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Climate of the Past Discussions

Interactive comment on "Re-evaluating the link between the Laacher See volcanic eruption and the Younger Dryas" *by* James U. L. Baldini et al.

Anonymous Referee #2

Received and published: 2 December 2017

I am not convinced by this paper, and so recommend rejection or major revisions. Perhaps the timing of the eruption is close to indicators of cooling (not my specialty), but the hand-waving arguments about why this eruption caused cooling and larger ones did not are not convincing. Indeed more work is required, including climate model simulations that include all the relevant processes. Even if the timing was close, there is no proof that this was not just a coincidence.

The authors claim that the climate system was particularly sensitive to volcanic forcing at the time, but this is just speculation. Where are the model results to show this? In fact, in Fig. 4 there are two larger eruptions during the same period. Why did only Laacher See produce cooling? They claim in the Fig. 4 caption that the Hekla eruption was more proximal, and therefore should be discounted, but the way it works is that

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Icelandic eruptions into the westerlies have to go around the world before the acid snow deposits on Greenland, and so there is no reason to think that it would have a smaller climate impact than Laacher See.

Even the size of the eruption is speculation, and the authors mix mass of SO2 with that of elemental Sulphur with that of stratospheric aerosol. What do they claim actually was the stratospheric loading for this eruption? And each time you talk about mass, please convert it to the same chemical so it can be compared.

The title is confusing. Why is it "re-evaluating?" There is no initial evaluation that is addressed in the abstract or in the paper.

Since the Laacher See eruption was high latitude, we would expect that for the same stratospheric loading, it would have much less of a climate impact than an equivalent tropical eruption, since the atmospheric lifetime would be much shorter, and there is less insolation at high latitudes. When you compare to Toba, this must be addressed. And if the eruption was in the fall or winter, most of the aerosol would have fallen out of the stratosphere before the Sun comes up the next summer and there would be minimal impact on climate.

The paper is replete with undefined acronyms, making it very confusing. All acronyms have to be defined the first time they are used. For example, what is LST? It is never defined. Is it LSE and a typo? What are TOMS, NGRIP, GISP2, GICC05modelext, ITCZ, GS-20, GI-19 (in Fig. 6), ...?

Please keep in mind that there will be readers not from your specific discipline, and so jargon needs to be defined. GS-1 is finally defined long after it is used, but the authors still never say what Greenland Stadial 1 is. What is a stadial? Why does Greenland have one? How many does it have?

The paper talks about magnitudes for volcanic eruptions, but never says what the scale is. Magnitude of what? If not of sulphur injection, then what is the point? And where

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do the data come from? There are no references to that.

As for the Toba eruption, the paper is missing key references on the climate impact. Robock et al. (2009) found a larger short-term impact, but no long-term effect. Timmreck et al. (2010) claim that it would have had a small impact, as the particles would have grown and had a smaller impact per unit mass.

Robock, A., C. M. Ammann, L. Oman, D. Shindell, S. Levis, and G. Stenchikov, 2009: Did the Toba volcanic eruption of ∼74 ka B.P. produce widespread glaciation? J. Geophys. Res., 114, D10107, doi:10.1029/2008JD011652.

Timmreck, C., et al., 2010: Aerosol size confines climate response to volcanic supereruptions. Geophys. Res. Lett., 37, L24705, doi:10.1029/2010GL045464.

In any case, I find the Haslam and Petraglia (2010) Figure 1 very convincing that it got cold before the eruption. By the way, that reference is missing from the reference list. Why does the timing of the Toba eruption in Fig. 6 here differ from that in Fig. 1 of Haslam and Petraglia (2010)? Which is correct, and why?

The paper ignores all the work that has shown that the 1257 Samalas eruption caused the Little Ice Age (Zhong et al., 2011; Miller et al., 2012; Slawinska and Robock, 2017). What does this tell us about the claim that a much smaller eruption of Laacher See caused a much larger climate response?

Miller, G. H., Á. Geirsdóttir, Y. Zhong, D. J. Larsen, B. L. Otto-Bliesner, M. M. Holland, D. A. Bailey, K. A. Refsnider, S. J. Lehman, J. R. Southon, Ch. Anderson, H. Björnsson, and T. Thordarson, 2012: Abrupt onset of the Little Ice Age triggered by volcanism and sustained by sea-ice/ocean feedbacks. Geophys. Res. Lett., 39, L02708, doi:10.1029/2011GL050168.

Slawinska, J., and A. Robock, 2017: Impact of volcanic eruptions on decadal to centennial fluctuations of Arctic sea ice extent during the last millennium and on initiation of the Little Ice Age. J. Climate, doi:10.1175/JCLI-D-16-0498.1,

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Zhong, Y., G. H. Miller, B. L. Otto-Bliesner, M. M. Holland, D. A. Bailey, D. P. Schneider, and A. Geirsdottir, 2011: Centennial-scale climate change from decadally-paced explosive volcanism: a coupled sea ice-ocean mechanism. Clim. Dyn., 37, 2373-2387.

It would have been nice to have used hanging indents or additional spacing for the reference list to make it easier for the reader to find each paper in the list.

In addition, there are another 35 comments in the attached annotated manuscript that need to be addressed.

Please also note the supplement to this comment:
https://www.clim-past-discuss.net/cp-2017-147/cp-2017-147-RC2-supplement.pdf

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