Comment on cp-2022-7

Michiel Baatsen (Referee)

Referee comment on "The sensitivity of the Eocene–Oligocene Southern Ocean to the strength and position of wind stress" by Qianjiang Xing et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2022-7-RC1, 2022

The authors present the results of eddy-permitting ocean-only simulations to shed light on the development of a proto-ACC around the Eocene-Oligocene Transition. In contrast to most earlier work, they provide a substantial increase in model resolution and a more realistic set of model configurations to represent the effects of a gradual gateway opening. A very detailed analysis is made of the momentum balance and the overall results are of great value to understand the role of Southern Ocean Gateways in much of the Cenozoic climate. Regardless, the manuscript still needs work to clearly present/explain the scientific set-up of the experiments, their motivation, the results and their implications. The manuscript is generally well written and mostly free of errors, especially the figures should be adjusted to improve readability and clarity.

Main comments:

- I am missing additional background, motivation and some explanation of the choices for wind stress profiles/latitudes (other than referencing to Scher et al 2015).
- It is not always clear which questions are asked, what the hypotheses are and how they are answered.
  There is an extensive introduction and study of the different components in the momentum balance, but it is tough to see the role these play in the larger picture. Especially the part on bottom form stresses is rather tedious to read and does not seem to answer many questions. In general, the manuscript is quite lengthy and lacking some clear structure/connections to see the overall story. It may therefore be better to focus on some specific topics, rather than treating all aspects in such detail.
- Using an ocean-only model is a big limitation, especially regarding the feedback between temperatures and wind stress as well as missing the atmospheric component of meridional heat transports. As they are restored to fixed distributions (of which the treatment and implementation could use some more explaining), sea surface temperatre and salinity fields are challenging to interpret and one should be careful drawing conclusions from these.
- Decreasing SSTs in the Southern Ocean are said to be an indicator of the thermal

isolation of Antarctica.

I am missing a clear explanation how those SSTs would be representative of temperatures on the Antarctic continent. Apart from a single figure showing SSTs, I am missing an assessment of meridional heat transports and how these would be linked to Antarctic temperatures altogether. What happens in the ocean is no doubt interesting and relevant, but statements regarding Antarctic temperatures as a whole are not well supported by the results presented here.

- I am missing a section on the geographical configurations used, as shown in Figure 1, motivating the different time intervals and explaining the different Gateway configurations. This makes it hard to interpret many of the results.

Specific comments:

- L53: maybe trivial, but good to specify that this is for the present configuration
- L57: are there any more recent observational-based estimates of ACC strength?
- L60: I see you refer to figure 1 here, but this is mainly showing the applied wind stress forcing. I don't mind this reference, but it seems more intuitive to move this figure further down.
- L100: This was already greatly improved in more recent model efforts, see e.g. Hutchinson et al. 2018, Kennedy-Asser et al. 2020, Baatsen et al. 2020. The latter shows a comparison to the Huber et al 2004 results, with subtropical waters reaching much of East Antarctica in the newer CESM simulations.
- L116: This is a very nice paper, you could also consider referring to Viebahn et al. (2016) who did a similar experiment (although using PD bathymetry) comparing HR/LR simulations.
- L129: Baatsen et al. 2020 already find a ~45Sv Tasmanian Gateway Transport with relatively shallow (500-1000) TG and DP, see supp. Figure 3.
- L145: I would expect westerly winds to be aligned with the polar front, do the authors refer to the oceanic (i.e. SST) front? Does this infer a mismatch between atmospheric/oceanic polar fronts?
- L162: It is unclear to me from the text what exactly is meant by eddy-saturation and what it results to.
- L174: Does 0.2N/m^2 agree with observed wind stress across the Southern Ocean? If so, is there an explanation for the underestimation of ACC strength in these idealised simulations?
- L200: I expect this also depends on the depth of the bathymetry ridge?
- L225: Is 0.25deg resolution sufficient to be eddy-permitting across the region of interest? For this, I would like to see a comparison of e.g. the local Rossby radius of deformation and model resolution.
  See e.g. LaCasce and Groekamp 2020, who show a first surface mode deformation scale of 10-20km at 60S in the present-day ocean.
- L238: a visual representation of the model domain, resolution and boundary conditions would be very helpful here.
- L265: Does this mean the authors use a continental slope from paleogeographic reconstructions, or present-day observations? Especially the East-Antarctic margin (both extent and slope) has changed significantly since the Eocene
- L288: I believe this is a very nice overview of possibilities, but I am struggling to find the motivation for these choices. Are there any simulations or theoretical considerations that motivate the applied shifts in max wind stress latitude?
- L295: I am missing some information here on how well equilibrated the simulations are after 45 years, especially regarding the zonal transports and isopycnal slopes through

TG/DP

- L329: Does this mean v=0 is applied at the northern boundary of the domain? This would imply the complete absence of a meridional overturning cell extending beyond the model domain.
- L379: Also here, there are quite a few more recent simulations showing this
- L409: 'higher/lower' is a bit ambiguous, especially on the Southern Hemisphere, consider using 'equatorward/poleward' instead.
- L460: At 300m, the relative strengthening of TG transport is larger with the more southerly wind stress max. This is reversed and much less sensitive to wind stress latitude at 1500m, can you explain why?
- L484: Is there also a formal definition of what you refer to as 'nearly homogeneous'? Why does this separate the thermal wind component from the bottom slope contribution? A non-uniform bottom slope would imply zonal flow throughout the column.
- L508: The DP transport is substantially weaker than the TG one (this is also the case in Baatsen et al. 2020, using a similar paleobathymetry). What does this imply for a possible proto-ACC?
- L527: I would re-phrase this sentence and not refer to specific colours shown in a Figure quite a ways back at this point.
- L540: Could you also suggest how?
- L665-689: This part seems fit better in the introduction rather than discussion?
- L707: If I understand correctly, the bathymetry used here changes both depth and latitude of the gateways at the same time.
  It may therefore be also the latitude offset between TG vs DP that may be just as important as their depth. I am missing this in the discussion.
- L750: There are some very important nuances and limitations listed here, which would deserve more attention in the discussion but also up front.
- L780: you already explain the concept of eddy saturation in the introduction. Most of the remaining part of this paragraph is a repetition of what is said earlier.

Figures:

- Fig1: the contrast between title and axes font size is quite large, it would seem right to adjust those somewhat.
  Although rather straightforward, the axes are missing labels/units as well. The figure now has a rather extensive caption, this could be shortened considerably by putting some of this information into legends/labels
- Fig2: overall font size is very small in these figures, consider increasing these. The choice of colours is also not optimal, especially to people coping with mild colour blindness.
  Could you explain why the wind stress patterns deviate from those shown in figure 1, is this due to land/sea distribution?
- Fig3: this is a nice overview of the different stream function patterns. As the figure mostly shows the extent/strength of the different gyres, I would redefine the reference value of the stream function.
  As shown now, especially the cases with stronger zonal flow are hard to compare as the entire stream function simply becomes more negative. Since you already define the ST/SP gyre boundary, why not use the zonally averaged BSF value as 0 reference?
- Fig4: The quality of this figure needs to be improved, especially the arrows are unclear and missing a reference . Like figure 4, the entire figure seems to be squeezed vertically, so I would suggest to change the original aspect ratio.

- Fig5: It is interesting to see that the bottom flow component is small, especially with the deeper gateway configurations.
  To interpret this, it would be very helpful to show the zonally averaged depth profile of these gateways as used in the model.
- Fig7: this figure is very hard to read due to font and panel sizes, consider e.g. putting 3 cases in a single panel instead.
- Fig8: same remarks as for Figure 2.

Technical comments/typo's:

- L105: pre-Eocene; do the authors mean the Paleocene or is this a typo?
- L357: Is the atmospheric pressure (gradient?) zero, or is it negligible compared to the pressure gradients in the ocean?
- L405: 'Here our'
- L421: southward?
- L766: double bracket not needed?
- L778: 'due to as'