

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

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Received and published: 14 July 2013

Response to Interactive comment of Dr. Oliver Bothe, 20. June 2013

We would like to thank Dr. Bothe for his careful reading and his numerous comments and suggestions. We respond to the comments in detail here below, where the comments of Dr. Bothe are in boldface.

I am astonished that the authors completely do without relating the basic tropospheric and surface climatology of their comparison 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

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about temperature (and to a lesser extent circulation) during the early 19th century.

We do totally understand the reviewers concern. However, the present study purposefully concentrates on the processes and mechanisms, and on their significance for climate, without already glancing at correlations with observational reconstructions (which have their own problems). That is why we decided to write a separate publication on “Impact of solar and volcanic variations on tropospheric temperatures during the Dalton Minimum”, which is currently in preparation. We agree with Dr. Bothe when he states that he is “aware, that the manuscript is not intended to be a paleo-climate study but rather details potential effects of the various possible forcing perturbations”.

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

This is a good point, but again, we would like to point at the manuscript in preparation. The notation of the Bottom-Up and Top-Down mechanisms was used for the design of the scenarios in the present ACPD paper in order to properly analyze the stratospheric dynamics and chemistry, which show clear responses to the different scenarios, while the globally averaged tropospheric responses are much more subtle (and topic of the follow-up paper).

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.

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We do not agree with Dr. Bothe on this point. One may or may not like our approach for selecting the chosen period, which choosing the 1809-1829 period would pay less attention to the important volcanic eruption in 1809 and to the preceding years showing the development up to this event.

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.

This is totally true, and caused the largest discussion among us. However, by changing the reference period from experiment to experiment, the comparability of the results suffers loss of significance. Triggered by Dr. Bothe's comment we will consider adding a figure showing the temporal evolution of NO_x , ozone and temperature, e.g. at tropical tropopause height over the entire simulation period. This could not only allow showing the ensemble spread, but also allows for showing the extremes, e.g. after the volcanic eruptions.

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.

We made a thorough analysis of not only annual, but also seasonal values and the most important results have been mentioned in Section 3. Some have been illustrated (like the EPP changes), some not, yet the latter patterns have been explicitly explained in the text. Still, in answer to Dr. Bothe's comment, we will consider putting seasonal

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average illustrations in the supplementary material section.

I understand the benefits of choosing 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).

We agree here and will add a section to discuss the different implications of having chosen the Shapiro et al. (2011) forcing. We however do not see by how much the quality of the paper would be improved by starting to discuss different volcanic forcings. Advantages and disadvantages of different reconstruction methods have been discussed already, e.g. in "Volcanic forcing for climate modeling: a new microphysics-based dataset covering years 1600-present" by Arfeuille et al. (Clim. Past., 2013).

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.

If we understood the comment of the reviewer correctly, a comparison of past e.g. energetic particle precipitation sensitivity studies to our model results is lacking. Unfortunately, to the best of our knowledge, no work is known to have performed similar studies during a grand solar minimum period in preindustrial conditions. We would be happy to learn differently from Dr. Bothe, if this were not true. Any comparison with studies looking at the influence of energetic particles in modern times is unfortunately not comparable due to the far higher CFC content in the stratosphere. (Yet, it is true that some of our results of the volcanic eruptions sensitivity study could be compared to the work of Arfeuille (PhD thesis, ETHZ, 2012).)

It would help to get some feeling of the spread of the various ensembles. Since the forcings are not constant over time, I would like to see some supplementary

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information on the temporal evolution of the anomalies in the different ensembles.

This is difficult, but we will consider to provide some illustrations or a table of temporal evolution of certain species at one or two selected locations in the stratosphere, including ensemble spread.

page 15063 line 9 page 15078 line 10: Could you provide more, independent or more recent evidence for a hypothesized Grand Solar Minimum in the 21st century?

We would like to point to the very recent work of Steinhilber et al. (2013) [Prediction of solar activity for the next 500 years, JGR] or of Lockwood et al. (2011) [The persistence of solar activity indicators and the descent of the Sun into Maunder Minimum conditions, JGR] and will add these citations to the two Abreu et al. publications.

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

The characteristic duration of a grand solar minimum is given by Denton and Karlen, 1973 [Holocene climatic variations pattern and possible cause, Quart. Res.]. Also, it is well known (e.g. Beer et al. 2000 [The role of the sun in climate forcing, Quart. Sci. Rev.]) that different cycles of activity (22 years Hale-cycle, 88 years, 208 years cycle (Sonett and Suess, 1984), 2000 years cycle (Mayewski et al. 1997) exist.

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

Yes, this is true. But we preferred to stick to the original, indigenous name and will add "Laki" in brackets.

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

We apologize, it seems that the final typesetting process confused the Figure-references. Several times, the HO_x and water vapour references have been inversed.

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We will of course correct this issue.

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?

Normally, one would. However, the significance was too low to talk about clear changes. Hence we focused especially on the "ALL" and the volcanic cases.

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.

This is true. We will reformulate the sentence to "We present in this paper a modeling study of the different forcing which could have led to dynamical and chemical changes in the stratosphere during the DM from 1805 to 1825 AD".

page 15084, line 28: 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.

We will rework this sentence.

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

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