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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.

D.M. Pyle

david.pyle@earth.ox.ac.uk

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This is an interesting paper which revisits the hypothesis that the eruption of the Laacher See Tuff may have provided a trigger for the onset of the Younger Dryas (YD). The weakness of the paper as it stands at the moment is that there is little new evidence to present, so the arguments haven't really changed since the hypothesis was first introduced. In some ways the paper might be better framed as a critical review of competing hypotheses for the 'cause' of the onset of the YD, in that this would help to bring some clarity to the current discussion, and to identify the key gaps in knowledge for the commuinity to tackle. As it stands, the paper is perhaps too much like a pitch in favour of a particular hypothesis.

For me the leading questions (which come out of this paper) are:

- was the LST eruption the source of a sulphur 'spike' in Greenland ice cores; and how could we test this assertion?

- what was the total volatile yield from the LST eruption; and what new measurements are needed to improve on this assessment. (And did the halogen release have any impact?)

- what cascade of physical processes could lead to the observed pattern of response seen for the onset of the YD; and is a volcanic eruption a sufficient driver, on its own.

Detailed points

Line 44 – there is also a documented enrichment in noble metals (e.g. platinum), at this stratigraphic level both in North America and Europe (Moore et al., Scientific Reports 7 Article Number: 44031, 2017; and papers by A Andronikov). Line 50 – 'new support' yes, but not much new evidence. Line 50 – ages: some explanation is needed about the framing of time in the paper, as the model ages are derived from multiple approaches.

Lines 53- 56: the emphasis here isn't quite right. The evidence for a 200-year time break between LST and the onset of 'YD' conditions in continental Europe remains firm. What has changed – since Lane et al., 2015, is the recognition that the onset of YD is time-transgressive. So – by inference – LST overlaps with GS-1 and the onset of YD as recorded in ice chemistry in Greenland.

Line 57 – does the 'GICC05modelext chronology' need a word or two of explanation and a citation?

LST Impacts

Line 74: see also the extensive work by Felix Riede on the impacts of the LST:

Book, 2017 - Splendid Isolation : The eruption of the Laacher See volcano and southern Scandinavian Late Glacial hunter-gatherers. / Riede, Felix. Aarhus : Aarhus Universitetsforlag, 2017. 214 p.

2016 - Changes in mid- and far-field human landscape use following the Laacher See eruption (c. 13,000 years BP). / Riede, Felix. In: Quaternary International, Vol. 394, 02.2016, p. 37-50.

2012 - Bayesian radiocarbon models for the cultural transition during the Allerod in southern Scandinavia, Riede, Felix; Edinborough, Kevan, JOURNAL OF ARCHAEOLOGICAL SCIENCE 39, 744-756

2008 - The Laacher See-eruption (12,920 BP) and material culture change at the end of the Allerod in northern Europe, Riede, Felix, JOURNAL OF ARCHAEOLOGICAL SCIENCE 35, 591-599

Volcanic Emissions

Lines 78 - 85 – this section needs some critical revision and updating.

Harms and Schmincke (2000) estimated, using mass balance, an SO2 yield of 20 Tg. Harms et al. (2004) did some experiments on LST magmas and determined the P, T , H2O conditions under which the magma was stored; you could revisit the calculations of Harms & Schmincke to re-estimate the S and water budgets of the system – taking account of the work that Bruno Scaillet and colleagues have done on other systems. The '150 Mt' value should be cited as Schmincke et al (1999, Quaternary International, 61, 61-72) – it is, as the authors say 'highly speculative' and based on using the 'Pinatubo multiplier'; this can certainly be improved upon, rather than being taken as a starting point for the argument.

Similar calculations have been attempted by Textor et al., 2003, (Geol Soc London Spec Pub, 'Volcanic Degassing', 213, 307-328); who also estimated the total halogen yield.

Discussion on volcanic emissions

Recent papers may also add a little to the discussion here: for example - - Colose, C.M., A.N. LeGrande, and M. Vuille, 2016: Hemispherically asymmetric volcanic forcing of tropical hydroclimate during the last millennium. Earth Syst. Dyn., 7, 681-696, doi:10.5194/esd-7-681-2016. - LeGrande, A.N., K. Tsigaridis, and S.E. Bauer, 2016: Role of atmospheric chemistry in the climate impacts of stratospheric volcanic injections. Nature Geosci., 9, no. 9, 652-655, doi:10.1038/ngeo2771.

Line 178 – 'five years' may be an overestimate: in Graf and Timmreck's model, sulphate aerosol had an e-folding time of 11 months; and the detectable signal of volcanic stratospheric sulphate aerosol is usually considered to be less than three years. Lines 180 – 195 – there's not really any new evidence here? Line 208 – the magnitude of the eruption is not relevant, it's the magnitude of the gas release that is the key point. The LST magma is an unusual composition, so surely this is the starting point for why it may have had an exceptional impact? Lines 274 – 284: there still is no way of linking a sulphate peak in an ice core to a particular eruption, in the absence of any tephra so this remains speculative. It remains possible that sulphur mass-independent isotopic fractionation signals may help to identify plumes that entered the stratosphere (e.g. Martin et al., 2014, Volcanic sulfate aerosol formation in the troposphere, JOURNAL OF GEOPHYSICAL RESEARCH-ATMOSPHERES Volume: 119 Issue: 22 Pages: 12660-12673), but this still won't help with source identification.

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