Response to reviews of manuscript:
Projecting the release of carbon from permafrost soils using a perturbed physics ensemble

We appreciate the thoughtful comments of both reviewers and have responded to each comment below. The reviews are copied verbatim and are italicized. Author responses are in regular font.

Response to Reviewer 1:

Anonymous Referee #1
Received and published: 15 January 2016
Comments on the manuscript by MacDougall and Knutti "Projecting the release of carbon from permafrost soils using a perturbed physics ensemble" submitted to Biogeosciences Discussions.

Overall Evaluation This manuscript presents the results of a study that uses a new version of the UVic ESCM to conduct a parameter uncertainty analysis to report uncertainties in the release of carbon from the permafrost region to the atmosphere for four RCP scenarios through the year 2300. In general, the study finds that the mean response and range of uncertainty of the new version of the UVic ESCM are more in line with other recent syntheses. Some of the conclusions are similar to those of previous studies: (1) the permafrost carbon feedback is most important for scenarios with substantial mitigation, (2) that permafrost soils are expected to release carbon for a very long time because of the long time lag between forcing and response. The analysis does identify among the parameters considered that better constraints on the size of the non-passive soil carbon pools and on the equilibrium climate sensitivity of the model will substantially reduce uncertainty of responses of carbon in the permafrost region. Finally, the analysis includes an analysis of permafrost carbon responses out to year 10,000.

In general, I like the design of this study, and the analyses are quite competent. The parameter sensitivity analysis is very welcome and valuable with respect to building on the recent data synthesis of Schadel et al. (2014) on the quality of soil carbon in the permafrost region. However, there are a three shortcomings in presentation and discussion that I think should be addressed in a revision: (1) a clear communication in the Introduction of the objectives/questions of this study is needed, (2) what about uncertainty with respect to parameters not considered by the analysis, and (3) better justification of the deep future analysis. Below I provide more of my thoughts on these issues followed by other specific comments in the manuscript.

What are the objectives/questions raised in this study? The Introduction is very vague with respect to communicating the key objectives/questions of this study. The Introduction has paragraphs on uncertainty in soil carbon quality, methods for analyzing model uncertainty, and multi-millenial simulations of anthropogenic *climate change. However, these are somewhat disjointed and the Introduction needs to tie them together more effectively and communicate the key objectives/questions of the analysis after these "motivation" paragraphs. A key deficiency along these lines, is that the key take home of the parameter uncertainty analysis with respect to Schadel et al. (2014) didn't even make it into the abstract.*

We have revised the introduction and the abstract to better communicate the key objectives of this study. In the introduction we have expanded on the sentences from page 19503 lines 6 to 9 to create a new paragraph. The lines did read:

"Here we will propagate the newly quantified uncertainty in permafrost carbon parameter values through an improved version of the University of Victoria Earth System Climate Model (UVic ESCM) to quantify the uncertainty in the release of carbon from permafrost soils to the year 2300."

The new paragraph is placed on page 19501 after line 15 and reads:

"The objective of this study is to use the new constraints on the quantity and quality of the permafrost carbon pool to explore key questions about the effect of the permafrost carbon pool on climate change. The questions we will investigate are: 1) How much carbon will be release from permafrost soils by year 2100 and 2300, and what are the uncertainty bounds on these estimates? 2) Which of the uncertain parameters identified by Schädel et al. (2014) and Hugelius et al. (2014) contribute the most to uncertainty in the release of carbon from permafrost soils? 3) How much time will pass before the permafrost carbon pool comes into equilibrium with the anthropogenically perturbed climate? The following paragraphs briefly review how uncertainty is treated in the framework of Earth system models and the expected lifetime of anthropogenic climate change."

The abstract has been re-written to include the key result from the parameter uncertainty analyses. Note that the abstract has also been altered in response to comments documented below and adjusted to maintain the 250 word length limit. The abstract did read:

"The soils of the northern hemisphere permafrost region are estimated to contain 1100 to 1500 Pg of carbon. A substantial fraction of this carbon has been frozen and therefore protected from microbial decay for millennia. As anthropogenic climate warming progresses much of this permafrost is expected to thaw. Here we conduct perturbed physics experiments on a climate model of intermediate complexity, with an improved permafrost carbon module, to estimate with formal uncertainty bounds the release of carbon from permafrost soils by year 2100 and 2300 CE. We estimate that by 2100 the permafrost region may release between 56 (13 to 118)Pg C under Representative Concentration Pathway (RCP) 2.6 and 102 (27 to 199) Pg C under RCP 8.5, with substantially more to be released under each scenario by 2300. A subset of 25 model variants were projected 8000 years into the future under continued RCP 4.5 and 8.5 forcing. Under the high forcing scenario the permafrost carbon pool decays away over several thousand years. Under the moderate forcing a remnant near-surface permafrost region persists in the high Arctic which develops a large permafrost carbon pool, leading to global recovery of the pool beginning in mid third millennium of the common era. Overall our simulations suggest that the permafrost carbon cycle feedback to climate change will make a significant but not cataclysmic contribution to climate change over the next centuries and millennia."

And now reads:

"The soils of the northern hemisphere permafrost region are estimated to contain 1100 to 1500 Pg of carbon. A substantial fraction of this carbon has been frozen and therefore protected from microbial decay for millennia. As anthropogenic climate warming progresses much of this permafrost is expected to thaw. Here we conduct perturbed model experiments on a climate model of intermediate complexity, with an improved permafrost carbon module, to estimate with formal uncertainty bounds the release of carbon from permafrost soils by year 2100 and 2300 CE. We estimate that by year 2100 the permafrost region may release between 56 (13 to 118) Pg C under Representative Concentration Pathway (RCP) 2.6 and 102 (27 to 199) Pg C under RCP 8.5, with substantially more to be released under each scenario by year 2300. Our analysis suggests that the two parameters that contribute most to the uncertainty in the release of carbon from permafrost soils are the size of the non-passive fraction of the permafrost carbon pool and the equilibrium climate sensitivity. A subset of 25 model variants are integrated 8000 years into the future under continued RCP forcing. Under the moderate RCP 4.5 forcing a remnant near-surface permafrost region persists in the high Arctic, eventually developing a new permafrost carbon pool. Overall our simulations suggest that the permafrost carbon cycle feedback to climate change will make a significant contribution to climate change over the next centuries and millennia, releasing a quantity of carbon 3 to 54% of the cumulative anthropogenic total."

What about uncertainty with respect to parameters not considered in this study. The manuscript needs to better justify why it focused on the 6 parameters it chose vs. other parameters it could have chosen. For example, a large component of uncertainty of the application of earth system models to analyzing the permafrost carbon feedback concerns the NPP response to increases in atmospheric CO2, yet this was not even mentioned in the discussion. I'm not suggesting that the study conduct analyses of additional parameters, but that it adequately discuss the relevance of the parameters it chose to include vs. those it chose not to include in the analysis.

Unperturbed parameters clearly also contribute to the unquantified uncertainty in the release of carbon from permafrost soils. This point is now better clarified in the manuscript. We have added a paragraph in the experiment design section (2.3) to explain why we chose the six parameters. The paragraph is placed on page 19508 after line 13 and reads:

"Besides the parameters we have chosen to perturb many other parameters in the UVic ESCM could affect the magnitude of the release of carbon from permafrost soils. In particular parameters from the Triffid dynamic vegetation model that control net primary production determine the input of carbon into the soil and therefore the net change is soil carbon in response to warming. However, for this study we have chosen to focus on uncertainty inherent to the permafrost carbon system instead of taking a global focus implied in perturbing the whole terrestrial carbon cycle (e.g. Booth et al., 2012)."

To the Discussion we have added a paragraph following line 18 on page 19517 to better acknowledge the unquantified parameter uncertainty. The paragraph reads:

"We have not quantified all of the parameter uncertainty that could affect the simulated permafrost carbon system. In particular the parameters in Triffid that control net primary productivity will determine the flow of organic carbon into soils and therefore the net release of carbon from permafrost soils."

Better justification of the deep future analysis? By the end of the paper, I wasn't convinced that the "deep future" analysis was very insightful. It was somewhat interesting to read through, but its relationship to mitigation in the near future didn't come across to me. It just seemed glommed onto the rest of the paper to me. I suggest either better justifying it in the Introduction and more effectively discussing its relevance, or dropping it from the paper.

The release of carbon from permafrost soils is conventionally given for a specific date in the future, with year 2100 being the favourite date. The deep-future experiments were intended to explore the ultimate release of carbon from permafrost soils. That is, carbon release as time->∞. Climate change will not end in 2100 (e.g. Clark et al. 2016) and we believe it is intrinsically of interest to project the effect of climate change on systems until the system comes into equilibrium with the new climate. We therefore would like to keep the deep-future analysis component of this study. To make the deep-future experiments better flow with the rest of the paper we have re-written the introduction of these experiments. For better clarity we have also changed the name of the experiment to the 'multi-millennial experiment'. In response to other comments from both Reviewers other changes have been made to the methods and results of the multi-millennial experiments that we hope has improved the clarity of these experiments.

The paragraph describing the multi-millennial experiment in the introduction (Page 19503 lines 10 to 19) has been changed from:

"Multi-millennial simulations of anthropogenic climate change suggest that the temperature anomaly caused by the burning of fossil fuels will last over 100 000 years (Archer, 2005). Such simulations suggest that 10 000 years into the future global mean temperature will remain approximately two-thirds of its peak temperature anomaly above the pre-industrial mean (e.g. Eby et al., 2009). Much of the permafrost carbon pool is highly resistant to decay (Schädel et al., 2014), however the long lifetime of anthropogenic climate change implies that the pool will eventually decay and its carbon will be added to the ocean-atmosphere system. To explore the ultimate fate of the permafrost carbon pool we have extended a sub-selection of model simulations to common era year 10 000."

To:

"Anthropogenic climate change will not cease in year 2100 (e.g. Clark et al., 2016) and the intrinsic timescale of decay of the passive component of the permafrost carbon pool implies that the permafrost carbon system will continue to evolve far into the future. Multi-millennial simulations of anthropogenic climate change suggest that the temperature change caused by the burning of fossil fuels will last for over 100 000 years (Archer, 2005), a period of time long enough such that the permafrost carbon pool may come into equilibrium with the new climate regime. To explore the long-term fate of the permafrost carbon pool we have extended a sub-selection of model simulations 8000 years into the future."

Specific comments

Title: I think the analysis of uncertainty is the most important aspect of this study, but "uncertainty" doesn't appear in the title. Also, why a "perturbed physics ensemble"? Don't some of the parameters analyzed represent biological phenomena? Wouldn't a "perturbed model ensemble" better wording?

'Perturbed physic ensemble' is a technical term that has been used by most of the other climate modelling studies that have used this method. For example: Collins et al. (2007), Collins et al. (2011), Shiogama et al. (2012), and Rowlands et al. (2012). The term flows from climate (and weather) modelling convention to divide models into 'model dynamics' the explicitly represented parts of the model, such as the equations of fluid dynamics, and 'model physics' all of the parameterized components of the model, including chemical and biological systems (e.g. Neelin, 2011). Stepping back, this jargon does seem odd when applied to the carbon cycle. Therefore to improve clarity we have changed the title of the paper to:

"Projecting the release of carbon from permafrost soils using a perturbed parameter ensemble modelling approach"

Abstract, Page 19500, Line 17: You need to define "common era" for the reader. I could only guess at what was meant by the term.

Common era is the secularized rendering 'Christian era' which itself is a secularized rendering of 'Anno Domini' (in the year of our Lord). The term is used to refer to calendar years of the Gergorian calendar system following that calendar's reference date. The term has been used without explanation in many papers in the Earth sciences. For example GRL paper Smerdon et al. (2011), which is titled "Spatial performance of four climate field reconstruction methods targeting the Common Era".

We have removed to two direct references to 'common era' in the introduction and abstract. However we maintain the use of this term in the figure caption by changing: "common era year 10 000" to "common era year 10 000 (8000 years into the future)."

Introduction, Page 19503, Line 19: Again, "common era" needs to be better defined.

For clarity we have changed "common era year" here to "8000 years into the future."

Methods, Page 19504, line 25: "organic matter content" is mentioned, but does the model consider organic horizons? Note that Schadel et al. (2014) analyzed carbon quality for both mineral soils and organic horizons, so this is the reason why I'm asking. Also, I'm wondering about the role that organic horizons play in the soil thermal dynamics of the model. I think all of these issues should be elaborated upon in the Methods.

The UVic ESCM does not have explicit organic soil horizons. Instead each model soil layer (in the top 6 layers) has an organic carbon density, this density helps determine the thermal properties of the soil along with the sand, silts, clay, water, and ice fractions in the soils. At the beginning of the model simulations the highest density of carbon in the surface layer of any grid-cell is about 400 kg m^{-3} which would make organic matter about 25-40% of the dry mass of this layer. The permafrost carbon in the model was assigned uniform global properties. A weighted average of the values for the organic, shallow, and deep mineral soils was used to create the PDF of the available fraction of permafrost carbon.

To clarify these points we have added the following sentences to the methods section. On page 19505 after line 2 a sentence has been added to describe how soil thermal properties are parameterized in the models.

The line reads:
"The thermal conductivity of each soil layer is determined by the sand, silt, clay, water, ice and organic carbon fraction of the layer (Avis, 2012)."

The lines that describe the how the available fraction is treated have been augmented to make it clearer that organic, shallow and deep mineral soils are treated separately by Schädel et al (2014). The lines did read:

"The available fraction is described by the sum of three weighted gamma distributions with each distribution respectively describing the PDF of the organic, shallow mineral (<1\,m), and deep mineral (>1\,) soils. The weights for the PDFs were derived from the relative fraction of permafrost soil carbon in organic, shallow mineral and deep mineral soils from Hugelius et al. (2014). The parameter values for the PDFs were derived by fitting gamma functions to the data in Figure 3 of Schädel et al. (2014)."

These sentences have been changed to:

"Schädel et al. (2014) reports the size of the fast, slow and passive pool of soil organic carbon separately for organic, shallow mineral (<1m), and deep mineral (>1m) soils. Here these three categories of permafrost carbon have been combined to produce a single value for the available fraction. The sum of three weighted gamma distributions with each distribution respectively describing the PDF of the organic, shallow mineral, and deep mineral soils are used to describe the available fraction. The weights for the PDFs were derived from the relative fraction of permafrost soil carbon in organic, shallow mineral and deep mineral soils from Hugelius et al. (2014). The parameter values for the PDFs were derived by fitting gamma functions to the data in Figure 3 of Schädel et al. (2014)."

Methods: Nothing is mentioned about inputs into the soil in the model description. How is NPP calculated and what are its sensivities.

Page 19503 line 25 to page 19504 line 4 of the original manuscript describe the Triffid dynamic vegetation module and how soil carbon is created. The lines read:

"The terrestrial carbon cycle is simulated using the Top-down Representation of Interactive Foliage and Flora Including Dynamics (TRIFFID) dynamic vegetation model. TRIFFID is composed of five plant function types: broadleaf trees, needle-leaf trees, shrubs, C3 grasses, and C4 grasses. These plant function types compete with one-another for space in each grid cell based on the Lotka-Voterra equations (Cox et al., 2001). The simulated plants take up carbon through photosynthesis and distributed acquired carbon to plant growth and autotrophic respiration. Dead carbon is transferred to the soil carbon pool as litter-fall and is distributed in the soil as an exponentially decreasing function of depth."

Following these sentences a short description is now given of the state variables that drive Triffid:

"Production of plant litter (and therefore new soil carbon) in Triffid is a function of temperature, plant function type, soil water availability, and atmospheric CO_2 concentration (Cox et al., 2001; Booth et al., 2012)."

Results,

Page 19511, Line 4: What do you mean by release of carbon from permafrost soils? Do you mean net changes in soil carbon, do you mean net loss of carbon from previous frozen soils, do you mean net changes in ecosystem carbon?

The ‘release of carbon from permafrost soils’ means the change in soil carbon in grid cells that contained at least one permafrost bound soil layer in year 1850. To clarify this definition the following sentence has been added following the cited line. The sentence reads:

“This quantity is calculated as the change in soil carbon in all soil layers (including the historic active layer) in model grid cells that had at least one soil layer below 0°C for two or more years under year 1850 forcing at the end of model spin-up.”

How is the permafrost region defined in Figure 1 (it differs from Hugelius apparently). Please clarify.

The permafrost region is defined as all model grid cells that contain at least one soil layer that has been below 0°C for two or more years. This has now been clarified in the caption for Figure 1. The following sentence has been added to the caption:

“The permafrost region in the UVic ESCM is defined as the area where the model simulates at least one soil layer that is perennially frozen at the beginning of the model integration in year 1850.”

Figure 6: Should the X axis be labeled “transformation” instead of “transmutation”?

Yes, thank you.

Page 19513, Lines 18-19: Is this true of Hugelius’s map, or just the UVic map? Isn’t the issue that more permafrost carbon is exposed at the southern boundary because the thaw is deeper?

The low quantity of permafrost carbon in the High Arctic if a feature of the Hugelius map that is generally captured by the UVic ESCM. The figure below shows the quantity of frozen (sequester) soil carbon from the UVic ESCM in year 1862 (blue) and year 2087 (red, RCP 8.5) in each latitude band. Values are averages over all 250 model variants. This figure clearly shows that most of the sequestered carbon is held blow 70°N in year 1850.

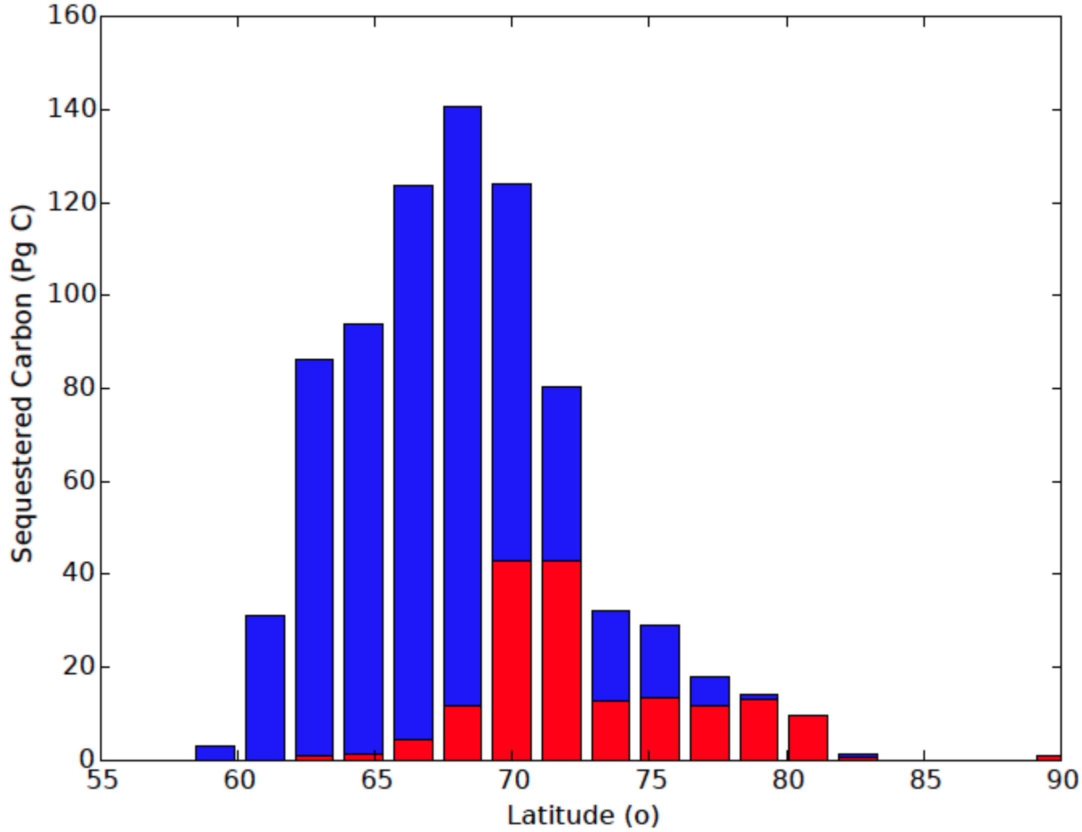

Figure Response 1. Latitudinal totals of the sequestered carbon in year 1862 (blue) and 2087 (red) in the UVic ESCM.

The deeper thaw in the south likely contributes to more carbon being un-sequestered in this region but the dominant effect is that much more of the permafrost carbon is held in the southern region.

Page 19516, Line 4: Change "Incorporating this new data" to "Incorporating these new data".

This has been changed.

Page 19517, Lines 1 and 2: Change "effect" to "affect"? Rewrite sentence so that it doesn't end in the preposition "for".

The sentence has been re-written from:
"There are many processes that effect the thawing of permafrost and decay of permafrost carbon that the UVic ESCM does not account for."

To: "There are many processes that affect the thaw of permafrost and decay of permafrost carbon that are not accounted for in the UVic ESCM."

Response to Reviewer 2:

Anonymous Referee #2

Received and published: 24 January 2016

The authors present a modeling analysis of future projections of carbon emissions from thawing permafrost. These results contribute the general knowledge of the permafrost carbon feedback. The new contribution includes the new statistical technique and the projections extending out for 8000 years to evaluate the long-term effect on climate. I found the paper well written and the results interesting. I suggest the paper can be published after minor revisions.

I have several specific comments:

P500, L17-19: The authors should rewrite this statement to reflect the estimated fractions of anthropogenic emissions. The current wording implies previously published papers imply 'cataclysmic' emissions. While common in the blogosphere and media, the published literature never makes such assertions.

The statement has been re-written from:

"Overall our simulations suggest that the permafrost carbon cycle feedback to climate change will make a significant but not cataclysmic contribution to climate change over the next centuries and millennia."

To:

"Overall our simulations suggest that the permafrost carbon cycle feedback to climate change will make a significant contribution to climate change over the next centuries and millennia, releasing a quantity of carbon 3 to 54% of the cumulative anthropogenic total."

The phrase "but not cataclysmic " has been deleted from the final line of the conclusions.

P501, L25-7: The authors should remove this statement for two reasons: 1) it is unrelated to the subject of the paper and 2) the broader community of soil scientists and modelers do not agree with the assertions of Schmidt et al [2011]. Schmidt et al. make a number of useful recommendations, but they base their analysis on a very small set of global models. The large spread in simulated soil carbon fluxes result as much from differences in simulated GPP as from the problems they identify.

The statement has been deleted.

P502, L8: 'emissions'

This has been changed.

P502, L15: Replace 'Montecarlo' with 'Monte Carlo.' The technique is named after an actual place, the casinos of Monte carlo.

Yes thanks.

P503, L15: I suggest rewording this.

The statement has been re-worded from:

"Much of the permafrost carbon pool is highly resistant to decay Schädel et al. (2014), however the long lifetime of anthropogenic climate change implies that the pool will eventually decay and its carbon will be added to the ocean-atmosphere system."

To:
"Anthropogenic climate change will not cease in year 2100 (e.g. Clark et al., 2016) and the intrinsic timescale of decay of the passive component of the permafrost carbon pool implies that the permafrost carbon system will continue to evolve far into the future. Multi-millennial simulations of anthropogenic climate change suggest that the temperature change caused by the burning of fossil fuels will last for over 100 000 years (Archer, 2005), a period of time long enough such that the permafrost carbon pool may come into equilibrium with the new climate regime. To explore the long-term fate of the permafrost carbon pool we have extended a sub-selection of model simulations 8000 years into the future."

P506, L10: What is the value range for the saturation factor and how is it calculated.

The saturation factor is a dimensionless quantity with a value greater than zero and less than 1. The parameter is used to tune the size of the permafrost carbon pool such that the pool takes on the size given by Hugelius et al. (2014).

This has been clarified in the manuscript by adding the following sentence after line 14 of page 19506:

"The factor S can take on values between zero and one and is used to tune the size of the permafrost carbon pool."

P507, L16-18: The reason for this is a problem common to all models: sub-grid representation of permafrost distribution. A model grid cell is either all permafrost or no permafrost, so simulating permafrost in areas like south of Hudson Bay is extremely difficult.

We agree. We have changed the sentenced to indicate that this is a common problem and given a citation to Koven et al. (2013). The sentence did read:

"The model does not capture the large permafrost carbon density in the Hudson Bay lowlands and permafrost (and therefore permafrost carbon) is absent from the Labrador peninsula."

And has been changed to:

"The model does not capture the large permafrost carbon density in the Hudson Bay lowlands and permafrost (and therefore permafrost carbon) is absent from the Labrador peninsula, a bias common to many Earth system models (Koven et al., 2013)."

P510, L15: Use '10,000 AD' rather than 'deep future.' I had trouble figuring out exactly what you meant.

"deep-future" has been changed to "8000 years into the future".

P511, L7-25: Make all these numbers into a table. I found it very difficult to read and impossible to remember the numbers. A table is a much more effective way to present a lot of numbers than sentences in text.

The tables have been created and is copied below. These two paragraphs have been re-written to:

"The release of carbon from permafrost soils for each RCP and for each of the 250 model variants is shown in Figure 3. Averages values and ranges for this quantity are given for all RCPs in Table 1. Model results in this section are quoted as the mean value of all model variants with the 5th and 95th percentile range in brackets. This is equivalent to the "very likely" range from IPCC AR5, although the numbers here are of course conditional on the model structure and parameter PDFs chosen. By year 2100 the model estimates that 56 (13 to 118) Pg C will be released under RCP 2.6, and 102 (27 to 199) Pg C released under RCP 8.5. By year 2300 the model estimates that 91 (32 to 175) Pg C will be released under RCP 2.6, and 376 (159 to 587) Pg C released under RCP 8.5. These results are generally consistent with the inter-model range of 37 to 174 Pg C, mean of 92 Pg C by 2100 under RCP 8.5 from Schuur et al. (2015).

The emission rate of CO_2 from permafrost soils is shown in Figure 4 and peak emissions for each RCP given in Table 2. Peak emissions under RCP 2.6 is 0.56 (0.13 to 1.29) Pg C a^{-1} and under RCP 8.5 is 1.05 (0.28 to 2.36) Pg C a^{-1}. The timing of peak emissions of CO_2 from permafrost soils varies by model variant and scenario followed (Figure 4) but generally occurs in the mid to late 21st century or early 22nd century in the case of RCP 6.0. The emission rate from permafrost soils is a function of both the rate of permafrost thaw and the depletion of the available fraction of permafrost carbon in thawed soils. The similar trajectories of emissions in the early to mid 21st century for the different RCP scenarios is consistent with the lag between forcing and response of the permafrost system. These simulated peak emission rates are of similar magnitude to modern land use change emissions, 0.9 ±0.8 Pg C a^{-1} averaged over the year 2000 to 2011 period (Ciais et al., 2013). Even in the most extreme bound emissions from permafrost carbon are projected to be far lower than modern CO_2 emissions from fossil fuel burning and cement production (9.5 ± 0.8 Pg C a^{-1} in 2011) (Ciais et al., 2013)."

Table 1. Release of carbon from permafrost soils by year 2100 and 2300 for each RCP scenario. Ranges are 5th to 95th percentiles. All values are in Pg C.

	Mean (year 2100)	Range (year 2100)	Mean (year 2300)	Range (year 2300)
RCP 2.6	56	(13 to 118)	91	(32 to 175)
RCP 4.5	71	(16 to 146)	149	(45 to 285)
RCP 6.0	74	(15 to 154)	204	(63 to 371)
RCP 8.5	101	(27 to 199)	376	(159 to 587)

Table 2. Peak emission rate of carbon from permafrost soils for each RCP scenario. Ranges are 5th to 95th percentiles. All values are in Pg C a^{-1}.

	Mean	Range
RCP 2.6	0.56	(0.13 to 1.29)
RCP 4.5	0.66	(0.16 to 1.57)
RCP 6.0	0.75	(0.19 to 1.59)
RCP 8.5	1.05	(0.28 to 2.36)

P511, L1: Why is there a peak in emissions in 2050?

The emission rate from permafrost carbon is a function of the rate of permafrost thaw and the depletion of the available fraction (fast and slow pools) of permafrost carbon. The model projects that a large amount of permafrost will thaw in the first half of the 21st century before the RCPs have diverged much, such that the near-term behavior of the model is similar under each RCP.

To clarify the manuscript we have added to following sentence after Page 19511 line 21:

"The emission rate from permafrost soils is a function of both the rate of permafrost thaw and the depletion of the available fraction of permafrost carbon in thawed soils. The similar trajectories of emissions in the early to mid 21st century for the different RCP scenarios is consistent with the lag between forcing and response of the permafrost system."

P511, L1: The authors need to include loses in simulated permafrost area.

On page 19512 we have added a short subsection to describe the loss of simulated permafrost area. The subsection reads:

"**3.2 Reduction in permafrost area**
In year 1850 the UVic ESCM has a northern hemisphere permafrost area (including the Tibetan plateau) of 14.87 million km^2, comparing well to the total of continuous and discontinuous permafrost area in the natural world (e.g. Tarnocai et al., 2009). By year 2100 the northern hemisphere permafrost area has been reduced by 5.91 (2.25 to 8.43) million km^2 under RCP 2.6 and 9.30 (7.49 to 9.90) million km^2 under RCP 8.5. By 2300 a small recovery of permafrost area occurs under RCP 2.6 with a net reduction from year 1850 of 4.78 (1.71 to 8.13) million km^2 while under the other RCPs loss of permafrost area continues until at least year 2300 (Table 3)."

Table 3. Reduction in the size of the northern hemisphere permafrost region by year 2100 and 2300 relative to year 1850 (14.9 million km^2). Ranges are 5th to 95th percentiles. All values are in million of km^2.

	Mean (year 2100)	Range (year 2100)	Mean (year 2300)	Range (year 2300)
RCP 2.6	5.9	(2.2 to 8.4)	4.8	(1.7 to 8.1)
RCP 4.5	7.6	(3.8 to 9.6)	8.8	(4.7 to 11.3)
RCP 6.0	8.3	(4.8 to 9.7)	10.3	(7.3 to 11.8)
RCP 8.5	9.3	(7.5 to 9.9)	11.7	(10.3 to 12.1)

P512, L19: The authors need to be careful about relative vs. absolute importance. The relative importance is much less for RCP 8.5 vs. 4.5, but the absolute magnitude of the fluxes is still 3x those for RCP 4.5.

The sentence has been re-written to be more careful. The sentence did read:

"These results suggest the the permafrost carbon feedback to climate change will be a more important climate change feedback in scenarios with substantial mitigation, consistent with previous studies (e.g. MacDougall et al., 2012)."

And has been changed to:

"These results suggest the permafrost carbon feedback to climate change will be more important in a relative sense to the magnitude of climate change in scenarios with substantial mitigation, consistent with previous studies (e.g. MacDougall et al., 2012)."

P513, L7: What about the importance of these parameters in 2300?

The next paragraph already describes the importance of these parameters in 2300. Page 19513 Line 14 to 16 read:

"The correlations with initial quantity of carbon in the permafrost region, permafrost carbon decay rate, and arctic amplification remain weak in this time frame, at 0.13, 0.02, and 0.11 respectively. These results demonstrate that the relative importance of uncertainty in parameters changes depending on the time frame of interest."

For clarity the sentence has been changed to:

"The correlations with initial quantity of carbon in the permafrost region, permafrost carbon decay rate, and arctic amplification remain weak by year 2300, at 0.13, 0.02, and 0.11 respectively. These results demonstrate that the relative importance of uncertainty in parameters changes depending on the time frame of interest."

P513, L25-7: A major field campaign is not required. What we do need is a strategy to collect the right samples from the right locations and set up incubation experiments at the right temperatures.

The sentence has been re-written to reflect the Reviewer's view. The sentence did read:
"A major field campaign to collect samples of permafrost carbon and conduct incubation experiments could therefore significantly reduce uncertainty in the strength of the permafrost carbon feedback to climate change."

And have been changed to:
"A dedicated field campaign and set of laboratory experiments to collect samples of permafrost carbon in optimal locations and conduct incubation experiments at the optimal temperatures could therefore significantly reduce uncertainty in the strength of the permafrost carbon feedback to climate change."

P513, L27: The authors should discuss dissolved organic carbon (DOC) as a factor, with some references.

A discussion of DOC has been added to the paragraph describing processes not accounted for by the UVic ESCM (Page 19517). The new sentences read:

"Transport of permafrost carbon from soils to surface waters as dissolved organic carbon (DOC) is a process that is unaccounted for in the UVic ESCM. Field studies in Arctic regions suggest that once DOC is transported to the surface and exposed to sunlight much of the DOC can be mineralized to CO_2 potentially providing a pathway to degrade otherwise passive permafrost carbon (e.g. Cory et al., 2013, 2014). "

P514, L26-8: What causes this warming?

Following cessation of emission, the temperature will either increase, decrease or stay the same depending on how fast carbon is incorporated into the ocean (MacDougall et al. 2013). If atmospheric CO_2 concentration is held fixed following the cessation of emissions then the Earth will warm until ocean heat uptake diminished to zero. However in most Earth system model simulations the ocean continues to absorb carbon after emissions cease such that atmospheric CO_2 concentration and therefore CO_2 radiative forcing also decrease. Therefore there is a trade-off between the unrealized warming and the reduction in radiative forcing.

The sentence has been re-written to briefly explain the source of the warming and to cite some of the relevant literature.

The sentence did read:

"Temperature continues to slowly increase following cessation of emissions, reaching a peak in the fifth millennium CE."

And now reads:

"Temperature continues to slowly increase following cessation of emissions, indicating that radiative forcing from atmospheric CO_2 is declining to slowly to compensate for the unrealized warming of the system (e.g. MacDougall et al., 2013; Frölicher et al., 2014). Temperature change reaches a peak in the fifth millennium CE in these simulations."

P515, L3-5: How does this cause the difference?
Both studies use the UVic ESCM. The only difference in forcing and model structure is the existence of the permafrost module and forcing from non-CO_2 radiative agents. Therefore the difference in model output must be due to these factors. This has been clarified in the text by altering the sentence from:

"The continued existence of non-CO_2 forcing in these scenarios and the inclusion of the permafrost carbon module are probable causes of the differences between that study and the present study."

To:

"The continued existence of non-CO_2 forcing in these scenarios and the inclusion of the permafrost carbon module are probable causes of the differences between that study and the present study, as both studies use similar versions of the UVic ESCM."

P517, L23-4: The authors should delete this statement. I do not agree at all that Schmidt et al. calls into question the multi-pool model.

The statement has been deleted.

References in this Response:

Clark, P. U., et al., 2016: Consequences of twenty-first-century policy for multi-millennial climate and sea-level change. Nature Climate Change, doi:10.1038/NCLIMATE2923.

Collins, M., B. B. Booth, B. Bhaskaran, G. R. Harris, J. M. Murphy, D. M. Sexton, and M. J. Webb, 2011: Climate model errors, feedbacks and forcings: a comparison of perturbed physics and multi-model ensembles. Climate Dynamics, 36 (9-10), 1737{1766.

Collins, M., C. Brierley, M. MacVean, B. Booth, and G. Harris, 2007: The sensitivity of the rate of transient climate change to ocean physics perturbations. Journal of Climate, 20 (10), 2315–2320.

Frölicher, T. L., M. Winton, and J. L. Sarmiento, 2014: Continued global warming after CO_2 emissions stoppage. Nature Climate Change, 4 (1), 40–44.

MacDougall, A. H., M. Eby, and A. J. Weaver, 2013: If anthropogenic CO_2 emissions cease, will atmospheric CO_2 concentration continue to increase? Journal of Climate, 26, 9563–9576, doi:10.1175/JCL-D-12-00751.1.

Neelin, J. D., 2011: Climate Change and Climate Modeling. Cambridge University Press.

Rowlands, D. J., et al., 2012: Broad range of 2050 warming from an observationally constrained large climate model ensemble. Nature Geoscience, 5 (4), 256–260.

Schädel, C., et al., 2014: Circumpolar assessment of permafrost C quality and its vulnerability over time using long-term incubation data. Global change biology, 20 (2), 641–652.

Shiogama, H., et al., 2012: Perturbed physics ensemble using the MIROC5 coupled atmosphere–ocean GCM without flux corrections: experimental design and results. Climate Dynamics, 39 (12), 3041–3056.

Smerdon, J. E., A. Kaplan, E. Zorita, J. F. Gonz_alez-Rouco, and M. Evans, 2011: Spatial performance of four climate field reconstruction methods targeting the common era. Geophysical Research Letters, 38 (11).