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Interactive comment on "Early westward flow across the Tasman Gateway" *by* W. P. Sijp et al.

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Response to Reviewer 1 for "Early westward flow across the Tasman Gateway" by Sijp et al.

The reviewer comments are in *italic*.

Sijp et al have conducted some middle-to-late Eocene modeling sensitivity studies, primarily with the UVIC model to study how flow direction through the Tasman Gateway changes as various southern ocean gateway parameters are changed. This is a follow on to an extensive set of papers recently published by Sijp et al with the same model,

and by others with other models, although motivated in this particular case by an interpretation of dinoflagellate cyst biogeographic patterns.

Contrary to the reviewer suggestion, our study focuses on the early Tasmanian Gateway opening of the early Eocene, not the middle-to-late Eocene as stated by the reviewer. The manuscript represents a first effort to use numerical modeling to reconstruct early Eocene shallow opening of the Tasmanian Gateway. As such, it is not a follow on of Sijp's previous work on the Eocene-Oligocene transition (EOT), which dealt with Antarctic glaciation and a deepening of the Southern Ocean gateways in a completely different time period with a different research question and boundary conditions.

There are now dozens of papers exploring sensitivity of climate to southern ocean gateway changes, and within the past couple of years there has been a resurgence of interest in such studies, although generally speaking they all produce similar and convergent results when it comes to the circulation patterns and climate responses. And actually, this paper produces the same results as the others as well, in terms of those variables, and the modelling itself is weaker, so I guess the only reason to think of a study like this as novel is if the resolution of the dinoflagellate story is both important and convincing. I do not see evidence of that.

The "dozens of papers" the reviewer refers to without further detail, presumably concern the emergence of the ACC upon the deepening of the Southern Ocean gateways at the late Eocene. Our manuscript focuses on a time slice 15 million years earlier, when the Tasmanian Gateway first opened to a very shallow depth, and was located further south. As such, it has a completely different setup of boundary conditions and has a different research question. Unlike the reviewer suggests, it therefore produces a completely different oceanographic result compared to studies on the Late Eocene deepening of the Gateway. We try to understand the emergence of a westward current upon the early opening of a shallow connection through the Tasmanian Gateway, opposite to an eastward flowing ACC. This surprising oceanographic consequence of gateway opening was suggested from microplankton fossil evidence by Bijl et al. (2013), but not tested with numerical modeling experiments. We are not aware of any other modelling studies on this counter-current, and so the reviewer's statement that our main results is the same as the "dozens of previous papers" is manifestly false. Our numerical modeling results confirm the biogeographic patterns seen in dinoflagellate cyst assemblages, and provides the first numerical proof of oceanographic changes seen in early Eocene Southern Ocean microfossil assemblages.

Because of various weaknesses, but most importantly because I do not see this paper as novel or answering a well formulated and important question, I do not recommend publication.

In contrast to this reviewer, reviewer 2 clearly picked up on the intent of our paper, its time frame in particular, nicely summarised this in the first few lines of review 2, and finds this "a good example of applying paleoclimate model simulations to evaluate a hypothesis based on empirical paleoclimate data", finds that it adds to our understanding of Eocene climatic change due to ocean gateways, and "found the results and analysis to be sound and well described". Nonetheless, in response to this comment by reviewer 1, we clarify the intent of our study by changing the 3rd line of the abstract:

"Recent micropaleontological studies suggest the onset of throughflow of surface waters from the SW Pacific into the Australo-AntarcticGulf through a southern shallowopening of the Tasman Gateway from 49-50 Ma onwards."

to:

"Recent micropaleontological studies suggest the onset of westward throughflow of surface waters from the SW Pacific into the Australo-AntarcticGulf through a southern shallowopening of the Tasman Gateway from 49-50 Ma onwards, a direction that is counter to the present day eastward flowing Antarctic Circumpolar Current."

Main Weaknesses. Winds are not prognostic in the UVIC model and most of the results will depend sensitivity on the wind stress (curls). This seems to be a huge weakness. Simulations are conducted in which continents are shifted 6 degrees south, but as far as I can tell, the wind fields are fixed. What kind of sense does that make?

Of course we are aware that the UVIC model has no fully coupled ocean-atmosphere. We chose this model because for we required a model with high enough spatial resolution to represent the shallow, southern opening of the Tasmanian Gateway during the early Eocene, and long enough integration times to equilibrate the deep ocean to examine a possible global temperature signal. To provide backup for the realistic setup of our model and cover both uncertainty in the location of the wind fields, and their possible changes, our study prominently features experiments where the winds have been shifted, precisely to test robustness of the results and cover the changes a fully coupled system might exhibit. Wind shift experiments are shown in Fig. 1b, Fig. 2b and Fig. 3b. Also, the POP model shown in Figs 8 and 9 employs yet a different wind field to the UVic model, providing a further sensitivity experiment with respect to differences in wind, geometry and resolution. Our simulations show that the presence of a westward current near Antarctica is remarkably robust, and is strongly determined by Southern Ocean geography.

Also, feedbacks between ocean circulation and the atmospheric circulation are impossible, so I do not have confidence that the results, even as presented, would be reproduced in a truly coupled model.

Our wind shift experiments cover a range of possible wind feedback responses, and indicate that our results are robust. Furthermore, a precise reconstruction of the ocean is not the goal of our study, and some would argue of ocean modelling more generally. Rather, the model is intended to uncover and understand plausible mechanisms and conditions that allow a westward current to occur, and be consistent with very specific distribution patterns of dinoflagellate species.

Also, how might all this change as ice sheets grow, wax and wane over Antarctica, might this not be an easier explanation for the observed dino patterns?

Our study concerns the early Eocene (see above), a time without Antarctic ice sheets. See, e.g., Pross et al., 2012, Nature, for a reconstruction of tropical vegetation for the early Eocene, including Antarctic coastal air temperature reconstructions, Huber and Cabballero, 2011 Climate of the Past, and Bijl et al., 2009, Nature for temperature reconstructions for the early Eocene.

There is no explicit comparison with the dinoflagellate cyst biogeographic patterns, instead passive tracer trajectories are calculated and described in terms of their fit with paleogeographic patterns. This is related to the fact that the underlying conceptual model for these patterns is in question. The paper does not address the fact that the conceptual model explaining dino patterns in this region in the past, presented in Huber et al., 2004 is rather in conflict with that in Bijl et al., 2011. The former relies on cold temperature in a 'cold trap' in the Ross Sea, whereas the latter utilizes a

local temperature record to infer that environmental conditions had little to do with the interpreting biogeographic patterns. If the original conceptual model is used how does that affect the interpretation?

The Reviewer here poses a false claim again. Bijl et al., 2011 does not conclude that SST does not drive dinocyst biogeographic patterns. What Bijl et al., 2011 questions is that the onset of regional proliferation of endemism was forced by SST, because endemic dinocysts become abundant at peak warmth. Once the endemic community is set, SST is the dominant driver of the abundance of endemic dinocysts. Views on a potential mechanism for the dinocyst patterns have been progressing away from the cold trap idea by Huber et al., 2004 over the years, because later proxy data for SST (Bijl et al., 2011, 2013; Hollis et al., 2009; 2012; 2014) suggested the entire Proto-Ross gyre is equally warm, which makes it difficult to think of a cold trap. As yet, the exact mechanism is unresolved, but we do note a clear relationship between dinocyst biogeographic patterns and specific ocean currents, both in the past and in the present-day oceans. This justifies our use of a passive tracer in our experiments.

On a more technical level, how does variability fit in with the trajectories? Advection across streamlines is primarily by ocean eddies or by non-steady atmospheric forcing, how are those handled in these simulations?

The bulk effect of ocean eddies and non-steady atmospheric forcing on tracers, including our passive tracers, are handled by the isopycnal flattening effect of the Gent McWilliams parameterisation and along-isopycnal Redi-diffusion throughout the ocean, and horizontal diffusion in the surface layers. As stated in the Model Description section, the model also employs the turbulent kinetic energy scheme of Blanke and Delecluse (1993) based on Gaspar et al. (1990), which models vertical mixing due to wind and vertical velocity shear. However, in our model, as in the real ocean, tracer distributions are significantly influenced by the large-scale circulation features, such as ocean gyres and, if present, the Antarctic Circumpolar Current.

On balance I am not convinced that the results presented are robust or correct. And even worse, as this sentence indicates "Finally, our numerical study is not consistent with the idea that such an oceanographic change can cause a significant and uniform Antarctic cooling", which of course is the main reason that people work on this problem. So the results are equivocal and without much importance even if true?

The mention of other research into this topic, and ice sheets further above, again suggests that the reviewer is thinking about the Eocene-Oligocene transition, indeed a widely researched topic where Antarctic temperature changes are of significant interest. In contrast, links between the more moderate Antarctic temperature changes of the mid Eocene and Southern Ocean gateway changes, as first proposed in Bijl et al. (2013), are much more tenuous (see also Pross et al., 2012). Unlike studies of the 15 million year later Antarctic glaciation, the focus here is on the early westward Antarctic Counter-Current, a rather enigmatic feature proposed in Bijl et al. (2013). This, and its relationship to biogeography, have not been researched before, We find no significant impact on Antarctic temperatures of this counter-current, much so in line with proxy evidence.