Clim. Past Discuss., 8, C2990–C2999, 2012
www.clim-past-discuss.net/8/C2990/2012/

Climate
of the Past
Discussions

Interactive comment on "Modulation of Late Cretaceous and Cenozoic climate by variable drawdown of atmospheric $p\text{CO}_2$ from weathering of basaltic provinces on continents drifting through the equatorial humid belt" *by* D. V. Kent and G. Muttoni

D. V. Kent and G. Muttoni

dvk@ldeo.columbia.edu

Received and published: 31 December 2012

We appreciate the very useful suggestions by Referee #2 to add a list of Conclusions and an opportunity to clarify the section on the Himalayan uplift-erosion hypothesis, which with other comments motivated us to reorganize the latter parts of the manuscript. We also tried to use more consistent units and terminology, acknowledging the important difference between total and net CO2 consumption rates from silicate

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

weathering. Our detailed responses to specific points raised by Referee #2 are as follow.

** (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).

Excellent suggestion – the results have now been summarized in a Conclusion section as follows:

10. Conclusions

1. Contrary to what has sometimes been assumed including in our earlier work (Kent & Muttoni 2008), the degree to which decarbonation of pelagic sediments contributes to atmospheric CO2 is calculated to be rather modest compared to present-day total outgassing, suggesting that subduction decarbonation was not a decisive factor in the higher pCO2 levels associated with warm climates of the Late Cretaceous and early Cenozoic let alone in earlier times, before proliferation of pelagic carbonate deposition in the deep-sea.

2. The small estimated contribution of decarbonation along with the null hypothesis of a constant rate of ocean crust production (Rowley 2002] suggest that changes in the long-term budget of atmospheric CO2 are more likely governed by the carbon sinks rather than in response to variations in the carbon supply as typically invoked in many carbon cycling models for at least the late Mesozoic.

3. To explain long-term changes in atmospheric CO2, we consider a time-varying CO2 sink model largely driven by the amount of land area, and highly weatherable subaerial basaltic terranes in particular, brought by tectonic plate motions into the equatorial humid belt, the most potent venue for continental silicate weathering and associated

consumption of atmospheric pCO2.

4. According to the drift-weathering hypothesis, the long-term decrease in atmospheric pCO2 levels since the Early Eocene climate optimum was initiated by the arrival of Greater India with the highly weatherable Deccan Traps in the equatorial humid belt at around 50 Ma, and was sustained by the emplacement of the 30 Ma Ethiopian Traps near the equator, the southerly tectonic extrusion of SE Asia, an arc terrane that presently is estimated to account for ∼1/4 of the total CO2 consumption by continental basalt weathering and volcanic islands that in turn represents ∼1/3 of total continental silicate weathering (Dessert et al., 2003), joined by the northerly incursion of New Guinea also into the equatorial belt.

5. The equatorial humid belt is likely to maintain potent (warm and wet) conditions for weathering over a broad range of pCO2 levels with only weak negative feedbacks to limit weathering rates, which would tend to dwell on the high side. Low atmospheric pCO2 levels leading to global cooling and glaciation depend on enhanced and sustained presence of weatherable rocks, especially subaerial basalts, near the equator.

6. Paleosols and other evidence of more intense chemical weathering in the temperate belt tend to be associated with times of very high atmospheric pCO2 levels and warm global climates, for example, in subaerial lavas of the North Atlantic igneous province of Late Paleocene to Early Eocene age, which evidently acted as safety valves providing strong negative feedback that acts to squelch a runaway greenhouse when there are insufficient weatherable terranes in the equatorial humid belt to adequately balance CO2 outgassing.

7. The drift-weathering hypothesis bears some resemblance to the Himalayan uplift-erosion hypothesis of Raymo and Ruddiman (1992) to the extent that it does not seem to make much difference whether more weatherable lithologies are introduced by horizontal plate motion - as we suggest - or as vertical uplift in mountain building processes, as long as it happens close to a warm and humid equatorial belt.

8. Comparison of bulk (mainly planktic) carbonate and benthic carbon isotope records indicates that the biological pump (surface to deep ocean isotopic gradient) increased just after EECO (∼50 Ma) and continued at more or less the same high level through most of the rest of the Cenozoic, a pattern that could reflect higher productivity associated with increased availability of nutrients from more intense silicate weathering in the equatorial humid belt. The oceanic Mg/Ca ratio also started rising at very roughly the same time, allowing the possibility that sustained weathering in the equatorial humid belt of the Deccan and other basalt provinces contributed to an ocean chemistry comparatively richer in Mg.

9. The drift-weathering hypothesis provides motivation for obtaining accurate estimates of the latitudinal distribution of landmasses and especially basaltic provinces over time, independent of inferences about sea-floor production rates, and incorporating climate models that take into account factors like relief, lithology and vegetation and include the necessary feedbacks and adequate spatiotemporal resolution for a more comprehensive understanding of the carbon cycle and climate change.

** (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.

In our model, atmospheric CO2 levels are progressively reduced over the Cenozoic essentially due to a small but sustained imbalance between CO2 input from mantle outgassing (held constant) and output via silicate weathering as more weatherable basaltic landmasses drifted through the equatorial belt, which according to a variety of climate models has the tendency to remain warm and humid – and therefore conductive to chemical weathering – over a wide spectrum of CO2 levels. In our model, the concept of weatherability of rocks/continents refers essentially to the superior efficiency of basalts to subtract CO2 during chemical weathering relative to other silicates (e.g., granites), as extensively described by the weathering laws of Dessert et al. (2003, and references therein). This appears different from the less intuitive concept of weatherability introduced by Kump and Arthur (1997) for the uplift of the Himalayas (that we now mention), defined as representing the product of factors ‘other than climate change’ (effects of relief, glaciation, and plant coverage) that are somehow capable to increase the weatherability of the ‘continents as a whole’ without increasing global weathering rates (Kump and Arthur, 1997).

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

We modified P4532 lines 10-15, which now reads as follows:

‘Hence the smoothed curve in Fig. 6 [now Figure 7] may provide at this juncture a more substantiated representation of the secular change in total equatorial consumption of CO2. Even allowing that the total consumption flux of CO2 should be halved to account for the CO2 returned to the atmosphere-ocean during carbonate precipitation, it is still remarkable that the net CO2 consumption of up to 190/2 = 95 Mton CO2 yr^{-1} from silicate weathering of only a small fraction of total land with basaltic and mixed crust provinces currently residing in the equatorial humid belt may balance a substantial fraction (~35%) of the present-day global volcanic CO2 emission rate of 260 Mton CO2 yr^{-1}.’

Accordingly, we also modified the caption to Figure 6 (now Figure 7) as follows:

Fig. 7. Total CO2 consumption rates from silicate weathering since 120 Ma of land areas in equatorial humid belt (5°S–5°N) obtained by multiplying a nominal total CO2 consumption rate of 100 ton CO2 yr^{-1} km^{-2} for basaltic provinces and 50 ton yr^{-1}

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

km−2 for mixed basaltic-metamorphic provinces (Dessert et al., 2003) and an order of magnitude less (5 ton yr−1 km−2) for the remaining continental rock types (Gaillardet et al., 1999) to the corresponding cumulative distribution curves in Fig. 6. Note that these total consumption rates should be halved for net CO2 consumption rates to account for the CO2 consumed by silicate weathering that is returned to the atmosphere-ocean during carbonate precipitation. For example, a modern total CO2 consumption of 190 Mton CO2 yr−1 should correspond to a net CO2 consumption of 190/2 = 95 Mton CO2 yr−1, or ∼35% of the present-day global volcanic CO2 emission rate of 260 Mton CO2 yr−1. For reference, a net CO2 consumption rate of 1 Mton CO2 yr−1 can be sustained by introducing into the equatorial humid belt or rejuvenating roughly 20,000 km2 of SE Asia arc terrane every million years.

** (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.

We deleted the last paragraph (lines 6-11) as unnecessary and speculative.

** (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.

We tightened the discussion of the Himalayan uplift-erosion hypothesis including addition of the pertinent Kump and Arthur (1997) reference, as follows; this section will now come immediately after 5 Quantification of CO2 silicate weathering sinks.

6 Himalayan uplift-erosion hypothesis

According to the uplift-erosion hypothesis (Raymo and Ruddiman, 1992), uplift of the Himalayas and Tibetan Plateau as a consequence of the India-Asia collision resulted in enhanced silicate weathering rates and higher associated consumption of atmospheric pCO2, causing Earth's climate to cool with the eventual formation of Antarctic ice sheets by ~34 Ma. This was largely based on the supposition that the progressive increase in 87Sr/86Sr values of marine carbonates since ~40 Ma (Hess et al., 1986; Richter et al., 1992) was due to enhanced delivery of radiogenic Sr from increased global chemical weathering rates from mountain building, especially the uplift of the Himalayas (Raymo et al., 1988). It is not clear in this model what would have been responsible for initiating decreasing atmospheric pCO2 over the prior 10 Myr since the 87Sr/86Sr seawater curve is essentially flat from at least 50 Ma to 40 Ma. More importantly, others have argued rather persuasively that most of the overall increase in 87Sr/86Sr resulted from the unroofing and chemical erosion of particularly radiogenic Himalayan rocks, such as leucogranites (Edmond, 1992) and metasediments (Harris, 1995) including metamorphosed limestones (Quade et al., 1997), in which case the 87Sr/86Sr seawater curve would not serve as a simple proxy for global weathering rates of continental silicates.

The general principle of the uplift-erosion hypothesis is nevertheless difficult to dismiss and the drift-weathering idea proposed here bears a general resemblance to it: both call upon tectonic mechanisms (uplift, plate motion) to initiate and sustain higher CO2 consumption via silicate weathering. Compared to a carbon cycling model driven by the supply-side, a sink-side model like the uplift-erosion hypothesis usually has to contend with the absence of an obvious feedback mechanism that is well-coupled to the climate system to stabilize atmospheric pCO2 levels even over relatively short geologic time scales (e.g., Berner and Caldiera, 1997; Broecker and Sanyal, 1998). To account for this, Kump and Arthur (1997) modified the Himalayan uplift-erosion concept by invoking compensatory factors to high local (Himalayan) chemical erosion and associated CO2 drawdown that would result in lower global chemical erosion rates even as all the continents somehow became more susceptible to chemical erosion. Organic carbon burial was offered as an additional or alternative process to silicate weathering for drawing down CO2, at least since Miocene uplift of the Himalayas (Raymo, 1994) and interestingly, France-Lanord and Derry (1997) did find that Neogene CO2 consumption from net organic carbon burial in the Bengal fan was higher than pCO2 drawdown from silicate weathering on the Ganges floodplain that is ultimately sourced by intense physical erosion in the Himalayas. Nonetheless, as discussed in the following section, marine carbon isotope records show no obvious evidence for increased net organic carbon burial globally in the late Cenozoic.

But what if tectonic uplift occurred closer to the equator instead of in the temperate arid belt: global climate would cool with the drawdown of CO2 but the resulting decreased silicate weathering and reduced CO2 consumption elsewhere might not be able to compensate for the continued drawdown of CO2 due to the presence of more weatherable landmasses in the relatively warm and wet conditions of the equatorial humid belt. This is effectively the drift-weathering scenario and may be the ongoing case today with the high landmasses of the SE Asia islands like Java, Sumatra, Borneo (world's 3rd largest island at 0.75 Mkm2 with mountain peaks rising to over 4,000 m), as well as New Guinea (2nd largest island after Greenland at 0.79 Mkm2 with peaks rising to nearly 5,000 m) impinging upon the equatorial humid belt and shedding prodigious amounts of sediment intensely weathering in the heat and humidity of the lowland aprons, even though atmospheric pCO2 levels are very low (probably as a result). This is also reflected by the global annual fluvial sediment flux from the world's main drainage basins to the oceans that shows a disproportionately high contribution to the global sediment yield by the SE Asian islands straddling the modern equator (Milliman, 1990). Although the weathering flux per unit area on the Ganges floodplain may be appreciable (West et al., 2002), it seems to us that the specific Himalayan uplift-erosion hypothesis does not actually do enough early enough and for long enough in terms of CO2 drawdown from silicate weathering to account for the cooling over the Cenozoic that led to the formation of major polar ice sheets. Besides, there are mountains in many parts of the world that at any given time are being actively eroded. The action really needs to happen in the equatorial humid belt where whenever significant weatherable lithologies are inserted via horizontal plate motion and/or vertical uplift–as is presently happening–negative feedbacks may not be efficient enough to inhibit net CO2 drawdown and an eventual ice age.

** 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).

We made the units more uniform using Mton CO2/yr for flux throughout with conversion from/to 1012 mol CO2/yr where it seemed appropriate, e.g., 260 Mton CO2/yr = 5.91 x 1012 mol CO2/yr.

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

Good addition: Goddéris, Y., and Joachimski, M. M.: Global change in the late Devonian: modeling the Frasnian-Famennian short-term carbon isotope isotope excursions, Paleogeography, Paleoclimatology, Paleoecology, 202, 309-329, 2004.

** 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)

Units have been made more uniform as Mton CO2/yr with equivalent value in 1E+12 mol CO2/yr where approriate.

** 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).

Equivalent values in moles provided.

** 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).

Assuming the Referee is referring to previous Figure 6 (consumption rates) rather than

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

previous Figure 5 (estimates of land areas), this is true, there are no carbon-climate feedbacks in our calculations, which are for total CO2 consumption just in the 5°S-5°N latitude band, not for all continental areas, so there is no reason from these considerations that consumption rates should be constant - they can go up and down as more or less weatherable rocks are in the equatorial humid belt where feedback from atmospheric pCO2 levels may not be very strong. Weatherable rocks elsewhere can put the brakes on when atmospheric pCO2 levels get too high. We now do point out (response to point 3) that the total equatorial consumption rates of CO2 should be halved for net CO2 consumption to account for release associated with carbonate precipitation.

** 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).

'Rate' will be added to axis label. We now also use 4.08 1E+12 mol/yr (=180 Mton CO2/yr), the total CO2 consumption of basalt provinces (3.109 1E+12 mol/yr) plus an estimate for small volcanic islands (0.97 1E+12 mol/yr) (Dessert et al., 2003, p.267), as the benchmark for comparison. The revised text will say that net (i.e., half of total to account for CO2 release in associated carbonate deposition) CO2 consumption for basalts (plus mixed crust) just in the narrow equatorial belt (5°S to 5°N) constitutes ~35% (95/260 ±) of the global volcanic CO2 emission rate, which simply reinforces the point that weatherable rocks in a weathering place do most of the consuming.

We thank the Referee for careful consideration of our manuscript and the opportunity provided to us by the comments to improve the manuscript.

Dennis Kent, Giovanni Muttoni

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