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Climate of the Past Discussions

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Interactive Comment

Interactive comment on "Influence of solar variability, CO_2 and orbital forcing during the last millennium in the IPSLCM4 model" *by* J. Servonnat et al.

J. Servonnat et al.

jerome.servonnat@lsce.ipsl.fr

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We thank both reviewers for their helpful corrections and constructive comments. Their original specific comments are preceded by "C# =>", and the responses are preceded by "R# =>". The responses to the technical comments are preceded by "=>" and follow directly the comments.

Anonymous Referee #2

This paper describes a simulation covering the last millennium using the atmosphere-ocean coupled model IPSLCM4. The simulation with changing total solar irradiance (TSI), orbital forcing, and greenhouse gases is accompanied by a control experiment

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of similar length. The authors provide a statistical decomposition to attribute temperature changes to the individual forcings. The multivariate decomposition of Northern Hemisphere temperature does not provide any surprising results and the comparison with reconstruction data must be said to be not very revealing because one important external forcing, i.e. disturbances by volcanic aerosols, are not included. However, the authors include a spatial decomposition and provide estimates for the signal-to-noise-ration (SNR) for spatial scales from the sphere to the grid-point area. Since the relative role of internal variability and external forcing in judging observed temperature variations is still on of the most important issues in studies of past climate, this refinement is highly welcome and it shows how models can be used to evaluate proxy-based reconstructions and to guide the further development of proxy-networks. In particular the authors point out that the patterns shown by the variance explained can be very different for different forcing, seasons, and – most importantly – regions. An interesting example is the role of orbital forcing (that is often assumed to be of minor importance for last millennium's temperature evolution) for the high northern latitude summer temperatures. Overall, the manuscript is well written and concise. For the abstract, I would recommend to somewhat de-emphasize the nice agreement of the simulations with the range of reconstructions but focus on the really new findings for the local sensitivity and the local SNR estimates. The authors might also consider reformulating the title which could be a bit more catchy if it reflects the new (regional) aspects of this paper. I therefore recommend publication in Climate of the Past after minor revisions.

Specific comments:

C1 => P. 424, line 3: To my knowledge, Lamb (1964) did not use the term MCA but "Medieval Warm Epoch" Line 7: maybe include reference Trouet et al. (Science, 2009) here.

R1 => This is right. The problem with the notion of the Medieval Climate Anomaly is that there is no clear paternity for this expression. Recent papers as (Trouet et al., 2009) or (Mann et al., 2009) use the expression MCA but they do not refer to any previous study. There have been previous studies like (Graham et al., 2007) that use the term MCA, without making reference to any previous article. We have changed the sentence "the so-called Medieval Climate Anomaly (MCA hereafter, (Lamb, 1965))" by "the so-called Medieval Climate Anomaly (MCA hereafter, introduced by (Lamb, 1965) as the Medieval Warm Epoch)".

C2 => P. 427, line 10, and p. 429, line 24: if there is a trend of -0.11 degr/per 100 yr in the control run over the first century, it is very likely that this is also present in the forced experiment (where radiative forcing is quite weak). So there is not really a point to discuss a 1075 AD trough in temperature here.

R2 => There is indeed a trend in CTRL during the beginning of the simulation that is described in Section 2 (p 427, lines 5-13). On the whole 1000-1850 period, this trend has been estimated with a cubic spline function and has been removed from SGI (p 428 lines 2-4) for all the analyses presented in our article. It has also been removed from CTRL for the analyses. This latter point was not clearly expressed in our manuscript, as suggested by Referee #1. We have thus changed the last sentence of Section 2 for "Finally, we estimated the very low frequency trend in the CTRL with a cubic spline function (Green and Silverman, 1994) and removed it from SGI and CTRL for the following analyses.". Since this trend has been removed from SGI, it should not dramatically affect the NH temperature evolution in SGI decomposed in Section 3.2. The aim of the NH temperature decomposition presented in Section 3.2 was to quantify the contribution of the forced variability. We proposed a linear temperature decomposition (Eq. (1)) stating that the temperature variability in SGI is the sum of the linear contribution of the forcings, or signatures, and a residual noise. As shown in Fig 2.c, this temperature decomposition demonstrates good skills at reproducing the NH temperature variability in SGI. During the first part of the millennium (1000-1425 AD), the solar forcing signature on NH temperature in SGI explains ∼80% of the temperature variance. The correlation coefficient between TSI and the NH temperature in SGI is 0.86 during the first part of the millennium. The NH temperature variability during this period is thus largely explained by the solar forcing. Our temperature decomposition allows us saying that the trough around 1075 AD is mainly associated with the synchronous trough in TSI (Oort Minimum).

C3 => P. 430: Rather than showing how excellent the agreement is between the simulation and the reconstructions it would be helpful to discuss why there is agreement in spite of the fact that the volcanic forcing is missing. Amman et al. (2007), Hegerl et al. (2007), and Crowley "Volcanism and the Little Ice Age, PAGES Newsletter, 2008) have pointed to the important role of cumulative occurrence of volcanic eruptions. Since one of the authors (Swingedouw) is also first author of a paper describing the effect of external forcing in the Toulouse model (similar model set-up, different atmosphere model) where volcanic forcing was applied, the authors should at least discuss this issue a bit more thoroughly.

R3 => We point out that we nowhere claim that there is an "excellent agreement between the simulation and the reconstructions". The second sentence of Section 5 says that "the SGI simulation reproduces well the temperature evolution during the last millennium, especially around the LIA". The first sentence of the fourth paragraph states that "We found an important difference between the reconstructions and SGI between 1000 and 1200 AD [. . .]". This comment invited us to revise the structure of Section 5 to make our point clearer. We changed the second sentence of Section for "the SGI simulation reproduces well the temperature evolution during the last millennium around the LIA". Apart from this correction, the initial content of Section 5 did not change. Following this comment, we have added some discussion elements on the volcanic forcing. We added the simulation presented in ((Swingedouw et al., in press), SW2010 hereafter) in the set of numerical simulations used for the comparison with SGI in Section 3. Fig. 1 and its caption were modified accordingly, as well as the paragraph of Section 3.1 (p 428 line 12-17). In Section 5 we included a paragraph to discuss the relative roles of solar and volcanic forcing on secular NH temperature variability: "The IPCC AR4 and SW2010 simulations taking into account the impact of the volcanoes show

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secular NH temperature evolution very similar to SGI. Volcanic forcing seems to have a weak impact on secular NH temperature variability compared to the solar forcing used in SGI and IPCC AR4 simulations shown, i.e. with a TSI mean value during the Maunder Minimum 0.25% lower than modern value. With lower amplitude of TSI variability, the influence of cumulative eruptions on secular temperature variability should be different (Ammann et al., 2007;Hegerl et al., 2007;Gao et al., 2009)".

C4 => P. 435, line 9: why use "root mean square" ?

R4 => We apologize for this mistake. We initially wanted to apply the weight on the temperature time series, which was not correct. The SNR presented on Figure 5 was calculated with the good formula, without the root-mean-square on the weights ai. Eq. 4 has been corrected.

Minor points

Fig. 1 a: Is the radiative forcing (left axis) calculated from the simulation?

=> The radiative forcing associated with solar variability and CO_2 has been estimated independently of the simulation. This missing explanation has been added in Section 2: "The associated mean radiative forcing in Fig. 1a is estimated by dividing the TSI by four (distribution of the top of atmosphere flux on Earth surface), and multiplied by the Earth's albedo (~0.7)." [. . .] "The radiative forcing associated with CO_2 in Fig. 1a is estimated with the equation of (Myhre et al., 1998). This equation gives a good estimate of the first order radiative forcing of CO_2 in the IPSL model (J.-L. Dufresne, pers. Com.)." The total anthropogenic forcing is the radiative forcing of the well-mixed GHGs estimated by Dufresne et al (2005) (caption of Fig. 1a). The radiative forcing of the tropospheric aerosols is calculated from the simulation (caption of Fig. 1a).

Figure caption: TSI is right axis

=> Âń left Âż has been changed for Âń right Âż

Fig. 2: The differences in the reconstructions would become visible better when show-

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ing all in one figure.

=> We have tried to put all the time series (NH temperature reconstructions and SGI) on the same plot as suggested. The comparison between the temperature reconstructions is easier but the comparison between each temperature reconstructions and SGI is penalized because of too much information on the same plot. We preferred to show separated plots to favour the comparison between SGI and the temperature reconstructions.

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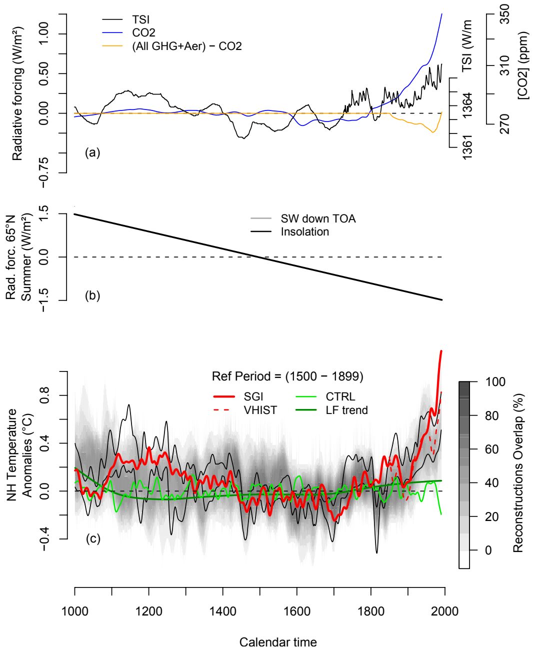

Fig. 1. Revised Fig.1

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