EGUsphere, referee comment RC1
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Comment on egusphere-2022-638

Anonymous Referee #1

Referee comment on "On the additivity of climate responses to the volcanic and solar forcing in the early 19th century" by Shih-Wei Fang et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-638-RC1, 2022

Summary

The authors analyse whether the volcanic and solar forcing response of the climate system is additive during the Dalton Minimum or not. Using ensemble simulations with a state of the art Earth System model, they find that the global climate responses are additive, but regionally some non-linear effect are present.

General comment

The authors present a well-structured study on a relevant topic, i.e., how natural forcing agents act separately and together on the climate system. The study is overall well written but the resolution of the figures need to be increased to at least 300 dpi. Overall, the study presents some new and interesting finding which desire publication in Climate of the Past, though I recommend some minor to major revision. One thing, which would make the study more convincing, is the inclusion of the 20CR reanalysis data which now spans back to 1806.

Specific comments

L13/14: Here the authors state that IN GENERAL the responses are additive. This is in contradiction to the statement later (L26-28) that regionally this is not the case. I suggest to write here that the superposition is only found for global mean considerations.

L22: The author state that the polar vortex strengthens when both forcing agents are present but weakens for each separate forcing agent. I did not find any explanation here or in the manuscript for this. The authors need to develop a mechanism/concept on why this is happening – just describing is not enough.

L36: change to "… (Timmreck et al., 2021) and the 1815 …"

L36-45: I think these studies are key for the discussion and should be used to really discuss the new findings of the manuscript in context to existing literature, so please use them in section 6.

L65-78: There is an interesting proposed by a colleague of mine which perfectly fits to the longer lasting effects after an external forcing event. In Lehner et al. (2013) they propose a sea ice-ocean-atmosphere feedback which can establish after a cooling in the Nordic Seas (induced by e.g. a volcanic eruption). So I think it is worth to mention it, to check whether this mechanism is also relevant in yoir study and to use it in the discussion part (Section 6).

Lehner et al., Amplified inception of European Little Ice Age by sea ice-ocean-atmosphere feedbacks. J. Climate, 26, 7586-7602, 2013.

L97: change to "… 40 years. The 20 ensemble members are generated…"

L98: The authors need to explain how they perturb the atmosphere.

L110: superscript 14 for ^{14}C.

L125: Line break before "Several".

L139: Which significance level is used 5% 1% ????

L140-148: The description of the rapid adjustment remains unclear.

L148: we are considering -> we consider

L154-155: The authors give a rate her, i.e. k/month but is unclear over which period this rate is estimated.

L171: Winter -> winter

L171: I see that the temperature reduction is strongest in the Arctic in winter, but it is not significant, so why is that and why do the authors discuss not sign. results here. The only sign. response is the tropical temperatures showing a weak cooling.

L171-184: The entire discussion is strange. On the one hand the authors tend to focus on changes which are not significant and also ignore some strange behaviour, e.g., the Arctic sea ice in summer is strongly extended (significant) but the surface temperature shows almost no change, how can that be? Similar the AO reacts in winter but no imprint on the other variables. Figure 3 is not easy to compared with Fig 5 as the authors do not use the same colour scales.

L186: Yes, but the meridional temperature gradient at the surface is enhanced, which leads to higher lower tropospheric baroclinicity. I think the authors need to be clear that their model tends to show the top down process more pronounced compared to the surface processes.

L191: Aerosols?

L195-199: I think here the mechanism described by Lehner et al. (2013) would be interesting to be assessed.

L214-217: So if the authors discuss it in anyway in section 4.2 it is not necessary to give a presentation of the results here, so I suggest to remove this part or move and merge it with section 4.2.

L252: Line break after “forcing.”.

L252-257: Only a few points are stat. significant at the 5% level in Fig. 9d. One would expect that 5% of the grid points are significantly different assuming independence so the authors cannot interpret the results of this panel to be different to pure noise.

L252-270 Interpretation of Fig.9: I think the authors tend to over interpret the significant changes, as only 50 % of the panels show clear signals and the rest can be seen as white noise (or no change). I suggest that the author revise this paragraph and concentrate on the significant changes in panels b, c, e, and g of Fig. 9.

L300-301: What about the wind driven gyre circulations – are there signals to be found?

L333: Please change to "Summary and Discussion"

L347-349: This a much better formulation than the corresponding one in the abstract.

L335-380: The discussion part needs to be substantially extended by the publications used in the introduction (see above) and Lehner et al. (2013).

L361: "… experiment, an additional tropical cooling …"

L365-370: A discussion here with using the 20CR reanalysis data would be very helpful here. I clearly recommend that the authors should compare their results with this reanalysis as it goes back to 1806 and at least over Europe it is reasonable.

L371-372: The sentence is awkward, so please revise it.

L373-375: Here you can include the discussion of the Lehner process as they involve also sea ice changes in the GIN Sea.

L381-386: Sorry to say but this is not a conclusion so please write a proper one.

L409 and following: I only checked the first 2 publications and important information is missing. Here, it is the journal name. I have not checked the rest but now-a-days there are a lot of tools helping the authors to avoid errors in the reference lists, so please,

please, use these tools.

Fig 2: Caption: please change to "… (e)-(h) are for SST (K) and the same regions. The thick …"

Fig 3, 5 and maybe 8: It looks like the authors changed the aspect ratio of the panels, which lead to a strange projection. Please use the original aspect ratio as the projection should be kept. Please also Say the significance level here. By the way, you have two options either you say "the 5% significance level" or "the 95% confidence interval". Other combinations (like "95% significance level") makes no sense (in statistics).

Fig. 7: Only few of points are significant so what do we learn from this figure (and the text in the manuscript). I suggest to remove it and also adjust the text. I recommend that the authors should stick to stat. significant results.

Fig.9: change to 5% significance level. (also throughout the manuscript).

Fig 12 Here I suggest to include also 20CR reanalysis or add it as a separate panel. In the caption at the end: ", and Schneider2015 is dashed (Schneider et al., 2015)."