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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 #1

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Baldini and colleagues present a previously published volcanogenic sulphate record from GISP2 ice cores and compile age estimates for the Laacher See tephra to argue that the LS eruption is recorded in Greenland ice cores as a large sulphate spike positioned approximately at the onset of Greenland Stadial 1 (i.e. Younger Dryas). The authors finally suggest that the LS eruption triggered the YD through a chain of feedbacks resulting from the initial volcanic-induced cooling in the Northern Hemisphere.

Even though their hypothesis is tantalizing I find the manuscript excessively speculative and the conclusions very much stretch what can be observed in the reconstructions. I should also point out that the putative link between the LST and a volcanic sulphate spike in GISP2 records was originally suggested by Brauer et al. (1999). In my opinion, the present manuscript doesn’t offer anything new but I brief review of the climatic

implications of volcanic cooling and far-reaching speculations on the triggers for the YD. Besides the speculative aspects of the paper, I have three major concerns that I think should be taken into account.

Main comments:

1. Assigning the LS eruption to a nearly synchronous large sulphate peak in GISP2 is a flawed assumption until proven otherwise by tephochronological analyses. The authors should consider alternative hypotheses. For instance, volcanic records from Greenland are particularly sensitive to Icelandic eruptions due to the eruptive frequency and proximity (e.g. Abbott and Davies, 2012). Hence, even small-size Icelandic eruptions characterised by moderate sulphate emissions can result in disproportionally large sulphate anomalies in ice core stratigraphies.

2. I think the paper would greatly benefit from exploring some of the mechanisms the authors discuss (e.g. southward wind shifts, AMOC decline, sea ice expansion, ect.) by looking into climate model output and/or historical reanalyses data sets. The authors present output data from MAECHAM4 simulations but they don’t provide any additional analysis of the atmospheric parameters in the model. CMIP historical climate simulations and PMIP last-millennium simulations offer valuable model data to examine the role of volcanic eruptions on the coupled atmosphere-ocean system. Model output based on volcanically-forced transient simulations with earth System Model (MPI-ESM) (Jungclaus et al., 2010) (available online) may also be useful.

3. I think some of the arguments proposed here (as well as the records presented in Figure 3) have conveniently been picked to craft a story where the LS eruption stands out as the INITIAL cooling that triggered the YD. This does not faithfully reflect the state of the knowledge around the dynamics that took place prior to and at the onset of the YD. Several reconstructions show that a gradual but substantial cooling across Northern Europe and in the Nordic Seas preceded the start of GS-1. Pollen records from the British Isles indicate cooling as early as 13,200 years BP (Walker et al., 2012). A drop

in air temperatures a few centuries before GS-1 has also been recorded in chironomid-based temperature records from Norway (Bakke et al., 2009; see supplementary information), Sweden (Muschitiello et al., 2015), and the Netherlands (Heiri et al., 2007). Similarly, sea surface temperature records from the Norwegian Sea indicate a rapid cooling approaching YD values as early as 13,500 years BP (Bakke et al., 2009) with temperature values dropping by ca. 2 °C. Well-dated paleoceanographic records from the coast of Norway also show a progressive aging of surface waters starting at ca. 13,200 years BP (Bondevik et al., 2006), which is suggestive of a slowdown of surface-water circulation and reduced advection of warm subtropical waters prior to the start of GS-1. I therefore believe that the authors should do a better job in contextualizing the LS eruption in the regional climate picture since proxy reconstructions from Central Europe (e.g. Grafenstein, 1999; Rach et al., 2014) are evidently not fully representative of large-scale North Atlantic climate. In particular, the records mentioned above clearly suggest that cooling and climate deterioration was long underway before the start of the YD, which implies that the cooling associated with the LS eruption cannot be the trigger for the YD.

Specific comments:

L30: An alternative route has been proposed involving the drainage of the Baltic Ice Lake, the timing of which precisely coincide with the start of GS-1 (Muschitiello et al., 2016).

L42-47: Without entering into the discussion on the credibility of the "impact" theory, I think that the evidence is still undermined by the poor chronological accuracy of the platinum/iridium anomaly.

L54: What stratigraphic frameworks? Please specify and provide reference.

L57-58: "...consistent with the LS eruption..." Please add here "as recorded in varved and 14C dated lake records".

L148: Please provide reference here on the impact of volcanic forcing on AMOC (e.g. Otterå et al., 2010).

L162: Please specify what model. This is not clear.

L176-192: This section is all very speculative. Please see my comment on the possibility of examining climate model output to support these claims.

L181: None of the studies cited here present direct evidence of sea ice changes (only indirect and mainly based on terrestrial reconstructions). Please consider referring to marine reconstructions (e.g. Cabedo-Sanz et al., 2013).

L223: I'm not convinced that the data in Figure 5a and b follow a Gaussian distribution. Rather, the frequency distributions seem skewed. Also the authors cannot claim that "a Gaussian distribution exist" when they fit a Gaussian-best-fit model to their data. They should use a resampling/bootstrap method to draw from their empirical distribution and only then establish its shape.

L219-230: I suggest estimating the frequency of Greenland cooling relative to the number of volcanic events occurring at the end of the preceding interstadial. This should somewhat inform on the potential link between volcanisms and the onset of stadial cooling.

L246: The rate of cooling (as seen in the d18O ice-core stratigraphies) is similar among most of the stadials, not exclusively between GS-1 and GS-20.

L247-249: The initial cooling can be tested using ice-core (e.g. d15N) and other proxy-based temperature reconstructions and compare the magnitude of the temperature change with that associated with large historical eruptions.

L253: Is this claim based on ice-core d18O profiles? I don't see any substantial warming in Greenland during the second half of GS-1. d18O of ice can be misleading due to changes in moisture source of precipitation. Before making this claim I would check the ice-core temperature records (d18O diffusion or d15N). As far as I can tell from the d18O data both GS-1 and GS-20 in Greenland were characterised by cold conditions throughout the stadial.

L258-260: Again, this is extremely speculative and I don't think there is any evidence supporting this claim.

L275-284: The magnitude of the spike does not necessarily scale linearly with the magnitude of the eruption and could solely depend on the proximity of the volcanic source or the atmospheric circulation pattern.

L290: Age uncertainties for the LST and the sulphate spike should be reported on their respective time scales (i.e. GICC05, IntCal13, MFM varve time scale, etc.). In addition, time scale offsets between the 14C time scale and GICC05 should be accounted when comparing radiocarbon-based and ice-core ages (e.g. Muscheler et al., 2014).

L291: Please see my previous comment. Cooling in the North Atlantic started a few centuries before the onset of GS-1.

L305-306: As I mentioned above, it would be helpful to look into the frequency of stadials relative to the number of volcanic eruptions during the preceding interstadials.

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