

Interactive comment on “Potential impact of the 74 ka Toba eruption on the Balkan region, SE Europe” by B. Wagner et al.

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General comments

The paper by Wagner et al. provides a new contribute within a sustained debate on the climatic effects of the Toba super-eruption, from Sumatra caldera, occurred close to the MIS 5-MIS 4 transition (c 75 ka). This eruption is indeed one of the major explosive events of the recent Earth history and during the last 25 years several volcanologists, paleoanthropologists and paleoclimatologists have been engaged in defining its actual effects on global climate and ecosystems, but currently no consensus has been reached.

Recently, with the advent of the studies at very high temporal and stratigraphical reso-

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lution, the investigations on the 75 ka Toba super-eruption have undergone a notable increment, as shown by c. 40 papers focused on this subject published only since 2011. However, the debate is far to be ceased, indeed in some cases the disagreements have led harsh comments and related replies (see for instance Lane et al., 2013 [PNAS 110, 8025–8029] and comment by Roberts et al., 2013 [PNAS 110, E3047] as well as Mark et al., 2013 [Quaternary Geochronology, in press] and the comment by Haslam, 2013).

In the light of the current controversial debate, the paper by Wagner et al. should be welcomed if it brings new evidence which cast light on this unresolved issue. Ideally, in order to address and settle this issue, three requisites are needed: (i) one or more very high temporal paleoenvironmental records; (ii) the presence of the Toba tephra in the same records and (iii) a robust and precise chronological framework.

Unfortunately, the dataset presented by Wagner et al. suffers under several aspects with respect to the mentioned prerequisites. Firstly, there is no evidence of the presence of Toba tephra in the Lake Prespa. The authors correctly recognise this circumstance as a limitation for their general inferences, but in spite of this they propose to overcome this crucial problem via a simple chronometric correlation. Specifically, they use an extrapolated ESR dating at 73.6 ± 7.7 ka for correlating a dramatic lowstand of the Lake Prespa to the Toba eruption and thus interpret this aridity event - recognised as very peculiar in the whole Prespa record - as the environmental-climatic effect of the volcanic eruption.

This conclusion is however highly speculative and not adequately supported. In my opinion, the lack of the Toba ash and the large uncertainty associated to the age of the lowstand make the Prespa record hardly suitable for addressing such an issue.

Finally, also the paleoenvironmental record has not a sufficiently high resolution for documenting the potential short-term effects of the Toba eruption. In addition, in terms of potential long-term effects, the drop of the arboreal pollen associated to the shell-

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layer documenting the lowstand and correlated to the Greenland Stadial 20, which follows the Toba eruption, is not so surprising marked. Indeed the Prespa pollens provide evidence for other similar stadial episodes occurred during both MIS 5 and MIS 3, some of which are associated to more severe aridity conditions. This rises some doubts on the claimed exceptional climatic condition associated to the lowstand dated at 73.6 ± 7.7 ka.

The authors also mention the fact that the lake is in a tectonically and karst active area, but they rule out the possibility that a paleo-earthquake and/or karst processes could have been responsible for the dramatic lowstand of the Lake Prespa, because a similar tectonic/karstic event dated at the end of MIS 5 "has not been described thus far". However, I disagree with this affirmation; in fact the lack of descriptions is not necessarily a proof of no occurrence. Paradoxally, one could affirm just the contrary; i.e., that the lowstand could be seen as an evidence of a previously unrecognised late-MIS 5 tectonic/karst event. In other words, in the lack of independent and compelling evidence (e.g. Toba ash or paleo-seismic data), either Toba eruption or a paleo-earthquake are both, even if not firmly proved, plausible explanations.

In summary, the evidence of the Toba eruption impact provided by the authors are not fully compelling. It substantially rests on the fragile chronological data which would allow to associate the event of significant drop of the level of the Lake Prespa, dated at c 74 ka, to the Toba eruption. I do not discuss the peculiar paleoenvironmental significance of the the Perspa Lake lowstand, which is well documented, but its direct linkage to the Toba eruption, claimed by the authors, is not convincingly demonstrated and, for myself, it does not go beyond the speculative sphere.

Specific comments

It would be useful to show in Figure 2 a correlation between Prespa pollen profile with one or more paleoclimatic records containing the Toba ash (e.g. Arabian Sea) or its putative volcanic signal (e.g. NorthGRIP or GISP2).

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Sincerely,

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