

Letters to the Editor

A Forum for Commentary on Articles and Research Issues

A Critique that Misses the Point: Reply to Bricker

We wish to take this opportunity to respond to H. Bricker's comment (2010) about our assessment of the case for an Aurignacian-Châtelperronian interstratification at the Châtelperronian 'type site' of Grotte des Fées de Châtelperron (Riel-Salvatore et al. 2008). We are pleased to see that it has generated some interest, and thank Bricker for taking the time to express his concerns.

Bricker takes issue with our general approach to typology, which he considers unorthodox, if not downright heretical. His critique boils down to the fact that we used types that, in his view, do not define the Aurignacian as it was traditionally defined by de Sonneville-Bordes and Perrot. The irony is that we largely agree with him and were explicit about this in our study. Why did we resort to creating more inclusive versions of the Aurignacian Index? Quite simply because the only previous typological assessment of the Grotte des Fées Châtelperronian, based on the retouched tools from all five cuts from Level B, yielded a GA value of precisely zero. This value derives from typological data in an unpublished Ph.D. dissertation (Harr-old 1978) that Bricker considers "good" (i.e., reliable). Yet, Bricker asserts that "the recent detailed examination of the relevant material by Zilhão and his colleagues should be accepted as correct." Because that study indicates that up to 8.2% of the assemblage from the supposedly interstratified Aurignacian Level B4 consists of Aurignacian diagnostics, it is clear that something is amiss in the typological systematics used to make these assessments. Is it a matter of determining who is the better typologist (see Sackett [1988] for an insightful discussion of this issue)? Or is it more likely that Zilhão et al. (2006, 2008) and Gravina et al. (2005) simply used a list of Aurignacian diagnostics that is more inclusive than the traditional one devised over half a century ago? We think the latter, and that Bricker does not comment on this fundamental discrepancy effectively vitiates much of his critique.

Some of Bricker's remarks also strike us as contradictory. On the one hand, he chastises us for the seemingly uncontroversial observation that mobility patterns and sample size must be taken into account in these assessments. But, after dismissing these concerns as of "dubious relevance," he then asserts that we do not come to "a meaningful conclusion." As for the "putative" Aurignacian tools, we stand by our contention that laminar technologies can generate only a rather limited range of tool morphologies, all of them easily produced by competent flint knappers. This equifinality is, in our view, sufficient to explain the presence of such items in most Châtelperronian assemblages. We are thus puzzled by Bricker's observation that the Châtelperronian at Les Tambourets contains 1.77–2.15%

Aurignacian tools (Méroc and Bricker 1984), even though the site has, by his own admission, "no known Aurignacian occupation." Bricker's perspective appears incommensurate with the presence of these artifacts in a 'pure' Châtelperronian context, yet this too is left unaddressed. If Aurignacian diagnostics do exist, as Bricker repeatedly states, what are they doing in an undisturbed Châtelperronian assemblage? Our perspective, on the other hand, easily accommodates that observation and does not depend on the problematic notion of a 'culturally pure' assemblage. If Bricker prefers to think that retouched stone tools are little more than cultural calling cards in spite of decades of work showing the contrary, that is his prerogative (cf. Sackett 1988, 1991; Clark 1989; Freeman 1994; Barton et al. 1996). We maintain that these tools were manufactured mainly for quotidian purposes, and that lithic technologies were conditioned by a host of situational or contextual factors with which all foragers had to contend (e.g., group size, mobility, resource distribution, anticipated activities, etc).

If Bricker disagrees with this perspective, it is incumbent upon him to provide an alternative explanation for the empirical patterns we identify, rather than simply stating that our theoretical perspective is "so general as to be non-explanatory." In fact, we believe we can detect an emerging consensus that the factors just noted are important determinants of forager assemblage composition in general (Clark 2002, 2009; Clark and Riel-Salvatore 2006). Since we are likely not dealing with 'culturally diagnostic' artifacts but rather with the range of products of blade technology, we continue to think that the effect of sample size on assemblage diversity is quite significant and needs to be explicitly accounted for (e.g., Grayson and Cole 1998). Thus, contrary to Bricker's assertions, and to carry his metaphor to its logical conclusion, we are not comparing apples and oranges, but instead simply trying to identify the ingredients that go into the complicated fruit salad that is the Paleolithic archaeological record.

While we appreciate the spirit in which Bricker's critique was offered, we find it to have little substantive merit. Even if the chronological relationship between the Châtelperronian and the Aurignacian proposed by Zilhão et al. (2006, 2008) eventually turns out to be correct (but cf. Mellars and Gravina 2008), typological data alone are sufficient to invalidate the contention that there is credible evidence for an interstratified Aurignacian assemblage in the Châtelperronian deposits at Grotte des Fées.

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