Atmos. Chem. Phys. Discuss.,
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Atmospheric Chemistry and Physics Discussions

ACPD

Interactive comment

Interactive comment on "Robust winter warming over Eurasia under stratospheric sulfate geoengineering – the role of stratospheric dynamics" *by* Antara Banerjee et al.

Anonymous Referee #1

Received and published: 15 November 2020

This is a useful, well done study that I think makes a valuable contribution to the literature. It addresses a question that, in my opinion, has not been handled terribly well in the past – how different are the dynamical effects of pulse versus sustained stratospheric sulfur injections? The paper is clearly motivated, well executed, and well written. I am recommending mostly minor comments. There's one set of comments that could require some work, but it hopefully isn't too onerous.

Lines 38-39: I'm not sure where the justification for these lines comes from.

Line 47: increased equator-to-pole temperature gradient aloft

Lines 50ff: As phrased, this somewhat undermines your point. You're saying that the

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Discussion paper

winter warming happens by chance, even for very large eruptions. So why would you expect to see a forced signal under geoengineering? I'd suggest reframing this paragraph.

Lines 158-159: Agreed with the sentence, but it's coming across as though this has not been recognized before.

Lines 159-160: I don't think the comment about this being in a populated area is compelling. There are lots of reasons why unpopulated areas might be important. The Antarctic ice sheet is a great example.

Line 167: Using an SNR threshold of 1 without context isn't all that helpful because the values are dependent on your definition. 1 might be a lot, or it might not be very much. It would be more useful to contextualize these numbers, discussing an area where you know that the answer is meaningful or not, and then use that to calibrate your SNR values. I find Figure 2b a lot more compelling than 2a in this regard, because it's a clear metric.

Line 200: I might suggest removing the latitude and longitude bounds. They imply that you're looking at changes in that box, which is very large and likely has a heterogeneous response.

Lines 225ff: I like this approach, but I think more details are needed and quite possibly an augmentation to your methodology. First, if there is reason to think that your DJF timeseries and NAM50 are both dependent variables (which seems likely), you will need to regress using an errors-in-variables method. Second, ordinary least squares regression doesn't inherently include complexities (like higher order terms) – this will show up as errors in your fits, possibly like what you find in the subsequent paragraph. If you have reason to believe that there are more complex relationships between the two variables, you can build that into your model.

Related to the previous comment, regressions have error bars. Is there a way to report on that, perhaps with a figure that looks like the stippling that you show in Figure 6?

Lines 254ff: Some of your residuals resemble other known Pacific teleconnection patterns, at least at a glance. I agree that doing a good job with this is the subject of future work, but it might help if you could provide some candidate modes or mechanisms rather than being totally agnostic.

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