

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 #2

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Synopsis

This study uses a set of climate simulation ensembles performed with a chemistry-climate model to investigate the role of natural forcing factors on the climatic evolution during the early 19th century, a period characterized by a prolonged phase of low solar activity and by a series of strong tropical volcanic eruptions. Among others, the sensitivity experiments include experiments separating the effects of changes in solar radiation within different spectral bands and related to the so-called “bottom-up” and “top-down” mechanisms of solar forcing, as well as experiments accounting for the variable flow of incoming energetic particles. This study pairs with another study recently published by the same Authors in *Atm. Chem. Phys.*. This study focuses on

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tropospheric (and oceanic) dynamics whereas the latter study focused on stratospheric chemistry and dynamics.

By comparing the all-forcing simulation ensemble and the single-forcing sensitivity experiments, the Authors conclude that: (i) the bottom-up mechanism of solar forcing dominated the top-down mechanism during the Dalton Minimum of solar activity, due to negligible impacts from irradiance changes in the UV-C band alone; (ii) there is virtually no tropospheric effect associated to variations in the flow of incoming energetic particles during the Dalton Minimum; (iii) volcanic and solar (bottom-up) effects result in deviating trajectories during the 1820s (warming from volcanoes, cooling from the Sun), so that (iv) only when in combination they produce a simulated climate evolution compatible with available reconstructions. The study further shows how volcanic forcing and solar forcing leave a different imprint on the ocean heat content, and how precipitation changes during the Dalton Minimum detected in the all-forcing ensemble are likely related to volcanic forcing affecting the Hadley cell and the position of the inter-tropical convergence zone.

There are novel aspects in the design of this study that make it an interesting contribution to the ongoing discussions about naturally-forced climate variability before the observational period. In particular, one novel aspect is the use of a chemistry-climate model, which implements a chemistry module allowing for an online calculation of interactions between different gas species (at the cost of more computational requirements). Novel is also the use of the Shapiro's reconstruction of spectral solar irradiance, which describes stronger variability in solar activity compared to previous estimates. An important result of this study is that the Shapiro's reconstruction generates, under all-forcing conditions, 19th century climate variations compatible with reconstructed ones. The manuscript is well written, and results are presented in a clear way and generally support the main conclusions of the paper. I have, nonetheless, some comments and requests for clarification concerning both the design of the study and the interpretation of the results.

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Major/general comments

My major concern regards the very limited size of the ensembles. I understand that this is due to unavoidable computational limitations, and that this does not necessarily hamper the general validity of the conclusions (though some results are based on rather low confidence). However, as also noted by the Authors in their conclusive statement, representativeness is a potential issue. It should be made clear that the initialization allows at least spanning substantially different ocean conditions. In a recent modeling study focusing on the same period, Zanchettin et al. (2013) showed that internal climate variability can strongly spread the simulated decadal climate response to a strong eruption, with individual realizations differing for as much as 1 K in decadal NH temperature outputs during the first two decades after the 1815 Tambora eruption. Their results also showed the relevance of internal variability for simulations-reconstructions comparisons, since individual model realizations resulted to be either incompatible or closely tracing reconstructed trajectories. I therefore strongly encourage the Authors to describe more in detail the ensemble generation method (rather than simply referencing to Anet et al., ACP), and describe succinctly how much the initial states differ concerning key climatic features (for instance ENSO and the Atlantic meridional overturning circulation).

To this regard, it is also important to be sure that the control run provides a reliable characterization of internal variability. The manuscript somehow lacks information about the control run, and I would appreciate more details being reported. For instance, from the caption of Figure 3 I understand that only 60 years were considered for that analysis: if this were the full length of the control run, it would possibly be insufficient due to under-sampling of internal multi-decadal and centennial variability. I would also appreciate if some words were spent about general aspects of the climate variability simulated by SOCOL3-MPIOM, such as the spectral properties of the simulated ENSO (ENSO is known to be too strong and too regular in the similar ECHAM5/MPIOM model in T31L19/GR30 compared to observations). Such information helps putting confidence

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on the results and otherwise clarifies caveats to be accounted for in the conclusions.

The fact that the strongly-varying solar forcing produces temperature variations compatible with available reconstructions seems to be one of the most important conclusions that can be drawn from this study, since it makes the Shapiro's SSI reconstruction plausible. This is even more so since the VOLC ensemble considerably deviates from the reconstructed trajectory (often in opposite direction that the BU ensemble does). Given the importance of the simulations-reconstructions result, I encourage the Authors to expand a bit the discussion especially on its interpretation in the light of indications from previous studies (in particular Feulner, 2011, but also the abovementioned Zanchettin et al., 2013). Certainly the outcome of such a cross-validation depends on the combination of selected forcings and selected reconstructions: for instance, here the compatibility between the all-forcing ensemble and the reconstructions is largely originated by "compensation" between solar (BU) and volcanic forcings. So, how much does the use of Gao's aerosols matter for the overall comparison and related inferences about the validity of the Shapiro's SSI reconstruction?

Concerning the interpretation of the simulated oceanic evolutions, the Authors only briefly discuss changes occurring in the North Atlantic, although this is a key region for the climate response to volcanic and solar forcing in several models (e.g., Stenchikov et al., 2009; Swingedow et al., 2010; Otterå et al., 2012; Zanchettin et al., 2012, 2013). I would appreciate some extended discussion about the dynamical responses in the North Atlantic Ocean or lack thereof, especially concerning the thermohaline circulation. As shown by Zanchettin et al. (2013), the interplay between oceanic heat transport in the North Atlantic and Arctic sea ice strongly determines the dynamical climate response in the continental Northern Hemisphere. So, for instance, a lack of North Atlantic response to volcanic forcing in the SOCOL3/MPIOM simulations may prevent sea-ice related feedbacks to set in in the Arctic. This could also be important for the interpretation of the all-forcing ensemble as simply resulting from "compensation" between BU and VOLC effects.

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As a final general comment on the writing style, I found that Section 4 (Conclusions) contained a lot of material which is actually discussion of the results. I recommend the Authors to split this lengthy section in two parts (Discussion and Conclusions), and to put only the major concluding remarks in the latter.

Minor/specific comments

6182 L14: isn't the last IPCC report the AR5? Maybe it's better to specify you refer to AR4

6183 L13: comparing → compared

6184 L6-7: please check if acronyms CCM was already defined, otherwise define it here

6184 L19: “as A result”

6184 L25-onward: the part “significant cooling [...] leading to modified patterns” is unclear to me: is “lead” to be intended in causal sense? At least in the case of NAO/AO the response is due to the downward propagation of a strengthened stratospheric polar vortex associated to in situ thermal effects of volcanic aerosols.

6185 L10: Feulner . . . their

6185: it could be worth including Zanchettin et al. (2013) in the list of previous modeling works focusing of the early 19th century, especially since they demonstrate how the simulated decadal climate response to the Tambora eruption depends on the background climate state, including the set of considered forcings and the ongoing internal variability.

6186 L18: please include acronym MPIOM at this point. Also, please provide in the following also details for the configuration/resolution of MPIOM.

6188 L3: THE state-of-the-art (?)

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6188 L21/Figure 1d: how does the volcanic forcing compare with estimates from previous modeling studies? Along this line, it would be important to also provide top-of-atmosphere radiative fluxes (solar, thermal, net).

6188 L22: is the same QBO nudged to all experiments? Can you please discuss a bit more the associated caveats?

6189 L13: as a side note, I found intriguing that no simulation considering full-band spectral solar irradiance changes was performed (BU+TD).

6191 L11: warming → warm ?

6191 L15: is there any implication from sea-ice for the warm anomaly west of the Antarctic Peninsula? To this regard, dynamics in the Antarctic region simulated by ECHAM5/MPIOM in T31/GR30 resolution suffer from an unsatisfactory representation of the southern mid-latitude westerlies. Is this the case also for SOCOL3/MPIOM? Can this be discussed a bit more?

6191 L19: it may be worth discussing the proposed mechanism with the general response mechanism described by Wang et al. (2012)

6192 L21: basing → based

6192 L26: please be more accurate, like: “A subsequent surface warm anomaly at high latitudes is the consequence”

6193 L11: “mid-latitude westerlies”

6193 L22: I would avoid the formulation “normal condition”. Is normality the mean unperturbed state or does it refer to some range around the mean?

6194 L7: as well as

6194 L15: temperature increases

6194 L15-onward: as a side note, a similar delayed warming over continental regions

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after strong volcanic eruptions has been detected in the ECHAM5/MPIOM-based “Millennium” simulation ensemble (Zanchettin et al., 2012).

6195 L16-17: “persistence [. . .] is more constant” reads strange, maybe rephrase?

6197 L5: please report whether all these reconstructions are consistently ocean+land (or land only) estimates.

6198 L11: warming → recovery from the cold anomaly?

6198 L26: isn't it more over the Pacific warm pool region?

6199 L5: change in SST: I guess this is cooling, can you explicit it?

6199 L15: is the “both” at the right place?

6199 L18-19: significantly weakened: yes but only in its upper branch, right?

6201 L1-2: is the mentioned cool period referred to reconstructions? If so, I do not see it in Figure 6, where the gray shading stays mostly above the zero line during the 1820s. This seems also to contradict a later statement (6201 L19-23).

Figure 4: the temperature response to the 1809 and Tambora eruptions seems very similar, despite the different aerosol loads. Any comment on this?

Figure 6 caption: shouldn't be Figure S5?

Suggested references

Otterå, O.H., M. Bentsen, H. Drange, L. Suo (2010), External forcing as a metronome for Atlantic multidecadal variability. *Nature Geosc.*, doi:10.1038/NGEO995

Swingedouw, D., Terray, L., Cassou, C., Voldoire, A., Salas-Méla, D., Servonnat, J. (2010) Natural forcing of climate during the last millennium: Fingerprint of solar variability. *Clim. Dyn.*, doi:10.1007/s00382-010-0803-5

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Zanchettin, D., C. Timmreck, H.-F. Graf, A. Rubino, S. Lorenz, K. Lohmann, K. Krueger, and J. H. Jungclaus (2012), Bi-decadal variability excited in the coupled ocean–atmosphere system by strong tropical volcanic eruptions. *Clim. Dyn.*, 39:1-2, 419-444, DOI:10.1007/s00382-011-1167-1

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