Geoscientific Model Development Discussions

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Comment on gmd-2021-372

Anonymous Referee #2

Referee comment on "Effects of forcing differences and initial conditions on inter-model agreement in the VolMIP volc-pinatubo-full experiment" by Davide Zanchettin et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-372-RC2, 2021

General comments and recommendation:

This paper aims at describing preliminary results based on the VOLMIP initiative, and in particular a multi-model large ensemble of 5-year experiments including the forcing of a Pinatubo-like volcanic eruption based on a common protocol for all the models. This article is scientifically well constructed, well presented and very well written. Merging and analysing such a large dataset is a huge and valuable effort. One small weakness of this article is that some aspects could be detailed for a better understanding, in particular for some analysis that would benefit from more explanations. A list of comments and suggestions is presented below, these ones corresponding to minor revisions that need to be addressed before publication. Most of them are discussion points for which it could be interesting to get the view of the authors. A list of technical corrections is also presented at the end of this document.

Discussion points

- I understand the benefits to build a multi-model protocol in which the volcanic forcing is commonly defined, to allow a “good agreement between the different models » and highlight the differences between the models in terms of response to external forcing independently to the implementation of the forcing. Overall, is there any risk that the homogenisation of the climate models encouraged in model inter-comparisons support the building of a unique family of similar models, all of them showing the same uncertainties that would be therefore more difficult to estimate? Should we encourage more contrasted model developments for a better understanding of the processes at play?
- Could we expect an impact of the anthropogenic forcings on the climate response to volcanic eruptions? In other words, would we expect different conclusions starting the volc-pinatubo-full experiment from control experiments produced with constant anthropogenic forcings corresponding to those observed at the beginning of the XXIth

century, and/or in transient forcing experiments?

- P4, L.125-130: How do we deal the fact that the modes of variability typically show different patterns for the different models? Why not having considered an EOF approach, using for each model its mode of variability with its specific pattern?
- P5, Characteristics of volc-pinatubo-full, the multi-model ensemble: Would it be possible to give more details about the spectral resolution of the models? Does it differ among the models? The way that the VOLMIP forcing is distributed over the spectral bands could be detailed. More information about the vertical distribution of the forcing in the different models would be also welcomed: is the forcing vertically distributed in a stationary way, using monthly climatologies of the elevation of the atmospheric layers, or is the forcing vertically distributed on-line? I saw that this weakness is discussed at the end of the paper, but why not including more information about these model features in this publication?
- Initial conditions: Why not sampling the QBO on a similar way as the other modes? QBO impact on climate response to volcanic forcing has been evidence in Thomas et al., 2009. (Thomas, M. A., Giorgetta, M. A., Timmreck, C., Graf, H.-F., and Stenchikov, G.: Simulation of the climate impact of Mt. Pinatubo eruption using ECHAM5 – Part 2: Sensitivity to the phase of the QBO and ENSO, Atmos. Chem. Phys., 9, 3001–3009, https://doi.org/10.5194/acp-9-3001-2009, 2009). At lower frequency, why not considering different states of the AMV that might affect also the response of the modes of variability (Ménégoz, M., Cassou, C., Swingedouw, D., Ruprich-Robert, Y., Bretonnière, P.A. and Doblas-Reyes, F., 2018. Role of the Atlantic Multidecadal Variability in modulating the climate response to a Pinatubo-like volcanic eruption. *Climate Dynamics*, *51*(5), pp.1863-1883). This point is discussed at the end of the article. Nevertheless, we do not know the reasons for which these modes have not been considered in the first edition of VOLMIP.
- P10: the ENSO differences among the models based on a temperature average over the Niño 3.4 area only might be affected by the ENSO specific position in each model. The ENSO signature in models is often shifted Southward/Northward Eastward/Westward as compared to the observations, and it differs clearly from one model to another one. This could be discussed in the article. The same issue can be highlighted for the NAO signature, and this might be a much more important issue considering the typical spatial biases of the NAO pattern in the current generation of AOGCMs.
- P13, L. 402: "dynamical responses may be masked by broad tropical radiative cooling effects » -> So why not considering a relative ENSO index (Nino3.4 tas minus tropical tas) as done in several publications (e.g. Khodri, M., Izumo, T., Vialard, J., Janicot, S., Cassou, C., Lengaigne, M., Mignot, J., Gastineau, G., Guilyardi, E., Lebas, N. and Robock, A., 2017. Tropical explosive volcanic eruptions can trigger El Niño by cooling tropical Africa. *Nature communications*, *8*(1), pp.1-13.). This is discussed in the end of the article, but why not including directly such a "RENSO index" in the article?
- P13-14: feedbacks: more explanations about the LW and SW ratios would be welcomed, to allow a better understanding of the sign of the feedbacks (negative versus positive) as well as the processes that are suggested in this Section. It is delicate in particular to understand whether the LW and SW changes are related to aerosol changes or to changes in the atmospheric temperature.

Technical corrections

- L71: "sensitivity experiments aimed" -> which one? Maybe more information could be given here.
- P6, L. 165: there is a more recent description of the Orchidee surface scheme in Cheruy

et al., 2020 (Cheruy, F., Ducharne, A., Hourdin, F., Musat, I., Vignon, É., Gastineau, G., Bastrikov, V., Vuichard, N., Diallo, B., Dufresne, J.L. and Ghattas, J., 2020. Improved nearâ□□surface continental climate in IPSLâ□□CM6Aâ□□LR by combined evolutions of atmospheric and land surface physics. *Journal of Advances in Modeling Earth Systems*, *12*(10), p.e2019MS002005.)

- P10, L. 294: why referring to Table 1 here?
- P10, L.302: It is stated that IPSL-CM6 is warmer in the tropical Pacific, but this is only verified in the Nino 3.4 domain, since it is cooler on average over the whole tropical area if I understand well Figure 3?
- Figures 4-5-6-7-8-9: A vertical line could indicate the exact timing of the eruption.
- Figure 10 caption: it is not totally clear whether the y axis show simply LWt/LWs□ for one experiment (volc-pinatubo-full) or an anomaly difference between this experiment and the control (as mentioned in the text at Line 409).
- P14, L. 414: "a tendential lowering » -> lowering with the time after the eruption?
- P15: 4.5 effect of sampling strategy: Again, it could be relevant to consider relative ENSO index (Nino 3.4 versus all the tropical areas) to disentangle the dynamical response of ENSO from the radiative cooling. The fact that the winter NAO does not affect the climate response to the volcanic forcing might be also explained by the relative small persistence of this mode of variability as compared to ENSO or AMV for example.
- P15: Ensemble size: In Figure 12, what is the period considered to compute the GST? (First year post-eruption?)
- P16, L.480: compare -> compared
- P18, L. 560: to understanding -> to understand.