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Among others, cut-marks are archaeological evidence: reply to 'Archaeological evidences are still missing: a comment on Fariña *et al.* Arroyo del Vizcaíno Site, Uruguay' by Suárez *et al.*

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In Fariña *et al.* [1], we claimed that a rich fossiliferous locality, Arroyo del Vizcaíno (hereafter, AdV), with marked bones that are much older than widely accepted for human presence in the Americas, deserved 'to be included in the agenda of early American peopling, either as a not foreseeable discovery . . . or as an example of natural processes mimicking human presence'. The comments by Suárez *et al.* [2] fail to offer a hypothesis of such a process. Instead, they mention we incur in 'serious methodological problems and important interpretative errors' that remain mostly unexplained.

Besides, our chief proposal of the marks having the features of those made by humans (which would render them an evidence as archaeological as any other) is only superficially and even erroneously dealt with, including criticisms to a previous, preliminary publication [3]. Their attempt at refuting our extensive and detailed work on 15 marks boils down to literally quoting one single warning in Behrensmeyer *et al.* [4]: 'microscopic features alone are not sufficient evidence to distinguish human-generated cutmarks from the results of trampling'. With that only argument, they claim that the cut-marks we studied are 'suspect'. Apart from the term chosen, this would pose no great novelty, as implicit in our remarks, but the reasons for their considering 'suspect' as synonym of 'invalid' are not given.

Also, it must be stressed that since Behrensmeyer *et al.* [4] was published in 1986, substantial improvements in the methods and techniques of studying bone surface modifications have taken place [5–7], as normally happens in a scientific discipline along three decades. Indeed, cited in our paper but ignored by Suárez *et al.* [2], the presence of microstriations, Herzian cones, shoulder effect, V-section, asymmetry and several quantitative variables allow to minimize the probability of misidentification of trampling marks as cut-marks or vice versa [5–8].

Falling short of discussing the very centre of our paper, the authors also made a number of considerations of lateral aspects, many of which imply misunderstandings of our contributions, logical leaps and major errors in the knowledge of the involved disciplines. Lacking enough space, we will mention here only a few of them.

Suárez *et al.* [2] make the appalling assertion that: 'It is not true that San hunter-gatherers "schlep entire carcasses with masses in excess of several hundred kilograms to more permanent camps"', because the author, Bunn

[9], ‘never substantiated [it] with data’, for which they quote O’Connell [10]. It is unusual that in scientific arguments something published is so carelessly considered ‘true’ or ‘not true’ without direct evidence. Besides, there is a most serious mistake, since O’Connell [10] does not challenge Bunn’s [9] observations on the Kua San hunter–gatherers in the East Central Kalahari Desert in Botswana. Instead, he contradicts what is said in a different paper by Bunn *et al.* [11] on the Hadza people in Tanzania.

Another major conceptual mistake in Suárez *et al.* [2] refers to mortality profiles, which ‘are constructed using tooth eruption–wear of dominant species . . . while Fariña *et al.* used bone fusion. This makes their results not comparable with data from Stiner or other specialists’. It is common knowledge among South American vertebrate palaeontologists that ground sloths lack deciduous dentition and have a single set of ever-growing teeth, rendering it impossible to establish age classes based on stages of tooth eruption and wear as is commonly done with other mammals. We cannot see why our use instead of long bone features to classify the individuals in three categories (young, adult and old) can be considered invalid, nor Suárez *et al.* [2] give any ground in that respect. In fact, several age determination methods yield generally comparable results in humans [12]. It is noteworthy that conclusions in forensics affect people’s freedom and—where there is death penalty—even people’s lives much more than palaeontology or archaeology.

Also about the mortality profiles, it is claimed that ‘the basic problem with adult dominated assemblages is the bad preservation of the young’. Precisely, it is basic taphonomy that some remains are differentially affected by diverse processes and we proposed in particular that human action cannot be discarded in the case of the AdV site as the source of such bias, while Suárez *et al.* [2] offer no alternative.

Some miscellaneous statements are given below, followed by our replies:

‘. . . the efforts by Fariña *et al.* to reduce [AdV site] to a single event’. Wrong claim: actually, we acknowledged there are two facies and we speculated that there could be two populations of bones with different taphonomical histories. The only time we leave this possibility open is where we say that the ‘physical preservation is rather homogeneous in the vast majority of the elements, indicating fast burial, *perhaps* as a single event’. Emphasis was added to give a proper idea of the amount of our ‘efforts’.

Furthermore, Suárez *et al.* [2] argue that our ‘selection of four *Lestodon* ribs for dating is not helpful in assuring that they produce independent dates. Under those conditions it is not safe to pool the results’. This is completely misleading; they give no notice of other elements we sampled (our table S1), as a glyptodont scute and two pieces of wood that ensure independence.

‘Moreover, the output of the Oxcal 4.1 program is not provided’. Wrong claim: this is given in the table S1, right column.

‘It is very difficult to make compatible “little evidence of major fluvial transport” with several events of “high energy”’. This conceptual mistake confuses the source of the evidence for high energy events, i.e. the deposit of sediments, with that of transport, namely the fossil remains. Besides, the palaeosurface (shown in our figure 1b) was composed by microbasins of several metres deep that must have acted as traps for large particles such as bones of giant mammals. Thus, one or more high energy events could have contributed smaller pebbles without transporting the fossils away.

‘In the end, [AdV site] is a time-averaged bonebed, resulting from a variety of events and processes’. This is an unsubstantiated, speculative conclusion drawn without explanation.

‘The bone collection from [AdV site], including many from one species and few from many others, makes absolutely no sense in terms of human hunting activities’. Another wrong claim: as discussed [1], the proportions of the anatomical regions represented resemble those observed in some kill sites.

‘. . . it is difficult to accept that “long distance weaponry” was implicated’. What is difficult to accept is that Suárez *et al.* [2] have misread that sentence so much. Lacking any evidence in one sense or another, nothing can be said about this. We only repeated this as a general reason given in the literature why humans hunt prime adults efficiently.

‘How many pebbles and cobbles were removed during the excavations? Which were their shape, size and raw materials? Which were the criteria applied to collect and select the so called artifacts within the “natural” lithic assemblage?’ Those are surprising questions, since all this is properly treated in the numeral 6. Lithic material of our ESM. Moreover, they assert that ‘This important information becomes crucial when only very few artifacts . . . are recovered along with dozens of alleged butchered animals’. Once again, this affirmation is wrong, since the possible artefacts are 10% and the well-diagnosed scraper represents 1% of the whole of the lithic sample collected, which does not seem so scarce. Besides, it is irrelevant, since we state that ‘though processing this number of giant animals would require a correspondingly high usage of lithic material, other South American Pleistocene archaeological sites also show a paucity of such elements’ and that only a part of the AdV site has been rather exhaustively explored. Hence, the claim that ‘technological and taphonomic analysis of lithics . . . is absent’ is thoroughly false.

Also about this, Suárez *et al.* [2] questions that the lithics collected are ‘useless’ for processing so many huge animals. Surprising again: nowhere in [1] or its ESM is proposed that those lithics were the specific tools used to process all the individuals found.

Finally, we entirely and emphatically reject the accusation that we needlessly ‘squeeze [AdV site] in order to find a human imprint; when there is evidence it shows, and none was found so far at [AdV site]’. We were cautious in our interpretations due to the unexpectedly old age of the site but we described, among other facts, 15 bone surface modifications that have the features of cut-marks, a mortality profile compatible with human agency, a proportion of anatomical regions that resembles those of kill sites and a lithic element that shows micropolish, considered unequivocal evidence of human action. Why can all this evidence be put down? Should we have held the publication of those facts? Should we have drawn different conclusions until we find unexplained evidence that satisfy all the sceptical? We firmly believe that healthy science progresses with daring but sound proposals and, on the other hand, with criticisms that make allowance for sheer facts, especially those pointing towards a different direction from what it has been easier to accept for some time.

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