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Climate
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Discussions

Interactive comment on "Climate and carbon-cycle variability over the last millennium" *by* J. H. Jungclaus et al.

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After the manuscript has been evaluated by four reviewers we have responded to all points raised by the reviewers, corrected shortcomings and errors and reformulated a substantial part of the manuscript. We thank all reviewers for the constructive remarks and suggestions that helped to make the manuscript more mature and to clarify misunderstandings. In the following we respond (indicated by Authors' Response: "AR") to each of the reviewer's comments (indicated by the reviewer's initials, here Pierre Friedlingstein: "PF"). The modified passages from the manuscript indicated starting with "MS":

This paper presents the first ensemble simulations of the last millennium performed with a comprehensive Earth System Model that consists of an Atmosphere-Ocean

General Circulation Model coupled to land and ocean carbon cycle models. This is a remarkable achievement that deserves to be published in CP. I find the paper and the analysis very interesting, however I have several comments. I will start with one general remark. I think the authors are a bit too enthusiastic when presenting their results. They claim that the model simulates realistic temperature evolution over the last millennium ("the ensemble simulations reproduce temperature evolutions consistent with the range of reconstructions"). They also claim the model does reproduces well the climate-carbon cycle sensitivity ("The magnitude of gamma agrees with a recent statistical assessment based on reconstruction data"). However, they find that their simulated atmospheric CO2 variations are lower than the one recorded in the ice-core ("The simulated atmospheric CO2 concentrations exhibit a stable carbon cycle over the pre-industrial era with multi-centennial variations somewhat smaller than in the observational records."). If delta T is right and if gamma (delta CO2/delta T) is also right, then delta CO2 should be right: : : My interpretation is that the model is at the lower end of both the climate response and the climate-carbon cycle sensitivity response, making the lower than observed CO2 response. I would suggest the authors to clarify this in the text.

AR: We thank referee Pierre Friedlingstein for the positive evaluation and the constructive suggestions. In preparing the revised version of the paper we have taken all the reviewer's comments into account. We hope that shortcomings have been eliminated and that the new manuscript gives a more comprehensive account of our analyses. In particular we have paid more attention to the discussion of uncertainties and possible model shortcomings. We have to apologize for a misinterpretation of Frank et al.'s definition of the sensitivity parameter gamma. They define it as the change in atmospheric CO2 concentration (in ppm) per °C change in NORTHERN HEMISPHERE temperature, whereas we used GLOBAL temperatures in the analysis (Fig 7 in the original manuscript). We have corrected this in the revised manuscript and recalculated gamma accordingly. This leads to somewhat smaller numerical values for gamma (this is because the standard deviation of NH temperature variation is higher than the one for global means). The resulting gamma values are still well within the range given by Frank et al. (2010), but somewhat lower than their median over the 1050-1800 period (7.7 ppm/K) and certainly lower than their estimate for the time period when the LIA CO2 drop occurs. We have pointed this out more clearly in the text and discuss uncertainties at several places in the manuscript. In the discussion we write:

MS: The simulated climate and carbon-cycle response to variations in the external forcing is likely model-dependent. In particular, our model's sensitivity of global CO2 concentration in the atmosphere to NH temperature changes are on the lower end compared to the probabilistic estimate by Frank et al. (2010), in particular, for those estimates including the LIA CO2 drop. Frank et al. ruled out earlier findings with much greater numbers, but encompasses still a wide range (1.7 – 21.4 ppmK-1). Model-based estimates of γ in Frank et al. (2010) come from the short C4MIP simulations and are probably not representative for experiments with relatively weak external forcing. Carbon-cycle model intercomparison exercises over longer periods are necessary to identify the model dependency of the interchange between the carbon pools.

AR: We would like to add, however, that we cannot be sure what really caused the long-term CO2 variations in the 1000 years before industrialization. We therefore added also the following paragraph:

MS: The CO2 reconstructions show a rise by 4 ppm between 1000 AD and 1100 AD and a decrease by about 5 to 7 ppm in the following 600 years. After 1750 AD, there is a steep increase towards modern values. The CO2 decrease roughly coincides with a period of decreasing temperatures towards the LIA, suggesting that CO2 simply follows temperature. However, the relation is probably not that simple: For example, the cooling after the volcanic eruptions in the early 19th century, that drove the climate back into almost as cold condition as in the early 17th century does not show up strongly in the CO2 records and the coincidence of the MWP with high CO2 levels around 1200 AD remains questionable. Therefore, although temperature changes certainly explain part of the observed CO2 variations, we cannot rule out that carbon-cycle variations

related to mechanisms other than surface temperatures, such as redistributions in the oceanic/sediment pools, with timescales from century to millennia play a considerable role. In the simulations we start from a well-equilibrated carbon-cycle that may not exist in the real world.

PF: I also have some reservations on the evaluation of the carbon cycle both for the historical period (section 3.1) and for the last millennium (section 3.4). The simulated atmospheric CO2 concentration for the recent period is below the observations (Figure 1). The authors should give in the text the deviation from the observations (about 10 ppmv by 2000), and more important, propose an explanation. Is the land use source too weak or are the land and/or ocean sinks too large (e.g. when compared to IPCC or GCP budgets). It would actually be good to give the values (global land /ocean uptake, land use emission, airborne fraction, etc for the 1990s). Also the authors could make use of a full 20th century "observed CO2" as reconstructed from ice core data combined with atmospheric measurements from an average of Mauna Loa and South Pole. This would be better than the 50 years record of Mauna Loa only used here as this latter is (1) too short and (2) not representative of the global atmosphere.

AR: We have followed the reviewer's recommendations and added a quantitative comparison of the simulations with observation-based budgets and compare sources and sinks for the 1990s with those from a recent publication of the GCP. Figure 1b (Figure 2b in the revised manuscript) has been redrawn including a CO2 reconstruction combining measurements and ice-core data. The entire paragraph on the 20th century CO2 evolution has been rewritten:

MS: The simulated global atmospheric CO2 concentration in the 20th century (Fig. 2 b) stays below the observed record (a combination of ice core data and atmospheric measurements provided by the Paleo Model Intercomparison Project at https://pmip3.lsce.ipsl.fr/. By the end of the 20th century, the simulations arrive at 10 ppm lower values than the reconstructions. Part of this discrepancy can be explained by the roughly 3 ppm lower CO2 concentration at the very beginning of the experiment

(800 AD). For the industrial period (1850 – 2000 AD) the simulated carbon content in the atmosphere increases by 163 Gt C in the ensemble mean, the land inventory changes by -3 Gt C and the ocean takes up 119 GT. The respective numbers given from a combination of reconstructions and model estimates (Houghton, 2007) read 175 Gt C, 40 Gt C, and 140 Gt C. As has been pointed out by Pongratz et al. (2009) the primary emissions from land-use change simulated by our model are similar to other studies (DeFries et al. 1999; Olofsson and Hickler, 2008), though at the lower end. Therefore we attribute the lower-than-observed CO2 concentrations in part to an underestimation of land-use change emissions that are not compensated for by a somewhat too weak ocean uptake. In addition, the turnover of soil turnover may be too slow. For the period 1990 – 2000 AD, however, the simulated carbon sources and sinks for the 1990s are well in the range of observations: atmospheric growth is 3.2 Gt C in the simulations vs. 3.1 Gt C in the observations. The ocean sink is 2.1 (2.2) Gt C, the land-atmosphere net flux is 1 (1) Gt C and the land use emissions account for 1.3 (1.6) Gt C (numbers in brackets from Le Quéré et al., 2009). Overall, the differences between simulated and observed CO2 concentration at the beginning of the 21st century are well in the range of state-of-the-art climate carbon models, such as those carried out in the framework of C4MIP (Friedlingstein et al., 2006; Raddatz et al., 2007). The CO2 increase from land-cover changes is moderate compared to contribution from fossil-fuel emissions. Over the last millennium, land-cover changes contribute roughly 20 ppm (Pongratz et al., 2009).

PF: In section 3.4 on the CO2 evolution I have a problem understanding the behaviour of the land biosphere. What is the reason for such a radical change around 1950? The land switches from a small source to a large sink in a couple of decades. The authors attribute this to “CO2 fertilization” but I don’t get it. Seeing the smooth atmospheric CO2 increase (figure 1), I don’t understand why fertilization should suddenly kick in around 300 ppmv. This doesn’t make much sense from a physiological point of view. Could the authors explain what is going on?

AR: atmospheric CO2 and d13C records suggest that the land was a net source of CO2 during the nineteenth and early twentieth centuries, and that the land changed to a net sink around the 1940s (e.g., Joos et al. 1999). In model experiments, the land turns into a sink in between 1940s and 1970s (McGuire et al., 2001; Brovkin et al., 2004) depending on biospheric model and land use scenario applied. The explanation by CO2 fertilization alone is, indeed, incomplete: The rapid decline in emissions from the land biosphere is caused by a rapid decrease in the global deforestation rates in the land-cover-change reconstruction we use (which are based on Ramankutty & Foley; see Area Change curve in Fig. 4 of Jain and Yang, 2005). But a reduction in deforestation rate reduces only the source strength, but would not turn the land biosphere into a sink. This sink develops only, because in addition the CO2 concentration increases, so that only after 1955 CO2-fertilization overcompensates carbon losses from deforestation. One could say, that only because of the drop in deforestation the CO2-Fertilization gets visible in the land carbon balance.

MS: After 1950, a rapid decrease in the global deforestation in our land-use-change maps (based on Ramakutty and Foley, 1999) and strong increase in anthropogenic CO2 emission rates lead to a state where CO2-fertilization overcompensated carbon losses from land-use-changes and this turned the land biosphere into a sink (Joos et al., 1999). Therefore, since 1950 both ocean and land have acted as massive carbon sinks, again highlighting the exceptional role of that period.

P.F.: The analysis of the climate-carbon feedback page 1022 is also a bit weak. The discrepancy between the simulated and reconstructed CO2 over the 1600-1800 period deserves a better explanation. The authors argue that the strong and too early CO2 rise due to land use is one possible reason for discrepancies. Then they say that other explanation include underestimation of the MWP-LIA cooling or the climate-carbon feedback. I think there are several issues here that the authors should clarify: (1) The model does not reproduce the LIA CO2 drop, but this has nothing to do with land use. The observed decrease started in 1500 or so where there was no significant land use signal.

AR: We agree, the way we discussed the land-use signal could lead to misunderstandings. We have changed the paragraph to make these points more clear (note that Fig. 5 from the original MS is now Fig 6 of the revised MS; see response to Reviewer 2)

MS: Before 1700 AD, land cover changes modulate the CO2 record only by a few ppm, slightly exceeding the range of internal variability. While the CO2-sequestration effect of abandonment of agricultural areas due to warfare and epidemics is discernable in regional and global emissions (Pongratz et al., 2009), it is not sufficient to explain the apparent CO2 decrease during the LIA. On the contrary, after 1500 AD, atmospheric CO2 in the land-cover-change-only experiment is almost always higher than the mean of the control experiment. . . .

PF: (3) Then there is the issue of the too early CO2 rise due to land use change around 1700, not recorded in the ice-core. Would this imply that the intensification of land cover change is too rapid (and too early)?

MS:The atmospheric carbon loss through cooling by volcanic activity in the 18th to 19th century (Figure 6 b) is overcompensated by a massive effect from land-cover-changes. In fact, the strong rise in CO2 levels during the 18th century in the full-forcing experiments (Fig. 6 a) can largely be attributed to the increase of emissions from agriculture (green line in Fig.6 b) (see also Reick et al. 2010)). Compared to observations, the onset appears to start roughly 50 years too early in the simulations. While the inability of the simulations to reproduce the CO2 decline between 1500 and 1650 cannot be attributed to an overestimation of land-use-change, the emission increase from 1700 onward is one possible reason for the discrepancies between our simulations and the CO2 reconstructions in the 18th century.

PF: (2) The failure to reproduce this drop then comes from either a too weak cooling or from a too weak response from the carbon system (too weak gamma), as the authors pointed out. However, in the rest of the paper they argue the model does well on these

two terms. See my very first comment.

AR: We have corrected our estimate of gamma (see above) and also added a more detailed and critical in the discussion

PF: If I understood well from Figure 7, the estimates of gamma are based on the 800-1600 period, i.e. excluding the Little Ice Age. It is compared with the Frank et al. estimate (1.7 to 21.4 ppm/K) that does account for the LIA signal. Does this affect the comparison? Could the authors calculate gamma over a period that includes the LIA in order to compare with Frank et al, but also to other estimates (Cox and Jones 2008, Sheffer et al., 2006)?

ARÂă: We have re-evaluated the calculation of gamma using now a time period from 800 to 1700. This includes the LIA, but we deliberately excluded the time after 1700 because anthropogenic effects become visible from 1700 onward when land-use-change emissions contribute significantly to atmospheric CO2 (see above).

P.F.Âă: Minor comments: Page 1016, line 14: give the fossil fuel emission data used here.

The reference to Marland et al. (2003) has been included.

P.F.: Page 1019 related to Figure 3: why does the control run show a cooling? Is it acciden- tal?

AR: The black symbols don't denote a general cooling but the magnitude of the range of temperature variations for 30-year mean periods in the control experiment. Another measure would probably be to indicate the overall maximum-minimum temperature or four times the standard deviation of the control run (30-year averages) as an estimate of the control run variability.

Page 1020, line 14. Clarify the sentence by saying that red is significant warming and blue is significant cooling.

AR: The sentence has been modified accordingly:

MS: In Fig. 5 these periods are indicated by colour shading where the respective depth of the shading indicates the number of ensemble members that show a significant warming (red) or cooling (blue) at the same time. The darkest shades of red or blue, respectively, indicate that all ensemble members show a statistically significant deviation from the control experiment.

P.F.: Page 1022, line 24. The glacial-interglacial sensitivity is irrelevant in this context. I would suggest dropping that sentence.

AR: We prefer to keep the very-long term sensitivity as it gives an estimate of the background changes.

P.F.: Page 1023, line 2. Give value of gamma for the E1 ensemble as well.

AR: The value for E1 is now included and Fig. 8 (Fig 7 in the original ms now includes the E1 ensemble). As we have pointed out above, we have calculated gamma again using NH temperatures instead of global averages. This results in somewhat lower numbers for gamma:

MS: Similar to the temperature sensitivity to external forcing that we have discussed with respect to TSI variations, the response of the simulated carbon-cycle to climate variations is certainly model dependent (Friedlingstein et al., 2006; Frank et al., 2010). In the real world, the processes controlling carbon fluxes between the atmosphere, biosphere, and the oceans are temperature dependent and, on glacial timescales, the sensitivity of the global carbon cycle to temperature is roughly linear with a slope of about 8 ppmK-1 (Woodwell et al., 1998). While empirical estimates based on last-millennium data have reported values up to 40 ppmK-1 (Scheffer et al., 2006; Cox and Jones, 2008), a recent assessment (Frank et al., 2010) quantified γ (the sensitivity of atmospheric CO2 to Northern Hemisphere temperature changes) with a median of 7.7 ppmK-1 and a likely range of 1.7–21.4 ppmK-1. For our simulations γ falls well within

this range, though at the lower end (Fig. 8 a). Strikingly, however, γ is much larger for the forced as compared to the unforced simulations. The regression slopes read 2.7 ppmK-1 for the E1 ensemble, 4.3 ppmK-1 for the E2 ensemble, but it is considerably smaller for the control experiment (1.6 ppmK-1).

P.F.: Page 1023, line 8. A lag of 10 years is considerably lower than what was found before. Both Cox and Jones, 2008 and Scheffer et al., 2006 found a lag of about 70 years. Is this because of not including the LIA? Why would this change the lag? Please comment.

AR: Indeed, Cox and Jones (2008) and Scheffer et al. (2006) found a lag of about 50 years between low-frequency NH temperature series (Moberg et al., 2005) and a 75-yr smoothed CO2 series from Law Dome (Etheridge et al., 1996) between 1500 and 1600. However, one should be aware about uncertainties in the reconstructions and there is no guarantee that if another time series were taken, the lag length of 50 years would sustain. For example, the annual combined NH temperature reconstruction by Rutherford et al., 2005, does show a relative decrease in temperature during the 17th century, but the temperature minimum in 80-yr moving averaged temperature is shifted to the later years relative to the reconstruction by Moberg et al. (2005) (see attached figure) and the lag between temperature and CO2 is considerably smaller. We conclude that uncertainty in the reconstructions is too high to compare it with the model.

P.F.: Page 1023, line 23. It should read "(Fig. 7b)"

Corrected

P.F.:Figure 7: Would it be possible to show results for E1 as well?

In the new version (now Fig. 8), both E1 and E2 are displayed

P.F.: Not sure Fig 7b is needed. Only one short, descriptive-only sentence describes it.

AR: We prefer to keep the figure (now Fig. 8) as it underlines the important finding in section 3.4 on the time-scale dependence of the climate-carbon cycle sensitivity.

P.F.: Why a lag of 5 years? I thought the main text mentioned a lag of 10 years. The result doesn't differ much for lags between 5 and 10 years. In the new calculation, we used a 10-year lag.

Additional References Brovkin, V., S. Sitch, W. von Bloh, M. Claussen, E. Bauer, and W. Cramer, 2004: Role of land cover changes for atmospheric CO2 increase and climate change during the last 150 years. Global Change Biology, 10, 1253–1266.

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McGuire, A.D., S. Sitch, J.S. Clein, R. Dargaville, G. Esser, et al., 2001: Carbon balance of the terrestrial biosphere in the twentieth century: Analyses of CO2, climate and land use effects with four process-based ecosystem models. Global Biogeochemical Cycles, 15(1), 183–206. Reick, C.H., Raddatz, T., Pongratz, J., and Claussen, M.: Contribution of anthropogenic land cover emissions to pre-industrial CO2, Tellus, B, published online, doi:10.1111/j.1600-0889.2010.00479.x, 2010. Rutherford, S., M. E. Mann, T. J. Osborn, K. R. Briffa, PâĂĽD. Jones, R. S. Bradley, M. K. Hughes, 2005: Proxy-Based Northern Hemisphere Surface Temperature Reconstructions: Sensitivity to Method, Predictor Network, Target Season, and Target Domain. Journal of Climate, 18, 2308-2329

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Figure 2

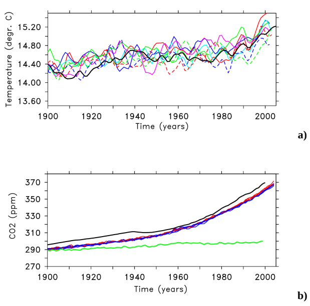

Fig. 2. NH temperature and global CO_2 concentration in the 20th century. **(a)** 20th century Northern Hemisphere (land and ocean) 2m air temperatures (11-year running means) simulated in ensemble E1 (solid coloured lines) and E2 (dashed coloured lines) in comparison with the HadCRUT3v dataset (obtained from the Climatic Research Unit, http://www.cru.uea.ac.uk/cru/data/temperature), **(b)** 20th century global CO_2 concentration (yearly data) simulated in ensemble E1 (red) and E2 (blue) in comparison with a combination of ice core data and atmospheric measurements (black) provided by the Paleo Model Intercomparison Project at **http://pmip3.lsce.ipsl.fr**. The green line is the respective curve for the land-cover-change-only experiment.

Fig. 1.

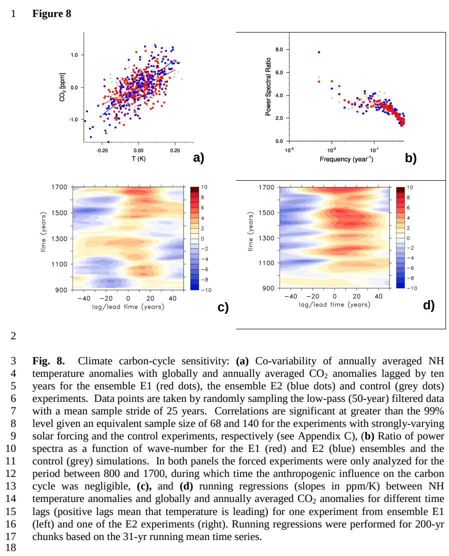
Figure 8

Fig. 8. Climate carbon-cycle sensitivity: **(a)** Co-variability of annually averaged NH temperature anomalies with globally and annually averaged CO_2 anomalies lagged by ten years for the ensemble E1 (red dots), the ensemble E2 (blue dots) and control (grey dots) experiments. Data points are taken by randomly sampling the low-pass (50-year) filtered data with a mean sample stride of 25 years. Correlations are significant at greater than the 99% level given an equivalent sample size of 68 and 140 for the experiments with strongly-varying solar forcing and the control experiments, respectively (see Appendix C), (**b)** Ratio of power spectra as a function of wave-number for the E1 (red) and E2 (blue) ensembles and the control (grey) simulations. In both panels the forced experiments were only analyzed for the period between 800 and 1700, during which time the anthropogenic influence on the carbon cycle was negligible, **(c),** and **(d)** running regressions (slopes in ppm/K) between NH temperature anomalies and globally and annually averaged CO_2 anomalies for different time lags (positive lags mean that temperature is leading) for one experiment from ensemble E1 (left) and one of the E2 experiments (right). Running regressions were performed for 200-yr chunks based on the 31-yr running mean time series.

Fig. 2.

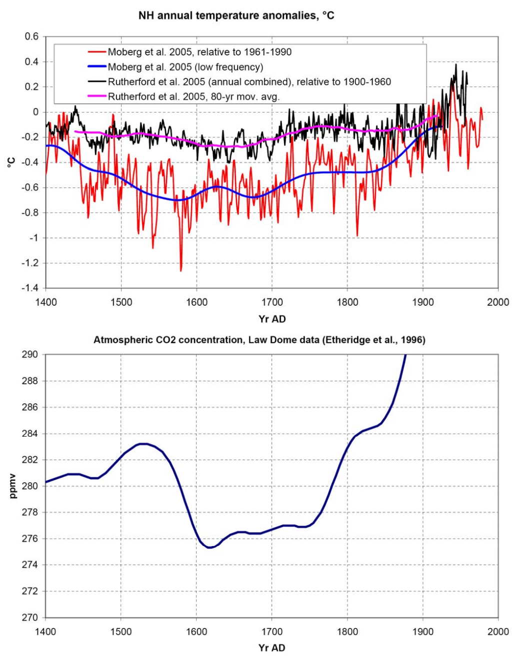

Fig. 3.