sium-argon dating ends some hundred thousand years before the present, and the oldest radiocarbon dates are about 40,000 B.P. (some laboratories try to reach an earliest date of about 70,000 B.P.). That means that there is a lack of dating techniques (except the relative stratigraphical sequences of local relevance and without exact dating) for a great part of the Pleistocene. Furthermore, potassium-argon dating depends on local volcanic activities, and there are only a few parts of the world where we find volcanic rocks occurring in an archaeologically relevant context. In Africa there are potassium-argon dates only on Pliocene and Lower Pleistocene deposits from East and West Africa.

I intended to take samples for potassium-argon dating at Tibesti with the help of a specialist because it was possible that the older tuffs and lava could be dated. But everyone who is familiar with the political situation in Africa knows that a short time after I finished my work, the Tibbu started a rebellion which is not yet ended, and until now it has not been possible to carry out fieldwork in Tibesti.

My publication deals with problems of stratigraphy and Palaeolithic archaeology in the *central* Sahara. Butzer's comment on a "lack of information on the literature in the Libyan Desert and the Nile, Chad, Niger, and Senegal basins"—areas outside the central Sahara—seems to be curiously unintelligible. A detailed comparison of stratigraphical sequences and Palaeolithic remains cannot be made at present because of the small number of stratified sites and the great distance involved. Now only a preliminary comparison can be made with the results of French colleagues from the Western areas of Hoggar, Maghreb, Mauritania, and Senegal, which is what I did. I intend to compare these results with the Nile region in a later paper, where it would be possible to consider the valuable papers of Karl Butzer.

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Rebuttal to Ziegert's Reply

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(1) Regarding the size and nature of Ziegert's "assemblages" my review speaks for itself.

(2) Ziegert's oldest "assemblage" was indeed attributed to the "Heidelberg industry" (p. 58, 1.3-4), with the provocative corollary statement that "It would be desirable that collections of older phases of the North African Acheulian be reexamined to show possible connections with the *'Heidelberg Kulturkreis'* on a broader basis" (p. 58; translation mine). Equally unequivocal are the identical tabular entries on Figs. 10 (p. 30) and 32 (p. 145).

(3) Ziegert described and illustrated materials potentially suitable for both C-14 and K/Ar dating but made no use of them. It is therefore strange to read that

> These investigations are to be evaluated as part of a program attempting to derive a series of individual stratigraphies along a diagonal from Northwest Africa to East Africa, in order to obtain a reliable basis of comparison for parallelizing the Pleistocene and post-Pleistocene cultures of the Mediterranean region and those of East and South Africa, as well as (to obtain) insights into the climatic sequence [p. ix; for similar statements see p. 138; translation mine].

To this I counterpose three facts: (a) The 40,000-70,000-year span of C-14 dating does cover most subsaharan industries and techno-complexes other than the Oldowan (amply dated by K/Ar) and the Acheulian. (b) The key climato-stratigraphic sequences of subsaharan Africa (reviewed by Butzer, 1971:334 ff.) are only of relative stratigraphic value (by extrapolation) in so far as they have been dated isotopically. (c) Geological correlations *per se* have continued to be prone to gross error as exemplified by the earlier work in the Nile Valley (see Butzer and Hansen 1968) or by the recent demonstration that the Gamblian Pluvial at the type site, far from being a suitable stratotype for the East African Upper Pleistocene, is of Holocene age (Butzer, Isaac, Richardson, and Kamau 1972).

(4) Ziegert does indeed look "Towards a Pleistocene Stratigraphy in North Africa" (major head, p. 137) and he does present a comparative stratigraphic chart (Fig. 31, p. 143) for Morocco (based on A. Ruhlmann!), three south Algerian areas, and Mauritania. The nature and the relevance of the literature that I felt to be *apropos* can be sampled in my own review of the Saharan region (Butzer 1971:312 ff., 581 ff.).

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A Comment on Gray's Review of THE WAHI WANYATURU

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I wish to comment on Gray's review of my book, *The Wahi Wanyaturu* (*AA* 73:1341-1342) in order to point to a strange twist of the thesis of the book which it contains. I don't know how to explain this twist, which may be due to his misreading of the book or the inadequacy of my presentation, or to both. It may also be due to a problem of perception resulting from my use of a paradigm (to borrow a phrase from Kuhn) which is strange to anthropology, the paradigm of formal economics. But the fact is that the review does not accurately represent the book.

Gray notes that I begin the book by declaring that it will develop a competitive view of African society which is to be contrasted with the "communalistic" (a term borrowed from Williams 1960:478) view still current in much of anthropology and which usually goes under the name of functionalism or substantivism. But, Gray concludes, the book is more communalistic than most which one encounters these days.

My point is, either Gray has misread the book or I have misrepresented the contents of the book. Which is the case?

Gray's argument is that, as in the discussion of obligations between kin in Chapter 6, I specify in great detail the system of obligatory exchange of livestock showing thereby, presumably, that rather than being economic, relations between kin are highly altruistic or obligatory. Yet Gray does not mention that in the same chapter (p. 102) I assert that "true altruism is as hard to assert in Turu society as in any other." Furthermore, he fails to mention that in Chapter 3, the heart of the book, which deals with "The Market System," I spell out how Turu individuals operate their farms for profit, how cattle are used as money both for exchange and finance, that land is a commodity (even in transactions between members of the localized lineage), and that marriage is formally equivalent to the labor market. One whole chapter is devoted to "People as Wealth" in which the idea of exchange of rights in people is fully developed. Throughout the book, in fact, the thesis is continually stressed that the individual Turu man is an entrepreneur seeking to enhance his resources.

In the introduction to the book I noted that for purposes of economic analysis a society is profitably viewed as a kind of game in which strategic decision-making behavior with respect to maximizing utility and profit is viewed as bounded not only by natural constraints but social rules. These parameters to economic behavior include ideas about obligatory giving. Gray does not note this thesis nor the fact that throughout the discussion of obligations I underplay their effectiveness and stress the strains put upon them by individual maximizing behavior.

If I were to write that book again today (it was actually put in the form in which it was published about eight years ago), I would note that it is possible to view reciprocal obligations between persons in any society as constants framing the individualistic exchanging of material means and labor, or it is possible to release the social dimension from constraint and allow it to