Clim. Past Discuss., 8, C2609–C2623, 2012
www.clim-past-discuss.net/8/C2609/2012/

Climate of the Past Discussions

Interactive comment on "Historical and idealized climate model experiments: an EMIC intercomparison" *by* M. Eby et al.

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Received and published: 1 December 2012

We would like to thank the referees for their careful assessment and review of our manuscript. What follows is our response to each referees comments, outlining how we will improve the paper by incorporating their suggestions. Referees' comment paragraphs are in indicated by an initial ">". Referee comments and our response are separated by a line with a dash ("-"). The referees' suggestions have been separated into manageable pieces and this sometimes means a similar suggestion is repeated in different sections. We have tried not to repeat our responses, so some referee comments may be addressed in other response sections.

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Response to Referee #1

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> The paper by Eby and co-authors is an impressive work aiming at addressing key issues concerning historical climate and carbon cycle changes over the last millennium. Many appropriate and specific sensitivity studies were dedicated to evaluate model's standard characteristics, the individual and summed influence of external natural forcings vs the influence of initial state on global temperatures and climate-carbon feedbacks over the last millennium as compared to model internal climate variability. They used a variety of EMICS to address the possible model dependency of each results and extract the robust signal. Even though the paper is well written and the authors tried to be concise, I think more work is still needed to address the outstanding questions raised by the authors.

> If the authors want to evaluate the relative contributions of the applied natural external forcings they should at least be more specific and give more information on the forcing data set used and how they were applied. Obviously due to EMICS inherent simplified physics and their coarse resolution, external solar and volcanic forcings will merely be applied as anomaly to the solar constant, while tropospheric aerosols as anomalies to the surface albedo. But by doing so, several additional assumptions and uncertainties might be added and these should be presented and discussed as most of the following results and conclusions concerning temperature and carbon cycle responses will directly depend on these assumptions.

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The main purpose of the paper is to evaluate the models through a series of performance metrics, which can be compared against data and other models. Our intention was not to provide an extensive validation of forcing protocols over the last millennium. However, as the referee suggests the way in which EMICs implement applied forcings is diverse. Although the forcing was specified to follow PMIP3 and CMIP5 protocols (which are explicit and well documented), not all EMICs are able to apply these forcings in the same manner. As an example, while some models use CMIP5 specified sulphate concentrations and calculate their own radiative forcing (similar to some more complex models that do not have sulphate chemistry), others must apply radiative forcing estimates from the radiative forcing Task Group: "RCP Concentrations Calculation and Data". In order to document more clearly the diversity and uncertainty in forcing implementation, we will expand the section describing the CMIP5 and PMIP3 forcing data and add a paragraph to the end of each model description in the Appendix, briefly stating how the specified external forcing is applied.

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> In that sense, it is quite misleading to specifically write in the text, that “sulphate and volcanic aerosols” were included in the models to run the simulations since it is actually not the case. No information is given in the paper on how these forcings were diagnosed, parameterised in the EMICS and evaluated against observational dataset over the instrumental period for the tropospheric sulphate aerosol for example or even for the volcanic aerosols.

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In the manuscript we state that simulations include changes in forcing from "sulphate and volcanic areosols". While we did not view this as misleading, we recognize and accept that more details should be provided. This paper is not meant to be an evaluation of how well each modelling group implemented externally specified forcing. This is described in the reference material. We felt that the diversity of methods of implementation would be reflected as part of the diversity of the models themselves. While some models may not be able to implement sulphate or volcanic aerosols in a "sophisticated" manner, most did include these forcings. If a model excluded any specified forcings, we will make this clear in the main text or in the appendix. Also see the previous response.

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> Figure 7 is limited to 250 years while in figure 10, the most striking feature that stems out of the simulations with and without free CO2 are cooling excursions far more important than the MCA/LIA differences. How realistic are these cooling events as compared to temperature reconstructions and how do they impact the conclusion concerning the role of land and ocean carbon fluxes on atmospheric CO2?

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Figure 10b is the same as Figure 7 but for the period before 1800. It is difficult to tell if these early cooling events are realistic. They are mostly the result of a series of large volcanic eruptions between 1250-1300 and 1450-1500 (see cyan line in 10b). If you look at multiple temperature reconstructions over the last millennium (as in Mann et al., 2008 or Frank et al., 2010), there is little agreement between them. There are excursions in some of the reconstructions that are as large as these simulated cooling events but it may be that these events are not realistic. It is interesting that these cooling events are not seen in the CO2 record. This implies that: the specified volcanic forcing is not realistic, or that the CO2 record is not reliable, or that the climate-carbon cycle feedback is weak or masked in the CO2 record by other factors. It is beyond the present experiment to say which is the case but this does warrant more research. It is possible that there are some inconsistences between the forcing and data reconstructions as suggested by Mann et al. (2012). As we tried to suggest, this is an area where rapidly integrating EMICs, carrying out multiple sensitivity tests, could make a contribution. These early cooling events, however, do not appear to be very important when looking at just MCA-LIA differences (although volcanic forcing during the LIA and the lack of it during the MCA is important). Volcanic forcing is relatively short lived in these simulations and the effects of these early cooling events have disappeared by the time of the LIA. See figure 10b. Similar early cooling events are seen in most of the simulations in Fernández- Donado et al. (2012) and the CMIP5 past1000 experiment, which is not surprising since the volcanic forcing for many of these experiments is similar. Results will be compared to these studies.

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> The authors' main conclusion and discussion concern the underestimation of temperature drop between the MCA/LIA while ignoring most important cooling periods preceding LIA. From the figure 10a and 10b, this doesn't look like a cooling trend at all, but rather like a succession of cooling cycles of more or less the same amplitude. The realism of these cooling cycles need to be evaluated respectively to the discussion on MCA/LIA amplitude anomaly, which definition referring to Franck et al (2010), is not meaningful is this context. Without such discussions and analyses, we can't judge if the related impact on the carbon cycle is relevant, as compared to other studies such that of Junclaus et al 2010. How these results do compare to other GCM studies?

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One of the purposes of using coupled carbon cycle models over the MCA to LIA period was to test how well they could reproduce both the temperature and CO2 record, to changes in radiative forcing, over the century time scale. This is an important test of a coupled model's ability to simulate a reasonable carbon cycle feedback, which in turn is important for future climate simulations. It is perhaps disappointing that the models seem to underestimate the somewhat uncertain temperature change over this period, but this is not a requirement in order to make a crude estimate of their carbon-climate feedbacks. We will compare these model results with those of Gerber et al. (2003), Goosse et al. (2010), Junclaus et al 2010, Fernández- Donado et al. (2012) and the recently released CMIP5 past1000 experiment. In general the results in this paper reinforce these other studies. Carbon cycle models seem hard pressed to reproduce the CO2 record and many simulations appear to also underestimate the cooling from the MCA to the LIA - especially when using the more recent lower estimates of solar variability. We will state this more explicitly in the paper. Also see the previous response.

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> What are the differences and added values/information of the present EMICS study respectively to other recent studies addressing the same questions? These need to be discussed and authors have also to take into consideration all recently published modeling work addressing the same issues.

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While there have been several studies that look at the temperature response to changes in forcing over the last millennium, there have been few previous studies (that we know of) that have simulated the carbon cycle feedbacks, Gruber et al, (2003), Goosse et al. (2010) and Juncaus et al. (2010). This is one of the reasons why we have emphasized results for the carbon cycle response. Carbon cycle results over the last millennium will be compared to these previous model studies.

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> The authors should also provide at least a figure showing time series of each applied external forcings in each models, in W/m2, so the reader can have an idea of eventual trends and relative amplitude of anomalies existing in the applied forcings in each model. This is very important piece of information when discussing the supposedly linear response of surface temperature to individual forcing, possible errors in these forcings, the role of initial state on the following climate/carbon responses for the last 1000 years as compared to paleoclimate reconstructions. All theses issues need to be carefully considered before accepting the manuscript for publication.

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It is not always trivial for models to disentangle the direct radiative forcing from feedbacks. It is easy enough for solar forcing changes and reasonable estimates can be made for volcanic aerosols and trace gases but tropospheric aerosols and land use change are more difficult to specify in terms of radiative forcing. Unlike PMIP3, all models in this study used the same forcing data sets and so at least in terms of volcanic, trace gases and solar forcing, the models' forcings are nearly identical. For land-use change and sulphate aerosols, the forcings may be slightly different due to different implementations, but the diagnostic calculations of these radiative forcings are also more uncertain. However, given these caveats, we will provide a figure showing the models best estimates of radiative forcing for most of the single component and "all" forcing experiments.

For the most part, small errors in these forcings will not affect whether the forcings add linearly or how well the models carbon cycle responds to climate change over this period. As for the role of the initial state, it is clear (from Figure 1a) that these models are starting from very different initial equilibrium states. The fact that the models respond so similarly, at least to small forcing perturbations, would indicate that the equilibrium state is not very critical in determining the response over the last millennium. If the referee is referring instead to the effect of different initial states, due to internal variability, this is generally not an issue with EMICS since they usually show low levels of interannual variability. Of the models that participated in the last millennium experiment, only LOVECLIM, and to a lesser extent IGSM, show much in the way of internal variability. The other models really only show the forced response, so differences in the initial condition due to internal variability is not important. However, it is possible that some EMICS are too simplified to show nonlinearities. We will make this clear in the introduction.

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Response to Referee #2

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> This paper by Eby and co-authors presents an inter-comparison of the results of a large number of earth system models of intermediate complexity (EMICs). It will undoubtedly be very useful especially for the upcoming IPCC report and presents and discusses the results in a clear, well written and concise manner. Although the findings

in this paper clearly warrant publication I do however feel that before this is done the authors should do more to place the results presented in the wider context of other results within the field. With this in mind I find the introduction a bit short and think that it could be substantially improved by including more about why EMICs are useful particularly in the context of the Last millennium. The problems and key questions about the last millennium should be detailed and why EMICs are a good tool to help solve them discussed.

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We will expand the introduction to include some additional discussion as to why EMICs are particularly well suited to address some of the key issues about the climate of the last millennium.

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> Furthermore I feel that more could be done to compare the findings to past studies and results from GCMs. In particular I find the section discussing the pre-industrial period particularly weak. A comparison of the SAT results for the whole last millennium with the available CMIP5 simulations (or pre CMIP5 simulations eg Jungclaus et al 2010) (perhaps just by including the mulit-model mean CMIP5 results on figure 10) , would be useful . Alternatively some reconstructions are available globally for this period eg Mann et al 2009 so perhaps you could compare to them . At the moment the only comparison you make is for the MCA-LIA, but if the annual (or decadal) SAT variability is incorrect you could be getting the right answer but for the wrong reasons. In addition comparing the EMIC calculated values for the carbon cycle over the last millennium to the values obtained by GCMs (eg Jungclaus et al 2010) would be interesting to show whether the problems you discuss are applicable to all models or just EMICs.

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We will add another panel to figure 1 in which we will focus on the temperature anomalies before 1850 and we will include some estimates from paleo reconstructions (Mann et al., 2008) and CMIP5 past1000 experiment results. We will also compare our results to those of Junclaus et al. (2010), Fernández- Donado et al. (2012). The mean of the CMIP5 models could be added to figure 10a although this might make the figure even more cluttered. CMIP5 results are not applicable to the other panels in Figure 10. The SAT comparisons we make over the MCA to LIA period are 100 year averages (1100 to1200 and 1600 to 1700) in order to be roughly comparable to the maximum between 30 year range averages given in Frank et al. (2010). We also give the results over 300 year periods (950 to 1250 and 1450 to 1750) and show that the diagnosed carbon cycle response is not very sensitive to the averaging period. Both the 100 and 300 year averaging periods are likely long enough to eliminate any annual (or decadal) SAT (or CO2) variability in the models. Finding an estimate of the maximum change in SAT between the MCA and LIA from data is much harder but we feel the estimate from the analysis of Frank et al. (2010) is probably a best estimate. We will add an estimate from the Mann et al. (2008) reconstruction. Longer averaging periods for the models likely reduces our estimation of the maximum SAT change slightly but we were mostly interested in looking at ratio of SAT to CO2 changes (a rough estimate of the climate-carbon sensitivity). As long as both carbon and SAT are treated similarly, and we look at long time scales, we feel that short term variability is unlikely to affect our estimation of climate-carbon sensitivity. We will discuss the role of variability and compare the carbon cycle results to those of Goosse et al. (2010) and Junclaus et al (2010).

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> The organisation of sections 3.3 and 3.4 should be made clearer . For example at the moment the effects of the forcings on SATs is discussed in two places. Where section 3.3 “Forcing Components” just refers to figure 7 which only shows the period 1850-2000 , section 3.4 the “freely evolving CO2” describes the effect of the forcings over the pre-industrial period. This should be combined somehow.

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The intention of Figure 7 was to focus on the period when most of the external forcings were active and changing. Before 1800, only changes in volcanic, solar and to a lesser extent CO2 forcing are important. The similar figure for the period from 850 to 1800 (10b) has a very different scale and combining the figures (7 and 10b) would reduce the utility of both. We could add a second panel to Figure 7 so that the forcing contributions over pre-industrial potion of the last millennium simulations can be discussed in section 3.3 (although this is really duplicating a figure panel). The discussion in section 3.3 pertains to the SAT results for 11 models while section 3.4 really is discussing the carbon cycle response of only 7 models. The reason the components are plotted in figure 10b and discussed in section 3.4 is because an estimate of the change in SAT (and CO2) between the MCA and the LIA is required in calculating the models climate-carbon sensitivity. Showing the forcing component contributions to SAT in figure 10 allows us to speculate on the reasons why the SAT change between the MCA and LIA might be underestimated. We will try to make this distinction between these sections clearer in the paper but we would like to maintain the current section structure.

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> A few minor comments:

> p4123 – line 25+ - should mention reconstruction uncertainty as well.

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We will mention reconstruction uncertainty as another reason that the models appear to be underestimating the drop in SAT.

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> p 4127 – paragraph starting line 13 – Bit confusing. Should reword to make it clear that these are 7 individually forced simulations, one for each forcing included. 1 simulation with all forcings and 1 control simulation.

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We will reword this description of the experiments.

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> P4127 – line 24 – bit confusing – should reword for clarity. Eg one simulation included all the forcings and one simulation none of the forcings.

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We will reword this description of the experiments.

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> P4129 I feel that the headings like “climate” and “carbon” are a bit brief and generic, perhaps these could be expanded slightly to be a bit more specific.

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The organization of the paper was to first look at climate characteristics and then the carbon-cycle characteristics of the simulations. Each section describes both transient historical and idealized experiments. It is organized in this way partly because only a limited number of models provided carbon cycle results. The climate section describes results from up to 16 models while the carbon section describes results from only 7. We felt that the general short titles were sufficient to describe the types of results that are being discussed but we will try to think of more descriptive short titles for these sections.

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> P4131 line 18 – should compare climate sensitivity results to previous studies.

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We will compare sensitivities to the CMIP3 and CMIP5 models.

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> P4132 line 10 change to “models with a complete”

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We will change this.

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> P4136 line 28 –I think the description of the effects of volcanoes needs expanding. What do you mean by having a small overall effect? From figure 10 it looks like the they are having quite a large effect (they are causing multi-decadal temperature reductions see eg early 1800s), and indeed later in the paper you suggest that a part of the MCA-LIA transition is due to volcanic eruptions.

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We mean that the century time scale effect of volcanoes is small in this study. We say that: "Volcanic aerosol forcing has a large but short lived negative effect on modeled SAT". Individual volcanic eruptions certainly have large effects on annual to decadal time scales although large groupings of eruptions may appear to act on a longer time scales. In these experiments volcanic forcing is applied as an anomaly and the long term average forcing is zero. This is different from some earlier studies that applied absolute volcanic forcing, which causes a long term downward drift in SAT, when starting from an equilibrium simulation which did not include “average” volcanic forcing. We mention that the SAT change in this study, due to just volcanic forcing, is very close to zero when averaged over the entire millennium. This agrees with the conclusions of Shindell et al. (2003) although Schneider et al. (2009), Goosse et al. (2012) and Miller et al. (2012) indicate the possibility of longer term persistent cooling in polar regions. We will make this section clearer.

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> P4136 line 3-4 Can you compare these values eg effects of land use and aerosols to previous studies perhaps by GCMs.

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We assume you mean page 4236 line 3-4. We will compare the effects of the forcing to previous studies.

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> P4141 line 25 Could mention that other land use schemes exist eg Kaplan et al 2010 which has larger pre-industrial land use change than the Pongratz et al dataset used here. And that potentially large land-use emissions due to reforestation in the Americas eg Ruddiman 2003, Faust et al 2006, Nevle and Bird 2008 for this period could be important (but see also Pongratz et al 2012).

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Other land-use datasets do exist, as do other data sets for other forcings. We have chosen to use one of the recommended PMIP3 data sets. We will mention that there are other choices and that this may affect the conclusions.

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