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Interactive comment on "The Eocene-Oligocene transition: a review of marine and terrestrial proxy data, models and model-data comparisons" *by* David K. Hutchinson et al.

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We thank the reviewer for their thoughtful and constructive comments on the manuscript. Here we outline our proposed response to each comment in blue text.

Kind Regards,
David Hutchinson

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General Comments

This paper provides a comprehensive review of the current state of understanding of climate change during the Eocene-Oligocene transition, and in the process, attempts to assess the cause of this change, CO2 versus paleogeography, the added role of ice. Published observations from marine and terrestrial archives are compiled and compared against climate simulations from a collection of modeling studies (i.e., a model inter-comparison). The observations include SAT (SST), SSS, continental ice extent, sea-ice and ocean circulation. The comparisons are for 2 broad intervals, late Eocene and early Oligocene. Presumably, the observations are binned over long (1-4 my?) windows. For the modeling studies, the boundary conditions, including ice sheets, are generally similar though not identical given the lack of coordination, and each of the models are run to equilibrium (thousands of years). The study looks at the simulated responses to changes in paleogeography (i.e., gateways), GHG levels, and Antarctica ice-sheets. For GHG levels, 900 ppm (just above the threshold for ice accumulation on Antarctica,) is used at the Eocene pCO2 and 560 ppm for the Oligocene. Because the published model experiments were not coordinated and thus run with a range of CO2, for comparison the output of some models are scaled to approximate the same ΔpCO2. The equilibrium climate states for each model and an ensemble are then assessed for a best fit with observations. Given differences in resolution and other parameters, the absolute T in the models vary widely, with a cool group and warm group, so the focus is primarily on Δ SAT. In general, most of the models are showing $\sim 2°$C cooling in mean global SAT (figure 5). There are minor regional discrepancies which are attributed primarily to differences in ocean mixing/heat transport. In the end, the ensemble is deemed to show a good fit with observations supporting the conclusion that a reduction in atmospheric CO2 was the primary driver of the EOT.

Thank you for the overall assessment and constructive comments on improving

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the manuscript.

I have mixed feelings about this paper. Clearly a considerable amount of time and effort went into compiling the observations, the synthesis of modeling work. This alone will be a valuable contribution to the EOT literature. However, I am not convinced that the modeling comparison is a useful exercise, at least not in the way it was intended or designed.

We have edited the model-data comparison to better acknowledge the limitations and uncertainties that were previously missing from this section. See further discussion below.

The main issue concerns the finding/conclusions about the forcing behind the transition, specifically a ∼halving of pCO2 (from 900 to 560 ppmv) with the EOT. To my knowledge, based on observations (see figure 5) or theory, there is no basis for a 40 to 50% reduction in atmospheric CO2 across this transition. The existing B isotope data, albeit sparse, even suggests a slight increase, and the alkenones suggest a decline but not nearly of that magnitude. More importantly, just from a purely theoretical perspective, there is no reason to expect such a large decline. Recall that when the first detailed, high resolution benthic O isotope records were produced, it became clear that the EOT, or at least the appearance of continental ice-sheets on Antarctica was relatively abrupt, consistent with the threshold hypothesis; a relatively small drop in GHG would be sufficient to trigger the rapid accumulation of ice on a polar continent (i.e., climatic threshold/tipping point, e.g., Crowley and North, 1988). This concept was reinforced by the ice-sheet modeling of DeConto and Pollard 2004 which demonstrated how the local albedo feedback on summertime T could accelerate ice accumulation. Granted that by today's standards the ice-sheet model of the D/P study was relatively coarse and simplistic, but the general theory of a bifurcation point in the climate system still seems valid. We can debate the exact magnitude of the

CO2 drop required, but it was probably small (∼100 ppm), especially with the proper orbital configuration. And even with the large uncertainties, the CO2 proxies are consistent with this hypothesis. This is the most compelling and important aspect of the EOT, a relatively large change in climate in response to a relatively small change in forcing. The observations of a few degrees of cooling, switches in the mode of ocean circulation are consistent with this hypothesis. The Goldner et al (2014) paper nicely illustrates the regional/global effects on ocean T of just adding the ice-sheet (w fixed pCO2). Also, why such a rapid and large reduction in pCO2 at that time? Positive feedbacks involving biogeochemical cycles, ocean uptake, could potentially draw down CO2 but the effect would likely be relatively small, <100 ppm, as suggested by a variety of modeling studies (& observations). More than likely, the decline in pCO2 from the latest Eocene to earliest Oligocene was probably minor.

The reviewer raises some important points here regarding the conclusions drawn from the modelling work. We have considered these carefully and agree that the interpretation of the model-data comparison needs to be changed to acknowledge the limitations and uncertainties of the modelling approach. It was not our intention to suggest that the models are entirely correct and that it was a 350 ppm drop in CO2 that caused the climate transition, but can see that the reader could think that from the way it was written. In order to address the reviewer's comments, we now include a discussion of the caveats; including the mismatch between observations and models, implications for model sensitivity and feedbacks, and recommendations for setting up future experiments. We have also updated the conclusion and the abstract to reflect these changes.

Regarding the CO2 change, the multi-proxy compilation of Foster et al. (2017) suggests 'best-fit' CO2 estimates of 893 ppm and 806 ppm for our late Eocene and early Oligocene windows respectively, or a drop of 10%. We have now acknowledged in the manuscript that our scaled estimate of CO2 change is larger than this best-fit estimate, however we note that there are some CO2 proxies that are consistent with such a change. For example, the alkenone records of Pagani et al. (2011) are consistent with a decrease of 300 ppm within a 1 Myr window of the EOT. The boron and stomatal records are more equivocal, showing that a drop of 200-300 ppm is plausible, though this can be considered an ‘end-member’ rather than a ‘most probable’ change.

The point about crossing a threshold of glaciation is an important limitation on the models used. These models must prescribe an ice sheet to be on or off, with no scope for dynamic feedbacks when crossing a glaciation threshold. We have added further comments here about the limitation in the ice sheet components of the models. However, it is still useful to compare in our ensemble: which forcing mechanisms provide the best explanation of the proxy temperature change, and how should each be relatively weighted. One clear result from our ensemble study is that the far-field temperature change (i.e. much of the global delta T at the EOT), is much better explained by CO2 forcing than by imposing an ice sheet or paleogeographic changes. This highlights the fact that a global cooling mechanism is needed, whether from CO2 or other feedback mechanisms, and such a change is not easily triggered by the gateway or ice sheet forcing experiments.

The results of Goldner et al. (2014), included in our ensemble, also bear this out: they show a large change in Antarctic and Southern Ocean surface temperatures, but little change in the global mean. However, those results do show a cooling of the deep ocean (since the deep ocean waters are sourced from Southern Ocean sinking), which could help to explain some of the benthic temperature proxy trends. Since the models are in general not fully equilibrated, we cannot undertake a model ensemble assessment of benthic temperature changes.

With all this in mind, the fact that the model ensemble is in agreement with observations is problematic. In other words, to match climate observations, a much larger

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change in forcing (1.6x) is required than justified by observations or theory. This is the same recurring issue with simulating high CO2 climates of the past, that a much larger forcing is required than justified by observations? The bottom line is that the models are under sensitive to GHG forcing. Arguably, this should be one of the main conclusions of this paper. As the paper is currently written, I almost get the opposite sense. Has this paper achieved the stated goal of identifying what drove the EOT, at least the abrupt appearance of ice-sheets ∼34 Mya? Am sure we can all agree that over the long-term, a reduction in GHG was the primary driver of Eocene cooling and key to triggering Antarctic glaciation. What we won't agree on are the specifics of timing and magnitude, the appropriate alignment of GHG forcing with the climate response.

It is true that some of the simulations appear to be under-sensitive to CO2 forcing when reconstructing Eocene climates. The climate sensitivity to doubling CO2 implied by this ensemble is 3.3°C, which may be lower than necessary to fit to the Eocene proxy record, but not radically so. A recent synthesis of climate sensitivity estimates based on the DeepMIP proxy ensemble suggests that climate sensitivity during the latest Paleocene, PETM and EECO was 4.5°C, 3.6°C and 3.1°C respectively (Inglis et al., 2020), with 66% confidence intervals ranging from 2-7°C during the three intervals combined. Thus, while there is scope for a higher climate sensitivity in the models, the current best estimates are not much higher than the ensemble mean climate sensitivity.

A large component of the Eocene warmth (compared to present day) can be attributed to non-GHG changes, including paleogeographic changes, vegetation, soil and albedo effects. These effects are illustrated more fully in the DeepMIP model ensemble of the early Eocene (Lunt et al., 2020). This shows that stronger GHG forcing is just one element that is needed to properly solve the model-data mismatch in the Eocene (or Oligocene). We have added further discussion to Section 7.2.3 to address these issues.

Recommendation: A key question for revision - what is the real purpose of this paper? If it's simply to provide a comprehensive review of the existing literature on observations and modeling of the EOT, recommendations for future research, minor/moderate revision (see comments below) would suffice. For the reasons stated above, the data model comparison (section 7) could be dropped. If retained, it would be essential to include a discussion of the caveats; the aforementioned mismatch between observations/models, implications for model sensitivity and feedbacks, and recommendations for setting up future experiments.

We have chosen to retain section 7, and as noted above, we have revised the interpretations to clarify the issues raised here. In line with the reviewer's comments, we have acknowledged the need for missing feedbacks to enable the global cooling signal seen in the proxies.

Additional Comments/Recommendations:

Window of Observations (Apples vs. Oranges); Considerable effort is spent in defining the duration of the EOT, "Hence the stratigraphic interval of the EOT according to our preferred definition is now given an estimated duration of 790 kyr (fig 1)". The problem is that the collected observations (Figures 3-5) span a much wider range of time, millions of years of the late Eocene/early Oligocene. I assume this is by necessity, especially with the inclusion of terrestrial climate proxies. However, to make the model-data comparison more meaningful, it would be best to only include climate observations that straddle the O isotope excursion at 34 Ma, lets say within windows of 500 kyr immediately above and below. This might exclude a lot of data but the comparison pre- and post EOT conditions would be more meaningful.

Using the larger window was necessary to capture an acceptable spatial extent of proxy data. This is especially the case with the terrestrial data, which generally has lower temporal resolution.

1065 -1067 A figure showing the change in sea-ice distribution would be useful.

We have added a supplementary figure showing the sea ice distribution for each model. This cannot be integrated with our scaled temperature responses, because unlike temperature, sea ice exhibits threshold behaviour that cannot be interpolated smoothly between different levels of CO_2.

7.1.3 SAT response to paleogeographic change, ΔTGEO I think the assessment of simulations with differing tectonic configurations, ΔTGEO, is useful only for assessing how the sensitivity of a model to a given change in GHG levels varies under different configurations, e.g., an open or closed Drake passage. We already know from previous studies that the geographic changes alone produce relatively minor changes in global climate, and sometimes in the wrong direction. Also, the uncertainties about the timing of the gateways are large enough that this is not really worth focusing on. The section should be condensed or simply moved to SOM.

While we agree that the TGEO changes produce overall minor changes in global climate, this section is important to include here because the gateway cooling hypothesis is a long-standing theory of what caused the EOT cooling, and some aspects of the gateway hypothesis are still actively debated in modelling literature of the EOT (Toumoulin et al., 2020, e.g.).

1121-22 yes, the CO_2 is somewhat arbitrary.

Agreed – but it is important to flag this here.

1124 - As we all know, the error in CO_2 reconstructions is quite large. Nevertheless, it is unlikely that CO_2 was halved pre-EOT to EOT. More likely, the change in pCO_2 was much smaller, at least initially with inception of glaciation (at 34 Ma) which involved a threshold CO_2 enhanced by regional feedbacks via ice-sheet growth. It is possible that biogeochemical feedbacks enhance the drawdown of CO_2, but probably not more than 100 ppm or so.

We agree that halving CO_2 is not a realistic scenario, but it is a clear and useful experimental protocol for establishing the climate sensitivity to GHG changes. We scale the CO_2 forcing to best fit the data, and in doing so our derived estimate is much less than a halving. We have clarified that certain feedbacks are missing from these models, and thus the threshold behaviour (which requires dynamic ice sheets) cannot be simulated in them.

1165 – Based on the goodness of fit, you derive an estimate of ΔCO_2? I understand the strategy here, but it seems backwards when the primary motivation behind reconstructing paleoclimates is to assess climate models. As stated above, I think the observations suggest that the models (as they were prior to 2017) are under sensitive to GH forcing.

As noted above, we have added further discussion on the models' climate sensitivity to CO_2, including recent advances from the DeepMIP intercomparison. We note that the warmer models from that ensemble also have considerable non-CO_2 warming effects, i.e. climate sensitivity is not the only factor that can potentially resolve the model-data mismatch.

Figure 5 (pCO_2 reconstruction) – This figure made me cringe. The terrestrial proxy

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pCO2, given the coarse stratigraphic control, low temporal resolution, could be misleading. And let's be honest, given the concerns about the reduced sensitivity of the stomata proxy to higher CO2, who would really expect that proxy to accurately capture the ΔpCO2 across the EOT? Let's not even get into the issues with soil carbonates. As most of the climate data are marine based, it would make sense to only include the pCO2 estimates from marine proxies plotted along with the benthic d18O of figure 2 over a narrower window of time. This would eliminate any uncertainties about the relative timing of changes in climate versus forcing.

We have better acknowledged the challenges of reconstructing CO2 from terrestrial proxies, as noted in our response to Reviewer 1. However, these are an important branch of the EOT literature and we think it is best to include those data, with some added discussion around its limitations. We have also adjusted the age limits of Figure 5 to be shorter (38 to 30 Ma), to be more coherent with our temperature proxy compilations.

References

Foster, G. L., Royer, D. L., and Lunt, D. J.: Future climate forcing potentially without precedent in the last 420 million years, Nature Communications, 8, 14 845, https://doi.org/10.1038/ncomms14845, 2017.

Goldner, A., Herold, N., and Huber, M.: Antarctic glaciation caused ocean circulation changes at the Eocene-Oligocene transition, Nature, 511, 574–577, https://doi.org/10.1038/nature13597, 2014.

Inglis, G. N., Bragg, F., Burls, N., Evans, D., Foster, G. L., Huber, M., Lunt, D. J., Siler, N., Steinig, S., Wilkinson, R., Anagnostou, E., Cramwinckel, M., Hollis, C. J., Pancost, R. D., and Tierney, J. E.: Global mean surface temperature and climate sensitivity of the EECO, PETM and latest Paleocene, Climate of the Past Discussions, 2020, 1–43, https://doi.org/10.5194/cp-2019-167, 2020.

Lunt, D. J., Bragg, F., Chan, W.-L., Hutchinson, D. K., Ladant, J.-B., Niezgodzki, I., Steinig, S., Zhang, Z., Zhu, J., Abe-Ouchi, A., de Boer, A. M., Coxall, H. K., Donnadieu, Y., Knorr, G., Langebroek, P. M., Lohmann, G., Poulsen, C. J., Sepulchre, P., Tierney, J., Valdes, P. J., Dunkley Jones, T., Hollis, C. J., Huber, M., and Otto-Bliesner, B. L.: DeepMIP: Model intercomparison of early Eocene climatic optimum (EECO) large-scale climate features and comparison with proxy data, Climate of the Past Discussions, 2020, 1–27, https://doi.org/10.5194/cp-2019-149, 2020.

Pagani, M., Huber, M., Liu, Z., Bohaty, S. M., Henderiks, J., Sijp, W., Krishnan, S., and DeConto, R. M.: The Role of Carbon Dioxide During the Onset of Antarctic Glaciation, Science, 334, 1261–1264, https://doi.org/10.1126/science.1203909, 2011.

Toumoulin, A., Donnadieu, Y., Ladant, J.-B., Batenburg, S. J., Poblete, F., and Dupont-Nivet, G.: Quantifying the Effect of the Drake Passage Opening on the Eocene Ocean, Paleoceanography and Paleoclimatology, 35, e2020PA003 889, https://doi.org/10.1029/2020PA003889, 2020.

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2020-68, 2020.