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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 #2

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GENERAL COMMENTS

The study analyses simulated NH temperature anomalies induced by short-term volcanic forcing and by relatively long-term solar forcing obtained with ECBilt climate model in comparison to various reconstructed NH temperature anomalies over the time interval 1000-1850 AD. The study discusses relevant scientific aspects, such as climate sensitivity to external radiative forcing and the relation between the timescales of the forcing and the response with focus on the long-term climate change. Since some discrepancies between simulated and reconstructed temperature changes still exist, this analysis can contribute to improve our understanding of the origin of the climate changes over the preindustrial period of the last millennium. The main conclusion presented in the study appears to be that the long-term downward temperature trend determined from solar-volcanic forced simulations is smaller than the trend determined

from reconstructed temperature series and thereby giving support to the reliability of the temperature reconstructions in that time interval. This result is highly important for increasing our confidence in both modelling studies and paleoclimate data analysis, however, the presentation is sometimes difficult to follow and statements should in some cases be more clear and less speculative.

For instance, the term regression, which is related to climate sensitivity, is used in a sloppy manner. A comparison with the climate sensitivity (namely equilibrium, effective or transient climate sensitivity) as determined in other modelling studies is missing. The aspect of seasonal temperature changes is touched in some places but the discussion is blurry (see specific comments). Findings which are not in line with the main conclusion, i.e., a negative correlation between solar forcing and two reconstructed temperature series and an exceptional large negative temperature trend in one reconstructed temperature series over the preindustrial period, are mentioned openly but not discussed in sufficient detail.

In conclusion, I suggest a revision of the analysis to provide a more comprehensible and convincing analysis of a scientifically relevant climate change study.

SPECIFIC COMMENTS

1) The title should indicate that the study analyses the temperature response from the time interval 1000 to 1850 rather than over the past millennium.

2) The study of Mann et al. (1998) analyses data over the last six centuries not from the past millennium (page 138, line 21).

3) It should be said clearly that the analysis of Hegerl et al. (2003) is on the signal detection in temperature series in response to volcanic, solar and greenhouse gas forcing. The success of signal detection depends largely on the amplitude of the forcing (signal-to-noise problem). Since the radiative forcing amplitude from volcanic forcing is much larger than from solar forcing the volcanic signal was more easily detected than

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the solar signal even if the timescale of the volcanic forcing is relatively short.

4) Is it correct, that the present study considers the evaluation of the quality of the temperature reconstructions with the help of climate simulations under the assumption that the reconstructed forcings are of reliable quality? The last but one sentence in the Introduction is difficult to read (p 139, l 20). If the message of the sentence is different some comments might be obsolete.

5) The description of the solar forcing is not clear (p 140, l 9). It should be specified which of the four different solar forcing factors introduced in Crowley (2000) is used in the present analysis. From comparing Fig. 1, upper panel, green/grey line with Fig. 2c in Crowley (2000) it appears that the factor B10/Lean splice (blue line) is chosen in the present study, but with a different scaling. Conventionally the "net radiative forcing" is used for estimating climate sensitivity, but in the present study the radiative forcing is said to be defined as the anomaly of the Total Solar Irradiance (TSI) divided by four. This could explain the different scaling seen in Fig. 1. However, it remains unclear why the present study does not use the "net radiative forcing" which can be described as the absorbed short wave radiation at the "top of the atmosphere" and which accounts for the planetary albedo of the system. This comment is also relevant for the interpretation of the regression coefficients obtained here from radiative forcing and temperature response in comparison to climate sensitivities obtained from other climate change studies (e.g., Cubasch et al., 1997, Cubasch et al., 2001).

6) It is not possible to recognize the 0.2 % decrease in TSI during the Maunder Minimum at ca. 1690 AD (p 140, l 10). Using the author's definition of radiative forcing a 0.2 % decrease with respect to the mean TSI results in 0.2 % x 1366 Wm-2 / 4 = 0.68 Wm-2 which cannot be seen from Figure 1, upper panel.

7) The usage of simulated temperature data from JJA, north of 20N appears justified for the comparison with reconstructed temperature data representing the warm season, as in the case of temperatures derived from tree ring data (p 140, l 20). Presumably, even

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more appropriate would be to take simulated temperature data from land areas only. But for the comparison of reconstructed temperatures representing annual means also annual mean temperature data should be used. In this context it would be interesting to see how large the difference is between simulated annual NH temperatures and JJA temperatures north of 20N.

8) The solar forcing based on Crowley (2000) has a annual resolution which is evident from the grey line in Fig.1, upper panel in which the Schwabe cycle is resolved. Could it be a typing error that the reconstruction shown in Fig. 1 does not resolve annual-decadal timescales (p 140, l 25)?

9) It should be mentioned in the caption of Fig. 1 that the y-axis in the middle panel is cut and volcanic events up to -11.8 Wm-2 are suppressed. Is there any cut of the y-axis in the bottom panel of Fig. 1?

10) Some further explanations are needed in support of the statement that reconstructed temperatures show a much weaker response to volcanic eruptions (p 141, l 15). Which reconstructed temperatures are meant? Fig. 1 shows no reconstructed temperatures while elsewhere in the text seven different temperature reconstructions are mentioned?

11) As said in item 8) above, the raw data of the solar forcing are annually resolved. Since the solar forcing timescale is relatively large with respect the response timescale of the atmosphere and the upper ocean, the solar forcing and the temperature response are expected to be correlated on decadal timescales. It is not clear why the correlations between solar forcing and temperature response are not shown in Fig. 2 for filter period below 40 years (p 141, l 27).

12) Is it correct that the regression of the temperature from the solar-volcanic forced simulation is done only against the solar forcing and the volcanic forcing but not against the solar-volcanic forcing? If so, would it be informative to perform also the latter regression? Then it could be possible to test the linear superposition of the long-term

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trends caused by volcanic forcing and solar forcing. Or is this proposal unsuitable?

13) The effect of seasonal temperature changes due to orbital forcing is mentioned in the Discussion to be considerably large in May and September but zero in July (p 147, l 22). In the regression analysis before (p 143, l 3) differences in regressions for summer and annual-mean data were mentioned? Are the latter differences obtained only for the volcanic forced simulations?

TECHNICAL CORRECTIONS

Table 1 contains in the bottom row and the column "Ma03" an unrealistic value.

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