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***Interactive comment on* “Linking the lithogenic, atmospheric, and biogenic cycles of silicate, carbonate, and organic carbon in the ocean” by S. V. Smith and J.-P. Gattuso**

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The authors, to some credit, have refused to blindly accept established theories about the long-term regulation of atmospheric pCO<sub>2</sub> via (silicate) rock weathering (the ‘silicate weathering thermostat’), and present a paper questioning the accepted theory. Furthermore, they offer a constructive ‘solution’ to the dilemma they purport to identify. The problem is that there is nothing actually ‘incorrect’ about the theory they are questioning, and the paper presented here appears at best confused (or confusing), but elsewhere, simply incorrect.

Rather than jump straight into (calcium) silicate weathering on which they focus, it is

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easier to consider the weathering (and subsequent precipitation at steady state) of (calcium) carbonate ( $\text{CaCO}_3$ ), if only because their reasoning can be equally applied to this simpler situation (and in any case, silicate weathering is 'free' to regulate  $\text{CO}_2$  and climate regardless of the authors assertions about 'missing'  $\text{CO}_2$ ).

The author's argument, in essence, is this:

(i) Take a mole of  $\text{CO}_2$  from the atmosphere. Dissolve it in (rain) water to form carbonic acid and react it with one mole of  $\text{CaCO}_3$  in rocks on land. ( $\text{CO}_2 + \text{H}_2\text{O} + \text{CaCO}_3 \rightarrow \text{Ca}^{2+} + 2\text{HCO}_3^-$ )

(ii) Transport the resulting solutes ( $\text{Ca}^{2+} + 2\text{HCO}_3^-$ ) via rivers to the ocean.

(iii) When one mole of  $\text{CaCO}_3$  is eventually precipitated and preserved (to balance the long-term calcium cycle, and in the shorter term, any imbalance between weathering and burial via the preservation efficiency of  $\text{CaCO}_3$  in deep-sea sediments) one model of  $\text{CO}_2$  is 'released' to the ocean. ( $\text{Ca}^{2+} + 2\text{HCO}_3^- \rightarrow \text{CO}_2 + \text{H}_2\text{O} + \text{CaCO}_3$ )

But, and the crux of the paper, is that (for modern seawater chemistry)  $\text{CO}_2$  released to seawater in the precipitation step (iii) partitions only about 60:40 between atmosphere and ocean according to Frankignoulle and others, and hence the atmosphere only receives 'back' 60% of the initially lost mol of  $\text{CO}_2$  (that was through weathering).

BUT, when you remove 1 mol of  $\text{CO}_2$  from the atmosphere in the initial weathering reaction, carbon re-partitions between ocean and atmosphere. Hence, it is not 1 mol of  $\text{CO}_2$  that one must replenish in the atmosphere through carbonate precipitation and  $\text{CO}_2$  release, but rather less. There is also a repartitioning of  $\text{CO}_2$  between the ocean and atmosphere upon addition of the weathering products ( $\text{Ca}^{2+} + 2\text{HCO}_3^-$ ) to the ocean (but before precipitation). In fact, the cycle is closed, and atmospheric  $\text{CO}_2$  is left unaltered at steady state and the atmosphere is in no danger of becoming 'exhausted' of  $\text{CO}_2$  as the authors suggest.

We can play a simple thought experiment to see this, since the act of weathering and

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removing  $\text{CaCO}_3$  from the land surface and re-depositing  $\text{CaCO}_3$  in accumulating sediments at the same rate (at steady state) is a zero sum, i.e., all that is happening in (i) + (ii) + (iii) above is that  $\text{CO}_2$  is being initially removed from the atmosphere and added to the ocean. Given a hypothetical machine that does this – removing  $\text{CO}_2$  from the atmosphere and pumping it into the ocean (actually, a serious and planned geoengineering strategy), would the authors really claim that given enough cycles of transferring  $\text{CO}_2$  and air-sea re-equilibrium, that all the  $\text{CO}_2$  would eventually end up in the ocean and a new steady-state would be reached with no  $\text{CO}_2$  in the atmosphere? This is in effect what they are proposing.

Couching the argument in (calcium) silicate weathering rather than (calcium) carbonate weathering makes no difference. In fact, because silicate weathering removes  $\text{CO}_2$  permanently from the atmosphere at a rate that is dependent on climate, even if (and it does not)  $\text{CO}_2$  went ‘missing’ from the atmosphere because of the ‘0.6 rule’, climate would cool, weathering would reduce, until sources (mantle out-gassing) was re-balanced by weathering consumption. Actually, the authors incorrectly omit the climate dependency of weathering in their discussion immediately following their Equation (3) as well as the buffering of atmospheric  $\text{CO}_2$  by the ocean reservoir to make the inference that the atmosphere exists on a knife-edge of losing all its  $\text{CO}_2$  in the event of even the slightest imbalances from steady state – except on the shortest, anthropogenic time-scales, atmospheric  $\text{CO}_2$  is firstly buffered by the ocean reservoir, then by marine sediments, then controlled via climate by weathering.

The underlying and fatal flaw in the reasoning and hence the entire hypothesis is that the authors consider the ocean-atmosphere  $\text{CO}_2$  partitioning due to only one ( $\text{CaCO}_3$  precipitation) out of the 3 occasions when re-partitioning occurs in the complete weathering cycle of the real World (the other 2 being the initial removal of atmospheric  $\text{CO}_2$  in rainwater as carbonic acid, and the addition of weathering products to the ocean). There is a reason why some of us spend our lives constructing global carbon cycle models (that the carbon cycle is not always initiative). I must admit to not having got as

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far as reviewing the 'solution' later in the paper of the (non existent) problem.

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