Clim. Past Discuss.,
https://doi.org/10.5194/cp-2020-68-RC2, 2020

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.

Anonymous Referee #2

Received and published: 2 July 2020

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 ΔSAT. In general, most of the models are showing ~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.

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.

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 ($\sim$ 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.

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 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 $\sim$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.

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.

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.

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

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.

1121-22 yes, the CO2 is somewhat arbitrary.

1124 - As we all know, the error in CO2 reconstructions is quite large. Nevertheless, it is unlikely that CO2 was halved pre-EOT to EOT. More likely, the change in pCO2 was much smaller, at least initially with inception of glaciation (at 34 Ma) which involved a threshold CO2 enhanced by regional feedbacks via ice-sheet growth. It is possible that biogeochemical feedbacks enhance the drawdown of CO2, but probably not more than 100 ppm or so.

1165 – Based on the goodness of fit, you derive an estimate of $\triangle$CO2? 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.

Figure 5 (pCO2 reconstruction) – This figure made me cringe. The terrestrial proxy 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 $\triangle$pCO2 across the EOT? Lets 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.

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