Biogeosciences Discuss., 9, C213–C217, 2012
www.biogeosciences-discuss.net/9/C213/2012/

Biogeosciences Discussions

Interactive comment on "How will organic carbon stocks in mineral soils evolve under future climate? Global projections using RothC for a range of climate change scenarios" *by* P. Gottschalk et al.

C.D. Jones (Referee)

chris.d.jones@metoffice.gov.uk

Received and published: 5 March 2012

Review of Gottschalk et al., "How will organic carbon stocks in mineral soils evolve under future climate? Global projections using RothC for a range of climate change scenarios"

This manuscript addresses the clearly very important question of how global soil carbon stores will change under future climate and environmental conditions. Specifically it tries to assess some level of uncertainty in this change, due to uncertainty of climate

projections from a range of GCMs and a range of emissions scenarios. It is clearly very relevant to BG readership and the climate/carbon cycle community at large. The paper is well written and clearly presented with good discussion of the relevance of its results.

My one serious concern about this paper is its use of the MIAMI model to derive future NPP changes as input to the soil cabron model. You correctly state that future SOC changes will be the balance of future climate change (where warming typically increases turnover rates) and vegetation inputs (which are sensitive to both climate and rising CO2 and will vary regionally). Getting this balance right therefore is clearly crucial - or else your results might be wrong even in their sign of response not just magnitude... While you take a wide sample of future climate projections from CMIP3 GCMs, the use of the MIAMI model to generate future NPP scenairos is very much a weak link I think.

For example, one of your main conclusions is that future SOC increases in the tropics but decreases in the high-latitudes. This surprised me, and is a direct result of the future NPP changes you assume. You seem to get strong NPP increases in the tropics and much lower incerases in high latitudes (figure 7). This is at odds with most predictions of future NPP you would get from more up-to-date, process-based vegetation models (such as C4MIP GCMs of Friedlingstein et al., 2006, J.Clim, or offline DGVMs from, say, Sitch et al., 2008, GCB). I would expect a strong increase in NPP in high latitudes in most models where growth is currently temperature limited. I would also expect a spread of results in NPP in the tropics where growth is more often moisture limited and future climate can get drier or wetter regionally with high uncertainty. See for example, Raddatz et al (2007, Clim Dyn, their figure 6), for an exact opposite pattern.

Specifically, the MIAMI model equations you show, have no limitation on NPP from high temperature, which means that progressivle warmer and warmer tropics will get higher and higher NPP - which I don't believe is realistic. In the tropics, seasonality is

BGD
9, C213–C217, 2012

also important and so use of annual precip will hide the fact that a long dry season can reduce NPP, even if the annual total has plenty of rainfall.

Similarly, Qian et al (2010, GCB) show a strong concensus from C4MIP models of INCREASES soil carbon in high-latitudes - I don't understand why your NPP wouldn't incerase here too.

So overall, I find this a good and worthwhile paper, but would really like to see a way round the clear weakness of using future NPP inputs that don't agree with the wide range of more modern process-based models. For example, could you use the NPP in some of the GCMs you use? both HadCM3 and HadGEM1 will output NPP despite not being "carbon cycle GCMS". i don't know if this is possible for other GCMS you use. At the very least I think you should quantitatively compare the NPP scenarios you use with others for the future (at a regional basis). If it turns out that your scenarios are within the spread of future tropical/high-latitude outputs from other models then it becomes OK to use. But if your NPP turns out to be inconssistent with other models you might need to find a better way of specifying this input.

I think it is really important to get this right because it is clearly a driving force behind your conclusions. If you can solve this, then the work becomes an important piece of work.

My other comment that would help out this in context would be to discuss literature on the other aspects of uncertainty. You nicely span the axis of climate uncertainty from other GCMs/scenarios. Papers like Sitch et al span the orthogonal axis of vegetation/soil models driven by a single climate model. Can you compare the range of uncertainty from each approach? A further paper you might like to compare with is Falloon et al (2011, GBC) which directly looks at the osil moisture control on future SOC.

other, minor comments follow.

BGD

9, C213–C217, 2012

Chris Jones

Abs. line 19. While I agree it is key to understand the processes underlying the different regional response of SOC stocks, and hence to improve the models, I think we still need to ask the question, at a global level, what the future change iin

SOC will be. This is one of the major carbon store changes which will affect future atmospheric CO2 - so a global change in SOC is one of the big climate-relevant quantities we do need to quantify.

p.414, line 2. 0.7 degrees warming is the total warming since pre-industrial - your text makes it sound like this is the component due to SOC changes.

p.420. eqns 2,3 - can you explain these equations better? I didn't understand what the "=" sign within the expressions means? can you also define the units (gDM m-2 yr-1) - DM=dry matter? (presumably means carbon?)

p.421. if I understand this right, you also use model precip (and evap?) to drive the RothC bucket hydrology. As with NPP, would you get better results to use the GCM soil moisture directly as these typically have more sophisticated hydrology models than RothC

p.422. use of NPP as plant input to the soil - this seems entirely appropriate for natural ecosystems. I wouldn't expect the difference between NPP and litterfall to be significant on these timescales. But for harvested ecosystems this isn't true. I've just noticed after writing this review that this is already covered in the BGD discussion - how does your approach deal with regions that begin as either natural or managed and then change to being managed or natural? The scaling wouldn't hold if the land-use CHANGES (which is part of your experiment design). So how do you deal with this?

p.422. More widely, can you explain more about how land-use affects SOC in this set-up? As far as i can tell, it changes the DPM;RPM ratio - so that more plant input goes into DPM and decomposes faster. Does it include any treatment of: - harvest -

BGD

9, C213–C217, 2012

Interactive Comment

is any plant input removed before going into the soil? (see above point) - fertiliser - is NPP higher on agricultural ground? - physical disturbance 9eg tillage) of the soil? If these important processes are not included (which is fine - most global models neglect these too), then you probably shouldn't make quantitative statements about the impact of LULUC on SOC.

Fig.1 - just to clarify. Panel (c) says just for simulated cells - which presumably means vegetated land points. Is this also true for the other panels? e.g. is panel (d) just the temperature from vegetated land points? This figure needs to be consistent

Fig. 2 - Can you explain why the land-use values you plot here are different for different GCMs? surely the land-use is a part of the scenario and should be the same for each GCM. So why is "AOGCMs A1B" different from "HadCM3 A1B"?

Fig 9 - I like this way of showing the dominant uncertainty. If I had to be really picky about all your maps, can you shift you projection left by 10 degrees so that the end of Siberia doesn't get chopped off?

Interactive comment on Biogeosciences Discuss., 9, 411, 2012.