

Clim. Past Discuss., author comment AC2
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Reply on RC2

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Author comment on "Ice core evidence for major volcanic eruptions at the onset of Dansgaard–Oeschger warming events" by Johannes Lohmann and Anders Svensson, Clim. Past Discuss., <https://doi.org/10.5194/cp-2022-1-AC2>, 2022

Below are our responses to the issues raised by the referee (*italic*).

Page 6, line 9: I suggest here to also try to discuss the amplitude of the eruptions considered not only as compared to Tambora, but also as compared to Samalas eruption. From what I understand, the mean amplitude of the eruptions considered here to help triggering a DO onset is about 2-3 times larger than that of Tambora, which might correspond about to the amplitude of Samalas (e.g. Jungclauss et al., 2017). There exists a number of models that do consider this eruption in PMIP4 Last Millennium simulations, which might be interesting to consider in follow up studies to evaluate the associated climate impacts, etc.

Thank you for this suggestion. We specifically chose a comparison with 1-in-500 year eruptions (greater or equal Tambora), because the return period of eruptions in our data set is actually 500 years. The return period of a "Samalas-magnitude" eruption in our data set is also consistent with the expected return period of a Samalas-type eruption estimated previously from ice cores, which is however less certain than a Tambora-type. In the revised manuscript (Sec. 2.3 as well as Fig. 3) we will include a comparison with the Samalas eruption.

Pages 7, line 19: it should be stated more clearly here that the model is an OGCM. This choice has strong consequences as it is not properly considering interactions with the atmosphere, and more importantly, the restoring terms might strongly affect the stability of the AMOC.

We will be more specific in saying that it is ocean-only, and that processes not considered in the model may alter the stability of the AMOC. Note, however, that the stability of the AMOC both in present-day and the glacial is still very much a matter of debate and very model-dependent. Thus, the point here was to propose a mechanism where we simply

assume bi-stability of the AMOC, and to use an ocean model that fulfills this assumption. We will state explicitly in the revised manuscript that our proposed mechanism is contingent on the sea ice and atmospheric response to not destroy the bi-stability of the AMOC and the dense water formation in the North Atlantic as a volcanic trigger of an AMOC transition.

Page 8, line 9: I assume it is 35 g/kg and not 3.5 g/kg, is that correct?

Indeed, thank you.

Page 9, line 14: the citation of Fig 6a that early in text is raising an issue, since normally, the figures appear in the order of their citation of the text. Also, I have the feeling that too much result materials are presented here, while "Material and Methods" should mainly describe the tools, not the results obtained with them.

Ok. We will move some material from Sec. 2.4 to the main text.

Page 9, lines 8-10: this reference to data from Lin et al. (2021) is a bit surprising at this stage. I think it should be introduced in the section 2.1 and better compared with the other reconstruction used.

We assume that with "other reconstruction" you mean the Svensson et al 2020 data set? We will add a Section 2.2. where we better describe the two volcanic data sets. Section 2.1 is dedicated to the isotopic data sets.

Page 9, line 13: typo: "evidence" should be "evident"

Ok.

Figure 3: I suggest to put also estimate from Samalas eruption here.

Ok.

Figure 6: as compared to hysteresis from various EMICs shown in Rahmstorf et al. (2005), this one seems a bit different. Present-day state is in particular not bistable, contrary to what is found in a number of AMOC in Rahmstorf et al. (2005). I suggest to discuss this somewhere in the last section and compare the bifurcation figure with Rahmstorf et al. (2005).

We are happy to briefly discuss this in the revised manuscript (Sec. 3.7). A detailed study of the bifurcation structure and the landscape of stable and unstable states in the model is currently underway. The main feature of the model that is important here is that there is a tipping point of the AMOC. The shape and width of the hysteresis loop depends on boundary conditions and other very much unconstrained parameters in the context of ocean-only models. We don't think it is at present possible to argue which ocean-only model or EMIC gives an accurate representation of the AMOC stability in the glacial period.

Page 19, line 18: the use of "likely" has a quantitative meaning in climate community due to IPCC reports. I suggest to reformulate this sentence which is not clear enough, notably the use of "some DO warming transitions" into a more quantitative IPCC-like assessment. Given the sentence just before, I would say that: "it is thus very likely that volcanic eruptions occurring a few decades before a DO warming contribute to this onset". Indeed, in IPCC terminology "very likely" corresponds to above 90% confidence level. This still does not mean that all DO events are triggered by volcanic eruptions... I let the authors further improve such a formulation to be entirely in line with the high precision of their results.

Thank you for this suggestion, we agree that the sentence is vague and will adapt accordingly.

Page 20: From my understanding of the impact of large volcanic eruptions on the AMOC, I think numerous processes including sea ice and salinity might be at play. Generally speaking, I would interpret the results obtained from the ice cores a bit differently than what is proposed here using the simple OGCM. Volcanic eruptions are inducing strong excitation of the main variability modes of the North Atlantic (e.g. Swingedouw et al., 2017). In this respect, they can be considered as an exciter, an

energy provider of variability of the AMOC following volcanic forcing. Here, following an eruption of the intensity of Samalas, there might large oscillations in the AMOC (e.g. Mignot et al., 2011, their Fig. 2). These oscillations can be positive ones, which clearly might increase the chance of noise-induced bifurcation highlighted here, but interpreted mainly through thermal buoyancy forcing at the surface from the volcanic eruptions, while many other processes might be at play. Thus, it is the fact that volcanic eruptions induce a strong variability (or noise) in the system that explain the shift, whatever the exact processes, which might deserve a more comprehensive climate model. This is more or less already what the authors depict, but this is not stated very clearly in my opinion. The exact mechanisms behind this excitation of larger noise are then not that essential at this stage of the hypothesis (also because AMOC response to volcanic eruption might be model dependent...even in more comprehensive ones).

We agree in principle with this interpretation, and with the fact that the exact processes that occur after the volcanic eruption and an excitation of the AMOC deserve a more comprehensive model. We will stress this better in the revised manuscript.

The point of our specific proposed mechanism, as opposed to a more general excitation of AMOC variability, is that it may explain why the eruptions seem to excite AMOC resurgences and not shutdowns. We are of course aware that our explanation is only one of many others that would also involve other processes in sea ice and atmosphere. We will state more clearly that the eruptions may also just lead to a general excitation of AMOC variability, and that this could lead to a spontaneous AMOC resurgence. We will also more clearly point out the varied impacts of different types of eruptions (latitude, season etc.) on the AMOC, referring to Mignot et al 2011.