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Letter to the Editor

Climate effects of the 74 ka Toba super-eruption: Multiple interpretive errors in 'A high-precision $^{40}Ar/^{39}Ar$ age for the Young Toba Tuff and dating of ultra-distal tephra' by D. Mark, et al.

To the Editor,

Mark et al. (2013) recently proposed a new age for the Sumatran Youngest Toba Tuff (YTT) super-eruption of 75 ± 0.9 ka, based on $^{40}Ar/^{39}Ar$ dating of proximal and distal tephra deposits. This age falls within previously measured ranges for the eruption (Oppenheimer, 2002; Storey et al., 2012) and is not under dispute here. However, the authors' interpretation of the palaeoclimatic implications of this finding suffers from fundamental errors.

Mark et al.'s assessment of potential climate effects from Toba relies on their discussion of large sulfate spikes, and associated spikes in electrical conductivity and other ions, in the GISP2 and NGRIP Greenland ice cores dated to around 75–70 ka. Although debated (e.g., Gatti and Oppenheimer, 2012), these spikes have previously been suggested to result from the YTT event (Zielinski et al., 1996; Svensson et al., 2012). Mark et al. accept this association in their discussion of the Greenland record (see their Section 6.4), reporting their new c. 75 ka age as 'supporting the correlation of YTT to the SO_4^{2-} and ECM spike in the GISP2 ice core', and describing the sulfate peak as a 'YTT spike' in their text and figure captions. They then posit that the YTT eruption occurred 'immediately prior to a 1 ka cooling event between interstadial 20 and stadial 20', but suggest that as there is a climatic recovery just prior to the onset of Greenland Stadial (GS) 20, Toba was not therefore responsible for the onset of GS-20. The cooling identified by Mark et al. prior to the end of Greenland Interstadial (GI) 20 is further suggested as evidence supporting 'forced climatic cooling on a centennial scale' by the YTT eruption. This is the only mention by Mark et al. of climate forcing in their study, and it was clearly considered a significant enough finding by the authors to include 'Forcing of Quaternary climate' in the study's title. Further, it is claimed that the study's findings bear on questions of environmental change in India attributed to YTT, and by extension on the inhabitability of the subcontinent at that time (see Haslam et al., 2010; Haslam and Petraglia, 2010; Haslam et al., 2012 for further discussion of these issues). Mark et al. go on to suggest that their new YTT age can serve as a tie point for multiple palaeoclimate records.

Critically, however, the position of the Greenland spikes relied upon by Mark et al. refute their positing of a 1000 year 'cooling event' following the YTT eruption, as well as the relevance of the recovery at the end of GI-20 to discussions of Toba's climate impact. Located during the decline from GI-20 into GS-20 (Zielinski et al., 1996; Huang et al., 2001; Oppenheimer, 2002; Xia et al., 2007; Storey et al., 2012; Svensson et al., 2012), the position of these spikes is fixed in relation to the climate data from the same core, and is unaffected by the new $^{40}Ar/^{39}Ar$ data as it is independent of any ages assigned to the YTT event. If the sulfate spikes are correctly assigned to Toba, as Mark et al. accept, then the eruption happened after the GI-20 climatic recovery discussed by Mark et al., and after any 1 ka cooling event that may have occurred 'between interstadial 20 and stadial 20'. The authors' readiness to attribute forced climatic cooling to the Toba eruption, when the eruption demonstrably post-dates the cooling to which they refer, is without merit. Oddly, Figure 9 of Mark et al. provides the data that refute their assertions of Toba-related climate forcing between GI-20 and GS-20, but this figure is referred to instead in the paper as supporting the non-existent 1 ka cooling event, and the figure caption specifically points out the irrelevant climatic upturn at the end of GI-20.

Mark et al.'s distortion of the indirect YTT evidence in the Greenland cores is compounded by their treatment of the other major palaeoclimate record they discuss, from Hulu Cave in China. Based on their placement of a Stadial 20 tie point at just over 69 ka in the Hulu sequence (in their Figure 10), Mark et al. suggest that either the Hulu Cave chronology is wrong, or that the monsoonal Asian climate lagged behind the North Atlantic by a c. 3 ka 'offset'. However, Mark et al. inexplicably place their Hulu 'Stadial 20 Correlation' in a position that is thousands of years too recent, in the stadial that followed Chinese Interstadial A19, as demonstrated by detailed reconstructions of the Hulu Cave and correlated Sanbao Cave climate sequences (Xia et al., 2007; Wang et al., 2008) (see Fig. 1). Note that again, this is not an error attributable to incompatible chronologies, but a mis-representation of the relative positions of interstadial and stadial events in the Hulu Cave record (specifically in stalagmite MSL in this instance). By incorrectly placing their Stadial 20 correlation thousands of years later than its currently accepted position, Mark et al. invent an unnecessary dilemma over mis-matched chronologies. In its correct position in the Hulu Cave sequence (Fig. 1), the tie point to the relevant Greenland sulfate spike is dated by the Hulu Cave sample MSL-199 (Wang et al., 2001: Supplemental Table 1). This sample has a

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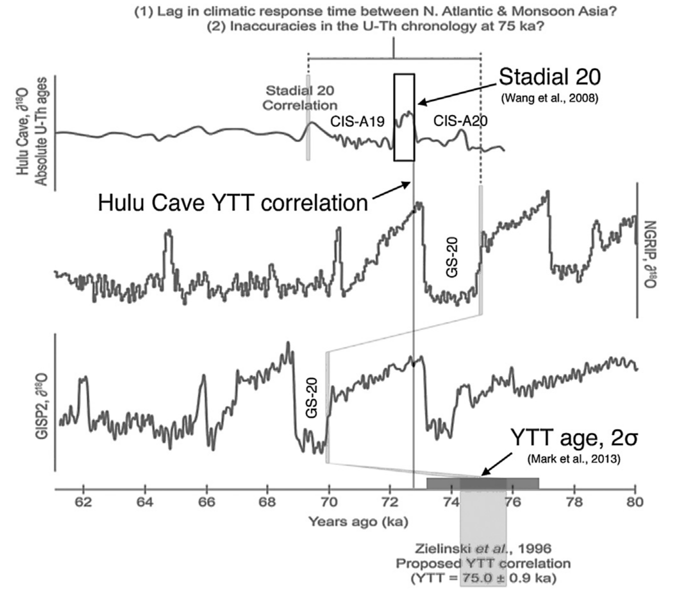

Fig. 1. Position of Stadial 20 in the Hulu Cave sequence (open box; Wang et al., 2008), overlain on Fig. 10 of Mark et al. (2013). Note that Mark et al. (grey shading, text and lines) err significantly in their placement of the correlation point. The correct tie point for YTT in the Hulu Cave sequence, based on the position of the Greenland sulfate spikes, is indicated. Adjacent interstadials and stadials are labelled for clarity (black text; CIS: Chinese Interstadial; GS: Greenland Stadial), and the two-sigma uncertainty in the YTT age reported by Mark et al. is shown.

^{230}Th age of 72.87 ± 0.4 ka, which is indistinguishable at the two sigma confidence level with the YTT age reported by Mark et al., eliminating the proposed 3 ka 'offset'.

Overall, while Mark et al. have produced a YTT age in agreement with several existing estimates, re-confirming the long-standing and widely-accepted Late Pleistocene age of YTT tephra in India (Westgate et al., 1998), this finding adds nothing new to discussions of the eruption's impact on climates, environments or hominins. Aside from the dating itself, which is not disputed here, in its attempt to draw wider conclusions about the climate record the study demonstrates a consistent lack of rigour, leading to misinformation and confusion. Future information on volcanic events recorded in Antarctica (Parrenin et al., 2012; Svensson et al., 2012), and high-resolution dates that allow fine adjustment of the Greenland record (Boch et al., 2011) will provide the corrections needed to resolve chronological discrepancies, not the mislabelling of existing records. Mark et al.'s recommendation that their c. 75 ka age be used as a tie point for linking disparate records may be valid (its validity is not assessed here), but their own attempt to apply and discuss such a tie point is flawed, at odds with the records they purport to calibrate, and should not be relied upon. Given these multiple inaccuracies, the recent work of Storey et al. (2012) on the same topic should be preferred as a current source of reliable information on the timing and impact of the YTT super-eruption.

Editorial handling by: R. Grun

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