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

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This paper attempts to test the proposition that demonstrably warmer climate in the early Cenozoic was due to greater output of CO2 from a carbonate subduction factory in high gear leading up to Tethyan closure. The underlying assumption is that warm periods like the Early Eocene climate optimum require special explanations. Barring changes in CO2 outgassing linked to varying global ocean floor production rates that drive GEOCARB models but for which there is scant evidence (e.g., Rowley 2002), subduction of Tethyan pelagic carbonates seemed to be an attractive and timely additional source of CO2 until its shutdown with collision of India–Asia at around 50 Ma (Schrag 2002; Edmond & Huh 2003; Kent & Muttoni 2008). However, subsequent calculations taking into account more precise plate tectonic motions showed that there was not

enough decarbonation to make an appreciable contribution to the CO2 budget, neither to the global warming in the Early Eocene nor by implication with the shutdown of the decarbonation factory to ensuing decrease of atmospheric pCO2 and cooler climate (Kent & Muttoni 2013).

The present paper makes an independent estimate of the output of the Tethyan carbonate subduction factory and uses a coupled climate-carbon cycle model (GEOCLIM) to evaluate the impact of the CO2 fluxes. The authors conclude that Tethyan decarbonation was unlikely to have been consequential for early Cenozoic warming, in substantive agreement with the conclusions of Kent & Muttoni (2013). This would seem to leave early Cenozoic warmth unexplained. However, the problem may be the other way around: more or less steady CO2 outgassing with slowly varying continental configurations and overall silicate weathering drawdown may be the norm to produce the predominant warm climate mode whereas low atmospheric pCO2 that results in polar ice sheets, such as over the past $\sim$30 Myr, is what needs explanation. Drastically reduced CO2 outgassing is hardly a viable explanation (and thus a general problem for GEOCARB models) so it must be CO2 sinks that vary greatly. Erosion and weathering associated with mountain building, as in the Himalayas, may be one way (Raymo & Ruddiman 1992). Another mechanism that was favored by Kent & Muttoni (2013) is emplacement of highly weatherable continental basalts into the warm and wet tropical belt where the Walker feedback brake is not strong on drawdown of CO2, which can thus descend to levels that allow ice sheets to develop.

Simply put, consideration of early Cenozoic warming could be placed in a broader context.

A few other suggestions from the foregoing:

#p.2868 l.23: "Atmospheric CO2 concentration may have only been able to reach significantly high values during the EECO (up to 25 770 ppm), but only if decarbonation efficiency was at its maximum at that time."

Decarbonation efficiency is extremely important in estimating CO_2 flux and warrants further discussion. Kent & Muttoni (2014) cite a recycling rate of up to 10% (typically lower) based on ^{10}Be data in arc volcanics in Central America (Tera et al., 1986). On the other hand, Hoareau et al. (this paper) cite decarbonation efficiencies of 60% and more following the modeling approach of Johnston et al. (2011), who unfortunately don't cite the Tera paper. The authors should make an attempt to sort this out.

#5.2.3 Organic carbon sources.

Beck et al. (1995) is cited tangentially elsewhere in the manuscript but their work on burial and subsequent exhumation of organic-rich Tethyan sediments in the early stages of the India-Asia collision really needs to be front and center in the discussion of organic carbon sources.

#The Abstract ends on a rather desultory note and should at least hint to a way forward, for example, maybe variable CO_2 sinks are more important than sources in the long-term atmospheric pCO_2 balance and need to be modeled better!

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