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Climate of the Past Discussions

Interactive comment on "Mechanisms for European summer temperature response to solar forcing over the last millennium" *by* D. Swingedouw et al.

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This paper addresses feedback processes responsible for the spatial differences in surface temperature response to solar irradiance forcing over the past millennium over Europe.

Big picture: The approach of using a real-world reconstruction to look for forced climate signals, and comparing the results with model-derived signals is welcome. The authors present good indications for a feedback process that had been suggested before, and properly document its occurrence in the model environment. The role of soil moisture and thus of evaporative cooling come the warm season is important, and it deserves

the attention beyond this particular application. From this perspective I regard the objectives, the general approach and the primary results of the paper as very much worthwhile the publication.

There are a few reservations that are tied to – in my opinion – omissions in the discussion that would have convinced me more about the actual identification of the underlying processes. They fall into two categories:

First: the discussion is quite unilaterally aimed at the direct effect of solar irradiance forcing and mostly ignoring dynamical changes in circulation that can lead to some of the spatial patterns attributed to feedbacks. I would like to point out that the indicators used for identifying the feedback are not restricted to direct radiative forcing but can just as much be initiated by climate response that is due to indirect, more dynamically induced changes over Europe. The discussion about previous winter precip is important in this regard. Figure 3 should be informative. Though the precip increase doesn't say that its due to previous winter, it can also be summer! This needs to be made clearer. I have a hard time thinking of the region of the Mediterranean through Central Europe all the way to Northern Europe as showing the response to one feedback process. The Mediterranean is heavily influenced by subsidence in the Subtropics, the northern sections are under the influence of the westerlies that are very sensitive to advection changes. Both large scale systems have different ties to solar radiation . . . The solar forcing itself is not just radiation that reaches the surface, its certainly more complex. Some more discussion of this fact, and its possible implication on the approach and conclusions is warranted. For example see the Gray et al. (Rev. Geophys., 2010) is a good starting point. ((on the side: the overall referencing is rather minimal. . . maybe some more linking might be possible))

Second: the description of how well the model is reproducing the climate, even if the core signals are somewhat stronger, are not supported (or contrasted!) with a more informed discussion of how climate in Europe, and particularly western Europe, has fluctuated over the past. If one would directly take the conclusion and project the link between solar forcing and climate into general descriptions, then one would claim that warm periods were wetter, while cool periods were drier in Central Europe, with that moisture acting to dampen. Looking at the historical record, and particularly the periods of extended warmth, I'm not sure if such a claim is easily supported by the evidence. Of course the links will never be as simplistic as this claim, but I'd have appreciated if the authors would at least attempt to make this comparison and discuss where it appears to fit, and possibly where not. Even by discussing more about what is seen and interpreted in the Guiot et al. reconstruction would have been helpful to me, not to speak of the many other reconstructions that exist, particularly the documentary records that allow so nicely the comparison of temperature and moisture for some of the famous minima in solar activity. I could come up with examples when cold years in the late 16th century were very wet / and some in the late 17th century that were dry showing the complexity and possibly mix of signals in addition to large regional noise, against which the results need to be measured. In the model this looks awfully "clean" where in the real world things might be a bit more varied.

More details:

page 1302, line 21-23: This last sentence of the abstract is well intended, but somewhat loosely connected. Correlations are low, the magnitude of the signals are different between model and reconstruction, and the reasons for the difference are not well explored. There are different issues at play.

p 1302, l 25, opening of introduction: IPCC is not a good source here because IPCC is a summary of the literature. Therefore cite the real literature, or maybe even more importantly state what is physical reality: a forcing doesn't generate the same signal everywhere because of regionally and locally different feedback processes. And then make the reference to summaries within IPCC, for example.

p. 1303, line 2ff: is the Mediterranean warming only due to feedbacks or is increased subsidence (with a northward shift of the storm track . . .) the actual signal? The reader

is led to believe its all local feedbacks. I don't think this is true. Check and better reference this claim that no fundamental circulation change is a work here. I think the authors will quickly realize that since a couple IPCC reports this region has been one of the few significant change areas identified. . . and it has to do with circulation, not only local feedbacks.

p. 1303, l. 12: parameterization (spelling)

p. 1303, l. 26: the reason for better detection in winter is that dynamics are involved rather than pure local radiative forcing

p. 1304, l. 9-10: I would like to hear more than model reproduces Northern Hemisphere temperature. I'd like to hear, without going to the paper, that the multidecadal variations that are often associated with forcings are reproduced, how they fall within the range of reconstructions (at the hemispheric level), etc.

p. 1304, l. 25: in a comparison where particular episodes are important, I'd like to hear that solar minima are independent of co-existing volcanic activity. Similarly, high solar activity should be compared with volcanic activity. An overall correlation is maybe quite misleading when we are mostly interested in specific periods . . . timing (see various papers in literature: Hegerl et al, Ammann et al.,).

p. 1305, l. 8ff: Looking at Figure 1, I'm not sure that the fit of the red-line and the black line (not solar) is actually that good. The flat period in black up to about 1420 is not well represented, the coolest episode around 1600 is not captured, the sustained rel. warm interval 1680-1890 is not picked but sees more of a trend . . .The model has certainly a much better correlation with the solar forcing than the real world. Its worth pointing this out.

p. 1305, l. 13: "respected" is not a good term.

p. 1305, l. 14: see above. . . I'm not very impressed by the correlations. Why is it good that the early part has a higher correlation? Aren't these simply artifacts from low frequency variations with very few degrees of freedom?

Figure 3a and p. 1305, l. 20ff: the color scheme is not well chosen. Use just the range that is present in the figure. Right now I would jump to the probably wrong conclusion that the model is extremely low resolution and thus the signal described might simply be due to the coarseness of the grid... which I'm sure its not. Figure 3a can be done much better to show what is actually happening in terms of radiative forcing.

p. 1306, l. 7-8: given that radiative forcing is even, there is still no a priori expectation that the response is as well! Local radiative response and dynamics are different things and they express themselves in different ways spatially.

p. 1306, l. 11: is the cloud increase related to the local increase in solar radiation, or could it be a dynamical response that is large scale? I'd like to see something about the large scale circulation indices ... see next comment:

p. 1307, l. 18: how was the "weather regime" issue tested? I'm not talking about individual events, but circulation indices. And here is the issue raised above that links the discussion to the historical record. Was it really wetter during warm periods in central Europe? Because only then large scale warming from the sun could be buffered by increased moisture... So are we just looking at Clausius Clapeyron? Is the signal in the model (and reality) much stronger of a negative feedback early in the season than late season, which one would expect given late winter soil moisture?

p. 1308, l. 1: how far away is the sea ice from Scandinavia and the other areas of largest warming? What indications exist that this played a role in real world?

p. 1308, l. 12: reconstructions can be useful to evaluate low frequency variability... well, given Figure 1, I'm not so sure that this has been established. I certainly agree with the statement, but the authors could do a better job to show this.

p. 1308, l. 16: what real world data exists for summer at 75 N?

p. 1309, l. 6: could it be that the signal over Europe is very mixed in the real world

because the actual causality is weak and we are more looking at a combined volcanic-solar effect?

p. 1309, l. 13-16: yes, I agree with this statement.

p 1310, l. 12: Mennendez typo

p. 1311, l. 1: check last author of Nature Geosci reference. It's not Lingling, it should probably be: "Suo, L."

p. 1311, l. 12, remove "vol"

Interactive comment on Clim. Past Discuss., 8, 1301, 2012.