Clim. Past Discuss., 8, C2295–C2303, 2012
www.clim-past-discuss.net/8/C2295/2012/


Climate of the Past Discussions

CPD

8, C2295–C2303, 2012

Interactive Comment

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

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Received and published: 8 November 2012

In their manuscript "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," D. V. Kent and G. Muttoni deal with multiple issues in their consideration of controls on climate. One is estimation and calculation of how much carbonate on the Tethyan seafloor got subducted in the runup to the Himalayan orogeny. Their question here is whether there is enough carbon being subducted to significantly alter climate when those subducted sediments eventually release their sequestered carbon dioxide back into the atmosphere during orogeny-related outgassing. The authors also address the question of how effective is basalt weathering with regard to altering climate. For example, do large igneous provinces (LIPs) become net CO2 sinks? Even though LIPs emit a lot of carbon dioxide and/or methane when active, does the weathering of their silicates draw down more carbon dioxide than the LIP emitted? Does a large basalt province exert an increased and significant weathering/cooling effect as it drifts through tropical latitudes where it is more likely to undergo chemical weathering than in an arid climate? Does basalt weathering in warm and moist settings serve as a negative (cooling) feedback as climates become increasingly warm?

General Comments

1. Regarding the degree to which subduction of Tethyan carbonates contributes to atmospheric CO2 from the Late Cretaceous to Eocene via metamorphism of those carbonates, and eventual outgassing of the resulting carbon dioxide, Kent and Muttoni have taken an interesting approach. They have calculated how much sediment should accumulate in the equatorial bulge zone beneath high productivity waters. Next, they have calculated how much of this sediment may have been subducted at various times and rates. They point out that determining how much of and when this subducted carbon reservoir will resurface is hard to establish. Nevertheless, the key point is that their numbers show that the mass of carbon involved in this mechanism is greatly dwarfed by normal, ongoing release to the atmosphere by volcanism operating at current rates of outgassing. It is important to note that they are assuming constant rates of seafloor production and hydrothermal activity. Whether or not this controversial suggestion from Rowley (2002) gains acceptance, this assumption in Kent and Muttoni's manuscript allows for testing of other aspects of the work they are presenting (and see further discussion of this below). The key point here is that when the Himalayan collision closed this

part of the Tethys ocean, the cessation of subducting Tethyan sea-floor carbonates and their subsequent outgassing was not a volumetrically significant enough loss in carbon dioxide emissions to initiate Cenozoic cooling following the warm Early Eocene.

2. A second major issue they address is that of drawdown of atmospheric carbon dioxide via chemical weathering of basaltic provinces such as those generated by the activity of large basaltic regions such as mafic Large Igneous Provinces (LIPs) in geologic history. They point out that such weathering will be most effective when relief is high and climate in which the LIP resides is warm and moist, such as in the tropics. Dry climate will not favor significant silicate weathering.

3. Their approach, assumptions, and methods set up a series of calculations that are instructive, but not everything is taken into account. For example, they assume that ancient climate belts are similar to those in the past. This may not be so for some warm ancient climates, but their approach does suggest a thought-provoking first approximation that could be adjusted as the paleolatitudinal distribution of ancient climate belts becomes better characterized.

4. Examples of potential climate-belt differences include suggestions that some Cretaceous terrestrial climates were fairly dry at tropical latitudes (Hay and Floegel, 2012), at least on a seasonal basis. Regardless of how dry these paleolatitudes might have been, there is a marked lack of vegetation. I suggest an alternative interpretation that the lack of vegetation might be due to high temperatures even though the tropics remained fairly wet. Regardless, my main point is that reduced vegetation cover will hamper silicate weathering, making these particular tropics less effective basalt-weathering settings. On the other hand, moist and mild climates have been suggested for high paleolatitudes during globally warm to hot intervals (e.g. Kidder and Worsley, 2010; 2012). The presence of high-latitude forests (e.g. Retallack and Alonso-Zarza, 1998; Taylor et al., 2000; Jahren, 2007) supports this shift. As a direct example of what I am focusing on here, I note that the authors emphasize (bottom of page 4527 and top of 4528) that the Siberian Traps presently do not weather much. This is exactly so in

our present climate. However, in the Late Permian-Early Triassic, those basalts were at high paleolatitude in the moist and probably mild climates mentioned above. Their presence in a paleoclimate setting favorable to weathering might help explain the Early Triassic pulse in chemical weathering (e.g. Sheldon, 2006; Algeo and Twitchett, 2010) that occurs on a world otherwise marked by widespread dry climates. Taken together, these considerations suggest that some basalt provinces might weather less than expected in some tropical settings and more than expected in some high-latitude settings. I am not suggesting that the authors correct for this in their present manuscript. Calibrating their present approach for changing of climate belts through time is an entirely new project. However, the authors should acknowledge that ancient climate belts probably do not match modern ones in many cases.

5. Kent and Muttoni review the major basalt provinces present on land in the Cretaceous. They report their paleolatitudes so as to set up consideration of how much weathering there might be as those provinces drift through warm and moist paleolatitudes. In addition to the concerns mentioned above regarding climate belts, I find myself wondering just how much of a given basalt province was actually weathered vs. how much is preserved. The authors assume that most of the basalt province is consumed by weathering shortly after emplacement, when the plume head provides relief that will contribute to rapid weathering and erosion. My sense is that once they have an approximation as to how much atmospheric carbon dioxide decreased in a given interval, the authors call on basalt weathering to remove that carbon dioxide from the atmosphere. This provides a ballpark figure as to the potential impact of basalt weathering, but it also ignores other cooling contributors. I think they are on the right track, but future work will likely lead to revision of their numbers.

6. Further thoughts in regard to the above, on p. 4530, line 1: Kent and Muttoni state that "The bulk of the CAMP lavas were probably weathered, eroded or buried soon after their emplacement in the earliest Jurassic." I wonder how they can get a sense for how much was actually removed at this time as opposed to later. Still, much remains.

The CAMP presently hosts the largest existing mass of basalt of the large igneous provinces reported on the LIP website (www.largeigneousprovinces.org/). I think the approach the authors are taking in terms of trying to calculate effects of weathering LIP basalts is simply fascinating. My main concern at this stage is to question just how much of the basalt is actually accessible? On the one hand, the authors point out (p. 4514, line 25-26 and p. 4538, line 14-15) that covering by cation-deficient soil will make basalt less susceptible to chemical weathering. I am also concerned about covering older basalt layers with new ones. If only the youngest basalt is exposed, that puts much of the LIP off limits to weathering. However, river erosion will slice through these layers, but only in certain areas. I'm not saying their approach can't work. I'm suggesting that other factors need to be taken into account such as: How much of the basalt is inaccessible? How much of the carbon dioxide drawdown being documented could be getting removed by processes other than basalt weathering? I think the authors are headed in an interesting direction here. My suspicion is that they will ultimately be able to quantify the amount of carbon dioxide drawdown that is being controlled by basalt weathering. My sense is that there is more than one factor at work here, and that the basalt weathering is not the entire driver of carbon dioxide drawdown.

7. As a test of the basalt weathering model, I consider the rapid cooling (e.g. Jenkyns, 2003) just after the extremely warm Cenomanian-Turonian interval. I am not aware of a basalt province at a suitable latitude for rapid weathering that might drive this cooling. I am not saying that basalt weathering does not happen, and it may well be a cooling factor. I merely point out that it does not seem to work in this case, and that other cooling mechanisms need to be considered to see the full picture. Some of the other cooling mechanisms may well operate more quickly than basalt weathering.

8. In their section on the Uplift-Erosion hypothesis (starts on p. 4535, line 12), I think some revision is needed here. Chiefly, I think they may want to rethink their discussion on Himalayan silicate weathering. On page 4536 (lines 2-3) they point out that



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mechanical weathering is vigorous in the high elevations, but that silicate weathering intensity is low, "even in the sediment basins like the Ganges system at lower elevations (France-Lanord and Derry, 1997)."

I fully agree that silicate weathering intensity must be low at high elevations. However, I suggest that it may not be so low in the sediment basins as Kent and Muttoni indicate at this point in the manuscript. For example, France-Lanord and Derry (1997) do seem to accept that there is significant chemical weathering in the Ganges Basin floodplain. On page 65 of their paper, they state (with regard to sediments recovered from the Bengal Fan): "From 7 to 1 Myr, clays in the Fan are dominantly pedogenic smectite and kaolinite (SK assemblage), reflecting more intense weathering in the GB floodplain." I think the key point of the France-Lanord and Derry (1997) paper is not that silicate weathering is minimal. Rather, they emphasize that carbon burial in the Bengal Fan appears to be higher than is likely generated by carbon drawdown from the atmosphere via silicate weathering. In my opinion, this does not necessarily mean that silicate weathering is low. It means that carbon burial in the Bengal Fan is higher than expected. Three untested stimulants for this additional carbon burial could include: (1) monsoonal upwelling in the Indian Ocean results in increased productivity, (2) such productivity might be further enhanced because silicate weathering will release more phosphate and nitrate to the Indian Ocean than may have been previously accounted for, and (3) dry-season winds will carry dust from India and adjacent countries from land into the Indian Ocean where it may fertilize nitrogen-fixing bacteria so as to augment productivity. Finally, and more to the point, there is work suggesting that the silicate weathering in the low-lying floodplains of the Himalayan system is indeed significant. West et al. (2002) did an interesting study on just this issue in the Ganges floodplain, showing that, when normalized to area, this region is undergoing weathering fluxes comparable to those of ocean island basalts and terrestrial basalts from tropical regions.

9. In their section 8 on calcite and aragonite seas (p. 4536; line 16), they suggest the



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possibility that control of the oceanic Mg/Ca ratio that influences aragonite seas vs. calcite seas could be driven by weathering of basalt provinces on land if ocean-floor influence on this ratio is minimal because of Rowley's (2002) suggestion that rates of ocean crust production and seafloor hydrothermal activity were steady. This is an intriguing consideration. If correct, it could explain why the Mg/Ca ratio started rising at roughly the same time as the Deccan Traps eruptions. Their plot (Fig. 6) of exposed subaerial basalts within 5 degrees (latitude) of the equator shows an interesting increasing trend, with one negative spike. The general trend roughly matches the Cenozoic increase in Mg/Ca ratio. Testing their model by going further back in time raises some interesting points. The plot of Mg/Ca ratio (e.g. Lowenstein et al., 2001, which Kent and Muttoni cite) shows spikes in the Early Triassic and Early Jurassic. These could correspond to chemical weathering of the Siberian Traps and CAMP LIP, respectively. However, going back further in time is less instructive. A positive spike in the Mississippian does not appear to correspond to weathering of any LIP. Alternatively, it might correspond to assembly of Pangea and the silicate weathering expected at that time.

10. A very important issue here is that if Rowley (2002) is correct in that rates of ocean crust formation were fairly steady in the Cretaceous (and perhaps in other intervals?), then weathering of large basalt provinces might well be highly significant in their effects on both Mg/Ca ratios in the oceans and on climate change.

Specific Comments

p. 4534, line 12: It is stated that climate "deteriorated" here. Why not say that climate cooled? You're implying that warm is good and cold is bad.

p. 4532, line 16: "Burial of organic carbon can also sequester pCO2." It would be better to say either "sequester CO2" or reduce "pCO2."

p. 4532, line 25: "reasonable" should be changed to reasonably.

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