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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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We thank referee D.V. Kent for his review. Following his comments, we propose to modify several sections of the manuscript as presented below. We hope that such revisions will meet expectations of the reviewer, and that the manuscript will be getting closer to being accepted and published. Hereafter are presented point by point our answers to comments.

R.C. 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).

A.C. We agree with the reviewer that the hypothesis of the "carbonate subduction factory" has been cited several times as a potential candidate to explain late Paleocene - early Eocene warming, including Johnston et al. (2011). However, before our study, none of these had used carbon cycle models to simply verify that proposed excess CO2 fluxes are large enough to have had a substantial impact on pCO2 and temperature at that time.

R.C. 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).

A.C. Kent and Muttoni (2013) have been the first to attempt a precise calculation of excess CO2 fluxes linked to Neo-Tethys closure, taking into consideration both tectonic plate motion and pelagic carbonate sedimentation on Neo-Tethys seafloor. Their pioneering work is reminded several times in our manuscript, and their CO2 fluxes are used as an input for GEOCLIM modelling. Whereas Kent and Muttoni (2013) have based their conclusions on the sole calculation of excess CO2 fluxes, we have used a state-of-the-art carbon cycle model to explicitly verify that calculated fluxes had no impact on Paleocene Eocene climate, which is indeed the case.

R.C. 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).

A.C. Hopefully, our conclusions are similar to those of Kent and Muttoni (2013), despite a slightly different approach and the choice of higher decarbonation efficiencies (as discussed later in detail comments). In light of the results of both studies, we are now quite confident that Neo-Tethys closure was not a significant driver of Paleocene-Eocene greenhouse.

R.C. 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.

A.C. On the long term scale, pCO2 variations are primarily controlled on the one hand by CO2 degassing, and on the other hand by CO2 drawdown through silicate weathering. We agree that the respective weight of each of these two poles needs to be discussed. Recent studies (Misra and Froelich, 2012; Froelich and Misra, 2014) have attempted to demonstrate that early Cenozoic warmth was due to a drawdown of silicate weathering, rather than to an increase of CO2 outgassing. Misra and Froelich's results have important implications that we have presented and discussed in the second half of section 5.3. To our mind, the most striking implication is that weathering rates still suffer from much uncertainty, and that further efforts should be geared towards completing this gap, as written in the text. However, we think that contrary to

the end Permian to middle Triassic period, characterized by a stable Pangaea supercontinent promoting very high pCO2 values (Godderis et al., 2008), the early Cenozoic does not perfectly conform to a period of "slowly varying continental configurations" that would fit to the "norm to produce the predominantly warm climate". This time period was characterized by the fast northward drifting of Greater India (up to 18 cm/yr), which according to Kent and Muttoni (2013) resulted in the arrival of a significant surface of continental terrane in the equatorial humid belt at $\sim$60 Ma (their Figure 6). As a result, CO2 consumption rate increased between 60 and 50 Ma (i.e., during the LPEE and the EECO) (Figure 7 of Kent and Muttoni (2013)). If calculations of Kent and Muttoni (2013) are correct, the evidence of increasing atmospheric CO2 at that time suggests that CO2 outgassing also necessarily increased, as proposed by several authors (e.g., Berner, 2006; Komar et al., 2013; Lefebvre et al., 2013). Anyway, as discussed above, calculations of CO2 consumption rates depend on estimations of weathering rates, which need to be better calibrated. Finally, we do not understand Reviewer's personal statement that "low atmospheric pCO2 that results in polar ice sheets, such as over the past $\sim$30 Myr, is what needs explanation". Our study is dedicated to precisely testing a hypothesis that may explain the late Paleocene to early Eocene greenhouse. It does not aim at explaining the cooling of global temperatures during the early Oligocene. Hundreds of studies have been centred on this complicate topic, which of course still deserves much work. In addition to decreasing CO2 outgassing (Van der Meer et al., 2014), we agree that an increase of CO2 uptake by the uplift of the Tethyan orogenic belt and the arrival of Ethiopian traps in the equatorial belt are the best candidates to explain the onset of permanent polar ice sheets during the Neogene (Raymo and Ruddiman, 1992; Kent and Muttoni, 2013; Lefebvre et al., 2013), but the weight of all these parameters is not fully understood. Nevertheless, a paradoxically low number of studies have focused on the mechanisms that triggered the LPEE-EECO greenhouse, and the question is still open, in particular in light of the high weathering rates computed by many models. Our study, added to that of Kent and Muttoni (2013), allows to confidently discarding the "carbonate subduction hypothesis" as a serious challenger.

We do not want to extend our work to the problem of post-Eocene global cooling, which as we think is outside the scope of the present study.

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

A.C. We think that the Introduction section of our manuscript places the study in a broad context. To sum up, we clearly explain that global temperatures have decreased during the Cenozoic, except during the LPEE and the EECO. Whereas modelling suggests that global cooling may result from “decreasing seafloor spreading and subduction rates, as well as increasing CO2 removal through silicate weathering”, the trend of increasing temperatures between 58 Ma and 50 Ma still lacks a convincing explanation. To add some clues to this problem, we test a hypothesis by using a carbon cycle model, which had not previously been attempted. This is the aim of our work. However, following suggestions of the reviewer, we propose to add several sentences to the manuscript, which would explain in more detail the mechanisms governing decreasing temperatures during the Cenozoic: - In the Introduction section (p. 2849, after l. 8): “During the Cenozoic, CO2 consumption was mainly governed by the erosion of the Tethyan orogenic belt, and by continental drift, responsible for the arrival of highly weatherable basaltic provinces in the equatorial belt (Raymo and Ruddiman, 1992; Kent and Muttoni, 2013; Lefebvre et al., 2013)”, - In the Introduction section (p. 2850, after l.14): “For Kent and Muttoni (2013), high CO2 could be explained by less efficient weathering close to the EECO, rather than by additional CO2 production.” - In section 5.1 (p. 2863, after l.12): “As a consequence, the strong decrease of CO2 production after India-Asia collision was not a driver of pCO2 decrease and global cooling recorded after the late Eocene (Kent and Muttoni, 2013)”

R.C. A few other suggestions from the foregoing: #p.2868 l.23: "Atmospheric CO2 concentration may have only been able to reach sig- nificantly 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 CO2 flux and

warrants further discussion. Kent & Muttoni (2014) cite a recycling rate of up to 10% (typically lower) based on 10Be 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.

A.C. We agree with Reviewer’s comment. Decarbonation efficiency is a critical parameter to estimate CO2 outgassing at arc volcanoes. Studies dealing with this tricky problem (e.g., Gorman et al., 2006; Johnston et al., 2011; Tera et al., 1986) give variable decarbonation efficiencies for a given modern subduction zone. For example, efficiency at the Alaska-Aleutian arc reaches 31% for Gorman et al. (2006), versus 0.1% for Johnston et al. (2011). Theses studies also show that efficiencies are strongly variable from one zone to the other. Efficiencies at sub-arc depth are typically in the range 0-65%, 0-70% and 1-8% according to Gorman et al. (2006), Johnston et al. (2011) and Tera et al. (1986), respectively.

All these estimates suffer from several uncertainties, as stated by the authors of these studies themselves:

- Johnston et al. (2011) (p. 146): “The major source of error in the calculation of decarbonation efficiency is the uncertainty in the carbonate content of the subducting sediment and oceanic crust”

- Tera et al. (1986) (p. 548): “Specific estimates of the percent sediment in arc lavas (1-10%) are largely unconstrained at this point, and must await detailed studies of 10Be inventories in near-trench sediments”

- Gorman et al. (2006) (p. 16): “Accurate modeling of decarbonation of subducting lithologies is dependent upon numerous parameters, many of which are subject to considerable uncertainty”.

Since decarbonation efficiencies at the Neo-Tethyan arc are likely to have varied with time, and given the uncertainties in estimating present day efficiencies, adopting a range of mean decarbonation efficiency values as an input to our modelling approach seems, to our mind, reasonable. We have chosen to rely on average values of Johnston et al. (2011) (i.e., ∼15 to ∼60%), for several reasons:

(i) This study is based on the re-evaluation of global decarbonation efficiency values of the widely cited review of Hilton et al (2002), which was based on volcanic gas composition data of 10 modern subduction zones. Johnston et al. (2011) used updated gas flux estimates of Fischer (2008) to recalculate new efficiencies;

(ii) Their calculation considers explicitly the role of crustal contamination on CO2 fluxes, which can substantially affect apparent efficiency values;

(iii) Their range of decarbonation efficiencies is consistent with results of Gorman et al. (2006), the most recent and advanced modelling study of this mechanism. In detail, if decarbonation at sub-arc depth alone is considered, mean decarbonation efficiency calculated from data of Gorman et al. (2006) (∼16%) is lower that that calculated from Johnston et al. (2011) (∼30%), but higher that that of Tera (1986) (∼3.5%). In contrast, if both fore arc and sub arc losses are major sources of volcanic arc CO2 fluxes -as postulated by Gorman et al. (2006)- their mean decarbonation efficiency reaches ∼60%.

These results show that most recent estimations of decarbonation efficiencies at modern arcs are variable but broadly consistent, with mean values comprised between 15% and 60%, i.e., above estimates of Tera et al. (1986). This does not mean that efficiencies at the Neo-Tethyan arc may have been lower (or higher!) at some time during the Paleocene and the early Eocene.

We propose to add the following sentences to the manuscript in section 3.3.1. (p. 2855, after l.25): "We have retained values of 15 to 60%. These are similar to mean values of modern efficiency from recent modelling study of Gorman et al. (2006) (∼16% and

∼63%, if volcanic CO2 is derived from decarbonation at sub-arc depth only, or both at fore-arc and at sub-arc depths, respectively). However, they exceed value used by Kent and Muttoni (2013) to perform similar calculations (i.e., 10%), based on 10Be data in arc volcanoes of Central America (Tera et al., 1986)".

R.C. #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.

A.C. We agree with Reviewer's comment. Beck et al. (1995) proposed a suitable mechanism to explain the onset of Paleocene-Eocene warming at ∼60 Ma. Similar to our study, India-Asia collision is seen as the trigger of Paleocene greenhouse, through the exhumation and subsequent oxidation of marine organic carbon. Their calculations are conditioned by early India-Asia collision (older than ∼60 Ma), earlier than ages used in our model (55 to 50 Ma) on the basis of several recent studies (e.g., Dupont-Nivet et al., 2010; Najman et al., 2010; Orme et al., 2014). In the last year, new estimations have proposed the onset of collision to occur during the lower Paleocene (∼60 to 58 Ma) (e.g., Hu et al., in press). These new findings show that the hypothesis of Beck et al. (1995) is not unfounded, and could be a good candidate to explain the negative shift in δ13C observed during the LPEE.

We propose to reorganize section 5.2.3, which is dedicated to organic carbon sources, in particular by adding an entire paragraph: "Finally, Beck et al. (1995) postulated that Neo-Tethyan marine organic matter accumulated on Eurasian and Greater Indian margins may have been oxidized during India-Asia collision and subsequent exhumation, provided collision occurred no later that ∼60 Ma. About 1.6x1018 molC/Ma may have been released during the first 4 Ma of the LPEE, enough to explain the concurrent negative shift in δ13C. Using our model, we calculate that the organic carbon contained within Greater Indian margin alone (∼3.8x106 km3) amounts ∼8x1018 molC (for a sediment organic carbon content of 1 wt%), corresponding to a flux of ∼2x1018 molC/Ma (i.e., close to estimates of Beck et al. (1995)) if all Corg was oxidized during exhumation. This was probably not the case, and our estimate is likely overestimated. Nevertheless, it shows that oxidation of Neo-Tethyan marine Corg may have contributed to the LPEE if collision occurred earlier than assumed in our model (e.g., Hu et al., in press), to an extent that deserves to be quantified more accurately in future studies."

R.C. #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!

A.C. The abstract and conclusion sections will be modified as requested by the Reviewer. For the abstract, we propose to add the following 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." For the conclusion, a close sentence could be added (p. 2869, after l.6): "Finally, an alternate explanation may be that CO2 consumption may have been lower than suggested by carbon cycle models, calling for a better calibration of early Cenozoic weathering rates."

Cited references that are not in the CPD article:

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