stem of the Greek word for sulfur—from which the generic term *Thioploca* derives—is *theio-* (latinized *thio-*). The root, to be sure, is *thei-* (latinized *thi-*), while the word is *theion* (latinized *thium*); but a stem is at once more than a root and less than a word. Buchanan errs in asserting that *all* intranominal *o*'s in such compounds are "connecting vowels." Most of them are; but where the first noun is a Greek *o-* stem (as it is in *theio-/thio-*), the *-o-* is an integral part of that noun rather than a neutral compound-formative.

However, Soulides errs in declaring flatly that Greek *deiktikos*, "indicative," and Greek *dēktikos*, "mordant," must be "pronounced the same." In modern Greek, *ei* and *ē* (as well as *i* and *u*) are identically pronounced; but in classical Greek (from which technical terms are preferably derived) they were differently pronounced, in non-Attic as well as in Attic dialects.

What is most needed in such questions, it seems to me, is not pontification but consensus. Yet, until the various international biological congresses make some sort of collective pronouncement on the merits and demerits of latinization as a general nomenclatural principle, such consensus will continue to be no more than a pious hope, and acrimonious debate will go on, to the edification of few and the enlightment of none.

R. W. WESCOTT
African Language and Area Center, Michigan State University, East Lansing

Genetic Composition and Cultural Structure

I would like to take issue with the passage in G. G. Simpson's review of T. Dobzhansky's *Mankind Evolving* [*Science* **136**, 142 (13 Apr. 1962)] in which Simpson asserts, "Many ethnologists follow Leslie White to the opposite extreme [from the position of C. D. Darlington], maintaining that genetic differences and changes in mankind can be completely ignored, that all normal individuals of our species are biologically identical as far as present status and future possibilities are concerned."

This statement appears to me thoroughly to misrepresent White and certainly corresponds to no opinion held by any ethnologist known to me. It is certainly contrary to what any anthropologist, ethnologist or otherwise,

learns when, in his graduate training or in his subsequent reading in physical anthropology, he studies the biology of man. All of us who are primarily ethnologists know perfectly well that there is individual genetic variability of great scope in any given human population, the more so if the population has been composed of strains hailing from quite dispersed geographical areas, as in the case of the population of the United States.

Rather, the argument is that culture involves relations, especially normative ones, which disregard the biological variations, genetic or otherwise, of *individuals*. Culture involves symbols whose form and content are independent of individual genetics and can be transferred from one individual to another regardless of their individual genetic constitution. Further, it involves symbol *systems*, or higher orderings of symbols which are still more remote from the genetic foundations of biological individuals. In short, there appears to be no evidence whatever linking *individual* genetic composition to cultural structure.

The argument, however, goes further. It is asserted that the processes by which culture is transmitted and generated are independent of the relatively minor intraspecies variations of gene pool distributions which may be called races. The fundamental position was clearly set forth by Diamond and Steward in letters to *Science* [**135**, 961 (16 Mar. 1962)]. The history of culture traits, indeed of whole cultures, is such as to prove again and again the independence of cultural traditions from any definable population, distinguished on genetic grounds alone. There is no satisfactory evidence that the dynamics of the gene pools of *populations* are linked with cultural structure or dynamics in any significant way.

I do not know of an ethnologist, Leslie White included, who would assert that there were *no* connections between genetical factors in humans and their cultures, but I think there would be fair consensus in the following assertions.

1) Genetic peculiarities of populations may (or may not) be adaptive, but if they are adaptive it is in the sense that they adapt man as a biological organism to the environment in which he lives. Though his living in any environment is done through the intermediacy of culture, the genetic feature is irrelevant to the content or structure of the culture, both of which are pri-

marily derived by diffusion, acculturation, or other means from populations other than the one under examination.

2) Culture may affect the distribution of genes in a gene pool. If gentlemen indeed, and persistently, prefer blonds and forbear, as a consequence, to mate as frequently with brunettes, the population will tend toward blondism. If, as is asserted to be the case in the Brazilian population, gentlemen prefer *morenas*, or darker-skinned, black-haired beauties, and mate with *morenas* at the reproductive expense of still darker and blonder persons of the population, the more extreme colorations will tend to decrease. If, by cultural means, malaria is removed from the environment of a group which has the gene for sickle-cell anemia in its gene pool, which proves nonadaptive in the absence of malaria, or even negatively adaptive, then persons with that gene will be reproductively at a disadvantage, the proportion of the gene will decrease, and the gene pool composition will in consequence change. Insofar as culture contributes to mixing or isolating groups through such phenomena as exogamy and endogamy, slave trade, and racist barriers, all of which are cultural phenomena, it will affect the processes of genetic change which are connected with geographic isolation or openness. Cultural effects, however, are relatively minor.

3) The direction is almost entirely one-way—from culture to gene pool.

4) The independence of genetic constitution, individual or populational, from cultural organization is further made indubitable by the entirely discordant rates of change of biological and cultural evolution. Though in the 40,000 or so years since the first appearance of *Homo sapiens*, biological variation has been at most subspecific, cultural variation has cumulatively evolved through several qualitatively different "stages." The rate of the change appears to have been quite independent of the rate of change in the biological population. The nature of the change is apparently also quite different, particularly with respect to the continual emergence of ever-larger geographical units, overarching local adaptational systems. There is no parallel to this in biological evolution. I refer of course to ever-expanding politically bounded systems for whose territories, despite local ecological variation, there are uniform institutional arrangements. Again, the "cumulative" character of culture has often been

pointed out as being characteristically different from biological development—that is, the transformation of the gene content of gene pools. Although I can define "cumulation" for culture, I have some difficulty in defining "noncumulation" for biological systems, for gene pools. In general, however, one feels intuitively that the two processes are different.

Taken together, these assertions or propositions imply that, for virtually all propositions in the analysis of culture or culture history, genetic constitutions of individuals or of populations can be taken as constants, and this is the position that Leslie White *has* taken. Whether this position is justifiable for the entire sweep of the history of culture is quite another matter. If the origins of culture indeed lie back a million and a half or two million years, as recent African discoveries seem to suggest (if the dating is correct), then, since the culture sequences from that time forward appear to be associated (even if not directly in archeological sites) with several quite different types of hominids, it may well be necessary to look toward the genetic effects on culture for those periods, insofar as they may be researchable at all. There is an interesting suggestion, however, that the rate of cultural development in those far-off times was even slower than the rate of genetic development. This would again suggest independence of the two, and the connection of cultural development with factors other than the genetic.

In conclusion, I should like to say that these remarks are addressed not merely to the very faulty statement made by Simpson but to the wider confusions which appear to be becoming all too prevalent of late as to biology and culture and what the anthropologists' position on the relationship between the two is.

ANTHONY LEEDS
Department of Social Affairs, Pan American Union, Washington, D.C.

It is a bugbear of some ethnologists that someone might maintain the existence of a genetic component in the capacity for different cultures or even suggest this as a hypothesis worthy of consideration. That is evident in Leeds's long and strong reaction to a single sentence in my review of Dobzhansky's excellent book.

The only issue really pertinent to my review is whether my necessarily ex-

tremely brief mention of White's position was "very faulty." If fault there is, it is only in my failure to specify culture; I thought this sufficiently implicit when *ethnologists* were specified. Leeds affirms that "for virtually all propositions in the analysis of culture or culture history, genetic constitutions of individuals . . . can be taken as constants." The difference between that and the view I ascribed to White is subtle, to say the least. In fact White has also maintained that current biological evolution in man is insignificant. Even if unduly succinct, my statement is an inescapable conclusion from White's two propositions. (Nothing was ascribed to White about human evolution in the past.)

The greater part of Leeds's long communication is devoted not to my alleged sin of misstatement but to defense of White's position. That is quite irrelevant to the review that Leeds is ostensibly discussing. I was reviewing Dobzhansky, not White, and extended discussion of my own views on White or other ethnologists did not belong in the review. Even less does it belong in this letter. The points that Leeds here raises are discussed, judiciously and at length, in Dobzhansky's book, to which I urgently refer both Leeds and the readers of *Science*. If Leeds's polemic has made anyone curious as to opinions apparently imputed to me but *not* expressed in the review, I might add that I agree substantially with Dobzhansky.

GEORGE GAYLORD SIMPSON
Museum of Comparative Zoology,
Harvard College,
Cambridge, Massachusetts

Conversion of Pyruvate to Lactate in Tumors

In their article on the pathways of intracellular hydrogen transport (*1*), Boxer and Devlin mention the observations of Busch (*2*) that injected labeled pyruvate was primarily converted to lactate by tumor tissues, in contrast to findings in a number of normal tissues, and that, in experiments with tissue slices, the percentage of added pyruvate that was converted to lactate increased in the presence of added glucose in tumors but not in normal tissues. The explanation was offered that increased flow through the glycolytic pathway requires increased oxidation of reduced diphosphopyridine nucleotide. However, Jedekein and Weinhouse (*3*) and