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Earth System Dynamics Discussions

Interactive comment on "Spatial Signature of Solar Forcing over the North Atlantic Summer Climate in the Past Millennium" *by* Maria Pyrina et al.

Anonymous Referee #2

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The authors analyze solar signals in simulations of the last preindustrial millennium with two different Earth system models. Their focus is on spatial patterns of surface climate responses during summer in the North Atlantic region. Three different analysis techniques are used, regression and correlation of interannual variability, comparison of the extended periods of the Medieval Climate Anomaly (MCA) and the Little Ice Age (LIA), and comparison of composites built from years for relatively high and low solar activity. The main conclusion, in my eyes, is that in the model simulations one can identify very little robust signals. Analyzed signals in, e.g., sea surface temperatures (SST) differ between the models, between different ensemble members of simulations with the same model, and between different analysis techniques. I don't think this result is surprising as the applied solar forcing is small, and there has, so far, no mechanism

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Discussion paper

been suggested for any amplification of solar signals for this space and time as it has been for example for the wintertime northern hemisphere where stratospheric signals may propagate downward through wave coupling. The question if solar signals can be simulated in NH summer can be of interest even if the answer in this case is rather a No, because it might help to understand signals in proxies that may be largely influenced by summertime conditions. I find it even useful to show that apparent signals in 1000-year long simulations may be spurious. However, I think the quality of the presentation and interpretation of the analysis is not sufficient to warrant publication, and I would suggest to reject the publication of the current manuscript. In the following I will provide reasons for this, mainly by discussing the conclusions provided by the authors, followed by a few more general comments. I won't provide a detailed line by line analysis at this stage of the review as I think a considerably reworked resubmission is necessary anyhow.

Discussion of conclusions: "Linear regression is not a robust method for the isolation of the climatic regional effects of solar forcing. This conclusion was demonstrated by the comparison of a fully forced simulation and a solar-only forced simulation conducted with the MPI-ESM model." As much of the manuscript this statement is not only problematic concerning its content but also concerning the often very approximate wording. I guess what the authors mean is not that the method is not robust but that it provides no robust results. Even then the statement is untrue. I don't dispute that the linear assumption can be very problematic. However, many solar signals have been robustly identified using regression techniques. Here, the authors very likely refer to the identification for the specific season and region they have studied. It's true that if at all very few patterns can be identified robustly in the two MPI-ESM simulations. If this is considered the first main conclusion of the paper, I'm wondering why the authors hide one of the figures (S3) supporting this statement in the supplementary material and use a different color scale than for the corresponding Fig. 2. Furthermore, from Figs. 2-4 it seems to me that the issue is not different for the individual ensemble members of the CESM and e.g. for the analysis of composites. If one of the conclusions should be that composite analysis is more appropriate than correlation analysis, I think it is necessary to substantiate this qualitatively.

“The SST response found by the method MCA-LIA cannot be attributed to the difference in TSI amplitude between those periods, as these periods do not fall into predominantly high/low phases of solar activity.” I'm not sure what the authors mean when they speak of the “SST response found by the method MCA-LIA”. Looking at Fig. 2, it again seems to me that there is not much of a robust signal, not different to correlation and regression analysis. If the goal is to identify solar signals then why chose periods from which one knows that they are not appropriate. Furthermore, this statement exemplifies again the careless usage of language. The authors very often speak of responses or signals when it is not at all clear if the analyzed anomalies indeed represent responses or signals.

“Robust conclusions about regional warming or cooling of the NA SSTs due to changes in TSI forcing cannot be drawn using neither a single model realization nor one ESM.” What would the authors like to identify robustly? The solar signal in the real world? I would argue that this can't be done with model simulations at all. Even if models showed robust responses, they might all suffer from the same mistakes, e.g. in the imposed forcing. I guess what the authors want to say is that the simulations analyzed here show virtually no robust responses. Analyzed anomalies differ between models and even between different ensemble members simulated with the same model.

“Diabatic heating links the TSI surface response to the atmospheric circulation response and induces wave-like pressure anomalies. Such atmospheric conditioning in high TSI periods might favour blocking-like patterns over middle and high latitudes in summer.” Here it seems like the authors argue they have identified a robust signal: a tendency for blocking at higher solar irradiance. It would be nice to quantify this so that one doesn't have to formulate as carefully as the authors do (“might favour”). I have to admit that I'm not a blocking expert. It may be difficult to analyze blocking from model output which I guess is only available at coarse temporal resolution. But from visual analysis of the figures alone I'm not sure about the blocking statement. Indeed, the SLP anomaly from the MPI-ESM composite analysis suggests more zonal inhomogeneity for higher TSI. But I'm less certain about the other simulations and hence robustness. Furthermore, I have difficulties to follow the reasoning concerning the diabatic heating and subsequent circulation changes. It is of course true that thermodynamic changes would entail changes in dynamics. But why identify diabatic heating from turbulent energy fluxes alone? What about radiative fluxes? And how does this analysis help me to identify if anomaly patterns may be of solar origin or due to internal variability?

A few more general comments: As mentioned with respect to the conclusion, I think the use of language is in most parts of the paper inappropriate for a scientific publication. There are almost no typos or errors of grammar, but formulations are often inexact. However, it is not only language, but also the descriptions of the simulations (e.g. how varies the SSI in the CESM simulations) and the figure captions (e.g. unit of column 2 of Figs. 2-4) are partly inexact. Given the experience of several of the co-authors this issue should be solvable with some more effort. This should not be a task for the reviewers.

One issue I have with the analysis of NA summer is that I don't know how the model performs for other parameters for which more information on solar signals exists. E.g., CMIP5 models have been shown to relatively robustly simulate global mean near surface temperature responses to 11-year solar variability, the maximum occurring about 2 years after solar cycle maximum. Do the models analyzed here show a similar behavior? If this were not the case, why should one bother to analyze NA summer. Similarly it would be interesting to learn about the models' responses for example in the tropics or NH winter. And there should be some information on the simulation of the NH summer climatological state. All this could go into the supplementary material.

Furthermore, it is necessary to formulate hypotheses about the expected responses analyzed with the three different methods. These responses might be very different, e.g. due to the different time scales involved. As alluded to above, I don't think that an analysis of interannual variability without testing time lags is sufficient. For the composite analysis it would be important to discuss the temporal distribution of the years contributing to the composites. Are they more or less alternating with the 11-year cycle so that one could interpret differences to the regression technique as method-related, or do they often sample longer periods with lower or higher solar irradiance?

As said initially I do think that the analysis attempted in this paper can be of value, so I would like to encourage the authors to invest more and resubmit the paper.

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