

Response to reviewers

We thank the reviewers for their positive and constructive reviews. We have two points which were questioned by all reviewers, which are addressed in the “General” section below and then proceed to answer the reviewers point by point.

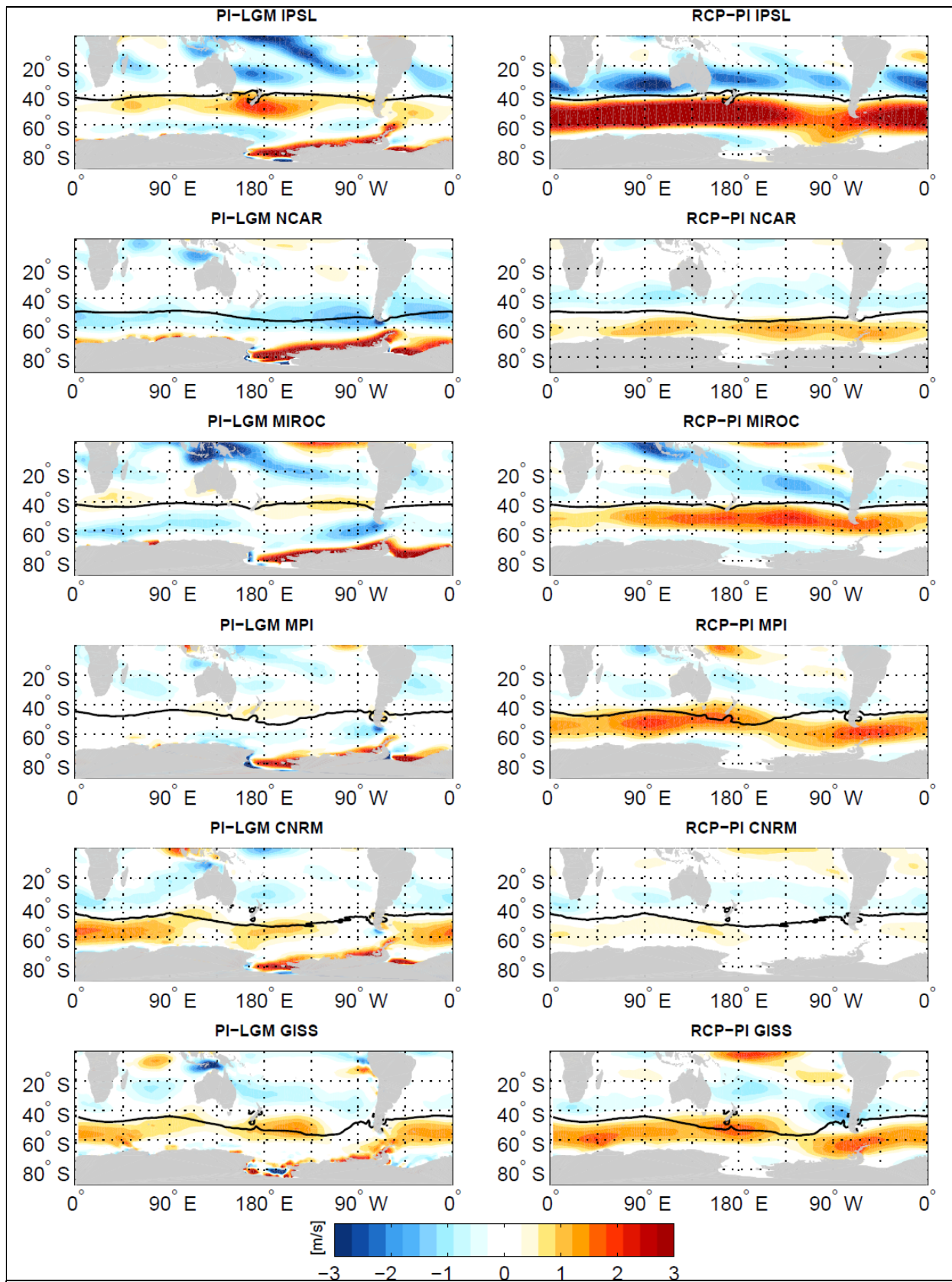
General:

A. Figure 2, wind changes.

Following suggestions by the reviewers, Figure 2 (showing maps of the future and past wind changes over the Southern ocean at 850 hPa) has been redone with the following changes:

- The blue / red lines (showing the mean jet position at the LGM and future, respectively) have been removed: they were hard to read and mostly added confusion.
- The color scale has been tightened, to make the anomalies for most models stand out more.
- Continents have been shaded with light gray, as a lot of the signal was due to spurious large anomalies linked to interpolation below the orography.

The new figure is shown below.



New Figure 2

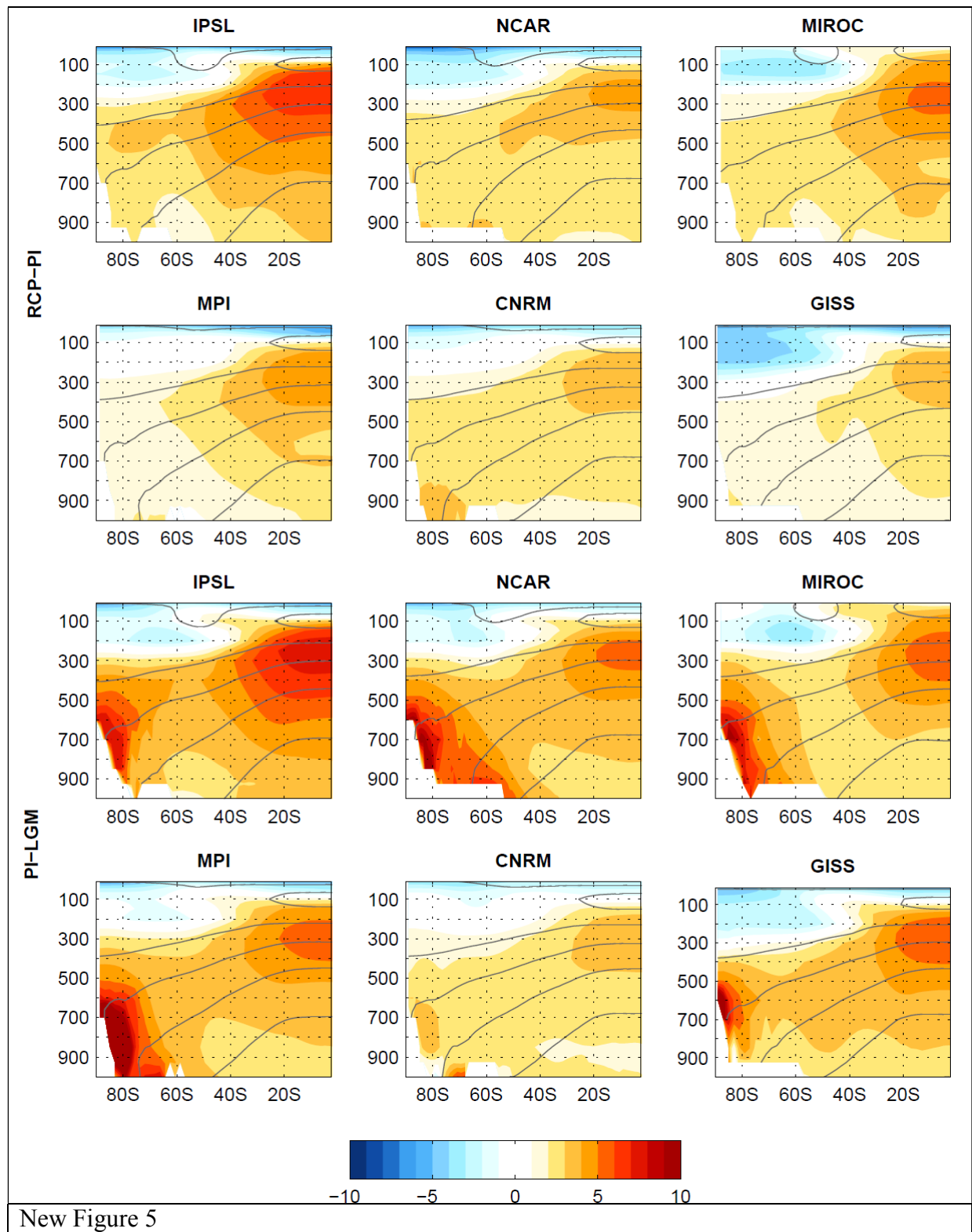
The position of the jet (latitude of maximum wind) was computed with a quadratic interpolation using grid points around the one of maximum mean wind. This has been precised in the text.

B. Mean temperature changes

Figure 5 (showing latitude-pressure cross-sections of the zonal-mean temperature changes) has been redrawn with the following changes:

- As asked by several reviewers, the panels have been reorganized to group future changes on top, and past ones at the bottom, instead of grouping by model.
- Grid points below orography are treated in an inconsistent way in the different model outputs, with some having missing data and others extrapolating from above. To apply a consistent treatment and plotting, we put all data at pressure levels below the local surface pressure to missing value.

This new figure 5 is shown below.



New Figure 5

Note that the latter treatment of missing values was already used for computing indices of temperature averaged over the polar or tropical regions, there is thus no change in these results.

Response to reviewer 2:

(1) p3696 l19. *The discussion of the Braconnot et al 2007 paper. What is meant by "discrepancies" in the models' response to LGM boundary conditions? Do you mean that there is a large amount of variability between the model responses? If so, I would describe it that way rather than there being discrepancies.*

This is in fact the discussion of the paper by Rojas et al (2009). Yes, we meant the models showed inconsistent responses and have modified the text accordingly.

(2) p3698 para 2. *I think this paper will be of interest to people, like myself, who are not so familiar with the palaeoclimate literature or the set up of the PMIP simulations. Since you are proposing that the behaviour of the ice sheets is key to the differences between model simulations I think it would be helpful here to have more of a discussion on what's going into the models, in particular in terms of the ice sheet. As far as I can tell, the ice sheet is prescribed? There is no dynamic ice sheet model in these simulations? So how then does the ice sheet end up being different in the different simulations? Is it to do with the interpolation onto the model grid and the horizontal resolution. Perhaps I am misunderstanding the PMIP website and there is indeed an ice sheet model, but either way I think a clarification of this in this paragraph would be helpful.*

In the PMIP3 experiments, the LGM ice-sheets are indeed prescribed to the climate models (which therefore do not include interactive ice-sheets). In practice, this means changing:

- 1- the land-sea mask, to account for changes in sea-level associated with the ice-sheets
- 2- changing the altitude of the continents and bathymetry for the oceans
- 3- changing the continental surface type to an "ice-sheet" type over the continents.

The way each group performs these changes varies according to the model characteristics, in particular whether the model is spectral or grid point. The final changes in altitude depend on the model type and its resolution. This is now described in the text.

(3) p3700 the section on jet stream definition. *I think some clarification of how the jet stream is defined is necessary. Is it simply the latitude of the maximum surface westerlies after interpolation onto a finer latitude grid or have you done some sort of quadratic fitting to the points around the jet maximum as in e.g. Kidston and Gerber (2010). It should also be clarified that you are looking at annual means?*

We used a very similar method to Kidston & Gerber, with quadratic fitting on the time- and zonal-mean jet (except at 850-hPa instead of 10-m). This was explicated in section 2.2. (see also our "general point" A, reviewer 1 had a similar point).

(4) Figure 2. *The degree symbols haven't turned out right.*

(5) Figure 2. *I'd suggest using a colour other than red for the right hand panels. It doesn't show up well for IPSL against the large zonal wind changes. It might also be helpful include more contours since it's hard to see the anomalies for CNRM and the blue anomalies for some of the other models.*

(6) Figure 2. *I'm a bit confused how you can end up with more than one jet latitude at a given longitude. e.g. for GISS the blue line seems to split and you have two jet latitudes from about 180E to 90W. The same is true for other models around New Zealand. What's going on there and how is this incorporated into your jet latitude differences?*

Figure 2 has been redrawn to remove these ambiguities: see main point A. Regarding the split line (point 6), this was because the time-mean wind can have several relative maxima at

different latitudes. This doesn't impact our computation of the mean jet, which uses the zonally-averaged wind that always has a single maximum.

(7) p3701 paragraph 2. I think some clarification of how you define the EOF is needed. This is calculated using the the whole years data? And it's the EOF of what? Zonal mean zonal wind on the 850hPa level? I assume this is the case from the caption of Fig 4 but I think this should be clarified in the text.

You assumed correctly: we used EOFs of month-to-month zonal-mean 850-hPa zonal wind. This precision was added in the text.

(8) Fig 4 caption. Suggest re-wording because at first glance it looks like this should be showing zonal wind on the 850hPa level. Perhaps something like. "Characteristics of the variability and response of zonal mean zonal wind on the 850hPa level for each of the models. Black lines: anomalies from 1sigma of the first (solid line) and second(dashed like) EOF of zonal mean zonal wind variability of the PI simulations.....etc"

(9) Table 1 caption: clarify here that these are the first and second EOFs of zonal mean zonal wind variability on the 850-hPa level.

Both captions have been rewritten and clarified.

(10) p3702 116-17: IPSL doesn't look all that different from NCAR, MIROC and MPI at high latitudes. Is it more the warming from 60S-70S at 900-1000hPa that you are referring to? If so, this should be stated explicitly in the text. People will be drawn to looking at what is going on over Antarctic and I think they will not agree that IPSL is behaving differently from those models. e.g. over the Antarctic continent IPSL and MIROC look very similar

IPSL (and GISS) are somewhat similar to other models in their strong PI-LMG warming over Antarctica; however this warming seems more confined to polar latitudes over Antarctica and does not extend as much over the Southern Ocean latitudes. This may explain a lower influence on the jet (too remote).

The paragraph has been modified to reflect this idea.

(11) Figure 5. Firstly it would be much easier to compare the models if you group the plots by scenario rather than by model. Secondly, there appears to be some inconsistency in how the Antarctic topography is dealt with and since you are concerned with temperatures over Antarctica I think this needs to be clarified. Four of the models (IPSL, NCAR, MIROC and GISS) appear to have NaN's under the Antarctic surface and so there won't be any temperature anomalies from e.g. 600hPa-1000hPa and 80S-Pole included in the temperature anomalies for fig 6. But, MPI and CNRM don't have NaN's under the Antarctic surface and so it appears that some different form of pressure level interpolation has been done. I'm a bit concerned about this since e.g. for MPI, you might be including large temperature anomalies in the high latitude lower troposphere that are actually below the Antarctic surface. Are MPI and CNRM on model levels? This should be checked over I think and it should be made sure that each of the models are being dealt with in the same way. Also, is the averaging done by pressure weighting or not? I'm not sure it matters either way, but if it is pressure weighted then this should be stated.

Figure 5 has been modified, to regroup the plots by scenario and to address the missing values issue (see main point B.) The treatment of missing values was already correct for the computation of temperature indices.

For the last question, averaging is done by pressure and area averaging for the temperature indices (as stated in the text), but it makes little difference (even area-averaging).

(12) Figure 6. What do the filled green and blue squares mean? Also, it should be clarified in the caption that the blue symbols are only accompanied by their model names in the left panel e.g. "The blue symbols (accompanied by model names in the left panel) are the PI-LGM results.

The filled squares are additional versions of the IPSL model (in squares for PI-LGM and RCP-PI). This has been added in the caption.

(13) p3705 l16. I would suggest softening the wording here a bit. It is true that comparing the different models gives you an indication of the factors that are important. But, it still doesn't unambiguously identify the causality e.g. maybe the Antarctic or equatorial temperatures are different because the jet shift is different. And there are also other ways in which you can test which factor is important within a single model e.g. running simulations with and without changes in the Antarctic ice sheet.

the "most probably" wording was changed to "probably", although we have complementary results that suggest otherwise!

All the typos and wording remarks have been corrected.