Clim. Past Discuss.,
https://doi.org/10.5194/cp-2017-68-RC2, 2017

Climate of the Past Discussions

Interactive comment on "Interhemispheric Effect of Global Geography on Earth's Climate Response to Orbital Forcing" *by* Rajarshi Roychowdhury and Robert DeConto

Anonymous Referee #2

Received and published: 11 July 2017

In this manuscript, the authors are investigating the role of geographic hemispheric symmetry or asymmetry on Earth climate. This question was raised already in the XIXth century and could indeed be an interesting scientific discussion. Unfortunately, as it stands, 1 - the paper does not provide a sufficient scientific background on this problem, 2 – does not present the severe limitations of the methodology used, 3 – does not discuss the mechanisms involved in their results, 4 – does not provide sufficient discussion for the theoretical interpretation of their work. As it stands, the resulting manuscript is merely a presentation of some AGCM simulations, without much scientific content.

1 - Overall, it is not clear what the main focus of the paper is.

1a: If it is a theoretical paper, then:

The authors should discuss in much greater details their critical assumptions, and how they impact the results. In particular, there is no dynamical ocean in their model set-up. A key theoretical finding in the 1980s was that the ocean heat transport cannot be symmetrical even under symmetric boundary conditions (see eg. Bryan, 1986; Thual et al, 1992, ...) : this symmetry breaking leads to multiple equilibria in the Atlantic deep circulation, and is now quite well understood on a theoretical point of view. So it is unlikely that a true coupled climate model would react symmetrically under symmetric boundary conditions, as found here with an atmospheric component only. This single omission is probably enough to reject the paper as it is now.

For a theoretical paper, it is not sufficient to get "almost symmetrical results" (line 174; line 267) without further comments. Indeed, it would be rather easy to go a step further and to give a proper account of the "remaining asymmetry". The obvious sources of asymmetry are linked to the astronomical forcing, in an explicit way through the precession and the location of the perihelion (as discussed rapidly in the paper), but also in an implicit way through the choice of a calendar. This last point is quite critical but is not even mentioned in the paper. For instance, (line 194) the authors do not specify if the number of summer days is fixed according to the present conventional calendar or if it is astronomically defined. Since this last specific point is at the center of the discussion on climate symmetry/asymmetry since the XIXth century (see below), it is a pity that the authors fail to address or even mention this key question.

The choice of an "averaged eccentricity" is not a very good one for a theoretical discussion on symmetry/asymmetry, since the role of precession will not be very clear, as discussed above (results are "almost symmetric"). Using a high eccentricity would provide an easier discussion of the asymmetry linked to the astronomical forcing. Using a zero eccentricity would remove the precessional forcing and, by difference, allow for a

true discussion of the role of asymmetry in the forcing.

1b: If the main topic is about paleoclimate changes, then:

The authors should explain how these symmetric/asymmetric configurations relate to actual past changes of the Earth geography. There is no real mention about this, expect in the introduction: “the global continental configuration has been close to its present form since the mid-Cenozoic Âż (line 29) and in the conclusion Âń the amplification (or weakening) of the response to insolation changes at precessional and obliquity periods might explain some of the important features of late Pliocene-early Pleistocene climate variability Âż (line 408). These two statements contradict each other : how the insignificant geographic changes of the late Cenozoïc could modify the response of the Earth system to astronomy ? Are these (insignificant) changes towards a stronger or a weaker symmetry ? Are we talking about amplification or weakening ? No only the manuscript is unclear on this conclusion, but it seems also quite contradictory. My impression is that the main topic of the paper is not about actual past climate changes.

1c: If the main topic is about atmospheric mechanisms involved in the simulated symmetrical/asymmetrical response of the atmosphere, then the authors should discuss these mechanisms. As observed also by Reviewer #1, this is obviously not the case here.

So my overall impression is that the paper could possibly be a rather theoretical one on symmetry/asymmetry of climate. Unfortunately, the key ingredients are missing.

2 - The title of the paper is about Earth hemispheric asymmetry, in the context of astronomical forcing. I was expecting some introduction to explain why this question is so central in the astronomical theory of paleoclimate. Unfortunately, there is no such background in the manuscript. This was a heavily debated topic in the XIXth century, with many important astronomical, physical, theoretical discussions (see eg. Lyell, 1830). The key point at that time was to understand whether the Southern hemisphere was colder because of the land-sea distribution (asymmetry) or because of the astro-

nomical forcing. This ended up with various ideas on present-day climate and various astronomical theories of Quaternary climates. So, indeed, the question of symmetry/asymmetry was quite fundamental since the beginning, and indeed it has not been investigated thoroughly with GCM tools. But as it stands, the paper does not provide a good introduction and a good theoretical incentive to look at the simulations results.

3 – The ocean is represented by a slab ocean. This is quite a critical assumption that should be discussed (see above). But some details should also be provided here: is there any representation of an oceanic meridional heat transport ? How is it parameterized ? Does such a parameterization reproduces present day SSTs ? Is it (almost) symmetrical when the atmospheric results are (almost) symmetrical ?

4 – line 174-175: “with some small remaining asymmetry due to the current timing of perihelion”. Indeed, this is precisely what is called the astronomical forcing linked to precession, as understood since the XIXth century, and to the "present day" calendar convention.

5 – line 187 and line 214: “the effect of Southern Hemisphere continental geography on Northern Hemisphere climate” (and reciprocally). Obviously, the authors are mixing up the notions of “effect” (something which involves causality, mechanisms, etc. . .) and the notion of “correlation” based simply on plotting maps without even discussing if there might be a causal link between the Southern continental geography and the Northern climate. This sentence is very symptomatic of the whole paper. Measuring such an “effect” (as attempted in this manuscript) has no real scientific value if there is nothing behind it, but mere coincidence.

6 – The choice an “extreme precession” (line 260) makes the implicit assumption that only high latitudes are involved in the question of symmetry/asymmetry. This is quite strange, since monsoons are likely strongly involved in this problem, through interhemispheric heat transport changes, something that could be captured by an atmospheric only model. But then, a more natural choice for “extreme precession” might

correspond to perihelion at equinoxes (not at solstices). Precession is fundamentally a cycle, and a circle has no “extremes” unless you look it from a specific point of view.

7 – line 299: “According to Milankovitch theory, the Northern Hemisphere should experience ‘interglacial’ conditions when perihelion coincides with boreal summer Âż. line 400: “At precessional periods, at which the high latitude summer intensity primarily varies Âż. Obviously, the authors have misunderstood Milankovitch. According to Milankovitch (1941), the “summer” intensity (defined as the caloric summer insolation) depends primarily on obliquity (therefore his prediction of 41 ka glacial cycles), not on precession.

8 – line 255: citation of Raymo 2006 for precession being out of phase in both hemisphere. Maybe Herschel (1835) or Lyell (1830) would be more appropriate, though scientists were aware of that fact certainly much earlier, probably since Hipparchus ($\sim$ 30 BC). Citing a recent paper for a very old (simple geometric) relationship seems quite inappropriate to me.

9 – line 121 et 168: meriodionally -> meridionally

References:

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