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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Forcing of stratospheric chemistry and dynamics during the Dalton Minimum" *by* J. G. Anet et al.

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The following short comment is not meant as a full review but rather collects the questions which arose while reading the manuscript. Before considering these, I'd like to encourage the authors to pursue the accompanying study on the tropospheric climate to which they hint in the last sentence. Our understanding of the dynamics during periods of reduced solar and/or increased volcanic activity would benefit from such a study and it would also help the paleo-community to separate the influence of solar and volcanic climate perturbations.

That said, already the manuscript on "Forcing of stratospheric chemistry and dynamics during the Dalton Minimum" by Anet et al. (2013) is a valuable contribution to our un-

derstanding of atmospheric, especially stratospheric and lower mesospheric chemistry and the related dynamical signatures. By separating potential external climate perturbations during the early 19th century, the study furthers our picture of this recent but still quasi-pre-instrumental period. The study further points out more generally how strong tropical volcanic eruptions and the various components related to a weakened solar activity may influence earth's middle atmosphere. The performed numerical experiments are also important testbeds for our ability to model the atmosphere and the climate.

My various questions on the submitted manuscript are best summarised as: Where is the discussion? However, some of my comments are probably beyond the scope of the present study.

I am aware, that the manuscript is not intended to be a paleo-climate study but rather details potential effects of the various possible forcing perturbations that may have acted during the early 19th century. However, related to my note on a possible tropospheric follow-up, I am astonished that the authors completely do without relating the basic tropospheric and surface climatology of their simulations to our previous knowledge for the early 19th century. Such a simple comparison would allow to relate the results of the performed ensembles (especially the all-forcing and the volcanic-forcing simulations) to documentary, observational and modelling knowledge about temperature (and to a lesser extent circulation) during the early 19th century. Again, the manuscript is not meant to be a paleo-climate study but I would welcome a minimum of validation of the ensembles where possible. Such validation is necessary to assess the merit of the performed simulations.

By constructing the Bottom-Up and Top-Down ensembles the authors refer to an interaction between troposphere and stratostphere and vice versa. The dynamics of such interactions, however, aren't discussed.

Further discussion is also welcome on the analyses. Especially the chosen period

(1805 to 1825) appears to be inappropriate at least but not only for the volcanic ensemble. From my point of view, the chosen window does not effectively separate the volcanic signal from the background internal variability. Furthermore, Figure 1 of the manuscript suggests that the chosen window does not capture the maximum of the perturbations and that other windows are more appropriate for all solar components (e.g. 1810 to 1830). If it appears necessary to use a common window for all runs, I would recommend using 1809 to 1829.

In addition, I wonder, whether the analysis is really done the best way. If I understand it correctly, the three ensemble members are concatenated and the mean of the such constructed annual mean zonal mean series is assessed against the control run. The analysis thus presents the mean annual mean anomaly over the period of choice (1805 to 1825). The chosen approach may indeed maximise the annual signal but likely only for those forcings which act over a multi-annual or the full 20 year period. Effects of shorter term forcings (the volcanic aerosols) may be captured incorrectly. Anyhow, the chosen procedure minimises any opportunity to benefit from the ensemble approach and to account for the initial state uncertainty and the ensemble spread.

Similarly, I do wonder whether it is appropriate to concentrate on the annual mean signal or whether it wouldn't be more reasonable to discuss the seasonal (summer and winter) signals. It is my understanding that we would expect quite different dynamical signals between the summer and the winter season or rather between the respective summer and winter hemispheres which the analysis possibly smears.

A discussion lacks also with respect to further choices made by the authors. For example, I understand the benefits of chosing the data by Shapiro et al. (2011) but since there is large uncertainty in our understanding of past changes in solar activity it appears necessary to discuss how this choice may influence the results. Again, this may imply discussing the surface signals. Similarly, I would welcome a discussion on the implications of choosing the data by Gao et al. (2008).

A thorough discussion would also help to relate the results to previous modelling work. Since even energetic particle precipitation events have already been studied, this would help to clarify the value of the results presented by Anet et al. relative to the diverse literature on all considered perturbations.

Since the study uses ensembles, it would help to get some feeling of the spread of the various ensembles. As an aside, the excuse (page 15071, line 23) for using ensembles of 3 members is rather trivial though one would of course prefer larger samples. That isn't meant as criticism, but rather the respective sentence is unnecessary.

Since the forcings are not constant over time, I would like to see some supplementary information on the temporal evolution of the anomalies in the different ensembles; but that's also only a sidenote.

Further Comments:

page 15063 line 9 and at other locations: Could you provide more, independent or more recent evidence for a hypothesized Grand Solar Minimum in the 21st century?

page 15064 line 3: Is there a reference for this definition of centennial scale solar variability?

line 23: Isn't Laki more commonly used than Lakagigar?

page 15066 line 18: Maybe use rather “highly unpredictable” instead of “of high unpredictability”

page 15067 line 15: Since the list of references is not exhaustive, an “e.g.” should start the list.

page 15069 line 15: If the PMIP3 protocol was used, shouldn't you then reference Schmidt et al. (2011).

page 15070 line 15: Maybe clarify the “and weighted . . .”-sentence?

page 15072 line 19: abolute -> absolute

page 15075 line 5: I am uncertain whether the use of “Though” is grammtically correct.

section 3.1.2: please check if all Figure-references are correct

page 15076 line 13: “over 20 yr long period”: insert “a” before of “20” or add an “s” to “period”. Anyway, does this really describe the way you analyse the data. Again I wonder whether you are really able to separate the volcanic effect by this long average. A measure of the ensemble spread would be interesting.

page 15078 line 10: Again, do you have a recent reference for expecting a Grand Solar Minimum?

page 15079 line 20: spacial -> spatial

Section 3.2.1: Maybe change order of Figures 8 and 9?

page 15080 line 22: radative -> radiative

Section 3.2.2: Wouldn’t we expect further changes in zonal wind in the other ensembles because of the temperature anomalies and because of dynamical effects of anomalies in, e.g. ozone? Are there seasonal effects which may counterbalance?

page 15081 line 15: 8d?

Section 4: I may be overinterpreting, but the first sentence of the section reads as if we knew the dynamical and chemical changes in the stratosphere during the Dalton Minimum but just not the relevant forcings.

page 15083 line 3: Insert “the” before “following”.

line 13 and 21: Are the seasons depicted correctly?

line 24: A bit of nitpicking: I wouldn’t speak of ”conclusions”. It’s just a summary of findings.

page 15084 line 3: More nitpicking: Here, and throughout the manuscript you write “experiments”. There are good arguments to make clear distinctions between computer simulations and experiments although more on a philosophical level.

line 15: Even more nitpicking: I would introduce a qualifier like “in our simulations”

line 28: treat? And if you want to write “threat”, I find this sentence overly alarmistic - not least because of the from my point of view less than satisfying evidence for a coming “Grand Solar Minimum” and the obvious uncertainties in our understanding of solar variability.

page 15085 lines 2ff: Similar to the last comment: Dependent on my mood I either regard the sentence trivial or unnecessarily stagy.

Figure 10: Could you show ensemble spreads and highlight the control run variability?

As noted at the beginning, some of my comments may be beyond the scope of the present work. Some others are more or less personal taste. However, I am indeed convinced that the study needs (1) to discuss the choices with respect to the forcing data and possible implications of using other data, (2) to change or at least sufficiently justify the chosen study period (1805 to 1825) and (3) to give, as a kind of validation, a basic impression of how the different ensembles influence the tropospheric/surface climate during the DM.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 15061, 2013.