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Climate of the Past Discussions

Interactive comment on "Modulation of Late Cretaceous and Cenozoic climate by variable drawdown of atmospheric pCO_2 from weathering of basaltic provinces on continents drifting through the equatorial humid belt" *by* D. V. Kent and G. Muttoni

Anonymous Referee #2

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Title: Modulation of Late Cretaceous and Cenozoic climate by variable drawdown of atmospheric pCO2 from weathering of basaltic provinces on continents drifting through the equatorial humid belt by D. V. Kent and G. Muttoni. Submitted to Climate of the Past, August 2012.

This manuscript highlights the impact, on the carbon cycle, of large igneous provinces (LIP) and decarbonation of Tethyan pelagic sediments associated with subduction

zones during the last 120Myrs.

The sections 1 to 5 are clearly very interesting and are potentially of considerable importance in understanding (1) the real effect of the decarbonation and (2) the evolution of the weatherability of continental surfaces. With regard to the first point, this manuscript shows that the decarbonation is a second order factor which means that the pCO2 evolution is mainly governed by the efficiency of the long-term carbon sink. This finding implies that the tectonic and LIP represent the most reasonable explanation for the CO2 decrease observed during the Cenozoic. This idea is clearly inspired by a previous work by D. V. Kent and G. Muttoni published in 2008 in PNAS. However, in this paper, all LIP are considered (not only the Deccan traps) and an important attempt has been made to quantify the response of the carbon cycle.

However this manuscript will require moderate improvements before it is ready for publication. The main issues are:

(1) The manuscript is very well-written and the current presentation is good, however the shape of the manuscript needs to be slightly changed. Indeed principal results should be summarized in the conclusion (a) effects of the decarbonation of pelagic sediments and (b) how Ethiopian traps and SE Asia have affected the weatherability of continents (The Deccan traps being largely discussed in PNAS 2008).

(2) The authors have to better explain the assumption behind their carbon model (section 5). Indeed weathering fluxes have to remain high enough to balance CO2 input at the geological time-scale, steady state levels of atmospheric CO2 being reduced without changes in global weathering fluxes but by increasing the weatherability of continents (LIP and/or tectonic). This important idea should be included.

(3) P4532 lines 10-15. I disagree with this sentence. The ratio (> 2/3) suggests that the flux 190Tton/Myr is the net CO2 consumption flux. However half of the carbon consumed on continents is released during the carbonate precipitation occurring into the ocean, so the net effect on the CO2 consumption is two times lower

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(190/2=95Tton/Myr) than initial suggested. The ratio should be 95/260 = 0.36 (close to Dessert et al. values (2003)).

(4) P4535 lines 6-9. The conclusion of this section needs more discussion. Indeed the climate-carbon feedback implies that the global chemical weathering is remained constant (see point 2). Hence, I would like to have more information concerning the process invoked to explain this supply of nutrients.

(5) P4535 line 13-17 Uplift-erosion hypothesis I suggest that this part be removed or better discussed. Indeed Kump & Arthur (1997) suggest that the increased rates of weathering demanded by the Raymo hypothesis lead to an imbalance in the carbon cycle and depletion of the exogenic carbon dioxide inventory.

I do have a number of suggestions that the authors may consider for improving the presentation of the paper

P4518 line 5. The authors should use the unit "1E+12 molCO2/yr", which is the conventional unit (for example 260 Tton CO2/Myr or 5.91 1E+12 molCO2/yr).

P4527 line 10. missing reference Godderis and Joachimiski 2004 (COMBINE model)

P4527 line 21. A manuscript tends to become harder to read when too many different units are used, so keep all fluxes in Tton CO2/Myr or 1E+12 molCO2/yr (Mton CO2/yr = Tton CO2/Myr)

P4530 line 27. The authors should use the unit "1E+6 molCO2.yr-1.km-2" to be in agreement with Dessert at al. (2003).

The figure 5 could appear misleading if the caption and text do not describe that the CO2 consumption rate is computed for a fixed pCO2, so without the carbon-climate feedback. This condition explains why the CO2 consumption rate is lower in the past. Indeed with the climate-carbon feedback, the consumption rates ought to be similar since 120Myrs (see point 2).

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Fig.6. the “CO2 consumption rate” instead of “CO2 consumption”. I suggest that the value of Dessert et al. 2003 be added (the CO2 consumption rate by basaltic provinces = 4.08 1E+12 molCO2/yr or 180Tton/Myr).

Interactive comment on Clim. Past Discuss., 8, 4513, 2012.