

**Answer to Levin.** – I am very sorry, if the author of "Semitic and Indo-European: The Principal Etymologies, with Observations on Afro-Asiatic", Saul Levin, took offence (*Anthropos* 94.1999: 646–647) at some critical comments expressed in my review of his book (*Anthropos* 93.1998: 625–627), it was not my intention to hurt anybody and him less than all. My annotation "provided that two roots are phonetically similar, their etymological affinity can be given for certain; then whether their meanings converge or not is irrelevant, since a plausible explanation of their semantic change(s) can be always somehow conceived with some imagination" (626) is obviously no author's quotation, but rather a caricature of his method (which, however, as already said above, was not intended to offend, but) which wanted rather to summarize in an exaggerated sense the spirit of his methodological proceeding way with all its weaknesses; at any rate, if this offended him, I apologize.

Nevertheless, I find a matter of fact that in Levin's book the lexical comparisons are essentially based on the simple phonetic similarity and, when the meaning of the compared cognates does not agree, as is for instance the case for the "displaced numerals," the author supplies the reader with some ad hoc explanations of the semantic development having the goal of rendering the comparisons somehow plausible, but which require the reader a considerable act of faith. It is generally known that in the course of time words change their meaning, therefore it is certainly naive to expect that cognates always have the same exact meaning in all languages where they occur, nevertheless one should absolutely not forget that the semantic component has to act as a checking device for the plausibility of the comparisons themselves and, if in order to justify the meaning diageement of some cognates, it is necessary to set up improbable and/or unprovable ad hoc theories (as the author does several times in his book), one has to be aware that all this invalidates the whole comparison set.

Finally, in spite of my full understanding for all the difficulties of indicating recurrent sound correspondences among languages to compare or of reconstructing the relative chronology of loans, I maintain that it is necessary because of methodological reasons to hold to sound correspondences (even by the search for presumed loanwords!) in order that the work earns the dignity of being "scientific", because the simple phonetic similarity is not sufficient (it could be in fact also due to a fortuitous coincidence!), and this is especially true, if one does not even accept any rigorous checking function of the semantic component. Marcello Lamberti

**Rejoinder to Lamberti.** – Marcello Lamberti and I are the way to a meeting of minds. Since my rejoinder to him was published (*Anthropos* 94.1999: 646–647), I have worked on the long chapter dealing with "Consonantal Correspondences" and showing how most of the pertinent etymologies fall into patterns. Either a word

of Indo-European origin underwent certain recurrent changes in Semitic, or on the contrary a Semitic word when taken into Indo-European. I would gladly share findings, as far as they go, with M. Lamberti or any interested reader. Saul Levin

**Reply to Aguilar.** – In reply to M. I. Aguilar's criticisms of my review of the Paul Spencer festschrift volume (*Anthropos* 95.2000: 314–315) I have only a few brief comments.

I have reviewed a great many volumes written by historians or covering historical topics, not only for *Anthropos* but for other journals. These have been favourable where the books have made proper use of anthropological or sociological writings and theories relevant to the topics and issues they raise, and they have been critical where such works have failed to do so. If historians choose to write about age sets, age grades, witchcraft, magic, lineage organization, kinship, and other such topics where anthropologists have written important works, then they are obliged to show proper mastery of the relevant ideas and literature. The same holds for anthropologists who have written about issues related to history. In my own case I have made frequent use of historical materials and historians' criticisms in my publications on African society, religion, ancient Greece, and the history of social theory. I was also honored by the history department of the University of Iowa by being invited to be the visiting Ida Beam lecturer in history in 1989. I think that my own record, far more than Aguilar's, indicates that I have had a long and intense commitment to the relation of history to anthropology. After all, it would be difficult for a student of Evans-Pritchard, as both I and Paul Spencer were at the same time, not to be keenly interested in the ties between history and anthropology. I am also interested in the ties between anthropology and literary criticism. Such interests, however, do not absolve scholars from neglecting relevant literature when they cross disciplines.

Regarding the fact that I have repeatedly criticized the English in some of the works I have reviewed (in many journals), I can only remark that the level of English has generally declined in writings in all areas of social science. This may be the result of poor training or of poor copyediting. Whatever the reason, it has not helped scholarly understanding or made readers' lives happier. It is certainly a feature of any work that warrants comment. If Aguilar's monograph had been as well written as his letter criticizing me, I should have liked his study much more. T. O. Beidelman

**Kurze Gedanken zu Hans Peter Duerrs Bemerkungen zu (m)einer Rezension.** – Hans Peter Duerr hat sicherlich viele bemerkenswerte Eigenschaften: Kritikfähigkeit gehört allerdings nicht dazu. Nun kann man nicht erwarten, daß Autoren zerknirscht in sich gehen, wenn man ihre Bücher nicht für gut befindet und dies