

represented by the Aurignacian assemblages. Difficulties in distinguishing between cores and tools (such as nucleoform endscrapers and burins, rabots, etc.) are indeed encountered in dealing with Aurignacian assemblages, but the problem does not arise with the Chatelperronian, the Ahmarian, or even the Dabban.

In sum, it seems that Chazan has arrived at the right conclusion (rejection of the language hypothesis) but for the wrong reasons. At least in my book, this is much better than the other way round.

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There is a striking discrepancy between the coarse grain of the general categories Chazan uses (such as language, linguistic abilities, cognition) and the fine grain of the small-scale and traditional archaeological measurements of his research. Throughout the paper he treats such concepts as "linguistic abilities," "cognitive capacity," and "economically rational" as virtually synonymous. Taking into consideration basic developments in the cognitive sciences as to, for instance, differences between linguistic and other cognitive capacities or the modularity of both language and "mind" might have permitted a more differentiated and refined operationalization of "linguistic abilities." Also, his analysis might have benefited from a look at the discussions of Bickerton's (1990) notion of protolanguage and Donald's (1991, 1993) notion of mimesis.

On top of this, the link between these concepts and "efficiency," "standardization," and "transport" is as vague as the link between these three and the quantitative data he has accumulated. The nature of the relationship between "language," on the one hand, and "efficiency," "standardization," and "transport," on the other, is unclear and very problematic indeed. His assertion that the "strength of an approach based on stone tools is that there are massive collections of stone tools from almost all geographical regions relevant to the transition from both the Middle and Upper Paleolithic" simply does not apply. Sample size is irrelevant here, as no attempt is made to "translate" stones into language. Instead, he simply tells us that he will (1) look at lithic industries (2) in terms of "cognitive capacities" (3) to make inferences on language.

Thus, even on the basic analytical level, the paper is seriously flawed: the relationship between "language" and "cognition" and that between these concepts and alleged archaeological "correlates" such as standardization and efficiency is merely implied.

There are also problems with the way in which these hypothesized correlates of language are measured archaeologically. For instance, Chazan cites Dibble, who, among others, has shown that archaeologists often measure "standardization" of discard, whereas in some cases—perhaps especially but not exclusively in the Up-

per Palaeolithic—we might be measuring standardization of design. There is more to tools than just length-width-thickness ratios, but this element is completely lacking in Chazan's analysis. A related problem is his treatment of cutting edges: as he states himself, many tools document only the final phase of a complex use-life, and Chazan's implicit *ceteris paribus* clause is much too simple here: assemblages are aggregates of individual pieces with individual "use-lives" and reduction and transport histories, discarded over many years, decades, or centuries of use of a site until sedimentation isolated them as "archaeological layers" that represent unknown amounts of time.

Likewise, the claim that archaeologists can hold "raw-material availability constant" by working with "assemblages . . . from the same or neighboring sites" is untenable. Raw-material availability, even at the level of individual sites, is to a high degree dependent upon climatic variations: interglacial vegetation or cold-phase slope deposits can make raw-material sources inaccessible, increased fluvial activity can uncover previously hidden flint-bearing chalk formations, and so forth, as described in detail for the eastern part of the Aquitaine by Turq (1992). Even on a fine time scale landscapes are usually not very stable, and consequently raw-material availability is not constant.

Finally, we do not see at all why a "revolutions" approach to the archaeological record—however debatable such an approach may be on the basis of archaeological evidence—entails a teleological perspective on human cultural evolution.

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Chazan's somewhat brief article makes some important points about the Middle-Upper Palaeolithic transition which supporters of the current orthodoxy ought to take into account. First, as I have argued elsewhere (Graves 1994), it is improbable that we shall ever know anything about the origins of language from the archaeological record. Whilst that record certainly tells us a great deal about the mental abilities of early humans, it tells us nothing about language itself because artefacts have neither syntax nor semanticity. They do not *represent* something else and so are not "symbolic" in that sense (see Graves-Brown 1995a, b). This underlines Chazan's point about points! Their form is largely a question of performance, not of economy or of symbolic standardisation. Whilst we might all agree that choices have to be made in terms of both the "final" form of the artefact and the *modality* by which it is produced (Lemmonier 1989, Pelegrin 1986), this does not constitute an equivalence to verbal symbols. Points are "standardised" because they are intended to perform the same function, not because they are intended to represent something, as is the case with words.