but even on this possibility we still have no reliable and convincing evidence, for which specious arguments are but a poor substitute.

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References

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Geodesy by Camel

W. R. Tobler, of the University of Michigan, has kindly pointed out a mistake in my article "Geodesy by satellite" (15 May, p. 803). In this article, I first stated that Eratosthenes, in his estimate of the circumference of the earth, used the length of a day's journey by camel as his primary standard of length, and that his estimate in terms of stades was derived secondarily. I also remarked that there has been consider-

able argument about the accuracy of his estimate, based upon various assumptions about the kind of stade he used, and that no one seems to have attacked the question directly by measuring the average speed of a camel.

Tobler has called my attention to the work of James Rennell [*Phil. Trans. Roy. Soc. London* **81**, pt. 2, p. 129 (1791)]. Rennell studied the speed of a camel by analyzing the records of a number of desert travellers, and actually proposed that the camel be used as the distance-measuring device in the initial surveying of Africa. Comparison of his results with the work of Eratosthenes reveals an interesting circumstance.

Briefly, Rennell found that the hourly distance travelled by a camel is almost independent of the loading and is remarkably constant. He found a rate of about 2.5 statute miles per hour, with deviations of about 2 percent. The daily distance does depend upon the loading; apparently the more heavily loaded camels simply stop sooner and refuse to continue. For the "heavy caravan," after allowing for stops and the sinuosity of the path followed, he found an average day's journey of 16.6 miles per day, and about 10 percent more for the "light caravan."

Eratosthenes gave 50 days' journey as the distance from Aswan to Alexandria. From an atlas, I judge this distance to be about 520 statute miles, giving 10.4 miles as the "camel-day" in Eratosthenes's time. Thus camels in 1791 travelled 60 to 75 percent faster than camels in 250 B.C., according to the assumptions made about the loading of Eratosthenes's camels. The most likely explanation of this large discrepancy is improvement in the breed.

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Genetic Code: Exaggerated Claims

In your editorial of 22 May you quote the statement by Glenn T. Seaborg of the AEC that "inevitably our recently acquired knowledge of the genetic code will be applied also to the improvement of the human species." As a graduate student of biochemistry and molecular biology I find the appearance of such a statement in the pages of *Science* profoundly unsettling.

Without a doubt recent advances in molecular biology have opened provocative new vistas both for the acquisition of knowledge and for its application; indeed I believe the coming decades will find this to be the most exciting field of science. Nevertheless, this very fact should make the scientific community aware of the need to guard against sensationalism. We are far from a full elucidation of the genetic code. Many of its most basic features are understood only poorly and some not at all (for example, the sequence of bases within a codon, intercistronic punctuation, and the various kinds of suppressor mutation). The 22 May issue of *Science* gives some idea of the situation in the dispute between Woese and Hinegardner and Engelberg ("Universality in the genetic code," p. 1030).

But leaving this aside, there is the much more fundamental question of the aims of scientific endeavor and the responsibility of scientists to make these aims clear to the public (which foots the bill) and especially to avoid suggesting melodramatic results which are not attainable with presently foreseeable means. Specifically, I know of no way whereby human heredity will "inevitably" be altered because of present studies on the genetic code. It would be fascinating to hear a concrete suggestion. Until the time when such suggestions become current and scientifically responsible, however, I think that a reputable and influential journal such as *Science* should refrain from spreading generalizations which *inevitably* will lead to disillusionment and mistrust of science, or an irrational fear of Frankensteins.

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We sympathize with the University of Skopje, Yugoslavia (Letters, 19 June, p. 1409), in the loss of their modern laboratories. However, they are far more fortunate than we are, for we have never had a modern laboratory. The Department of Pharmacology at Meharry is housed in converted army barracks; the renovations were carried out by the faculty and staff aided by minute grants from two pharmaceutical houses (the only responses to numerous letters sent out by members of the faculty). The building has no permanent heating system; the furniture