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Climate of the Past
Discussions

Interactive comment on "Emulation of long-term changes in global climate: Application to the late Pliocene and future" *by* Natalie S. Lord et al.

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The manuscript by Lord et al. presents a new statistical emulator based on a large set of GCM simulations. The authors tested their methods against climate reconstructions for late Pliocene and then applied it to produce a set of climate change projections for the next 200,000 years for different CO2 emission scenarios. Advantage of proposed method that it allows one to obtained high resolution climate scenarios on very long time scales with very low computational cost. The manuscript is well-written and requires only minor revision.

General comments

1. I have no doubts that the authors clearly realize not only advantages but also impor-

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tant limitations of their methods. Some of these limitations are discussed in different parts of the paper. However, I believe it would be useful for potential users of the methods it would be useful to present a more critical discussion of applicability of the methods and its potential limitations.

Firstly, it should be stated very explicitly that the emulator is not applicable for simulations of transient climate cahnge on time scales shorter than several millennia . However, on such long time scales, two major climate forcing – CO2 and ice sheets – strongly interact with each other, that cannot be accounted for in the method presented in the manuscript. On the page 11 the authors wrote that they "are able to simulate global climate development over long periods of time (several million years), provided that atmospheric CO2 level for the period is known, . . . ice sheets do not change outside the range considered... and the topography and land-see mask are unchanged". I believe the authors are too optimistic concerning "several million years" - even a much shorter time interval for which all these conditions are met would be difficult to find in the recent past or in the near future. Clearly, this method is not applicable to Quaternary. For Pliocene, CO2 concentration is not known sufficient accuracy. However, it is very likely that during the late Pliocene CO2 concentration experienced significant fluctuation at different time scales. It is also likely that during Pliocene, the extent of northern hemisphere ice sheets varied beyond the range used in this study (e.g. Willeit et al., 2015). I cannot see how all these problems can be circumvented without use of a comprehensive Earth system model. The less important but still not negligible problem is that according to the PRISM4 reconstruction, land-sea mask and orography during the late Pliocene in some regions (primarily North America and Europe) differed considerably from the modern ones.

The situation is even more problematic for the future. It is not known how good is performance of existing carbon cycle models on such long time scales, but a reasonable agreement between results obtained with different models gives some hope. However, future simulations with the stand-alone carbon cycle models are only valid till the next glacial inception. For the medium emission scenarios, the next glacial inception is immanent (of course making a brave assumption that humans will not influence climate after the end of fossil fuel era) before or soon after 100,000 AD. Beyond that time, the methodology described in the manuscript is not applicable any more. For the extreme Business-as-usual type scenarios (5000 GtC and more), the situation is even worse. Under such scenarios, most of the Greenland ice sheet will melt completely already within the next 1000 years and most of the Antarctic ice sheet will also melt eventually (e.g. Winkelmann et al., 2016). And, according to recent study by DeConto and Pollard (2016), this "eventually" may occur already within one or two millennia. Such rate of ice sheet melt would strongly affect the ocean circulation and stratification with unknown but long-term consequences. In addition, 70 meter sea level rise resulting from melting of existing ice sheets would strongly affect global land-sea mask and regional climates. In addition, submersion of the large part of northern Europe would also have serious implications for the geological storage of nuclear wastes in this area. As the result, the conditions required for applicability of the proposed method can be violated already after the first few thousand years.

Second, the emulator cannot be applied if the climate system possesses a strong non-linearity. AMOC shutdown is the most natural example. The authors mentioned non-linearity only once and assumed that "any non-linearities in the GCM response being absorbed by stochastic component of the Gaussian process" (p. 6). I am not sure I understand what this means. Please clarify.

By saying all that, I do not challenge the usefulness of the proposed methods. This method, if properly applied, can be used for different types of studies, like analysis of safety of long-term storage of nuclear wastes. However, for potential users the knowledge about limitations and potential caveats is crucial to prevent misuse of this method.

2. The model reveals strong response in annual mean temperature on precessional forcing. Since annual mean precessional component of orbital forcing is zero, I wonder

what causes such response. Is it really global or only regional phenomenon? May be it would be useful to add to the Fig. 4 annual SAT anomalies produced by other forcings: CO_2, obliquity and precession (say difference between the maximum and minimum obliquity and difference between the “warm” and the “cold” orbits).

3. I found the attempt to reconstruct Pliocene CO_2 from individual temperature records rather strange. These four temperature records are so poorly correlated with each other that it is hard to expect that any global factor (like CO_2) can bring them in agreement with modeling results. As the result, all four CO_2 “reconstructions” have very little in common. I wonder what one can learn from such exercise. Although I cannot be objective in this respect, but I do believe that using of stacked data (e.g. Willeit et al. (2015), Stap et al. (2016)) rather than individual records, is more appropriate approach to reconstruct past CO_2 concentrations.

4. I would strongly suggest to not use expressions like “fossil fuel emission” or “anthropogenic fossil fuel emission”. Unfortunately, this jargon is used in some publications related to energy and mitigation. However, I do not believe it is appropriate for climate modeling papers. In any case, burning of fossil fuel is the most important but not the only source of anthropogenic CO_2. Land use and cement production also play a role in rising of atmospheric CO_2 concentration.

Specific comments

L. 54 Which “system” is meant here?

L. 74 Typo. “precessional”

L. 110 I would change “modern day” to “Quaternary”

L. 128 “input configuration”?

L. 250 change “forcings” to “parameters”

L. 256 What about obliquity?

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L. 265. This is not estimate of “remaining reserves”. This is just “current estimate” of fossil fuel reserves which has a tendency to increase with time.

L. 277 I do not believe that 20 ppm CO2 change during Holocene (which is primarily transient response to the deglaciation) has something to do with the natural CO2 variability during Anthropocene.

L. 209 Emission cannot be removed

L. 298 CO2 will not return to preindustrial level because glacial cycles will resume before this will happened. But even without glacial cycles, it is unlikely that preindustrial level of 280 ppm is the true equilibrium CO2 concentration in the interglacial world. Even small disbalance between volcanic outgassing and weathering would cause significant CO2 drift on time scale order of 100,000 years.

L. 353 Please specify initial conditions for model runs.

L. 367 Which positive feedback is meant here? I guess this is just an artifact of models with prescribed present day vertical ozone profile.

L. 502 Why “linear nature of the plot increases “ confidence? In theory, this plot must not be necessarily linear.

L. 585 What is “SAT index”

L. 751 “Across the four sites. . .” This sentence is not clear

L. 758 What is the meaning of “emulated uncertainty” and how it was defined?

L. 763 What is meant under other “human activities”?

L. 776 “long atmospheric lifetime of fossil fuel emission”?

L. 776 Reference to the original Archer (2005) paper would be much more appropriate

L. 813 -820. The authors try to argue here that the fact that they cannot model ice sheet evolution is not very important for the future 200,000 years climate projections.

This is not true – see my general comments.

L. 899 “High latitude sites concentrations” Sounds like CO_2 concentration is different in different sites

Fig. 9. I guess Fig9a shows annual SAT difference due to CO_2 increase to 400 ppm. If so “modern annual SST” is misleading. What is shown in 9b is not clear to me.

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