low latitudes if they contain an amount of water comparable to that on Earth. (Stress τ ranges from 0.5 to 1.0 bar for ice sheets on Earth.)

If the ice sheets of Venus were to extend only to 60° latitude, τ would have to be of the order of 19 bars when $V = 1.5 \times 10^9$ km^3. Since the creep rate of ice is proportional to the stress raised to about the third or fourth power, the ice velocity of such ice sheets would be at least 10^4 greater than of those on Earth; it would require a rate of accumulation of snow upon them that is also at least 10^4 greater than the rate of accumulation on either the Greenland or the Antarctic ice sheet. Another unreasonable result for this value of θ_0 is that the maximum ice thickness at $\theta = 90°$ is 47 km.

In addition to the problem of the size of the Cytherean ice sheets, they also present problems of stability. On Earth, ice sheets of this extreme size appear unable to exist in a steady-state condition (*4*).

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Weertman finds my suggestion (*1*) that water comparable to that on Earth is locked in polar ice caps on Venus unreasonable because the sheets would have to extend too far toward the hot equatorial belt; he suggests that sheets several kilometers in thickness are unstable. Here are my reasons for concluding that ice sheets averaging 10 km in thickness and extending to 45° latitude may be reasonable:

The stability of the sheet depends only on the net rate of precipitation (after correction for vaporization) being great enough to compensate the melting caused by the motion toward the equator and by the hot winds from the equatorial desert; a net precipitation rate of ice of 1 m/year corresponds to a flow rate of 0.3 km/year (if the ice sheet ends at 45° latitude). These seem to me to be reasonable figures, especially in view of the fact that ice formed in the presence of 18 atm of CO_2 may have quite different flow and melting properties.

Takenouchi and Kennedy (*2*) have studied the dissociation pressures of the phase $CO_2 \cdot 5.75\ H_2O$ (*3*). Apparently its mechanical properties have not been measured, but it forms in equilibrium with ice and CO_2-rich water at 10 atm and about −1°C; at 18 atm it forms at about 4°C. At the higher pressures to which the ice caps may be subject, its melting point rises to 19°C at 2000 atm which is the maximum pressure envisaged on Venus (20-km thickness at the poles). This characteristic of melting point rising with pressure is opposite, of course, to that of ordinary ice which at 2000 atm has a melting point of about −20°C (*4*).

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African Agricultural Patterns and the Sickle Cell

The recent paper by Wiesenfeld (*1*) presents most ingenious arguments on the effects of developing agricultural patterns on the evolution and incidence of the sickle-cell trait in Africa. The value of the approach which Wiesenfeld has adopted makes it the more unfortunate that much of his argument is based on Murdock's so-called "Malaysian agricultural complex" (*2*). The concept of the east-to-west spread of this "complex" of root and tree crops across the African continent about 2000 years ago was widely promulgated by Murdock (*2*), but his theory has gained little acceptance by Africanists who have actually worked in the field in Africa, as it ignores botanical, archeological, and agricultural facts, especially insofar as the origin of root crops and their cultivation is concerned. There is, of course, no doubt that Madagascar and the east African littoral have been greatly influenced by peoples and crop complexes of Malaysian origin, but there is little to support the thesis that these peoples and complexes spread to west Africa before the 16th century A.D.

The botanical evidence is clearest in the case of the yam. Although the Malaysian *Dioscorea alata* is a fairly common crop plant in west Africa, *D. cayenensis* and more especially *D. rotundata* (the "Guinea yams") are cultivated on a far larger scale, and these are unquestionably indigenous (*3–5*). In many west African languages the common names of *D. cayenensis* and *D. rotundata* signify "proper yam" (*6*), and it is with the harvesting of these species that the peculiarly west African tradition of the New Yam Festival is associated (*3*). The Asiatic species is grown mainly near the older coastal towns, where European influence is strongest (*7*); furthermore it is certainly no more common to the east of the Dahomey gap than to the west, as Murdock, to support his theory, suggests it is. There exists, also, an area of central Africa more than 1000 miles (1600 kilometers) wide where the Asiatic yam is virtually unknown even today, clearly separating the east and west African areas where this yam is cultivated (*8*). There is, in fact, no reason to suppose otherwise than that *D. alata* was brought by sea to west Africa by Iberian traders of the 16th century (*9*). The introduction of the "Guinea yams" into cultivation is essentially a west African development (*5, 10*), specifically a development peculiar to the Kwa-speaking peoples of the part of west Africa that lies between the central Ivory Coast and the Cameroons (*7, 11*). Reports by the earliest European mariners who visited west Africa, about A.D. 1500, show that cultivation of and trade in yams were already highly organized at that time (*12*). The wealth of myth and legend associated with the cultivation of the "Guinea yams" also points to their great antiquity as crop plants (*3, 11*). Outside the Ivory Coast–Cameroons belt the cultivation of yams is of far less importance; beyond this belt they are grown mainly for use as an occasional vegetable relish or, at best, a secondary staple.

Of the other Asiatic yams mentioned by Wiesenfeld, *Dioscorea esculenta* was introduced into Africa only late in the colonial period (*13*), and nowhere on the continent is it important as a food crop. *Dioscorea bulbifera* is indigenous to both Africa and Asia. African farmers can distinguish between the African and Asiatic forms and prefer the latter, believing it to have been introduced by Europeans (*4*).

Although the biological evidence is less clear for taro (*Colocasia*) than for the yams, a substantial body of evidence suggests that this originally Asiatic crop reached west Africa by way of the eastern Mediterranean, Egypt, and the Sudan, rather than from the east African littoral. The very name *cocoyam*, by which it is widely known in west Africa, can be traced linguistically to the classical Greek *Kolokasion*, from which the botanical taxon is also derived (*14*).

Archeological evidence of communities with equipment suitable for agriculture or vegeculture is now beginning to be found in the tropical forests of west and central Africa (*15*). Dates in the 2nd and 1st millennia B.C. have been obtained for these communities. There is at present in this evidence no trace of any connection with southeast Asia; the earliest suggested movements between southeast Asia and east Africa are dated in the 1st millennium A.D. Local development of agriculture within the west African forests, resulting from contacts with indigenous sorghum growers of the tropical grasslands further north, seems more likely and is already supported by some of the evidence.

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16. The opinions expressed are our own and not necessarily those of the organizations with which we are affiliated.

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Although I am no expert in African prehistory, I believe that the evidence presented by Coursey and Alexander speaks strongly against the early presence of Murdock's "Malaysian agricultural complex" in west Africa. The hypotheses developed in my article (*1*) were based primarily on the differences between root and tree crops and other crop complexes. Since the indigenous "Guinea yams" and the Asian crops involve similar slash-and-burn agricultural patterns that contrast markedly with other forms of agriculture, I believe the general concepts developed are still valid. Those concepts are as follows: slash-and-burn agriculture causes development of endemic malaria, which in turn puts increased selective pressure on "sickler" heterozygotes; increasing frequencies of the sickle-cell trait act in a negative feedback loop to cause a partial reduction in malaria.

Other specific conclusions in my article must be reevaluated. To obtain sickle-cell trait percentages as high as 30 percent in many west African populations requires a period of *at least* 1500 years (*1*, Figs. 5 and 6). The only agricultural system that could have allowed both malaria and the abnormal hemoglobins to prosper so early in west Africa must have been based on cultivation of indigenous crops.

North and west Africa, while always considered separately, appear to have much in common in regard to hemoglobinopathies (*2*). Hemoglobins S, C, and K, thalassemia, and glucose-6-phosphate dehydrogenase deficiency are present in both areas and across the Sahara in high frequencies (*2*). Archeological evidence and data pertaining to taro as a root crop and to abnormal hemoglobins suggest that the cultural, economic, and biological development of west Africa may owe more to the eastern Mediterranean than to east Africa or southeast Asia.

The most important conclusion is that we are dealing with biological responses to a behavioral change of multicentric, not unicentric, origins. The peculiar pathology of malaria has brought the contents of the red blood cell under intensive selective pressure in many parts of the world. Hemoglobin E was associated with hill slash-and-burn agriculture in southeast Asia around 1500 B.C. (*3*). The Asian crops came to influence east Africa and were associated with hemoglobin S. Agriculture and thalassemia were associated in the Mediterranean (*4*).

The comment of Coursey and Alexander points to the need for a more detailed analysis of the relationship between crop types and the patterns of agriculture, malaria, and abnormal hemoglobins.

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