Biogeosciences Discuss., 8, C1204–C1206, 2011
www.biogeosciences-discuss.net/8/C1204/2011/

Biogeosciences Discussions

Interactive comment on "Estimating the permafrost-carbon feedback on global warming" *by* T. Schneider von Deimling et al.

Anonymous Referee #1

Received and published: 20 May 2011

GENERAL COMMENTS

This paper takes a novel approach to develop a general quantification, including uncertainty, of the impact of permafrost thaw and carbon cycling, and the feedback to warming. This is an important global change feedback issue that is not well-understood, and one that has gotten a lot of public and media attention. A good quantitative study like this is an important addition to the literature. The model is clearly presented, the manuscript is well-written, and the results are well-organized and not over-hyped.

SPECIFIC COMMENTS

1. I think it is important to make it clear that landscapes with permafrost have carbon fluxes while the permafrost is present, and whether or not the permafrost is changing.

They may have net CH4 emissions or uptake, net CO2 emissions or uptake. I'm sure you are well aware of this, but you might be misinterpreted. For example, in figure 3b and 3c, these are not really 'permafrost emissions'. There is still permafrost present at least until 2150 in the RCP8.5 (though deeper in the soil), and the organic matter in that permafrost is still essentially inert (frozen); the reported emissions derived from organic matter in newly thawed soil. In Fig. 3d,e it is not permafrost induced change, but thaw induced change. Any 'permafrost thaw' emissions will be in addition to the net emissions that were occurring before the thaw, which may also be changing with climate change due to warming, wetting/drying, elevated CO2, vegetation dynamics, . . . (do you account for this potential change in 'background emissions' in the analysis? If not, that should be made clear). I think that 'emissions derived from organic matter in newly thawed soil' is what you intend to report (a shorter name would be better), and, if so, I think that you should state it clearly early in the paper, and make sure your terminology is clear.

2. The nonlinear patterns in permafrost carbon sensitivity (Section 3.3 and Table 2) are an interesting result, but the pattern is not easy to understand. Could you expand this discussion a bit to explain why (1) the sensitivities increase over time, and (2) are higher for the high (RCP8.5) and low (RCP3-PD) scenarios than the intermediate scenarios (RCP4.5 and RCP6)?

3. Equation 3: the latitudinal amplification factor, alpha – should that be constant for the entire permafrost domain, or should it have a latitudinal gradient? How much would that affect your results?

4. I was a bit confused by the 'summer temperature' terminology. Is this really an estimate of summer temperature (e.g., temperature of warmest month or June/July/August average or something like that) or just a measure of effective thaw temperature – mean annual temperature above some threshold for each zone? What sort of values do you get for T-summer (1° would be a guess for warming above a threshold, 10-15° would be a guess for a real summer temperature)?

5. Table 1: it would be helpful to have citations for the default and/or sensitivity ranges in the table, either as an additional column or as footnotes. In the sensitivity range for the Q10 parameters, you report two parameter values but only one Q10 value. The high end of your sensitivity range for methane oxidation fraction (10-20%) seems low to me; I don't think this process is that well-constrained by observation at this point.

TECHNICAL CORRECTIONS

a. In some places you use 'MAGICC' and in others 'MAGICC6'. If these are different, please explain; if not, please use consistent notation.

b. lines 179-180: what do you mean by 'temporal' wetlands? Temporary? If so, for how long? Seasonal?

c. lines 193-195 or so: I don't think it is a given that precipitation increases will outweigh ET increases everywhere, particularly given the uncertainty in precipitation (relative to temperature, which may be a reasonable predictor of ET). I believe some macro-scale hydrology simulations show that runoff changes are relatively small, due to off-setting increases in ET and precipitation (Fekete et al. Global Biogeochem. Cycles, 24, GB0A12, doi:10.1029/2009GB003593, looking ahead to 2050).

d. line 206: Heimann, not Hermann.

e. The thermokarst methane bubble flux results of Walter et al. have gotten a lot of attention, but I don't think that they have been widely replicated. They are worth mentioning, but once is enough.

f. line 432: what do you mean by 'inert'? maybe 'inertia'?

g. line 524: I don't see a hyphen (i.e., –); do you mean a 'prime' (i.e., ')?

h. line 525: since the A term is a fraction, it would probably be better to say 'starting at 1.0' rather than '100%'

Interactive comment on Biogeosciences Discuss., 8, 4727, 2011.