Clim. Past Discuss., 9, C2918–C2923, 2013
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Climate of the Past Discussions

Interactive comment on "Impact of solar vs. volcanic activity variations on tropospheric temperatures and precipitation during the Dalton Minimum" *by* J. G. Anet et al.

Anonymous Referee #1

Received and published: 11 December 2013

This study follows on from the recent paper published by Anet et al. in ACP (doi:10.5194/acp-13-10951-2013). The authors examine simulations with a coupled atmosphere-ocean-chemistry climate model, SOCOL3-MPIOM, to attempt to separate out the effects of volcanic and solar forcing on surface climate during the Dalton Minimum (1780-1840). They conduct a simulation in which all known external forcings are included in the model, followed by various sensitivity tests in which the different components of the volcanic and solar forcing (low/high UV, energetic particle precipitation (EPP)) are included separately.

The main sources of uncertainty in the study relate to: the assumptions that must be made about the nature of the solar and volcanic forcing during the Dalton Minimum,

and the uncertainties associated with the fidelity of the climate model in simulating the processes relevant to the complex couplings between the solar and volcanic forcing and surface climate.

Using their single forcing experiments, the authors find no significant role for the top-down mechanism in the case of solar forcing, tested by perturbing only solar radiation at wavelengths < 250 nm or EPP, but argue that the bottom-up solar mechanism and volcanic forcing both play an important role in determining anomalous surface climate during the Dalton Minimum (temperature, precipitation and ocean heat content). The authors make some attempts to compare their model results to proxy based temperature reconstructions, and conclude that their results are comparable to the reconstructions analysed. However, these results disagree with an earlier study by Feulner (2011), which showed that a model forced with the Shapiro et al. (2010) solar reconstruction did not correctly capture the evolution of past surface temperatures.

The manuscript is well written and generally clear. I have a couple of specific issues that I would like to be addressed before I would recommend the manuscript for publication in Climate of the Past.

General points

1) P6192 L18 – P6193 L13

I found the discussion around the ability of SOCOL3-MPIOM to simulate the top-down mechanism, which is thought to involve a modulation of the strength of the stratospheric polar vortex, to be unsatisfactory and incomplete. On P6193 they describe how the post-volcanic European warming in their VOLC experiment is consistent with other studies that have highlighted a top-down mechanism following volcanic eruptions (e.g. Driscoll et al., 2012, JGR). However, in the conclusions (P6201 L24-25) they argue that this feature is only weakly significant and that the top-down mechanism doesn't appear to be operating properly in their model, which seems to be somewhat contradictory.

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If the model does simulate a top-down mechanism in response to volcanic aerosol forcing, then why would it not also capture a similar top-down mechanism due to solar forcing? The authors make some inconclusive statements (P6193 L1-2) about the role of the climatologically weak polar vortex in the model, but this issue is not addressed in detail. The reader is left wondering whether the results and their validity may be strongly affected by model deficiencies in being able to simulate the relevant processes to capture the surface response to volcanic and solar forcing. I deem this to be a deficiency of the manuscript it its present state, and recommend that a more detailed examination of this issue be included.

2) P6201 L11-13

The apparent contradiction of the results of Feulner (2011) requires a more complete discussion. Do the contrasting conclusions arise from differences between the model simulations presented in the two studies, or is it related to the fact that the two studies use different temperature reconstructions to evaluate their model results? In particular, how different is the temperature reconstruction of Frank et al. (2010) used by Feulner (2011) from the five reconstructions used in your study? Comparing Feulner (2011) Figs 2 and 3 to your Fig 6, it would appear that the modelled temperature anomalies might not be that different, but that the reconstructed temperature data are quite different.

I recommend that this issue needs to be addressed in more detail in the text. As it stands, the reader is left wondering why the two studies differ so markedly in their overall conclusions, and whether or not the Shapiro et al. (2010) solar forcing is likely to be a plausible reconstruction.

Specific minor points

1) Are the timings of ENSO events different in the various sensitivity experiments? If so, I would expect this to be important for the shorter timescale fluctuations in the temperature timeseries in Figs 4 and 6. Furthermore, the timings of ENSO in the

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model are unlikely to be the same as those which occurred in the real world, so can you factor in the possible effects of this variability into your comparison of the modelled vs. reconstructed temperatures in Fig 6?

P6182 L7 ejecting – replace with 'injecting'

P6183 L1 I would have thought the main issue here is not the size of the perturbation (0.3 W/m2), which could in principle be large enough to impact on surface temperatures, but the fact that most of this energy will be absorbed in the stratosphere.

P6183 L4-14 I think it is important to distinguish between the two types of top-down mechanism that have been proposed – one relates to a modification of the strength of the stratospheric polar vortex (winter-time only), and one relates to the direct impact of the tropical lower stratospheric 'secondary maximum' in temperature on the midlatitude jets (e.g. Haigh et al., 2006; Simpson et al., 2009), which could in principle operate during other seasons.

P6183 L20 This sounds very deterministic – would suggest changing to 'this is thought to lead to'

P6183 L22 Both top-down and bottom up mechanisms are described as having the potential to modify the Hadley cell – how might their relative effects be separated?

P6184 L16 Is there a reference for the 60Mt sulphur? Please add.

P6184 L25 Could this warming of the polar vortex also be due to dynamical modulation rather than purely radiative effects?

P6185 Which solar forcing did Shindell use? This 0.6-0.8 K seems large compared to other estimates for the effects of a grand solar minimum (e.g. Jones et al., 2012, JGR).

P6188 L11 Define 'phi'.

P6188 Yes, but is this likely to be an upper estimate of what the 'real world' influence is?

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P6189 L1 CMIP4 should be CMIP3, I think.

P6189 L25 How long is the control run? Please explain in more detail how the three ensemble members are initialised from the control run and how the differences between the three transient ensembles and the control run are constructed?

P6190 L9 Why is the Student's t-test computed for the ensemble mean differences for the 20 year period rather than the differences across all 3*20=60 data points as was done in Anet et al., 2013, ACP? How is autocorrelation taken into account? It appears that the df = 2N-2, as is the case for independent data samples.

P6190 L20 Is this annual-mean temperature? Please state this.

P6191 L9-10 Presumably you have the diagnostics to check this?

P6191 L28 Remove 'being'

P6192 L1 assure – replace with 'ensure'

P6192 L7 neither – replace with 'either'

P6192 L4 It does not look like Fig 2(b) + Fig 2(c) = Fig 2(a) in many regions, and therefore the solar and volcanic responses are not additive. If this is the case, why can some features, such as the warming over the Bering Sea (P6191 L11-12), be directly attributed to the BU or VOLC forcing by comparing Fig 2(c) to 2(a), whereas the combined (BU + VOLC) changes in other regions, such as the cooling over Northern Europe and Australasia, do not directly correspond to ALL?

P6192 L7 add '(not shown)'

P6192 L20 exchange – replace with 'coupling'

P6193 L1 reword to ', but it may originate in the weaker winter vortex'

P6193 L17 add units to standard deviations.

P6196 L19 amply – replace with 'considerably'

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P6197 L10 that means – replace with 'which shows'

P6197 L19 Do the differences in the timings of an 11 year like signal imply that the dating is not sufficiently accurate on decadal timescales to detect the effects of solar forcing. If so, what are the implications for investigating the detailed evolution of temperature over the Dalton Minimum?

P6201 L16 'famines' – this statement is highly speculative, I suggest removing it.

P6202 L11 for a certain time – this statement is vague, please clarify.

Interactive comment on Clim. Past Discuss., 9, 6179, 2013.