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Climate of the Past Discussions

Interactive comment on "Teleconnection between the climates of the Polar Regions during the last ice age" *by* Xiao Yang and Jose A. Rial

Anonymous Referee #1

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Yang and Rial compare three conceptual mathematical models of the bipolar coupling of millennial-scale climate variability, i.e. the relationship between Greenland Dansgaard-Oeschger (D-O) and Antarctic Isotopic Maxima (AIM) events. They use published ice core water stable isotope data (NGRIP and EDC) on previously established chronologies. They compare the transfer functions for each of these models, and evaluate how well the models can predict the Antarctic record given the Greenland record (G-to-A), and vice versa, how well the models can predict the Greenland record given the Antarctic record (A-to-G). They find that all three models are essentially equivalent for the G-to-A case, and that the PhaseSync method works best for the A-to-G case.

Based on this the authors argue that the PhaseSync method is the best framework to

describe the bipolar coupling, a conclusion with which I disagree (for reasons outlined below). Overall, the study does not contribute much to the literature, given that it is basically a (not very comprehensive) review of published models and no new dynamical insights are presented. One gets the feeling that the authors were aiming at the so-called "least publishable unit". I make a suggestion below on a natural way in which the study could be extended that provides more value to the literature.

My main criticism is that the conclusion that the PhaseSync method is superior over the I/D and TBS models is not valid.

First, this conclusion is based purely on the A-to-G test; in the G-to-A test, the I/D and TBS models actually score better. A recent paper by the WAIS Divide Project Members (2015), has clearly demonstrated the N-to-S directionality of the teleconnection. Coupled atmosphere-ocean GCMs clearly reproduce the SH signals after applying NH perturbations to the AMOC (Pedro et al., 2016). Since there is data- and model-based evidence that the teleconnection has a N-to-S directionality, it is obvious that G-to-A (and not A-to-G) is the most relevant test for these models.

The I/D and TBS models were designed around the assumption of a N-to-S teleconnection, and therefore Yang and Rial test these models outside of the scope intended by their authors.

Second, the PhaseSync model may perform well at the A-to-G test, to my understanding it is just a mathematical construct without a firm basis in climate dynamics. In the PhaseSync model, each polar region represents an independent climate oscillator, which are then synchronized like pendulum clocks. This implies that the oscillations would be present also in the absence of the teleconnection – simply not in a synchronized way. It seems unlikely to me that Greenland temperature could fluctuate by > 10 degree C in the absence of the coupled sea ice and AMOC perturbations (i.e. the teleconnection mechanism). I have not seen any realistic description of what these uncoupled oscillators are. By contrast, there is a wide range of paleoceanographic ev-

idence linking the D-O cycle to variations in Atlantic circulation (Lynch-Stieglitz, 2017), which is the physical basis/justification of the I/D and TBD models (which are identical for most practical purposes, as the authors also suggest). The I/D and TBD concepts are further well simulated in climate models (Ganopolski and Rahmstorf, 2001; Pedro et al., 2016; Pedro et al., 2018; Schmittner et al., 2003). This wide gap in supporting evidence should be acknowledged, and weighed in the evaluation of what model is most realistic.

As I mentioned, the paper in its current form does not have much new to offer to the paleoclimate community. The observation that the PhaseSync model is superior in the A-to-G reconstruction does open the opportunity to redo the analysis of (Barker et al., 2011), and provide an improved 800,000 year reconstruction of Greenland climate. Such a reconstruction would be a valuable product, that naturally extends from the presented work. The long-term orbital component would have to be added to the millennial component, but that should be straightforward.

Throughout the manuscript would benefit from editing for grammar and sentence structure.

Detailed comments:

P1L23: Note that Broecker 1998 is talking about a different type of seesaw, one in deepwater formation.

P2L2: Note that the I/D model also suggests a pi/2 phase shift, and so this observation should not be solely accredited to Oh et al (2014), as many other studies have precedence.

P2L5-10 and elsewhere: Please elaborate on what the individual oscillators would be without the teleconnection. And what physical mechanisms are responsible for the coupling? Simply stating “ocean and atmospheric teleconnections” is too vague.

P3L4: these are not the correct references for this statement. See some of the refer-

ences given above are more suitable.

P4L25: note the similarity between the I/D and TBD models, and the fact that they are well supported by sediment records and climate models.

P6 section 2.3. There is a big misunderstanding here. The authors argue that previous studies were hampered by using N-S synchronization, whereas this uncertainty is eliminated by using the AICC2012 chronology for both cores. However, the AICC2012 chronology is constructed via N-S synchronization! So the same uncertainties apply. Please rewrite this section. Also, the WAIS Divide ice core is much more accurately synchronized to Greenland, and would provide the better data set to test these ideas on.

P8L7: Note that the TBD model also gives a pi/2 phase shift at the relevant frequencies ($\sim$1ka).

P8 section 3.3: note that the A-to-G test is not relevant for testing the models, as the data are clear that the teleconnection operates N-to-S

P9L9: Buizert et al. (2015) should be cited as WAIS Divide Project Members (2015)

P11L3-9: another, more traditional explanation for the great Antarctic warmth during Heinrich stadials, is that these stadials are lengthened by iceberg delivered freshwater, allowing more heat to build up in the SH via the BPS.

P11L19-25: Note that there is a lot of ocean sediment and climate model evidence for the TBS and AMOC variations on which it rests, such evidence is currently lacking for the PhaseSync.

P128: “suggest potential physical coupling mechanisms”. Which ones? These are missing from the theory, as far as I can tell.

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