

ACPD: Review #1

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

We would like to thank the anonymous referee #1 for his/her great review of our publication and provide here the answers to his/her questions.

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.

We agree that a separate study of the nonlinear effects would be of a great help and that such a publication would be of significant interest for the scientific community. However, we preferred looking at the summed nonlinearities in a transient climate, as it was during the Dalton Minimum. The main purpose of the study was thus not to show nonlinearities, but this important piece of information of a - rather minor, we admit (locally 20% more NO_x and 12 % more NO_y in the "stacked" field compared to DM-ALL) - nonlinear effect had to be pointed out. We toned that down to "discrepancy between the sum of the contributions and the combined modelled effect":

Text change:

"Nonlinearities are only discussed and defined as such if the discrepancy between the sum of the contributions and the combined modelled effect are significant."

The solar reconstruction of A. Shapiro used in this study is highly debated as it gives a much 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 to 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) include 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.

As the referee #1 points out him/herself, the "correct" SSI in the past is not a settled topic. We modified slightly the conclusions to answer these uncertainties. It is true that most of the present reconstructions (including Shapiro et al. 2011) cannot reproduce the SORCE measurements of the solar variability in the course of the 11-year cycle as stated in the Ermolli et al. review. However, this manuscript is aimed at modeling of the climate response to the long-term solar irradiance variability. The modern physics-based models, e.g. Shapiro et al. 2011, SATIRE (see Krivova et al. 2003), and NRLSSI (Lean, 2000) attribute the 11-year SSI variability to the competition between contributions of dark and bright features and the long-term variability to the variability of the quiet Sun. The spectral profiles of these two types of variability are different and the SORCE result does not give any direct input about the spectral profile of the long-term variability. Our group is currently

preparing the manuscript which will discuss this question in details. We note that the study of Shapiro et al. (2013, JGRD) was devoted to the analysis of the climate response to the 11-year variability and its result cannot be transferred to the present study.

Additional simulations applying a different SSI reconstruction would be very helpful in order to conclude on the robustness of the results.

We totally agree an additional study would help to quantify the robustness of the results. However, this will not be done in the current study.

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.

We agree with this comment and now discuss the uncertainties in the volcanic eruptions implementation and the consequences on our results:

Text added:
P15071L3 ?

“Uncertainties in historical long-term volcanic aerosol datasets can be large, with more challenges than for the representation of the well observed Pinatubo 1991 eruption. Indeed, the lack of atmospheric observations leads to uncertainties arising from ice-core measurements and calibrations, and from the implementations of volcanic dataset, which generally involve further assumptions (e.g. altitude and size distributions of the aerosols). The volcanic forcing applied here is based on an aerosol model for the calculation of these variables, and the strengths of this method for the depiction of the aerosol latitude/altitude/size distributions for eruptions in the pre-satellite period are described in Arfeuille et al., 2013a.

As most CCMs, SOCOL tends to overestimate the stratospheric warming following the Pinatubo eruption (Eyring et al, 2006, SPARC CCMVAL 2010, Lanzante and Free, 2007), and the AER-based SOCOL simulation of the Pinatubo eruption (Arfeuille et al., 2013b) suggest that it is probable that the stratospheric warmings due to the eruptions in the Dalton minimum period are as well overestimated. This however remains within current uncertainties for the representation of volcanic impacts in the pre-satellite period and it can be noted that in the important tropical tropopause region, SOCOL forced by the AER method leads to an accurate warming after the Pinatubo eruption, even in better agreement with observations than many GCMs forced by satellite-based aerosol datasets.”

The non-linear effects found are not really a surprise and are rather moderate.

We toned the "nonlinearity"-paragraph down. See above.

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.

This has been already pointed out by Dr. Oliver Bothe in the interactive discussion. Although we totally agree that surface changes should be investigated thoroughly, we chose to put the focus of this publication on the upper atmosphere. Any discussion of the surface, of the AO or of regional effects is hence not possible. We are however already working on a next publication, which is considering the local temperature effects at the surface during the DM.

(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.

We agree that the term "not shown" has been used quasi-inflationary and suppressed the term where not needed.

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

We modified the abstract where misunderstanding according to the comment of referee #1 was possible.

There are many somewhat sloppy statements (P15063 L10: "similar decrease": where do you know from?;

We modified the sentence to "Given this, an assessment of periods in the past containing grand solar minima is helpful to understand...".

P15065 L25ff: what do you mean with "its effect is still not known" and "controversial? Please specify!

We decided to cite Marsh (2000), and Laut (2003), to illustrate the controversial issue.

P15066 L8: very stable: what do you mean, compared with, on what timescales?

It is known that the cosmic ray field is relatively homogeneous and does not vary significantly over climatic timescales (100-500 years). However, variations over thousands of years can be reconstructed. Hence, we modified to "millennial timescales" to avoid any misunderstanding and added a citation.

P15066L17: "are not always directed "mostly they are not directed to earth)

We reformulated that sentence to "Solar protons events (SPEs) - emerge from coronal mass ejections of the sun, which occur very irregularly and are rarely directed towards the Earth."

P15067 L24: "are general accepted as main rivers for global climate cooling"

We preferred not to change this sentence, as it seems correct and sound for us.

P15069 L20 "All solar related driver" expression;

We reformulated this sentence by "All forcings influenced by the activity level of the Sun were based on ..."

15070 L11: what is a classical proxy?;

We replaced "classical" by "...from proxies like ^{10}Be , which are usually used"

P15078 L8: "harmful effect for life" why mentioned when not also valid for the DM?

We reformulated that sentence to " While higher NO_x concentrations at the poles in high altitudes above 50 km do not have a harmful effect on the ozone layer, such a NO_x production at lower altitudes leads to a slightly accelerated destruction of ozone via reactions 1-3."

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.

We do not see what is wrong in that sentence. We say that "GCRs are formed by high-energy sources like supernovae". Without a SN, no GCRs would be formed, as no SN remnants would exist.

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.

We agree here with the referee #1 and changed the sentence to "They travel nearly at the speed of light and are thus highly energetic particles. Being capable in influencing our atmosphere, those particles can reach energies of several GeVs (Bazilevskaya et al., 2008)."

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

We added Baumgärtner et al. (2009). in both citations (atmosphere & chemistry).

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?

As the stratospheric aerosol information is fed as a boundary condition to our model, the fast BDC does not have a greater significant changing effect on the volcanic impact in our simulations. The volcanic aerosol information have been modelled with the AER model, driven by climatologies of temperature- and wind fields and thus should show reasonable values.

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?

SOCOLv2 underestimated the heating rates above 1hPa, this is correct. However, we were using SOCOLv3, basing on another GCM (ECHAM5 vs ECHAM4). The corrections for the different bands and continuums have been carefully checked and do not double-count the UV absorption in certain bands.

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?

We do not fully understand this argument. The original data have annual resolution and no additional smoothing has been applied. For the SOCOL runs the data have been linearly interpolated to the monthly resolution.

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?

We added in the conclusions section following paragraph:

"We are aware of the fact that by using a strong solar forcing, the temperature, wind and chemical responses might be at the higher edge and might need a comparison to a weaker forcing. However, in the recent work of Anet et al. 2013b, the difference in the ozone response between a weak and a strong forcing of Shapiro et al. 2011 did not seem to be high enough to repeat all experiments with a weaker solar forcing."

What concerns the photolysis and SSI, monthly resolved look-up-tables were used.

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

We added an additional figure of the Ap index, of the solar modulation potential and of the SEPs in the SSI panelplot.

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.

As can be seen in the comments of referee #1 and of Dr. Oliver Bothe, the period to choose seems to be very difficult to decide. We chose to take the 1805-1825 period because we include both major volcanic eruptions (1809, 1815) and - averaged over the whole period - the lowest solar irradiance in the time frame from 1800-1830. A longer time frame would moreover smear out the volcanic footprint.

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

We have considered this change, but chose to keep it to make the process understandable to a wider public.

P15080L1: why cooling not from additional H₂O?

As can be seen in Brasseur & Solomon, 2005, page 207, the cooling contribution from water vapour is very small above 60 km. Hence, the cooling cannot come - at least not in the simulated strength - from additional water vapour.

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

This is a severe flaw in our manuscript. The pictures show JJA values and not DJF, thus austral winter values. We have modified the figure title and captions. "DJF" has been changed to "JJA".

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

We are not able to understand the point made by the referee #1. The analysis has been done (W*) and is shown in the last figure.

P15083L1: I cannot reproduce 1% change in the visible

This is a sloppy formulation from our side. We decided to reformulate following: "This is mainly due to the drop of only 1% in the radiation band 3 from the Shapiro et al. (2011) reconstruction"

P15083 L 9: 250 nm is not VIS.

This is totally true and can be misleading. We reformulated to "A significant cooling of up to 0.6 K is observed in the middle stratosphere when reducing the irradiance of the bands 2 and 3 of the solar spectrum in our model (250nm-690nm)."

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

We have reformulated and shortened the last para the following way: "Looking for analogs in the future, the drop of ozone by up to 7% at ozone layer height due to a reduction of the UV radiation should be kept in mind when considering the possible future grand solar minimum of the 21st century. A similar - or an even greater - decrease in the ozone layer thickness due to ozone depleting

substances and UV radiation reduction then gets a possible health issue on Earth. As well, the effects of reduction of UV, volcanic eruptions and increase of oxidation by GCRs should be thoroughly investigated in future research of the 21st century with an AO-CCM due to halogen and anthropogenic NO_x loading. The evolution of the ozone layer might be of utmost interest to the whole scientific community, as e.g. crop yields or health of living beings could be influenced by the interaction of some specific anthropogenic emitted substances with the stratospheric chemistry."

**P15070 L 28: for for P15079 L20: spacial -> spatial P15082L27: excepted
P15084
L28: treat**

We have corrected those typos.

References:

Laut, P.
Solar activity and terrestrial climate: an analysis of some purported correlations
Journal of Atmospheric and Solar-Terrestrial Physics , **2003**, 65, 801 - 812

Marsh, N. & Svensmark, H.
Cosmic rays, clouds, and climate
Space Science Reviews, Springer, **2000**, 94, 215-230