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The Cryosphere Discussions

TCD

6, C790–C797, 2012

Interactive Comment

Interactive comment on "Uncertainties in the global temperature change caused by carbon release from permafrost thawing" *by* E. J. Burke et al.

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The authors appreciate these positive responses to their paper and the thorough reviews. Thanks!

Many of the processes and feedbacks involved in permafrost carbon release remain very uncertain. For example, we do not have full understanding of how rapidly the permafrost will thaw, how much C will be lost, the rate of decomposition, and whether this decomposition will be aerobic or anaerobic. Whilst some of the specific processes cannot be simulated directly in these types of models, by carrying out the sensitivity analysis across a broad range of parameters, this study investigated what the major uncertainties were given the process representation which is already in HadGEM2-ES. To our knowledge this is the first analysis of its type and should be useful to both modellers and empirical scientists. The processes which are currently missing are discussed in a little more detail in the discussion section.

RC C587

(1) Climate mitigation is indeed a complex problem which will be discussed in further detail in a subsequent paper. Any reference to climate mitigation in this document will be removed.

(2) Page 1369, line 21: Change “thus” to “this”. This will be changed.

(3) Page 1370, line 11: Change “an accumulative” to “a cumulative”. This will be changed.

(4) Page 1371, lines 24-25: I think you first need to have some description of the approach that HadGEM2-ES uses to simulate soil temperatures before you describe how you diagnosed the zero degree isotherm.

Indeed HadGEM2-ES assumes that the soils in northern high latitudes are mineral soils and does not consider the thermal and hydrological properties of organic soil horizons and moss. In addition, the snow scheme in HadGEM2-ES does not provide sufficient thermal insulation. Therefore the simulated temperatures are biased very low particularly in winter and HadGEM2-ES over-estimates the permafrost extent. HadGEM2-ES simulated permafrost extents are $\sim$ 23.8 million km2 which is larger than the 12-17 million km2 suggested by observations (Zhang et al. 2003). Adding a correction for this overestimate will reduce the total amount of permafrost carbon in the present-day. A new snow scheme is currently being tested within the land surface scheme of HadGEM2-ES (JULES) which significantly reduces the cold bias in the wintertime, although a slight cold bias remains all year round.

Dankers et al. (2011) did a comparison with the CALM active layer thicknesses for a version of JULES similar to that used in HadGEM2-ES and found the simulated active layer thickness was deeper than observed. Dankers et al. (2011) discuss these biases in further detail.

Koven et al., (2012) perform an evaluation of permafrost in the CMIP5 models (including HadGEM2-ES) and assess the thermal connection between the surface air and soil. They show that there is too strong a thermal connection in HadGEM2-ES. This will be reduced in future versions of HadGEM which include the new snow scheme.

The following paragraph will be added to the text under the model description section:

'HadGEM2-ES calculates soil temperatures using a discretised form of the heat diffusion equations with the soil thermal characteristics realistic functions of the soil moisture content. It also includes the latent heat from water phase changes in the subsurface calculations. There are no explicit snow layers in the model but the top soil layer is adapted to represent lying snow processes. HadGEM2-ES treats all soil as mineral soil and does not consider the thermal and hydrological properties of organic soil horizons and mosses which are particularly important when simulating soil temperatures in northern high latitudes.'

A further discussion of the consequences of these biases will be added to section 5. This is discussed in more detail in RC672.

(5) Page 1372, line 10: Reference to Fig. 1. I don't see any units on either of the axes in Figure 1. These will be added.

(6) Page 1372, lines 13 and 14: See my comment number 4 about the issue of accuracy. It would be good to establish the degree to which the 70 layer model is accurate vs. "assuming" it is accurate. See the response to (4) and response to RC 672.

(7) Page 1372, line 18: I noticed that Burke et al. (2012) is "in preparation". It doesn't likely that the cited in preparation manuscript will be published prior to a decision on this manuscript. This paper is currently under second review with Climate Dynamics.

(8) Page 1372, line 24: Change “interpolated” to “interpolation”. This will be changed.
(9) Page 1373, line 27: It seems that there should be a comma after “pedons”. This will be changed.

(10) Page 1374, line 13: “Turbles” should be “Turbels”. This will be changed.

(11) Page 1375, section 2.3: One assumption that is being made here is that there are no new inputs of carbon into the thawed permafrost layers. I think you need to make a statement that the methodology does not consider this issue, and it would be good to also return to it briefly later in the paper, perhaps in section 5. This sentence will be added in section 2.3: ‘It is assumed that there are no new inputs of carbon into the thawed permafrost layers and all vulnerable carbon is thawed permafrost carbon.’ This sentence will be added in Section 5: ‘The processes included within this model are highly simplified and do not include any interactions between the carbon currently within the carbon cycle and the thawed permafrost carbon’.

(12) Page 1375, line 15: Change “immediately the permafrost” to “immediately as the permafrost”. This will be changed.

(13) Top of page 1378: When I got to the end of section 2.3.2, I was surprised to not see any explanation of the last two parameters in Table 1. I see that the explanation of these parameters come in the Results section, but I was wondering I they could be explained in the methods, and it seems most appropriate to do this at the end of the full paragraph on page 1374. I will add a reference to Table 1 earlier in the document (section 2.2) where the uncertainties in the soil organic carbon content are discussed. I will also change the order in Table 1 so these parameters come first as in the document.

(14) Page 1379, line 10: Change “at each year” to “for each year”? This will be changed.

(15) Sentence spanning lines 26-29 of page 1381: I really can’t figure out what was

done “by perturbing the mean SOCC for each soil by 0.75 of the standard deviation and using . . .)”. Please rewrite this and justify this methodology better. This sentence will be replaced with: ‘The minimum/maximum of the vulnerable carbon were found by reducing/increasing mean SOCC to their minima/maxima (see section 2.3) and using the minimum/maximum of the soil SOC reduction factors shown Table 1.’

(16) Page 1382, line 25: “Schaeffer” should be “Schaefer”. It also seems that you may want to cite Zhuang et al. (2006; CO2 and CH4 exchanges between land ecosystems and the atmosphere in northern high latitudes over the 21st Century. Geophysical Research Letters 33, L17403, doi:10.1029/2006GL026972.) along with Schaefer, Schneider, and Koven. This will be added.

(17) Page 1384, line 15: “Eq. (3)” should be “Eq. (4)”. This will be changed.

(18) Page 1385, lines 21 and 22: I assume you mean “high latitudes” instead of “lower latitudes”. This will be changed.

(19) Page 1394: For the “soil reduction” parameters in Table 1, perhaps rename them to be “soil SOC reduction” to be more descriptive. This will be changed.

RC C672

The authors acknowledge other mechanisms for carbon burial (cryoturbation) and remobilization (thermokarst, fire, coastal and river erosion, possibly decomposition heat), as well as deep carbon pools (e.g. Yedoma), that are not included in this model exercise. As most of these would result in more rapid SOM remobilization it can be stated that the permafrost carbon feedback in this paper is probably under-estimated.

This will be added.

It would have been particularly interesting if thermokarst formation, expansion and drainage could have been simulated by ‘prescribing’ a transient increase in lake area, which would have exposed more carbon to talik formation and have increased the proportion of SOM decomposed anaerobically (methane emissions). However, this is probably much more complex to implement in a model environment.

This is an interesting comment! HadGEM2-ES simulates wetland extent and changes in wetland extent under future climate scenarios. We thought about using the HadGEM2-ES simulated wetland extent and its changes in this framework and decided against it. This is a difficult process to simulate and uses a number of additional assumptions which need to be justified. However, this is a process which could be readily incorporated into the framework as it stands to explore its potential impact on the permafrost carbon release. One for future work!

GENERAL COMMENTS

The authors appropriately indicate that the modeled extent of the current permafrost region and the total soil C pool for the upper 3m compare reasonably well with observations (Zhang et al., 2003; Tarnocai et al., 2009). My only main concern with the paper is that the authors do not address sufficiently the error resulting from an overestimation of the present-day active layer in the HadGEM2-ES model. There is only a vague reference to this issue in the final conclusions section (page 1388, lines 1-2). This implies that too much soil C is already in the active layer and, therefore, less will become available for future decomposition as permafrost thaws. There is likely a double bias here, because it is often the most organic-rich soils (peat deposits) that have the shallowest active layers.

I will add a section discussing this at the beginning of section 5. See also response to RC587 (4).

The authors should make an effort to compare the modeled depth of the active layer with observations (e.g. the CALM network), and quantify the amount/proportion of soil C that presently resides in their modeled active layer. This will provide a good indication of the extra amount of soil C that actually is perennially frozen under current conditions compared to model results.

The comparison with the CALM sites done by Dankers et al. (2011) was used as a rough approximation of the bias in the active layer. Very approximately, if the extent is assumed to be overestimated by ~1/3 (23.8 million km2 compared with between 12 and 17 million km2) and the active layer ~1/3 too deep (Dankers et al., 2011) the large-scale high latitude permafrost volume in the top 3 meters of soil has a relatively small bias. However, this may result in a slightly low estimate of soil organic carbon because the soil organic carbon content generally decreases with increasing depth and decreasing latitudes.

Of particular relevance here, and arguably more important, is the sensitivity of the maximum active layer thickness to global mean temperature. This is an important factor which was not fully discussed in the paper. An analysis of the CMIP5 global climate models shows significant differences in their sensitivities. This is an additional uncertainty which impacts the permafrost carbon release. Koven et al. (2012) did an excellent study examining the CMIP5 models and their ability (or lack thereof) to simulate current permafrost and active layer and their sensitivity to changing temperature. Sentences discussing this will be added.

SPECIFIC COMMENTS

page1372-line4: check sentence, snow is definitely part of the climate system

This will be changed to ‘snow cover’.

p1373-l6: modeled active layer depths across latitude in fig. 2 (p1396) seem to be exaggerated compared to observations (see also p1381-l1-2), and would probably only apply to well-drained upland soils with thin top organic layers. For instance, Histels near the southern limit of permafrost distribution can have active layers of only 50-60 cm. The specific thermal properties of different soil types are not considered in the model setup.

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Interactive Discussion

Discussion Paper

A fuller description of the simulation of soil temperatures in HadGEM2-ES will be added. See also response to RC587 (4).

p1376-l13. Turbels This will be changed.

p1385-l21: Thermokarst terrain is widespread at higher latitudes (in the whole permafrost region) This will be changed.

p1385-l22-24: Observations suggest that lateral erosion and ground subsidence increase thaw lakes area and number in continuous permafrost regions and drainage decreases them in discontinuous regions . . . This will be changed.

p1386-l12: after cryoturbation; add 'coastal and river erosion' This will be added.

p1387-l14: after cryoturbation; add 'coastal and river erosion' This will be added.

page 1388, lines 1-2:provide an estimate for these uncertainties; calculate proportion of soil C in the modeled active layer under current conditions which can be compared to the estimates in Tarnocai et al. (2009) and regional studies. This will be discussed in detail in section 5 and alluded to here. In addition uncertainties in the sensitivity of the active layer thickness to global mean temperature will be mentioned as these are important.

Interactive comment on The Cryosphere Discuss., 6, 1367, 2012.