Forum Reply

https://doi.org/10.1130/G46156Y.1

^{10}Be in Australasian microtektites compared to tektites: Size and geographic controls

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In his welcome commentary on our paper (Rochette et al. 2018), Mizera (2019) challenges our preferred interpretation for the Australasian tektite (AAT) source: soil-covered old continental sediments, i.e., ^{10}Be-rich onto ^{10}Be-poor material. Blum et al.'s (1992) analysis of Sr and Nd isotopic ratios points toward Jurassic-Cretaceous sediments, such as those found in abundance in south Laos and east Thailand (Singsoupho et al., 2014). Our point was that Chinese loess, or any other thick Quaternary sedimentary column, cannot account for the consistent ^{10}Be gradient observed from Indochina to Antarctica, interpreted as the translation of a vertical gradient in the target. Mizera put forward instead his previously published (Mizera et al., 2016) target hypothesis in northern China deserts (i.e., the source of Chinese loess dust), and specifically in the Baidan Jaran desert (BJD), using a stack of glacial period sediments (^{10}Be-poor) and interglacial paleosol (^{10}Be-rich).

Let us first point out that our ^{10}Be results on microtektites added on previous tektite data by Ma et al. (2004) cannot be used to derive precise inferences on the geographic position of the target, but on its vertical structure in terms of ^{10}Be content. In fact we did not discuss the "out of Indochina" hypothesis of Mizera et al. (2016) due to a lack of space, not because our data or interpretation constrains the impact location within Indochina.

Although we acknowledge that our preferred interpretation is based on a number of assumptions and simplifications, we found several debatable points in the arguments of Mizera:

(1) The tektite event is said to occur at the beginning of interglacial period M19, i.e., at a time a paleosol was developing. This is not in agreement with the most recent work of Valet et al. (2019) on Indian Ocean deep-sea cores that clearly put the tektite event at the very end of glacial period M20 or at the beginning of the transition to M19; i.e., at a time paleosol was not yet forming. It is unwise to use Chinese loess stratigraphy for precise chronology, as done by Mizera, as loess sequences do not follow a simple sedimentation pattern: paleosol may develop on materials that were deposited during a cold period due to down-going pedogenesis (see, e.g., Zhou and Shackleton, 1999). Thenceforth, the possible ^{10}Be stratigraphy of BJD invoked by Mizera becomes problematic. In our tropical soil over old rock scenario, the fact that the impact occurred during a cold period does not change the composition of the soil resulting from long-term alteration, independently of climatic cycles.

(2) Chinese loess/paleosol source, and by inference the BJD, is said to show an "exceptional" chemical and isotopic match with the AAT: this is not the case regarding ε_{Nd} (Mizera et al., 2016, their figure 7) and of course, ^{10}Be content (apart from soil and mud, but see point 1). We also note that Mizera et al. (2016) reported no bulk chemical analyses for BJD samples that would allow one to assess this match. BJD material at the origin of the AAT is said to be coarse sand with ~10% fine fraction (see Wang et al., 2015), otherwise the low ^{10}Be end member will not be reached. The BJD is praised for its exceptional dune thickness (>100 m on average; Mizera et al., 2016), implying that it is likely that the AAT material would be entirely derived from such dunes. In fact, Hu and Yang (2016) provide chemical analysis of BJD surface sands that shows bulk mean SiO_2 content of 79.5 ± 3.5 wt% (n = 18) against 73.8 ± 3.5 wt% for the AAT (n = 125). Additional significant differences include the abundance of TiO_2, 2.8 ± 1 wt% versus 0.7 ± 0.1wt%, and of another immobile element Zr, 106 ± 49 µg/g versus 310 ± 41 µg/g, respectively.

(3) The 1–10-m-thick soil/bedrock section inferred in our model "would probably not provide enough material for AAT formation." The volume of melt involved in the AAT was estimated to be <1 km^3 (Ma et al., 2004). A 250 km^2 surface (i.e., a 9 km radius circle) would correspond to <4-m-thick molten section. The order of magnitude is correct, although the volume estimate is quite poorly constrained.

(4) Our old rock plus soil model "has not been supported by field data and mixing calculations." This sentence implies that geochemical data from our suggested target do not support the model. In actual fact, no such data exist. We thus propose to wait for chemical and isotopic analyses of the suggested target in, e.g., south Laos to return to the issue.

In conclusion, we feel that, given the data presently available, our proposed interpretation stands against the Occam's razor test, and that alternative interpretations based on a thick Quaternary sedimentary target may need more-complex assumptions and ad hoc configurations in terms of ^{10}Be contents. In particular, a ^{10}Be-rich upper layer due to the onset of an interglacial period prior to tektite fall cannot be invoked. It is beyond the scope of this response to discuss the difficulty the "out of Indochina" hypothesis of Mizera et al. (2016) has in reproducing the geographic characteristics of the AAT field. Note also that no evidence for proximal ejecta is given by Wang et al. (2015) in a core from the BJD that samples a continuous sequence of the past 1 million years of sedimentary record.

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