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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

S. L. Weber

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This referee gives a number of useful suggestions for improving the manuscript. The analysis was extended (points 2 and 11 below). Most of the suggestions for clarification and further discussion were followed in the revised paper.

Main points
1) findings of Waple et al. are incorporated in the revised paper.
2) The question why the multi-centennial trends in the reconstructions are not compatible with the sensitivities derived at shorter timescales is intriguing. The suggestion of the reviewer to examine this is worthwhile. The regression analysis was carried out with linearly detrended temperature and solar forcing records. For the model, this does not make any difference, as trends are very small anyway. For the

reconstructions, detrending the records results in smaller values of the regression but a similar timescale dependence. This is briefly discussed in the revised text. Another way of approaching this is to examine the regression for band-pass filtered records. This was already done in the paper (slightly extended in the revised version).

3) It could very well be that the regression analysis is inconsistent at millenial (multi-centennial) timescales due to the absence of feedbacks in the climate model, which do play a role in the real world. Another possibility is that these timescales are simply not well resolved due to the limited record length. Both interpretations are mentioned in the discussion section. Taking these caveats into account, the present analysis indicates that the long-term trend in the data is overestimated wrt shorter timescales. In contrast to this, Storch et al. find that the low-frequency variability is underestimated. They state that the timescale separation occurs already at multi-decadal timescales, whereas in the present study it lies at centennial timescales. This difference in separation timescale is probably not essential, as the separation point may be sensitive to the experimental design and spectral characteristics of the forcing used in their model runs.

Particular points

4) correct

5) the description of the solar forcing has been modified. Fig. 1 displays the anomalies in TSI divided by four, without taking the effect of NH albedo into account. The Crowley (2000) reconstruction has a 0.2% decrease in TSI from the modern value to the Maunder Minimum. This number is given to put the Crowley reconstruction in the context of the comparison made by Bard et al. (2000) of different reconstructions in terms of the % decrease in TSI during the MM. This is stated clearly in the revised text. Figure 1 only displays the time interval 1000-1850 AD, so this decrease is not visible by definition (there is a pronounced trend in TSI over the last 150 year).

6) the definition is given in section 2 of the revised paper. It does not contain any es-

timate of the heat flux into the ocean. Obviously, the heat flux into the ocean reduces the regression coefficient at the shorter timescales (as indicated in section 4). At interdecadal and longer timescales, this factor does not play a role anymore.

7) The lack of cloud and moisture radiative feedbacks affects the climatic sensitivity of ECBilt, making it lower than in more comprehensive AGCMs which do include those feedbacks. Also, spatial response patterns will depend to a large extend on the inclusion of cloud dynamics, moisture and stratospheric chemistry and dynamics. However, it does not seem likely that these atmospheric feedbacks operate differently at different timescales. Hence it is assumed that this feature of ECBilt does not affect the timescale dependence of the regression of the hemispheric temperature. Clearly, this feature makes ECBilt less suitable to study spatial response patterns. The ocean heat flux, which is incorporated in the model, affects the regression for decadal timescales. The response seems to have equilibrated at interdecadal timescales, consistent with findings by Waple et al. (2002). The lack of relevant feedbacks in the climate model (related to the ocean, land surface, or sea-ice) may be a reason for the different behavior of the regression in the data and the model, as stated in the discussion section.

8) The regression has been computed by ordinary least-squares. I think that the estimate of the predictand/predictor error variances is a paper in itself.

9) It is stated very clearly in the ms. that the regression for the longest temporal scales is uncertain, due to the limited record length. There is a brief discussion on the statistical significance of correlations. The cross-correlation between the solar and volcanic forcing is smaller than 0.1 for all timescales. A multiple regression on the two forcings thus yields results that are very close to the single regressions for each forcing separately.

10) The simulated equilibrium regression of 0.3 C/Wm2 (thus, climatic sensitivity of 0.43) indeed implies a temperature change of 0.43*3.71=1.6C for a doubling of CO2. The value found by performing the 2 timesCO2 equilibrium run is 1.65C. The reason for this low sensitivity found in ECBilt are explained under 7) and in the revised text. I think that this low sensitivity is the reason for the small response found in the solar-forced

runs, not the small amplitude of the solar forcing used.
11) the sensitivity of the present results to seasonality is discussed in the revised text.

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