Biogeosciences Discuss., 11, C80–C84, 2014
www.biogeosciences-discuss.net/11/C80/2014/

Biogeosciences
Discussions

Interactive comment on "Challenges and opportunities to reduce uncertainty in projections of future atmospheric CO_2: a combined marine and terrestrial biosphere perspective." *by* D. Dalmonech et al.

I. C. Prentice (Referee)

colin.prentice@mq.edu.au

Received and published: 18 February 2014

General comments

This review is unusual in covering both terrestrial and marine carbon cycle feedbacks. It provides a wide-ranging introduction to the recent literature in the field. This is one function of a review. But I also look for new insights and clear messages. Both are lacking.

Much of what is said in the later part of the review about metrics and benchmarking

goes over ground already covered in the recent Foley et al. paper in Biogeosciences. The MS's claim to novelty therefore has to rest on its treatments of issues specific to the carbon cycle, as its title suggests. Unfortunately, these treatments are generally unsatisfactory.

A good starting point for the review would have been the IPCC AR5 (Working Group I report). AR5 made a comprehensive and quantitative analysis of the carbon cycle and other biogeochemical feedbacks based on the CMIP5 models. The authors should have demonstrated progress beyond the state of the art as represented in AR5, but this was not at all clear.

I was particularly disappointed by the superficial treatment of process uncertainties in the terrestrial carbon cycle. This should have been noted, and corrected, by the experienced carbon cycle scientists who are in the author list. I was also surprised to find instances of fractured English, given the presence of native English speakers among the co-authors. I detected many typos, and missing or incorrect words. I have not flagged these one by one as they were too numerous. But obscure clauses such as 'New marine dataset will allows to explore, along to stock data, information. . ..' (line 567) or '. . ..have been shown to exhibit a lower compared to. . ..' (line 762) betray a lack of attention to meaning.

Specific comments

Section 3.1 is exceptionally weak, and gives the false impression that very little is known about any of the processes. 'Down-regulation' of photosynthesis is said to be 'acting on physiological and biochemical adjustments', an empty statement. No reference is given. Down-regulation refers to a downward adjustment of Rubisco capacity that is nonethless almost always accompanied by an increase in photosynthesis. There are several hypotheses on the cause of this adjustment that surely deserve mention, if the process is to be mentioned at all.

The same paragraph refers to changes in allocation as follows: 'these [?] characterized by significant complexity'. . . and cites 'Chaplin' (presumably Chapin) for a general lack of understanding of below-ground processes. This is a travesty of the state of knowledge on CO2 effects on allocation. To be sure, there are controversies, but the text here gives the impression of total uncertainty rather than controversy, and conveys nothing of what the controversies might be about.

The next paragraph (line 290 et seq.) fails to make a distinction between sink limitation (quixotically championed by Körner and co-workers as the main control of terrestrial carbon uptake) and nutrient limitation. It mentions 'merismeric control': does this mean 'meristemic'? In any case, sink limitation is very far from being accepted as a universal regulator of plant growth. Nutrient limitation is a different issue, as there is clear experimental evidence that plant growth responds positively to (for example) N additions in boreal and temperate forests – even if there is no general agreement yet about how this process should be represented in models.

Lines 299 et seq. start confusingly by mentioning temperature controls of photosynthesis, autotrophic respiration and heterotrophic respiration – processes all with different temperature controls! – in a single sentence, and then referring only to Lloyd & Taylor's paper, which refers to heterotrophic respiration alone. As with 'down-regulation' in the previous paragraph, 'acclimation' is introduced here without definition or explanation. The paragraph's conclusion that there is 'no general pattern' to photosynthetic temperature acclimation is wrong: a general pattern has been described in a thorough analysis published by Kattge & Knorr.

Lines 310 cite Atkin's work showing that the base rate of respiration acclimates to temperature, but it's said only that 'plants might' adjust their respiratory rate in this way. . . the point is that they do. Subsequent mention of vapour pressure deficit (vpd) seems out of place: nothing is said anywhere about the key role of vpd in regulating stomatal conductance, and how this affects photosynthesis, whereas it isn't at all clear what stomatal conductance has to do with this paragraph. The basics are missing, in other words, and as a result the text make no sense.

The last paragraph on p11 omits key literature on the control of heterotrophic respiration. It is not explained how 'substrate limitation' might reduce temperature sensitivity. Holland (cited) was also an author on the Knorr et al. (1995) Nature paper that clarified the response of heterotrophic respiration to temperature.

p12 discusses the importance of demography, but strangely does not mention global vegetation models that include demography explicitly, such as LPJ-GUESS, ED and POP.

The last paragraph on p12 mentions N and P cycle constraints on C cycling, but it is hard to understand: what does 'while alleviating only little of terrestrial N limitation in response to warming' mean? What is doing the 'alleviating' and how? Then (p13) it is mentioned that only a few ESMs include these constraints. It's true, and I'm sure it's a good thing in principle to represent the interactions among biogeochemical cycles in ESMs. But the two CMIP5 models that do include interactive N cycling are unable to take up CO2 at a realistic rate, which suggests that they are simulating the coupling incorrectly – a caveat would thus be in order.

Lines 379-380: carbon stocks should be in PgC, not PgC/y.

Line 489: Cox et al. (2013) is surely relevant here.

Line 535: Piao et al. (2013) also voiced strong reservations about the Jung et al. product if used as a reference for interannual variations in GPP.

Lines 540-545: This comparison is useful and does not look good for remotely sensed greenness over the Amazon basin. However this is a much-researched topic now, and the MS does not do justice to the recent literature on the seasonality of primary production in Amazonia.

Lines 640-642: The uses of palaeodata are dismissed in three lines, referring to 'uncertainties in forcing data' and 'in the proxy data' as an excuse for palaeodata allegedly not providing a strong constraint. This is completely unacceptable, especially in an output from a project (GREENCYCLES II) with a strong palaeo dimension! The statement here implies either that palaeodata are useless, or that there are not large uncertainties in current observations (for example, Amazon greenness?!) Similarly with the forcing: orbital changes, greenhouse gas concentrations, sea level, ice sheet extents are all very well known at the major target periods for palaeoclimate modelling. If palaeoclimate data are excluded from this review, it should be simply said that they are 'beyond the scope', or nothing at all should be said, rather than attempting to dismiss them in a sentence.

Colin Prentice

Interactive comment on Biogeosciences Discuss., 11, 2083, 2014.