Biogeosciences Discuss., 8, C4732–C4741, 2011
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Biogeosciences Discussions

Interactive comment on "Effects of soil rewetting and thawing on soil gas fluxes: a review of current literature and suggestions for future research" *by* D.-G. Kim et al.

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Review

Kim et al., 2011 Effects of soil rewetting and thawing on soil gas fluxes: a review of current literature and suggestions for future research, Biogiosciences Discuss., 8, 9847-9899

The manuscript presents a literature review on soil rewetting and thawing effects on the soil gases CO2, CH4, N2O, NH3 and NO in field and laboratory studies, explores the mechanisms and drivers of the different gas flux responses to rewetting and thawing, highlights the need of a more process-based understanding and modelling of these

events and provides future suggestions for improved experiments and research. Future climate scenarios predict an increase in droughts and extreme weather and an increase in freeze-thaw events that might increase C and N-losses from soils, thus both negatively affecting carbon and nutrient availability and increasing atmospheric green house gas concentrations resulting in a positive feedback to climate change. Therefore, the work is important and topical and would appeal to the BGS readership.

A strength of this review is that effects of rewetting and thawing on various soil gas fluxes (not only CO2)are evaluated and that current literature information on general mechanisms and drivers of the different gas responses is nicely summarized. Another strength is that current knowledge gaps and future experimental needs on the topic become evident.

The literature research and analysis appear sound; however, there are a number of concerns in argumentation, justification and structuring that would require (major) revision.

A particular concern is the generalization of the term “response” of the gas fluxes to the events by the authors, which is not well defined. Fig. 1 does not reflect natural conditions, as the pulses are often transient and often do not return to pre-treatment values. It should be clarified if only maximum responses or general increases above pre-event values are meant.

Secondly, I disagree with one of the main outcomes that there is no significant difference in response among gases only because they fit on a power function when taking all data points into account. This power function is generated by large fluxes as CO2 and N2O being on the top of the curve and small fluxes such as NO being on the bottom. I do however, not always see the single flux response to the events rewetting and thawing fitting on this curve (as i.e. for CO2 after rewetting events).

The arrangement of the sections by gases, response patterns and mechanisms is nice. However, I found that the sections of “General patterns of response” would profit from a

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more general summary and discussion of the overall literature results, instead of listing data from a few single study examples as it is often exhaustive for the reader to extract the essential general context.

As the authors have done a considerable work gathering information from 338 literature studies from 1956 to 2011 in various ecosystems it would be nice to extract temporal changes in "response" over the last 50 years as probably already affected by global change. Also it would be desirable for the reader to gain information in which ecosystems (e.g. arid vs. humid climates, grasslands vs. forests) rewetting and thawing has the largest effects on the single gas fluxes and where the effect on annual budget is large or negligible. I suggest including such information, as it is needed for a comprehensive understanding of the importance of such events.

Section 4 Knowledge Gaps and future directions should be rewritten in a more general review-like manner as many of the discussed knowledge gaps are very detailed for the single gas flux responses and could be included into the respective Mechanisms and Drivers parts, rather concentrating on the general problems involved in experimental design and modelling such as temporal and spatial resolution of measurements and lab vs. ecosystem studies applicability for several purposes (e.g. understanding mechanisms vs. up scaling and calculating gas budgets).

Comments:

Abstract

Page 9848 Line12: It would be interesting to know if the responses of different gases have changed over this period from 1956 to 2010, thus, if climate change already plays a role in these events. This has not been looked at and may not be aim of this review but it would be nice to include in the general outcome.

Introduction

Page 9848 Line 25: I suggest rephrasing to: "The rewetting of dry soils and thawing of

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frozen soils represent abrupt step changes. . .”

Page 9850 Line 1: Please change to “. . .the mechanisms and impacts of changing rainfall regimes and freeze-thaw cycles on. . .”

Page 9850 Line 3ff.: I would not go as far as generalizing that most flux measurements are done in “coarse temporal resolution” (i.e.. frequency in eddy covariance as the most used method for measuring CO2 fluxes is 5 and 25Hz). It would be good to specify what flux measurements you are referring to. Further, please clarify “unrealistic simulations of dry-wet and freeze-thaw events”.

Page 9850 Line 7: The studies cited here studied ecosystem respiration (thus, only CO2). Please add some references on how the other fluxes (CH4, N2O etc.) are influenced or change to “annual carbon fluxes” to avoid generalization.

Page 9850 Line 13: The authors claim to have created a new database on published studies and claim that this is novel. I do not see the novelty here, as every literature review, particularly the ones doing meta-analyses with published data create a “database” of published studies to extract the current state of knowledge and research challenges”. I suggest removing “. . .creates a new database on published studies. . .”

Page 9850 Line 17: “discuss the underlying mechanisms and drivers of responses” This objective lacks specificity – please specify “drivers and responses of what?”

Methodology

Page 9850 Line 22: Please change to: “. . .where acquired by a literature cross-search of peer-reviewed publications between 1950 and 2010.” There is no need to say which search engines and key words you used for that. Also, in the abstract you state “studies between 1956 and 2010”. Please correct.

Page 9850 Line 24ff: Please change to: “Studies with field observations. . .”, “Similarly, studies of thawing. . .”

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Page 9851 Line 3ff. This sentence needs more explanation as it seems out of place and it is difficult to understand what is meant by “changing active layer depths” and why you are suddenly talking about permafrost soils and soil/plant succession effects on them.

Page 9581 Line 11: I have a doubt about the reference to Fig. 1. The soil efflux after rewetting and thawing does not always behave like it is shown in the hypothetical Figure: immediate increase and gradual decrease to pre-treatment values. E.g. in case of CO2 fluxes after rewetting often the initial increase is vary transient and the fluxes behave like this; immediate and transient increase followed by a strong decrease to well above pre-treatment values and then a slow decrease. How did you calculate the response in these cases or what was your period of peak-gas fluxes? Further, in natural studies various rain events are common impeding the return of gas fluxes to pre-treatment values as shown in Fig. 1. The authors should define how they calculated the peak-flux period in such cases.

A review of the effect of rewetting and thawing on soil gas fluxes

Page 9852 Line 5: Please change “multiple” to “various” and “experimental settings” to “experimental designs”

Page 9852 Line1-3: Reference needed

Page 9852 Line 14: It would be nice to have a general overview of ranges listed here of how much CO2 increase in terms of μmol m-2 s-1 has been observed in the different ecosystems

Page 9852 Line 15: Please remove “. . .pulses observed in. . .”

Page 9852 Line19: Consider changing to “. . .Rs increases after rewetting in soils. . .”

Page 9853 Line 5: What do you mean by responses? Please specify! Do you mean the maximum CO2-pulse or the general CO2 increase above pre-treatment values? We found in an irrigation experiment that the CO2-pulse in general is very rapid and

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does not last longer than some hours but that pre-rain values were increased during several weeks (Unger et al., Soil Biology and Biochemistry 2010) and that they were due to different mechanisms (rapid pulse is not explained by soil moisture (extent and longevity are related to antecedent soil moistures and change in soil water potential) = probably priming by rain-induced availability of substrate, longer-term increase of soil respiration above pretreatment values is correlated with soil moisture) = probably increased soil microbial growth and turnover of soil organic matter with increasing soil moistures. We have a very recent paper in Agricultural and Forest Meteorology that might be interesting for you in this regard: Unger, S. Máguas, C., Pereira, J.S., David, T.S., Werner, C. (2012) Interpreting post-drought rewetting effects on soil and ecosystem carbon dynamics in a Mediterranean oak savannah, Agricultural and Forest Meteorology, 154-155, 9-18, doi: 10.1016/j.agrformet.2011.10.007 - attached in the supplements Further, if studies observed a 30 days response, could it be that there were other rain events in between, triggering further respiration pulses?

Page 9853 Line 7: I do not see a >10000% increase in Fig. 2. Do the Whiskers represent maxima or SD? Please state in the figure legend.

Page 9853 Line 26: consider removing “increased”

Page 9853 Line 27: add comma between “...pore space...” and ”...enhances anaerobiosis...”

Page 9855 Line 14: bracket missing after references, what is meant with Rewetting, thawing and soil gas fluxes database in Sect. 6?

Page 9855 Line 28: Please give some explanation why colder thawing temperatures increase Rs as it seems unreasonable.

Page 9855 Line 29: It would be good to discuss the real mechanisms that were hypothesized by the studies cited as you did for the rewetting also for the freeze-thawing cycles. Thus, why does the first freeze thawing event gives the largest flux response?

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Page 9859 Line 25: Please change: "...at multiple..." to "...in various..."

Page 9860 Line 22: the term "15 d response" this needs to be defined as said above for CO2

Page 9861 Line 21-24: There are not really mechanisms described in 3.1. for freeze-thawing events.

Page 9863 Line 22-23: Please consider changing to "Rewetting studies have commonly reported a short-term increase in NO fluxes (ca. 1-3d)..." The term "response" needs to be defined as said above for CO2

Page 9864 Line 12: It is hard to tell what you mean by "physical mechanisms" for the different gases as there are many (decreased diffusion through pore clogging by water, gas diffusivity, increased substrate supply by shattering of aggregates? etc.) Maybe it would be good to go into more detail and describe which processes are important for which gas.

Page 9866 Line 2: Please change: "...at multiple..." to "...in various..."

Page 9866 Line 5ff: Although I see that taking all preflux and postflux data from all gases into the same function are giving the response shown in Fig. 3 it is hard to believe that all gases responded in the same way to rewetting or thawing with no significant differences. For instance CO2 values in Fig. 3 are only to be found in the upper part of the curve and at least combining the rewetting CO2 response into a regression would result in a different regression with a much smaller slope. I suggest the authors provide proof on the response functions of each of the gases in each of the treatments with information on statistical differences between slopes and intercepts of the single regressions in a separate table or rephrase their conclusion that the combined soil gas response (all gases, thawing and rewetting) would give eqation 2 without stating that there were no significant differences between thawing/rewetting and among gas fluxes. Also Fig. 2 already showed that responses of fluxes to rewetting are completely

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different between events and gases, which is conflicting to your generalisation in Fig. 3. Further it was stated earlier that in some of the fluxes single studies reported negative responses after thawing and rewetting responded not or negatively (e.g. CH4 Page 9856 Line 24ff. or Page 9867 Line 1ff.). These data points were obviously left out of Fig. 3 as there are no data points presented below the 1:1 relationship. It should be stated somewhere why the authors excluded these data.

Knowledge gaps and future directions

Page 9866 Line 20: please remove “of” from the headline

Page 9867 Line 9: You present a review of 338 studies. I would not say that that is “relatively few”

Page 9867 Line 10ff.: I agree with the authors that many studies do not go into detail of all the questions stated here. I think this is also a problem when trying comparing the overall responses of the single gases to rewetting and thawing events in this review as data are deducted from studies that lack this kind of information. E.g. did you exclude studies that did not state the time-lag between rewetting and peak-fluxes or duration of the peak-fluxes when describing overall responses of the single gases? I find that the general term “response” is a bit to broad for the reader to understand the whole dynamics of the pulses and should be defined better in the sections “General patterns of responses”.

Page 9867 Line 18ff.: The authors should go more into detail why it would be important to study changes in ratios of different gases in response to rewetting/thawing events, as it is not evident what extra information this would give on the impact on annual gas budgets.

Page 9868 Line 3: please remove “of” from the headline

Page 9868 Line4ff. Maybe the detailed description in the doubts on drivers of N2O fluxes should go to the section where mechanisms and drivers of N2O fluxes are dis-

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cussed.

Page 9869 Line21ff. Why would you want to extend information from pot experiments and lab studies to the regional or continental scale? Ecosystem scale measurements are more appropriate for these calculations. Thus, I would not say there is a critical issue here since these studies have a different focus on trying to understand the mechanisms behind the gas pulses instead of trying to derive regional or continental gas budgets from their data.

Page 9870 Line19: I suggest citing Unger et al., 2010; 2012 here, as they try to explain mechanisms in CO2 flux increases after rewetting by stable isotope methods.

Page 9871 Line 23: please correct: ". . .models can be used. . ."

A Blog for open discussion and web based open databases

Although I generally agree that data sharing and discussion on recent publications etc. help our understanding on the matter, I am not sure if it is appropriate to advertise the Blog in this review. I suggest removing this information or reducing it largely and putting it in the open Discussion Forum of BGSD.

Table 1

Please add the number of field and laboratory studies that enter this analysis to the table legend

Figure 2

Please correct hyphens at the x-axis. Please state in the table legend what the boxes and the whiskers represent (medians, 25% and 75% percentils, standard deviations, maximum values etc.).

Figure 4

Please correct hyphens at the y-axis.

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Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/8/C4732/2011/bgd-8-C4732-2011-supplement.pdf

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