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Climate of the Past Discussions

Interactive comment on "Did high Neo-Tethys subduction rates contribute to early Cenozoic warming?" *by* G. Hoareau et al.

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We thank referee J. Martinod for his positive review. Our answers to his comments are presented below.

R.C. This paper discusses the possible causes of the Early Eocene Climate Optimum, and shows that the closure of the Neotethys alone cannot explain the pCO2 increase at that time. This paper is very well written, easy to read even for geologists like me that are not specialized in paleo-climatology. Definitely, this work deserves being published.

A.C. We are greatly pleased to read that the reviewer has appreciated our work.

R.C. The paper recognizes huge uncertainties on many parameters that control CO2 release by the Neotethys subduction (as large that this kind of discussion sometimes

looks to me like discussions on the sex of angels). Nevertheless, G. Hoareau and coauthors show that, even considering the scenario for which the CO2 release by the Neotethys subduction zone is the largest, it does not account for the registered pCO2 increase. My only concern is that authors consider a 200 m sediment thickness on the subducting oceanic lithosphere before the onset of continental subduction. Is it really impossible that a significantly thicker sediment layer was lying on the oceanic plate close to the continental slope, e.g., because submarine fans would have developed somewhere on the Neo-Tethys passive margin? Could this significantly increase the released amount of CO2 or not?

A.C. The question of Neo-Tethyan sediment thickness is of primary importance. In the CFSM model, we have considered to keep a constant carbonate sediment thickness of 200 metres. This value has been chosen for several reasons: first, as stated in the text, it is similar to that chosen in previous estimates of CO2 degassing related to the "carbonate subduction factory" (Edmond and Huh, 2003; Johnston et al., 2011), which allows direct comparisons of CO2 flux calculations. Second, it is close to (but slightly lower than) the mean value calculated by Kent and Muttoni (2013) based on a time integration of carbonate accumulation rates in the Neo-Tethys ocean, north of the Indian and Arabian plates. This means that the maximum thickness of carbonate pelagic sedimentation may have been close to $\sim$200-300 m. We agree that sediment thickness may have been locally higher due to the presence of submarine fans or margin deposits such as carbonate platforms. However, it is likely that the impact of such deposits on the global excess CO2 was limited. Paleogeographic reconstructions indicate that north of Greater India, the Paleocene-Eocene Neo-Tethys ocean was deep (Heine et al., 2004), favoring the predominance of pelagic deposition (e.g., Kent and Muttoni, 2013), as assumed in our model. Submarine fans related to the vicinity of the passive Indian margin would have made part of the "Greater Indian passive margin", which is also considered in our calculations. For Arabia and Africa, palinspastic reconstructions of Barrier and Vrielynck (2008) indicate that subduction of deep deposits (clastic or carbonates) was also the rule during the Paleogene and the Eocene (i.e, the

time period considered in this study). While subduction of margin deposits can also be deduced from these reconstructions, only a small proportion of subduction trench length was concerned. Because of the low subduction rate of Arabia and Africa compared to Greater India at that time (1.5-4.5 cm/yr versus 14-16 cm/yr, respectively), we speculate that subduction of additional but limited volumes of sediments on the northern Arabian or African plates had a low impact on the global excess CO2 budget.

R.C. Another minor comment: the last phrase of the abstract is disappointing, because it looks like there is no alternative to investigate. Authors should put in their abstract the last phrase of the paper ("decrease of net carbon burial"), because if there's no solution, nobody will likely want to read the paper.

A.C. Referee D.V. Kent has made approximately the same comment ("The Abstract ends on a rather desultory note and should at least hint to a way forward, for example, maybe variable CO2 sinks are more important than sources in the long- term atmospheric pCO2 balance and need to be modeled better!"). We have proposed to modify the end of the abstract, by adding a sentence (p. 2848, after l. 25): "An alternate explanation may be that CO2 consumption, a key parameter of the long-term atmospheric pCO2 balance, may have been lower than suggested by modelling. These results call for a better calibration of early Cenozoic weathering rates."

References that are not in the CPD article: Heine, C., Müller, R.D., Gaina, C., Clift, P., Kuhnt, W., Wang, P., and Hayes, D.: Reconstructing the lost eastern Tethys Ocean Basin: Convergence history of the SE Asian margin and marine gateways: Geophysical Monograph, v. 149, p. 37–54, 10.1029/149GM03, 2004.

Sincerely, G. Hoareau on behalf of co-authors – 9 October 2015

Interactive comment on Clim. Past Discuss., 11, 2847, 2015.