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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 #3

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This manuscript is devoted to changes in sources and sinks of CO2 for the part 120 Ma in a paleogeographic context. The authors evaluated potential parameters impacting on past pCO2, and more particularly the role of decarbonation of pelagic sediments in Tethyan subduction and the role of continental basalts weathering.

Previous studies, focused on present-day systems, show that intensity of chemical weathering of continental silicates depend on lithology and that basaltic lithology is

one of the most weatherable. Meteorological parameters like temperature and runoff have also strong impact on basalt weathering, with optimal conditions in warm and humid tropical context. The weatherability of silicate continents evolved over the past (relative surface areas of plutonic versus volcanic lithology), that should have impacted CO2 consumption rates and then pCO2. This hypothesis is properly exploited by the authors.

The first part of the manuscript (sections 1-5) is clearly written, the hypotheses are well described, and results are interesting and convincing. It contributes to a better understanding of the evolution of pCO2 and climate for the past 120 Ma. It shows that decarbonation of pelagic sediments in Tethyan subduction from 80 to 50 Ma is significant, but is a rather small contribution of the high pCO2 and supposed warm climate in the Cretaceous-Eocene. This study shows that the efficiency of subduction decarbonation as a source of CO2 is arguable. The atmospheric pCO2 would be then triggered off by CO2 sinks instead of CO2 sources. If the long-term source of CO2 is constant, that could be questionable, the variation of pCO2 reflect variation of continental silicate weathering flux. This flux increases with increasing of weatherability of continents, whether an increase of the basaltic/silicate area ratio.

However, without modeling of the carbon cycle, interpreting the geochemical evolution of oceanic carbonates is more hazardous (sections 6-8). Indeed, the carbon and alkalinity budgets into the ocean-atmosphere system depend on weathering fluxes (carbonates and silicates), CO2 emissions and carbonate deposition.

The section 6 presents no new results. A compilation of carbonates data and d13C record is presented. The authors propose an interpretation of these records in term of organic fraction of the total carbon burial flux. I am not sure to understand the interest of this long (too long: 4 pages) discussion.

The section 7 should be included into the discussion of section 5 (or removed). The section 8: again, without modeling of carbon cycle (with for example an oceanic submodel with carbonate speciation), it seems hazardous to discuss precipitation of calcite and aragonite.

A conclusion with the main results is missing.

Specific comments: Section 5 : The study of dessert et al. 2003 shows that basalt weathering is also important into the temperate humid belts (5° to 30°). It would be interesting to present CO2 flux resulting from this climatic zone in fig 5 and 6.

Section 5: The authors cannot compare directly the CO2 consumption flux with the CO2 outgassing flux. Only one half of “this riverine CO2” is sequestrated into carbonate at geological scale.

Section 8: The authors compared the volume of Deccan Traps with riverine Mg flux. We cannot compare a volume of Mg with a flux. I suggest using the paper of Tipper et al. 2006 (EPSL) for the oceanic Mg budget.

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