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Interactive comment on "A timescale analysis of the NH temperature response tovolcanic and solar forcing in the past millenium" *by* S. L. Weber

Anonymous Referee #1

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The manuscript presents an analysis of several records of the Northern Hemisphere mean temperature, taken from empirical reconstructions and from a number of simulations with the climate model of intermediate complexity EC-Bilt. The main objective of the manuscript is to relate variations of f solar and volcanic forcing in the past millennium with simulated and reconstructed Northern Hemisphere temperatures at different time scales. This analysis should therefore yield an estimation of the sensitivity of the Northern Hemisphere temperature to external radiative forcing. The main conclusions are that: the sensitivities derived from the reconstructions and from the simulations are consistent with one another; that the sensitivity to volcanic forcing is smaller than for solar forcing, due to the short-term character of volcanic eruptions; and that the millennial trend in the solar forcing and the trend in the reconstructions are not quite compatible with the sensitivities derived at shorter timescales.

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There seem to exist different opinions in the community about the feasibility of estimating the climate sensitivity from paleo-records of the last millennium. The author of this manuscript certainly believes that this could be possible. The approach of combining the analysis of reconstructions with climate models simulation, i.e. every tool that is available, is in my opinion valuable. Unfortunately, many uncertainties in the estimations of past radiative forcing and past temperatures remain, but in my opinion this manuscript represents an interesting contribution to this difficult goal.

I see, however, several shortcomings, as I try explain in my comments below. The author may want to consider them as suggestions to improve the manuscript.

1) The analysis presented in this manuscript is quite similar to that found in Waple et al (2002), which somewhat surprisingly, is not cited. Perhaps the main difference is that here longer simulations (1000 years) and other and more recent reconstructions are used. However, Waple et al. additionally present spatially resolved sensitivities and identify areas that contribute more strongly to the global, or NH, sensitivity. In my opinion, the methodologies and the goals of both studies are quite similar and this manuscript should have incorporated, at least in the discussion, the findings of Waple et al.

2) The use of longer records allows this manuscript to reach some interesting conclusions, for instance ,the asymptotic behavior of the sensitivity with timescale, reaching for the model simulations a value that should theoretically coincide with the equilibrium sensitivity. Unfortunately, this behavior is missing in the reconstructions, so that the objective of estimating an equilibrium sensitivity for the real climate cannot be reached. I think this problem could be related to the mismatch between the estimated sensitivity at multidecadal timescales and that simply derived from the millennial trends in the reconstructions and the forcing. I think this mismatch leaks into the regression analysis at shorter timescales, and it could be interesting to see if a similar regression analysis with linearly detrended records yields a behavior that is more similar to the simulations.

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3) In any case, the problem of this mismatch remains. Weber interprets this result as an indication that empirical reconstructions do not underestimate the long-term variability (Storch et al, 2004). I think, however, that this interpretation is not quite supported in this manuscript. This underestimation of the low-frequency variability, as found by Storch et al, also occurs at multidecadal and centennial timescales, and not only at millennial timescales. A more parsimonious interpretation would be, in my opinion, that the regression analysis either is inconsistent at the millennial timescales or the errors in the estimation as too large to allow for a meaningful inference (see point 9 below).

I have a number of particular points that, I think, still need some elaboration. Some of them are just a matter of presentation, but others would require a more careful consideration.

4) It should be specified that volcanic eruptions are parametrized, in the simulation, by a reduction of the solar irradiance.

5) what is "solar intensity" in Figure 1? Is it the net radiative forcing at the top of the atmosphere? It is not clear to me if the effect of the North Hemisphere albedo is included in this figure or not. From figure 1, I cannot confirm that the changes in solar irradiance between the Late Maunder Minimum and the mean value (?) or between the Late Maunder Minimum and present values (?) is 0.2 % of 1366K, as stated in the text. From this figure, I can just see that the difference between LMM and today is about 0.4 W/m2. Multiplying by 4 to get the solar irradiance and dividing by 1366 W/m2, I get a change of 0.12%. Considering the differences between LMM and the long-term mean yields even a smaller change. Perhaps the curve depicted in Fig1a should be divided by the co-albedo and one would get a better agreement, but even in this case, the value of 0.2% does not come out. I think this point should be really clarified, since it directly affects the estimation of the regressions.

6)The actual regression equation that is being fitted is not stated explicitly in the manuscript. I think, this should be done to be completely clear about what is being computed. For instance, the estimated regression coefficients apparently do not take the heat-flux into the ocean into account, so that the energy balance equation is not complete. This missing factor probably influences the behavior of the regression parameter at different timescales (White, 1998), as indicated in the manuscript. Although, I think that a regression equation without the ocean heat-flux is perhaps correct in this context, this point should be explicitly stated.

7)The behavior of the regression coefficients will be also affected by feedbacks of the climate system, which may operate at different timescales. In this respect, the model EC-Bilt lacks two very important feedbacks (cloud cover and atmospheric humidity (?)). It has been shown that the humidity feedback is critical to correctly simulate the effect of volcanic eruptions (Soden et al. 2002), and cloud feedback is also very relevant for the overall sensitivity of a climate model (Stephens, 2005). The question then arises about which is the mechanism that more strongly affects the timescale dependency of the regression, ocean heat flux or other feedbacks. Humidity and cloud cover feedbacks probably operate at short timescales but, could the lack of these or other feedbacks be a reason for the different behavior of the regressions coefficients in the model and in the reconstructions?

8) The regression coefficients have been probably been estimated by ordinary least-squares, which is known to underestimate the true regression when the predictor (in this case, solar or volcanic forcing) contains noise. In the analysis of the simulations there is none, as correctly explained in the manuscript, but for the reconstructions this is not true any more. The bias in the estimation probably depends on the amount of noise in the reconstructed of solar or volcanic forcing at the different timescales. There are other estimation methods that in theory are unbiased, e.g. total least-squares, but it requires a knowledge of the ratio of the error variances predictand/predictor. This point is also discussed with other terminology in Waple et al.

9)An important aspect of the analysis which is missing is the estimation of the uncertainty ranges in the regression. This can be particularly relevant for the longer timescales, where the number of independent samples is small. For instance, a quick calculation for the case of the the Mann98 reconstructions at 100-year timescale, and for ordinary least-square estimation, yields a 90% error range of about +-0.15 K/wm2, which may potentially affect the conclusion that the regressions increase with timescale. A related aspect is whether solar and volcanic forcing are correlated. Colinearity between both influences the estimated regressions and uncertainties. Although there is no apparent physical reason for this, it does happen that periods of increased volcanic forcing coincide with minima of solar activity, as the response to individual eruptions can be identified in the simulated record. This is not possible in the simulations and therefore the estimations in this case may be additionally biased.

10) Another point that the manuscript does not discuss is the consequences of the numerical values of the regression coefficients, a point that is sometimes overseen. Taking into account an estimated forcing for 2xCO2 of 3.71 w/m2 (Myhre et al., 1998), a sensitivity of 0.3K/wm2 amounts to an equilibrium temperature change of 1.59 K (or 1.11 K if the effect of albedo was not included in the estimation of the regression). This is a small number compared to the equilibrium sensitivities of most models. A possible explanation could be that the variations in solar forcing have been overestimated. However, the solar forcing used in this manuscript is already in the very lowest range of solar reconstructions, which spans between 0.2% and 0.5% of LMM-to-present change (IPCC,2001). To be fair, there exists indeed studies that point to a smaller changes of solar irradiance (Lean et al., 2002), although they have been partially contested (Solanki and Krivova, 2004). A second explanation could be, of course, that the sensitivities to solar and greenhouse forcings are different. Still another explanation could be that the regressions estimated from the reconstructions are biased low (e.g. through ordinary least-squares) and that the model has too low a sensitivity (lack of cloud feedback), so that the true regressions should be higher. In any case, I think it could be of some interest to discuss the implications of the relatively low values of the regressions obtained in this manuscript. The values derived from the reconstructions seem to be at

variance with model sensitives, unless one accepts past variations of solar irradiance that are much smaller than currently assumed.

11) The analysis of the model data is based on Northern Hemisphere summer temperature, justified in the manuscript by the fact that most of the proxy indicators are sensitive to summer temperatures. However, even if this were true (which I think it is not, at least for some of the reconstructions) , some reconstructions used here have been calibrated to annual mean temperatures, so that their variations has been inflated to take this caveat into account. I am not convinced that the use of summer temperatures is correct. It could be useful to present some of the analysis also for annual mean temperatures and see how large the differences are.

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