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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.

Anonymous Referee #1

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Review of Forcing of stratospheric chemistry and dynamics during the Dalton Minimum by J.G. Anet et al., Msnr. acp-2013-296

This is another study of solar variability effects using the SOCOL model. The SOCOL model, as one of rather few, combines a chemistry climate model spanning the whole middle atmosphere with an interactive ocean and includes a number of physical parameterization necessary to model a spectrum of possible solar-terrestial interactions.

Here the authors study the period 1805 – 1825 which covers the so called (solar) Dalton minimum. They distangle the different effects of changes of solar UV, visible and particles, together with an analysis of the impact of the strong volcanic eruptions happening during this period. The authors show that the superposition of the different

forcing results in a non-linear behaviour of the model system.

The paper is generally well written, the analysis is presented in clear form, including well organised figures. Nevertheless, I have difficulties to see the scientific focus in this paper and I strongly miss a critical evaluation of the results presented. The latter is in my opinion a prerequiste for the paper to be published in ACP.

Major comments

1. The authors study a rather complicated period as the two solar cycles 5 and 6 interact with several strong volcanic eruptions. If the purpose of this study is to show the importance of non-linear superposition effects, the model has to be evaluated for the different impact mechanisms separately and beforehand. The solar reconstruction of A. Shapiro used in this study is highly debated as it gives a mucher higher UV solar variability in the past. A recently published study by Shapiro et al. concludes that the SOCOL model seems to be in reasonable agreement with solar forcing according SIM and SOLSTICE data when comparing with the solar response of some middle atmosphere species, on the other hand Ermolli et al. (2013, ACP Vol. 13, p 3945) conclude that most SSI models (including Shapiro's) cannot reproduce the SIM/SORCE spectral behaviour. Obviously, the use of the correct SSI in the past is not a settled topic. This has to be made clear for the reader already in the abstract and the consequences have to be discussed in the paper. Additional simulations applying a different SSI reconstruction would be very helpful in order to conclude on the robsutness of the results.

2. In addition, in Arfeuille et al., 'Uncertainties in modelling the stratospheric warming following Mt. Pinatubo eruption', Atmos. Chem. Phys. Discuss., 13, 4601-4635, doi:10.5194/acpd-13-4601-2013, 2013, the authors show "that the use of this dataset in the global chemistry-climate model (CCM) SOCOL leads to exaggerated aerosol-induced stratospheric heating compared to observations, even partly larger than the already too high values found by many models in recent general circulation model

(GCM) and CCM intercomparisons. This suggests that the overestimation of the stratospheric warming after the Pinatubo eruption arises from deficiencies in the model radiation codes ...“. Again, the authors do not mention and discuss any validation of their implementation of volcanic eruptions in their model.

3. The non-linear effects found are not really a surprise and are rather moderate. Much more interesting would be a specific impact of the combined forcing for example on the response of the AO influencing regional climate. It is surprising that the authors do not analyse their results for possible regional patterns. Globally, the surface effects of the DM period seems to be small when inspecting their figures, but with the both strong forcings the authors apply, some conclusions of surface effects would be very helpful for the scientific community in terms of possible influence of the MA on climate under disturbed natural forcing conditions.

Minor comments:

(not shown): I count 14x where a reference is given to a figure "not shown“. If it’s an important finding, relevant for the conclusion, please show, if not, you should consider to leave it out or put it in an appendix.

Abstract, L1: whereas the title states that the paper focusses in the stratosphere (but the whole middle atmosphere is dicussed) the abstract claims that climate effects are investigated. That’s a little bit misleading.

There are many somewhat sloppy statements (P15063 L10: "similar decrease“: where do you know from?; P15065 L25ff: what do you mean with "its effect is still not known“ and "controversial? Please specify! P15066 L8: very stable: what do you mean, compared with, on what timescales? P15066L17: "are not always directed“ (mostly they are not directed to earth) P15067 L24: "are general accepted as main drivers for global climate cooling“ P15069 L20 "All solar related driver“ expression; P15070 L11: what is a classical proxy?; P15078 L8: "harmful effect for life“ why mentioned when not also valid for the DM?

P15066 L5: GCR does not originate from SN themselves but from the SN remnants. Even in shock fronts of star forming regions particles can be accelerated to CR energies.

P15066 L9: the energy range of GCR itself is much broader and much higher energies are observed but you mean the GCR component which is mainly responsible for the ionization in the lower atmosphere.

P15067 L13: there are other simulations too, eg. Baumgärtner et al., from which you take the Ap dependent NOx parameterization.

P15068 L19: Models with low vertical resolution (as here used) often show an too fast BDC. What’s the mean age in your model, and what does it mean for the simulation of the volcanic impact?

P15069 L1ff: The radiation code of SOCOL seem to underestimate heating rates above 1 hPa (CCMVAL report). On the other hand, is there the possibility that UV is double counted in some bands from adding just the extra-heating?

P15069 L23/Fig.1: The SSI reconstruction shown give a smoothed impression compared to SSN. Is there any time filtering applied? If so, what consequences this would have for your experiments?

P15069 L25: the fact that this is an extreme reconstruction is mentioned but has to be discussed in the course of the paper, see above. What is the time resolution of the look-up-table, that is: how many realisation for the different SSIs were used in the model runs?

P15070L3 ff: please show the changes in the particle forcings as a additional figure.

P15071 L21: please explain why you do not cover at least solar cycle 5 and 6 for your analysis completely. The period chosen seems to be rather arbitrary.

P15073ff: The description of NOx-ozone chemistry can be left out.

P15080L1: why cooling not from additional H2O?

P15080ff: The Fig. 9 shows DJF values and not austral winter. If you explain by BDC changes please show the relevant analyses.

P15081L21ff: please show the relevant analyses to prove your hypothesis.

P15083L1: I cannot reporoduce 1% change in the visible

P15083 L 9: 250 nm is not VIS.

P15084 last para: for me this paragraph is essentially uncomprehensible and is somehow unrelated to the content of the paper.

Typos:

P15070 L 28: for for P15079 L20: spacial → spatial P15082L27: excepted P15084 L28: treat

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