Clim. Past Discuss., 7, C113–C120, 2011
www.clim-past-discuss.net/7/C113/2011/

Climate of the Past Discussions

Interactive comment on "Implications of the permanent El Niño teleconnection "blueprint" for past global and North American hydroclimatology" *by* A. Goldner et al.

Anonymous Referee #3

Received and published: 14 March 2011

Review of Implications of the permanent El Nino teleconnection "blueprint" for past global and North American hydroclimatology

Overall

This article describes an interesting experiment, which certainly merits publication in Climates of the Past. I do, however, feel it would benefit from some relatively major revisions before publication to improve its readability and to make its findings more obvious to readers without an extensive knowledge of the subject.

My two major suggestions are as follows:

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

1. There is so much data presented in the figures that it is unnecessarily hard to pick out the features alluded to in the text. I count a total of 85 different spatial fields being presented in the work, which seems excessive. I think this partly a consequence of a slight lack of focus in the piece – be more assertive about which conclusions you want to convey and it should become clear which figures need inclusion.

2. I was not certain what was gained by the inclusion of the RegCM3 results. I assume that the global model does not satisfactorily describe the paleo-observations of the hydrological cycle over North America. However, this was never actually shown. It felt like you were trying to solve a problem that I was not sure actually existed.

Some other more minor points are:

i. The description of the method is missing an essential sentence or two

ii. The citations did not always address the points made in the sentence

iii. Some of the discussion may benefit from inclusion in the results section, as it summarizes the findings and explains the importance of them

iv. I got a little lost in the section on superrotation. I was not expecting your model to show it, as none of Barreiro et al. (2006), Vizcaino et al. (2010) or Brierley Fedorov (2010) report such a feature. The text seemed to start off with this expectation, and then say that it was pretty common after all. I still do not understand why your simulation is shows superrotation, whilst these other do not (not that you necessarily need to address this here)

v. The soil-moisture feedback analysis seemed to address a different question to elsewhere in the piece (about processes impacting variability within the NINO simulation, rather than differences between NINO MOD)

The following are more detailed comments about the different sections of the article.

CPD

7, C113–C120, 2011

Introduction

I found the use of references in the introduction a little lop-sided. There are several instances of many references being cited, but without really verifying the point made in a sentence with multiple stated facts. The very first sentence provides an example of this issue. It states that the Miocene and Pliocene were 2-3oC warmer globally despite small changes in CO2 levels, yet of the 6 citations 3 deal with CO2 variations, 2 with the climate of North America and one with the Pacific. None of these describe a global mean temperature change of 2-3oC, which, as a quantitative estimate, must come from somewhere.

Kurschner reference is actually 2008 (p201, l6)

No references for wetter-than-modern conditions in pre-Quaternary (p201, l20). So haven't really shown the "regional aridity paradox" exists.

Alter commas in "jet, data, theory and models" (p202 l1) as it reads awkwardly.

No discussion of either Molnar Cane (2007) or Bonham et al. (2009) on p202, l15.

Description of Barriero et al (2006) method is incorrect. They extended dateline SSTs.

You describe the methods of previous studies permanent El Nino studies in the introduction, but do not summarize any of their findings.

There is considerable repetition between the first 2 paragraphs on P203.

Method

The sentence referring to Joseph Nigam is confusing (p204, l8). I'm unsure whether realistic teleconnections in a coupled model with an "incorrect" ENSO signal really show that the atmospheric model is correct. The following sentence could also benefit from rewording.

The description of creation of the SST field is highly deficient. Low-pass filtering alone

CPD

7, C113–C120, 2011

will still give an interannually-varying SST field, with features of both El Nino and La Nina. You need at least one other sentence explaining how you go from this to an El Nino anomaly. I'm a little unsure whether you add a single anomaly throughout the year, or a monthly varying one that does not change from year to year (both could fit with p204, l22). Henceforth, I'll assume that it is a constant one. You haven't stated the source or time-period of your observed dataset.

I believe the anomaly is a global field, as it doesn't state otherwise. Please be explicit about this, because obviously the teleconnections will be differently represented. However, the two references for the Pliocene are solely on the Equator – comparison with a more global reconstruction would be relevant (e.g. PRISM or Brierley et al, 2009).

"as the" on p205, l8

p206, first paragraph. I think this point would be better shown with the inclusion of some observations to act as a truth (probably replacing the difference plot). As you state this is only an illustration, a single period may be sufficient.

Results

You state the observations give an SST change of 2-3oC (p206, l16), yet figure 1a shows values in excess of 5oC, which is twice as large rather than "in close agreement".

Please alter colorbar in Fig1a, so that each level has an individual color, rather than 3-5oC sharing orange.

Please be careful when stating numbers: 0.272oC seems to imply excessive accuracy, 0.27oC would do.

There is no need to refer to Fig1b three times in consecutive sentences (p207, ll15-22).

Superrotation

I found this section a little awkward to understand. Firstly, I had some issues with the text discussing the winds in NINO state, whilst the figure only shows anomalies from

CPD

7, C113–C120, 2011

a reference state that is not shown. The discussion seems to imply that superrotation should be expected from the inclusion of any SST anomaly, yet I was left uncertain as to why this should be the case. I know that simulations with minimal SST gradients (e.g. those of Barriero et al. , 2006) do not show superrotation. I think in part this confusion goes back to my uncertainty of NINO SST field, and whether your SSTs lead to an SST maximum in the central Pacific larger than that of the west Pacific warmpool, or simply a relaxation/removal of the zonal SST gradient along the Equator. One could naively think that the zonal wind anomalies in fig 3f might cancel on the zonal mean (note there is no scale for this panel). Yet again I wonder if showing the actual circulations rather than just anomalies might be helpful here.

Regional Results

I was surprised there was no mention of Canada in the discussion, considering the largest DJF anomalies exist there. Perhaps it is too close to the edge of the domain, but even that should be mentioned. Another reason for mentioning Canada is the impact of a permanent El Nino on ice sheet formation has been a focus of previous research (e.g. Huybers Molnar, 2007 and Brierley Fedorov, 2010)

This section reads rather like a description of the figures, some additional interpretation (that would not be immediately gleaned from looking at the figures) would improve the style of the section.

In Fig 4, the conventional abbreviation of Sept, Oct Nov is SON.

In Fig 5, the color bar has many more contours that color. Has a significance test been applied to the features on this figure, as there are large regions of white?

With both figure 4 and 5, I was uncertain what was gained by including five separate panels – do the paleobservations have this level of temporal resolution?

Stormtracks

P211, l5. This sentence states (VQ), yet the figures use V'Q'. Please incorporate the

CPD

7, C113–C120, 2011

equation here, rather than in the figure caption and be consistent in your labelling

The stormtrack analysis appears correct, but I was left wondering how this tied into the temperature and precipitation feature you had just described (especially Canadian winter warming). Perhaps you could move your discussion to here, before heading onto other features.

Regional scale circulation changes

It required effort to grasp the point you were trying to make with this section. A little bit of rewording and adding a summary at the end of the section would help with this. I think it would also benefit from only figures necessary to the story being shown - perhaps just fig 10.

Soil feedbacks

I was a little uncertain as to what this subsection was attempting to prove. Are you saying the NINO-MODERN anomalies are enhanced by soil moisture feedbacks? As you concentrate on anomalies within an individual simulation, I am unsure whether you have tackled this issue. Instead, I think you are testing whether soil-moisture feedbacks were stronger within the permanent El Nino climate.

I would expect land surface changes to enhance the NINO-MODERN difference, however I suspect the simulations are performed using prescribed vegetation/land surface properties, which would actually have changed over the past few millions years. One way to tackle this question, would be to use alter the land surface in the simulation or to find a way to switch off the soil-moisture feedback in the RegCM3 simulation (perhaps by using inputting the modern soil moisture values somehow).

P213, l26. “preceding the summertime” is unnecessary.

Does white in the figure indicate a lack of statistical significance? If so, state this. Also there are only 7 colors used in a colorbar with 14 boxes.

CPD

7, C113–C120, 2011

I didn't feel that I needed to see the precipitation lag correlations as well as the relative humidity; simply stating that the correlations were weaker would be sufficient.

Discussion

I didn't fully understand the arguments in the first paragraph of p217. Are you trying to retrospectively justify using the RegCM3? Perhaps a single additional panel of the NINO-MODERN JJA anomaly in the low-resolution model in figure5 will make this point more effectively.

I was surprised that the point discussed in the second paragraph of p217 had not been mentioned previously. Typically El Nino's are strongest in DJF, with much more moderate SST anomalies in the summer. I would therefore expect the strongest differences between your permanent El Nino forcing to occur in the summer. (I think the differences between impact of permanent El Nino in Barreiro et al. 2006 and the patterns shown in Huybers Molnar, 2007 arise from this difference in methodology).

The discussion is not the place to introduce the possibility of increased El Nino frequency, especially without any further mention or citations (p218, l24). I am not sure that you need to discuss this possibility at all.

Has the existence of a permanent El Nino actually been proposed for the Miocene? I am not certain that it has (if it has been, just include a citation).

Conclusions

P219, l21 please use "atmosphere models", as "climate models" implies coupled atmosphere-ocean models to me.

P219, l22. Please could you reword your clause about ENSO deviation? You have solely explored changes in the mean climate state and not discussed changes in climate variability. Although I do expect the two to be related, we do not yet know the nature of the relationship. I feel it important to recognize this distinction, especially in the conclusion.

CPD

7, C113–C120, 2011

Interactive comment on Clim. Past Discuss., 7, 199, 2011.