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Climate of the Past Discussions

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

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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.

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.

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?

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.

(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.

(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)?

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)?

Minor comments:

Page 1016, line 14: give the fossil fuel emission data used here.

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

tal?

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

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

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

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.

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

Figure 7:

Would it be possible to show results for E1 as well?

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

Why a lag of 5 years? I thought the main text mentioned a lag of 10 years.

Interactive comment on Clim. Past Discuss., 6, 1009, 2010.