nature portfolio

Peer Review File

Large emissions of CO_2 and CH_4 due to active-layer warming in Arctic tundra

Corresponding Author: Dr Margaret Torn

This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
Margaret Torn and co-authors present respiration (+ NEE) and CH4 flux data from a filed warming experiment in the Arctic. Soil columns down to 50cm (=border to permafrost) were warmed during the growing season of two consecutive years and in addition the surface frost layer of the soil was artificially thawed once in autumn. The setup of the experiment is impressive and the many replicated measurements during two years provide a very solid flux dataset, which was used to assess the temperature sensitivity (warming response) of respiration. The 14C signature of the respired CO2 was determined, indicating that older C is respired in warmed soil. Apparently this is the first in situ experiment, which artificially warmed the whole active soil layer down to the permafrost in situ, in an Arctic ecosystem. Considered the high carbon stocks in Arctic soils the study is highly significant, and might therefore be considered for publication. Below you find some suggestions for potential improvement/clarification.

I do not work with Earth system models and hence I am not really familiar with which Q10 really are used in individual model to parameterize ecosystem respiration in the Arctic. However, to my knowledge, many of these models apply temperature sensitivities rather related to air temperature, than to soil temperature. See eg. Huntzinger et al. 2020 (Evaluation of simulated soil carbon dynamics in Arctic-Boreal Ecosystems 1,14, Environ. Res. Lett. 15 (2020) 025005). In this paper a whole punch of models is compared. Some of this models use much higher Q10 as 2, the value that is introduced as typical Q10 in Earth system models for Arctic in the current manuscript. I'd double check if it is really the case that all/most Earth system models use Q10 of 1.5-2 that are relating to soil temperatures (and not air temperatures). If this is not really the case, the text needed to be adapted accordingly. Nonetheless, it is no question that the current study provides a really robust Q10 for the growing season respiration at the site by comparing respiration from control to respiration from warmed plots. This exactly is what is Huntzinger et al called for: "This suggests that modelers need to revisit their Q10 values and perhaps employ biome specific Q10 values, particularly in high-latitude." The application of two different Q10 in a model in the current manuscript is nice in terms that it provides some numbers for the whole Arctic flux, but first, it is no bgreat wonder that respiration is much higher when a Q10 of 2.8 is applied when comparing to a Q10 of two. Second, it is not said that the Q10 observed at the experimental site is representative for the whole Arctic, and third it is not said that the temperature response will be constant during the 80 years simulated ahead. It often has been shown that warming responses decline or cease over time. Therefore the observed response during the two study years might not be 1:1 extrapolated until 2100. All this could be mentioned with some words in the discussion.

I would be careful with NEE (and GPP). It warrants some explanation already in the main text that these were all spot measurements, which were taken shortly after respiration measurements and do not represent day round (24h) NEE, but only the value during a single 60 sec spot of the day. When I read about NEE or GPP, I assume that these are calculated over full days or full seasons, such as in eddy covariance studies. Therefore it might be pointed at this (limitation) in the text. Since NEE was still negative in warmed plots, the plant CO2 uptake must have been quite significant. However, plant community, plant biomass, and especially root biomass and root distributions within the soil are not explained at all in the manuscript. I suggest adding this important information. It would be interesting to get a feeling how much of the additionally respired CO2 might be from heterotrophic and autotrophic sources. (Above ground biomass harvest is mentioned in L389, but results are not shown).

The experimental melting of a topsoil ice layer in October and the observed release of trapped CO2 and CH4 are striking – especially the amounts of released CH4 were high. However, I wonder a bit if the imposed scenario is really realistic. This setting would impose that we do not enter a generally warmer future, but a future of still cold periods, followed by extreme warm events. I guess a lot of energy input was necessary to melt down the ice layer. To which increase in air temperatures does the energy input that was required to melt a 5-10 cm deep frozen soil layer (+snow and surface ice) refer? Is it really possible that a short massive heat wave, such as manipulated, follows a longer (1 month) frost period in autumn? Also the soil does not typically warm from below (soil heating), but from the surface downwards (natural) – with consequences on aeration. This all might be discussed. The reasoning and exact experimental implementation of this "shoulder-season" experiment needed to be better explained in the methods. It would have been nice if the CO2 and CH4 fluxes would have been measured a little longer as the 3 days after switching the heating on – then one would get an idea how long this gas release effect had lasted.

Line comments:

Title: "High" could be replaced by "Accelerated". What does "high" mean?

Abstract:

L31: duration could be added
L33: see above Q10 in models
L34: it is unclear what "a controlled shoulder-season warming experiment" means here as it is not introduced.

Intro:

L49: not really important, but the scenario in the model runs you did are different (range up to +10°C not +7.7°C) (EDFig 4)
L52-56: Well, this is about heterotrophic respiration of decomposer microbes, not about the ecosystem respiration you have measured.
L74: water unsaturated soils
L78: there is another recent study about warming effects on soil CH4 fluxes, though not in an Arctic ecosystem (Heinzle et al 2023 AgrForMet)
The numbers presented in Table1 and the legend text of Figure 3 are repetitive. I suggest removing all according lines from the Fig 3 legend text and keep Table 1, or to integrate all the Q10 values from Table 1 into the Figure 3 and remove Table 1.
Fig5: You might delete the indications for solid and dotted lines in the box. It is anyway mentioned in the legend text – and confused at least me when I was searching for a black line and dots. If you take out the green scenario, the figure would be even clearer. The green lines overlap with others – without these lines the graph would be comprehendible within one glimpse.

Methods:

Please add detailed data about above ground vegetation and roots.
Table 1: the whole 13C story and "low glucose" treatment are a bit distracting for me. What has this to do with the current study?
L465: Fluxes were measured...

Extended Data:

EDFig2: why not showing the full year?
Table S1: There is something wrong with the legend – only half of the categories are explained.
Table S2. Seems there were some outliers when comparing mean with median of overall Q10
Figure S2: what do the two "older" in the graph mean?
NEE – see above

Reviewer #2

(Remarks to the Author)
The manuscript entitled "High emissions of CO2 and CH4 due to active-layer warming in Arctic tundra" by Torn et al. provides interesting and important insight into how mineralization rates may be impacted by arming in tundra soils. They present an in situ warming approach, that warms the entire active layer and causes thawing of the upper permafrost layers. this in turn leads to increased respiration rates and a contribution of deeper, older soil organic carbon to the CO2 respired at the surface. Most interestingly, they show that an increase in soil temperatures during the shoulder season leads to a rapid and large pulse of CO2 and CH4 from the soils, likely due to the release of trapped gas in an underlying talik. These results are highly relevant to the journal and will provide interesting discussion topics and lines of future research for the broader community. I have attached a PDF with comments throughout the manuscript. I do not have any major concerns, rather minor ones throughout that should be addressed.

Reviewer #3

(Remarks to the Author)
This paper heated the whole active layer, and then analyzed the greenhouse gases emissions and their temperature sensitivity. The findings are not groundbreaking, but the methods are novel and provide some solid data about the carbon

emissions in a warming climate. The main concern is that the authors measured ER, not Rh, making it difficult to understand the decomposition of soil organic matter under warming treatment. The paper is largely overall well-structured, I have some concerns for the authors to improve the manuscript. Some of the comments may be mean, but please try your best to explain or discuss them.

I would like to see some values of ER in the abstract. Also, please mention the land cover type in the study area. In the abstract, when you mentioned ER, you need to report the changes in vegetation. The last sentence is ambiguous, the authors heated the whole active layer, this is different with the deepening active layer.

L50, what is the timescale? Annual, growing season or non-growing season?
3. L72, Here or somewhere, please add a sentence to clearly describe that climate warming will increase the soil temperature for the whole active layer.

The first hypothesis is not well related to the introduction, why you expect higher Q10 values? Again, it should be clear to state the ER, NEE, Rs or Rh here. Warming usually promotes vegetation growth, and thus increase ER, but this not means the decomposition of soil organic matter.

L 116, please indicate the permafrost boundary in this figure.

L142, Figure 2, for the October 2015, the values are very high, not only about 50% higher, which was described in L135-136.
L176 For the Q10 values, the authors calculated for the treatment differences of heated-control ER and temperature. I have no objection to this idea. But how about using the values ER and temperature from control and heated? There are several ER values and temperature for control and treatment, you can calculate the Q10 values, respectively.
L142 and L209, how about the cumulative fluxes of ER and methane at the annual scale?
L223 what carbon fluxes? Please be specific about the soil organic carbon release,
L229-230, what is the difference between with or without plants? How do you know your results are better than fast cycling pools? Your study also has large uncertainty.
L233-251 please compare your findings with more previous studies about the effects of warming on ER, so potential readers can understand what is the differences between the surface warming and whole warming.
L260-269 Did you observed the rapid melting of snow and ice during the experiment? What about the effects of melting of snow and ice on ground temperature and soil water content?
L307-323, the authors used the higher Q10 values for CESM modeling. I think this is not necessary. First, these should be part of results. Second, you simulated the carbon emissions were higher, that is simply related to the model structure. If you want to keep this modelling results, you should use some data, e.g., field observation data such as eddy tower data, to validate the model, and then you assess the accuracy of the simulation, and then you can say that your Q10 values are better.
L353 What is about the time period?

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
Authors did a thorough revision. I have a couple of further comments for finetuning.

My main suggestion is to move Figure 3 into the supplements. This figure shows (a) co2 flux versus soil temperature and seasonal Q10 values in (b) and (c). Having Table 1 with all the substantial Q10 information in the main text already, these additional Q10 details (in fig 3) are not really necessary to be shown in the main part. Seasonal Q10 (Fig. 3) were not used for modelling anyway. The Figure 3 content and caption text are rather complicated to read for a non-expert in this topic and rather distract from the main content. Current Figure 3 could be replaced with current Figure S2 (radiocarbon) and/or Figure S3 (NEE flux), which both would be more interesting/informative for a reader. Figure S3 currently lacks a caption in the version I have downloaded!

Line comments:

L35: can a model “report” a Q10? It can, but in this case the Q10 is a model input parameter. Hence the sentence should be rephrased.
L39: comma after “whole”?
L52: I’d delete “respiration” here. Respiration is a ghg flux. You may simply write “natural GHG fluxes” or “ecosystem ghg fluxes”
L90: please indicate already here how deep the active layer is.
L99: issue as in L35 – can a Q10 in a model be “observed”?
L142: not really true - during certain warming-off periods CO2 flux was still higher in treatment plots (08.10, 08.13, 09.01). You ma rephrase here.

Figure 2 and Figure 3: In both figures the addition of labels a,b,c into the three panes would be very helpful. You could then in the text more easily refer to e.g. the shoulder season experiment (Fig. 2b) etc. From the current figure caption it is not clear that the “October 2015” is the separate shoulder-season warming experiment.
L166-175: based on the text here it would make sense to move the corresponding supplementary figures into the main ms.
L200-2001: please refer to Figure 4 here already.
L229: might not be only increased microbial respiration. Root respiration as well typically increases under warming. How many roots were in the soil?
L270: “many” might be replaced with “certain”

L272-306 discussion on CH4 fluxes in general: The overall effect of soil warming – namely decreased CH4 emission (fig. 4) somewhat goes under in the discussion. Although the effect was quantitatively smaller and statistically non-significant, it should be at least mentioned. Small effect over long duration also sums up quantitatively. The pulse CH4 during intensive shoulder season warming is impressive, but obviously was not occurring under natural (non-artificial warming) conditions in e.g. spring of the same year when the frozen soil thaws naturally. – or did measurements start too late in the season? Is there any explanation for the high CH4 emissions in September 2016 (fig 4) and the very low CH4 flux of warmed plots at the same time?

L522: The model could be better explained – repeated measurement design, etc.?

Reviewer #3

(Remarks to the Author)
I check the revision and response. The authors addressed all my concerns. I believe this experiment provide solid data about in situ warming on the active layer and its effects on carbon emissions, and such work should be encouraged.
I do notice some minor typos throughout the manuscript, but I believe the authors can do this during the proofing stage.

NCOMMS-24-14134A

Barrow Warming Reviews and response to reviews

We appreciate the constructive comments and suggestions from the three reviewers. We have addressed every point in the reviews, by changing the text, answering a reviewer question, or explaining how we addressed a suggestion without changing the text. These helped clarify our methods and results and added more nuance to the discussion.

Below we describe the reply to each specific comment. The comments are in **blue** and our response is in **black**. The line numbers in our replies refer to the *current* line numbers (the reviewer comments are copied verbatim, including their reference to line numbers in the original submission).

REVIEWER COMMENTS

Reviewer #1 (Remarks to the Author):

Margaret Torn and co-authors present respiration (+ NEE) and CH4 flux data from a filed warming experiment in the Arctic. Soil columns down to 50cm (=border to permafrost) were warmed during the growing season of two consecutive years and in addition the surface frost layer of the soil was artificially thawed once in autumn. The setup of the experiment is impressive and the many replicated measurements during two years provide a very solid flux dataset, which was used to assess the temperature sensitivity (warming response) of respiration. The 14C signature of the respired CO2 was determined, indicating that older C is respired in warmed soil. Apparently this is the first in situ experiment, which artificially warmed the whole active soil layer down to the permafrost in situ, in an Arctic ecosystem. Considered the high carbon stocks in Arctic soils the study is highly significant, and might therefore be considered for publication. Below you find some suggestions for potential improvement/clarification.

Thank you for the helpful comments.

I do not work with Earth system models and hence I am not really familiar with which Q10 really are used in individual model to parameterize ecosystem respiration in the Arctic. However, to my knowledge, many of these models apply temperature sensitivities rather related to air temperature, than to soil temperature. See eg. Huntzinger et al. 2020 (Evaluation of simulated soil carbon dynamics in Arctic-Boreal Ecosystems 1,14, Environ. Res. Lett. 15 (2020) 025005). In this paper a whole punch of models is compared. Some of this models use much higher Q10 as 2, the value that is introduced as typical Q10 in Earth system models for Arctic in the current manuscript. I'd double check if it is really the case that all/most Earth system models use Q10 of 1.5-2 that are relating to soil temperatures (and not air temperatures). If this is not really the case, the text needed to be adapted accordingly.

In Huntzinger et al. 2020, the model ensemble output that they used (MsTMIP) only provides certain high-level outputs and does not include soil temperature, which is why they used air temperature. Indeed this is fairly common when looking at the temperature sensivity of of ESMs. However, the papers that we reference (Todd-Brown et al. 2013, 2018) specifically refer to soil temperature when reporting Q10s for soil decomposition. The "intrinsic" Q10=1.5 reported by Lawrence et al. 2015 is also based on soil temperature, although one has to refer to the CLM4.5 Technical Note, Equation 15.5, to verify this. Similarly, we also used soil temperature in our calculation so that Q10 of our model estimate is calculated in the same way as Q10 of our experimental measurements.

Nonetheless, it is no question that the current study provides a really robust Q10 for the growing season respiration at the site by comparing respiration from control to respiration from warmed plots. This exactly is what is Huntzinger et al called for: “This suggests that modelers need to revisit their Q10 values and perhaps employ biome specific Q10 values, particularly in high-latitude.” The application of two different Q10 in a model in the current manuscript is nice in terms that it provides some numbers for the whole Arctic flux, but first, it is no bgreat wonder that respiration is much higher when a Q10 of 2.8 is applied when comparing to a Q10 of two. Second, it is not said that the Q10 observed at the experimental site is representative for the whole Arctic, and third it is not said that the temperature response will be constant during the 80 years simulated ahead. It often has been shown that warming responses decline or cease over time. Therefore the observed response during the two study years might not be 1:1 extrapolated until 2100. All this could be mentioned with some words in the discussion.

We appreciate the comment that "the current study provides a really robust Q10 for the growing season respiration at the site by comparing respiration from control to respiration from warmed plots."

For the first point: We agree that it is not a surprise that that respiration is much higher when a Q10 of 2.8 is applied as compared to a Q10 of two. This is especially clear for people who are familiar with applying Q10's or Arrhenius equations. For the readers (possibly majority of readers) who are not as familiar, we thought it was worthwhile to illustrate the order of magnitude and show the impact.

For the second point: We also agree that warming (experiments) in other locations of the Arctic might not produce the same Q10 values. We will clarify that we do not mean the temperature response from our experiment is representative of global arctic. However, the simple application in Figure 5 for CMIP models is consistent with the fact that all CMIP5 models and most CMIP6 models use a single, constant Q10 for the whole globe in the soil module.

A sentence was added to the Discussion (lines 420-422) stating that we followed ESM practice, although it is likely an oversimplification, in applying the same Q10 pan-Arctic and over the century.

I would be careful with NEE (and GPP). It warrants some explanation already in the main text that these were all spot measurements, which were taken shortly after respiration measurements and do not represent day round (24h) NEE, but only the value during a single 60 sec spot of the day. When I read about NEE or GPP, I assume that these are calculated over full days or full seasons, such as in eddy covariance studies. Therefore it might be pointed at this (limitation) in the text. Since NEE was still negative in warmed plots, the plant CO2 uptake must have been quite significant. However, plant community, plant biomass, and especially root biomass and root distributions within the soil are not explained at all in the manuscript. I suggest adding this important information. It would be interesting to get a feeling how much of the additionally respired CO2 might be from heterotrophic and autotrophic sources. (Above ground biomass harvest is mentioned in L389, but results are not shown).

Good point. We have clarified that all NEE and GPP measurements are instantaneous (lines 220-223). We describe aboveground biomass measurements in the methods section and show data in the new Figure S4. We have also added a reference to incubations we did with soil from the same area, which isolated the heterotrophic response, albeit under incubation conditions (Vaughn and Torn 2019). See paragraph starting line 309, in particular lines 315-316. These incubations showed a 39% increase in soil respiration due to a 5oC warming from 5o to 10o C. We have added a reference to this finding in the Discussion.

The experimental melting of a topsoil ice layer in October and the observed release of trapped CO2 and CH4 are striking – especially the amounts of released CH4 were high. However, I wonder a bit if the imposed scenario is really realistic. This setting would impose that we do not enter a generally warmer future, but a future of still cold periods, followed by extreme warm events. I guess a lot of energy input was necessary to melt down the ice layer. To which increase in air temperatures does the energy input that was required to melt a 5-10 cm deep frozen soil layer (+snow and surface ice) refer? Is it really possible that a short massive heat wave, such as manipulated, follows a longer (1 month) frost period in autumn? Also the soil does not typically warm from below (soil heating), but from the surface downwards (natural) – with consequences on aeration. This all might be discussed. The reasoning and exact experimental implementation of this "shoulder-season" experiment needed to be better explained in the methods. It would have been nice if the CO2 and CH4 fluxes would have been measured a little longer as the 3 days after switching the heating on – then one would get an idea how long this gas release effect had lasted.

These are good questions. Starting with the question whether the scenario of a future with cold periods punctuated by thaw or warm events is realistic, as opposed to a generally warmer future, climate models predict that in a warmer 21st century, the snow-covered season will become shorter, but it will not disappear. There will still be frozen ground and surface ice/snow in the much of Arctic. Current winter temperatures are often -20oC or colder and even with 8oC warming there will still be frozen ground (even if permafrost is lost) and ice in winter.

The second component to this comment is whether there can be short-lived or rapid thaw after snow or ice have accumulated on the ground and created a frozen surface. The rapid melt of ice/snow and thaw of the top cm's of soil is an observed, occasional event in the Arctic, in fall and spring. Warming is projected to lead to more frequent events in which surface ice/snow is rapidly melted. One of the main drivers of this thaw historically are rain-on-snow events, which bring a lot of energy quickly. Such events, and their impacts, are recognized and as such have been studied and monitored in the Arctic. These events are becoming more common, not only because warmer temperatures promote wet precipitation, but also because warming leads to more moisture (that can form rain) in the arctic atmosphere. Another driver of thaw are warm spells; these are less effective in the fall months than is rain-on-snow because the heat capacity of air is low and there is minimal direct solar radiation in the fall. In the spring, however clear skies and warm weather may cause an early or semi-abrupt thaw accompanied by GHG pulses.

The energy required for thaw of frozen water can be approximated by the latent heat of melting; this is approximate because the water's solute concentration reduces the energy demand. For pure water and ice, 334 J of energy are required to melt 1 g of ice at 0°C. Considered another way, liquid water at 0°C has 334 J g^{-1} more energy than does ice at the same temperature.

As a non-technical summary of the broad range of issues associated with a wetter, warmer arctic, I thought this recent article from the Yale School of the Environment might be helpful: https://e360.yale.edu/features/arctic-rainfall-climate-change. NOAA's annual Arctic Report Card is a good source for general trends towards a warmer and wetter Arctic.

Line comments:

Title: “High” could be replaced by “Accelerated”. What does “high” mean?

We have replaced "high" with "large."

Abstract:

L31: duration could be added

Text has been edited, lines 42-43.

L33: see above Q10 in models

As described in the reply above, the text and value are based on soil temperature.

L34: it is unclear what “a controlled shoulder-season warming experiment” means here as it is not introduced.

We have simplified to "a shoulder-season warming experiment" (line 48). To keep to the word limit for abstracts, we describe the experiment in the body of the paper.

Intro:

L49: not really important, but the scenario in the model runs you did are different (range up to +10°C not +7.7°C) (EDFig 4)

The +7.7C number refers to surface-air warming averaged over 14 CMIP5 models in Soong et al. 2020. Our EDFig4 is looking at soil temperature warming of one model (CESM).

L52-56: Well, this is about heterotrophic respiration of decomposer microbes, not about the ecosystem respiration you have measured.

Agreed. The text here is about microbial decomposition and (heterotrophic) respiration. We think the logical flow is ok here as we want to mention just one source of respiration in this point. Since it is very difficult to separate ecosystem respiration into microbial respiration and plant (autotrophic) respiration in the Arctic, we only report ecosystem respiration results. We consider both processes, heterotrophic and autotrophic respiration, when discussing possible mechanisms and implications.

L74: water unsaturated soils

Text now reads “soils that are unsaturated with respect to water,” line 103.

L78: there is another recent study about warming effects on soil CH_4 fluxes, though not in an Arctic ecosystem (Heinzle et al 2023 AgrForMet)

Thank you for recommending this article. This paragraph was focusing on the methane fluxes in the arctic, and thus it may be inappropriate to cite non-arctic studies here.

The numbers presented in Table1 and the legend text of Figure 3 are repetitive. I suggest removing all according lines from the Fig 3 legend text and keep Table 1, or to integrate all the Q10 values from Table 1 into the Figure 3 and remove Table 1.

Agreed. We removed the numbers that are shown in Table 1 from the Figure 3 caption, except we kept one median Q10 value as it helps clarify what is shown in the data plot.

Fig5: You might delete the indications for solid and dotted lines in the box. It is anyway mentioned in the legend text – and confused at least me when I was searching for a black line and dots. If you take out the green scenario, the figure would be even clearer. The green lines overlap with others – without these lines the graph would be comprehendible within one glimpse.

Thank you for raising this. We have changed the legend to show all of the line-color combinations. We have kept the scenarios, but have changed to a color-blind-accessible palette to improve comprehendability.

Methods:

Please add detailed data about above ground vegetation and roots.

We have added material to the Methods section on aboveground biomass, vegetation community, and roots. We added a figure showing aboveground biomass as Figure S4.

Table 1: the whole 13C story and "low glucose" treatment are a bit distracting for me. What has this to do with the current study?

Good point. Our intention was to describe all of the work done at this site, but the link to our current study was not clear. We have edited this section substantially (starting on line 480) and removed extra information from Table 2.

L465: Fluxes were measured...

The sentence starts with "Surface fluxes of CO_2 and CH_4 were measured." I am not sure if the reviewer is making a note for their purposes or requesting a change.

Extended Data:

EDFig2: why not showing the full year?

We used a subset of data in this plot to show the diurnal pattern. It was very hard to see the pattern if we put all time points in this figure. However, we agree showing the full measurement period is useful, and have done so in Extended Data Figure 3.

Table S1: There is something wrong with the legend – only half of the categories are explained.

Good catch, thank you. We have fixed the legend.

Table S2. Seems there were some outliers when comparing mean with median of overall Q10

Indeed. We use the median in the text. The mean is included for completeness, and as you suggest, to give more information about the distribution.

Figure S2: what do the two “older” in the graph mean?

I have added this sentence to the Figure caption” "The orange bar (average value of respired 14CO2 from heated plots in that year) is annotated as “older” if the radiocarbon value indicates the respired carbon was fixed from the atmosphere longer ago than the age implied by the carbon respired from the control plots in that year. "

NEE – see above

Please see our comments and edits described above.

Reviewer #2 (Remarks to the Author):

The manuscript entitled "High emissions of CO_2 and CH_4 due to active-layer warming in Arctic tundra" by Torn et al. provides interesting and important insight into how mineralization rates may be impacted by arming in tundra soils. They present an in situ warming approach, that warms the entire active layer and causes thawing of the upper permafrost layers. this in turn leads to increased respiration rates and a contribution of deeper, older soil organic carbon to the CO_2 respired at the surface. Most interestingly, they show that an increase in soil temperatures during the shoulder season leads to a rapid and large pulse of CO_2 and CH_4 from the soils, likely due to the release of trapped gas in an underlying talik. These results are highly relevant to the journal and will provide interesting discussion topics and lines of future research for the broader community. I have attached a PDF with comments throughout the manuscript. I do not have any major concerns, rather minor ones throughout that should be addressed.

Thank you for all the helpful comments.

The line-by-line comments from Reviewer 2 are copied here, with responses.

L. 31, Abstract. For how long?

This was added to the abstract ("...for two growing seasons"), lines 42-43.

L. 31, Abstract. Compared to what?

Abstract text was clarified, line 45.

L. 36, Abstract. They will need to explicitly show that they measured ch4 oxidation to make this statement

We did not measure CH_4 oxidation in this study. We have modified the text to say that the methane would likely be oxidized, and have added citations to support this point in the text, lines 49-50.

L. 38, Abstract. Would only be a suggestion and do not want to impose my writing style, but this seems like an unnecessary choice of non-plain language. Check up on this though before commenting

We replaced "and portends" with "contributing to," line 51.

L45: Introduction Provide some citations for this?

We provide citations in the Discussion and did not add here, for brevity.

L. 50,   Background/intro.   Borad readers of the journal may not be familiar with Gelisol terminology referring to permafrost affected soil. Also, this is a USDA identification

The text has been changed to: “Cyrosol (i.e., permafrost or Gelisol),” line 72.

L. 51-52,   Background/intro. If there is space this should be expanded upon, why are these things controls on respiration and why does temperature effect them

Thank you for the suggestion. We thought it might be too much detail and text to describe all the possible mechanisms, such as kinetic, thermodynamic, and other chemical effects; physical effects like thaw, and physiological effects. We added the following text to convey that there are many points of impact: “such as microbial community, metabolism, and enzyme activity, substrate availability, and vegetation types and inputs,” starting line 74.

L. 68,   Background/intro. Potential to include snow fence experiments also

Agreed. The cited study (Plaza et al. 2019) is a snow fence experiment. Text has been changed to refer to snow fence and snow relocation experiments, line 90.

L. 78,   Background/intro. Check kyra st pierre paper from greenland and ch4 oxidation

We were unsure if there was a comment for us or a note.

L. 81,   Background/intro. Why methane and not ch4 as in title?

Text has been changed to refer to the chemical formula CH_4 throughout except for a few specific cases.

L. 91,   Background/intro. Check to see how the approach is novel, confirm whether the approach is novel or the application the heat the entire active layer is novel

The approach of using a heating rod and warming the entire active layer by a controlled amount are novel in Arctic studies. A similar heating-rod approach has been used to warm mineral soil in a non-permafrost system (Castanha et al. 2018).

L. 97,   Background/intro. Ultra portable? If so I agree with their idea that it should have the ultra section dropped from its name!

Indeed, this has been corrected to the Ultraportable Greenhouse Gas Analyzer in the text, line 126. (I agree with reviewer here, it doesn't feel ultraportable when you are carrying it!)

L. 98-99, Background/intro. Slightly unclear what they mean here, warming increasing how sensitive the soil is to temperature? Or under warmer soils there is higher respiration (i.e., tundra soils are highly temperature sensitive)?

We meant that the change in respiration (CO_2 efflux to atmosphere from tundra surface) per unit change in temperature will be higher if the whole soil is warmed than it would be if only the surface soil were warmed.

L. 103, Background/intro. During the winter/spring? *Or just taliks at the site that have not connected with the active layer in the past?*

At this site, the surface freezes early in the fall, forming a talik until the whole active layer freezes. Gases are probably generated in the talik in fall, roughly October-December. In the high Arctic, there is a talik only in fall and early winter. In warmer parts of the Arctic, there can be a talik through the whole winter or even year round.
The rapid melt is most likely to happen in fall and spring because winters are too cold to have liquid precipitation that melts/thaws the surface

L. 115-116, Figure 1. Every depth had 0 (or negative) warming effect, what proportion of the warming depths did this occur in and why?

The proportion of temperature differences below 0 was 0.0212, 0.0003, 0, 00003, 0.0023, 0.0011, 0.0026, for 5, 10, 15, 20, 25, 35, 50 cm respectively. Thus, temperature difference was negative 2.12% of the time at 5 cm and less than 0.3% of the time for depths below that. We assume that small background differences in rainfall infiltration was the main reason, since rainfall can quickly cool the soil, and then it was heated again.

L. 118, Figure 1 caption. Indicate what dashed vertical line represents

We added to the caption that the vertical line represents the average value of 3.76oC warming

L. 118, Figure 1 caption. Maybe make a note in the caption that the negative value in the figure indicates below the surface and that the y-axis increments are not equal

We have modified the figure so that the Y-axis increments are equal and added the note about negative values.

L. 119-122, Figure 1 caption. Seems unnecessary for the caption,

We have moved this material from the caption to the main text.

L. 162, Results. Could it also be due to the turnover of older, refractory organic matter in the active layer that is mineralized due to warmer temperatures?

This is a good point. It has been added to the text in this section, line 220.

It is difficult to pick out the treatment plots from one another in this figure. Could you plot the median Q10 response curves for both treatments over these to show how they differ? It seems like the high rates in October 2015 are excluded from Figure 3, why is this so?

We agree that there is a lot of information in Figure 3a, with different symbol shapes for treatment and different colors for months. We thought that adding response lines (in other words, the exponential fit between CO_2 and temperature) would be too much, but we could add these lines to Figure 3a. As an alternative, we have provided the coefficients needed to plot the slope of the median Q_{10} response curves (i.e., the Q_{10}'s generated from seasonal temperature variation) for the control plots, heated plots, and the combination, in Supplementary Table S1.

The reviewer is correct, the values from the fall-season in October 2015 were not included in this Figure. We have added a note in the Figure caption that values from October 2015 were not included. These data were not included because the processes involved (post-ice melt) were qualitatively different than those in the rest of the dataset and the values were so high that they were off the scale of plot 3a.

L. 195, Figure 3a. This is not entirely clear from Figure 3a, they look quite similar in the plot. Can this be zoomed in on?

It is easier to see this point in Figure 4, which has an explicit time axis and has the October 2015 data in a separate panel. We have added a reference to Figure 4 in this sentence (line 195 in original submission).

L. 197, Discussion Figure 2. In the inset Figure o Figure 2 the gray bar indicates that the heaters were still off on October 15th

Thank you for catching this. The gray shading on October 15, 2015 was an error and has been removed from Figures 2 and 4.

L. 205, Discussion. This should be reported in umol to be consistent with Figure 4

Thank you, we have corrected this, line 285.

L. 229, Discussion. It may be useful to define what the authors mean by short-term. They use two growing seasons to estimate their Q10 values, but do some 2 growing seasons is short-term. Especially given the interannual variability in environmental controls on soil respiration.

We agree that two years is short term for many aspects of response. The emphasis in this sentence is meant to be "in the absence of plants" and the fact that few incubations extend beyond a year was secondary. Our experiment is also looking at short-term responses (i.e., direct responses without long term changes in vegetation and hydrology), but does include in situ plants and weather. We edited lines 316-317. We added a parenthetical that “We consider our field experiment to be relatively short term as well.”

L. 234, Discussion. How does this warming compare to winter warming due to snow fences and deeper snow packs?

In this warming experiment the warming effect was relatively stable (3-5 ºC) across the growing season and the soil profile (5-35 cm) (Figure 1). The warming effect was more variable (0-15 ºC) with time and soil depth (0-40 cm) for the snow fence and relocation experiment in Natali et al. 2011(GCB).

L. 258, Discussion. remove

Removed the extra parenthesis, line 346.

L. 264, Discussion. Do you explicitly measure the presence of this talik? I know that this is a difficult thing to do and it is most likely that there was the formation of a talik at the site. Does any of the soil temperature show this? Seems important to show if this study is to be the first to show in situ late season pulses due to gas release from taliks.

Soil temperature sensors in other locations at the BEO have/had power year-round to determine the duration and depth of talik (e.g., Romanovsky https://doi.org/10.5440/1126515), but we did not have probes running year-round in our experiment plots.

L. 355, online methods. Parentheses

Thank you. We have fixed this, line 468.

L. 366, online methods. Were these sealed at the bottom?

No, these were open tubes. We have edited the text, lines 473-474.

L. 368, online methods. It would be nice to include a photograph of this set up in Extended data figure 1 if possible.

The best photos we have are from other sites, so including a photo might be confusing for the reader. Here is an example photo so that the reviewer can see what the chamber-base looks like!

L. 392, online methods. How were these determined?

Brief methods descriptions were added. The vegetation was hand sorted, dried, and weighed. This text has been added to the methods section and caption of a new supplementary figure showing aboveground biomass, paragraph starting line 515.

L. 813, Supplementary Information. Sometimes a P value indicating significance is reported as "P < …" and others it is reported as "P = …". Please pick one approach and stick to it for consistency

We have changed to "P =" throughout except in Tables 1 and S1, where the very low p-values made that hard to read (actual values for the NLS fit were P=3.012e-06, P=3.706e-06, P=2.593e-07, P= 2.206e-06, from top to bottom in the table, respectively. For Table 1 we have changed P < 0.05 to P<0.0001, and for Table S2 we have used similar appropriate values.

L. 815, Supplementary Information. Figure number not included

Thank you. This has been corrected to Figure S3, line 1122.

L. 817, Supplementary Information. Was soil moisture monitored?

Soil moisture was measured manually, periodically.

Reviewer #3 (Remarks to the Author):

This paper heated the whole active layer, and then analyzed the greenhouse gases emissions and their temperature sensitivity. The findings are not groundbreaking, but the methods are novel and provide some solid data about the carbon emissions in a warming climate. The main concern is that the authors measured ER, not Rh, making it difficult to understand the decomposition of soil organic matter under warming treatment. The paper is largely overall well-structured, I have some concerns for the authors to improve the manuscript. Some of the comments may be mean, but please try your best to explain or discuss them.

Thank you for the helpful comments.

I would like to see some values of ER in the abstract. Also, please mention the land cover type in the study area. In the abstract, when you mentioned ER, you need to report the changes in vegetation. The last sentence is ambiguous, the authors heated the whole active layer, this is different with the deepening active layer.

We added the ER values to the abstract along with the p-value for the difference, lines 45-46.

We did not detect any changes in vegetation over the two years. We hypothesize that if the warming had continued longer than two years, we would have seen changes in vegetation as a result of (1) an (hypothesized) increase in nitrogen availability and (2) a longer active season with deeper growing volume for roots.

Good point that heating is different than deepening the active layer. The treatment did both.

L50, what is the timescale? Annual, growing season or non-growing season?

The predicted change in soil temperature reported in Soong et al. is annual; it was graphed in five-year increments in that paper.

3. L72, Here or somewhere, please add a sentence to clearly describe that climate warming will increase the soil temperature for the whole active layer.

We have added a sentence and referenced Soong et al., which analyzed >1 m depth over the century. In addition, Extended Data Figure 4 shows CESM soil temperature warming that is projected through 2100 to 35 cm depth which is nearly the whole active layer, line 72.

The first hypothesis is not well related to the introduction, why you expect higher Q10 values? Again, it should be clear to state the ER, NEE, Rs or Rh here. Warming usually promotes vegetation growth, and thus increase ER, but this not means the decomposition of soil organic matter.

For the editor's reference, the first hypothesis is "(1) when the whole active layer is warmer, tundra respiration rates show higher temperature sensitivity compared to those observed in shallow-warming experiments or in Earth system models."

Why do we expect more temperature response in surface co2 respiration for whole-active-layer warming rather than shallow warming? Here I try to break it down:

(1) We start from the common conceptual model that surface CO_2 flux reflects quantitatively the cumulative CO2 production in all depths below the surface at which flux is measured. This is based on conservation of mass and assumptions that microbial CO2 consumption (chemo-autotrophic), lateral transport exist but do not change appreciably day to day. Thus, more CO_2 production (i.e., respiration) translates to more CO_2 efflux. This underlies the approach of using chamber measurements to estimate CO2 production rates.

(2) We hypothesize that autotrophic and/or heterotrophic respiration increase with warmer temperature, based on many published field and lab studies of arctic tundra. We cite literature specific to the Arctic in the Introduction and Conclusion documenting a positive effect of warming on soil and ecosystem respiration. The Earth system models used the same Q10 globally so this aspect is not Arctic specific.

(3) We do not assume that the temperature sensitivity per gram biomass/material depends on temperature or depth (i.e., could have same temperature sensitivity of root tissues at 10 cm and roots and at 30 cm depth).

(4) We expect a larger increase in total column co2 production (and thus efflux) when we have heated the whole soil profile than when we have heated only the surface because in the former more biomass/material has been warmed. Therefore, the total change in CO2 production will be larger.

To make it clear that we mean ER, we have added the word "ecosystem" so that the phrase reads "tundra ecosystem respiration, line 128.

We share the same expectation as the reviewer about the long-term impacts on vegetation; a warmer Arctic would be expected to have more vegetation growth. In terms of our experimental treatment (belowground and short term), we are not aware of any empirical tests whether (or evidence) that belowground warming alone (without aboveground warming) has resulted in vegetation growth in the Arctic.

L 116, please indicate the permafrost boundary in this figure.

We added this information to the caption for Figure 1 "*Active layer depth ranged from 32-44 cm next to the control plots.*"

L142, Figure 2, for the October 2015, the values are very high, not only about 50% higher, which was described in L135-136.

Agreed. The 50% value is for the growing season, the October 2015 values are fall shoulder season. Here is the existing text, with emphasis added: In both years and all timepoints **except October 2015 (when the effect was larger),** heating increased ecosystem respiration by nearly 50% over unheated controls (n = 194 observations; control flux 0.67 μmol m^{-2} s^{-1}, heated flux 0.99 μmol m^{-2} s^{-1}, treatment difference $p<0.0001$).

L176 For the Q10 values, the authors calculated for the treatment differences of heated-control ER and temperature. I have no objection to this idea. But how about using the values ER and temperature from control and heated? There are several ER values and temperature for control and treatment, you can calculate the Q10 values, respectively.

Agreed. We have done this. These Q10's are reported in Table 1, Figure 3, and Table S1.

L142 and L209, how about the cumulative fluxes of ER and methane at the annual scale?

We agree that cumulative fluxes would be useful, but don't have a good way to extrapolate our growing-season point measurements to the whole year. We are showing the individual daily values and a season average. It would be possible to estimate the cumulative growing season value by multiplying the season average flux rate with the number of growing-season days, June-July-August.

L223 what carbon fluxes? Please be specific about the soil organic carbon release,

Here we wanted to keep a general phrase since the paragraph contents include soil (SOC or root) and ecosystem carbon fluxes, for CO_2 and CH_4. We did add a qualifier to our reported Q_{10} in this paragraph that it referred to CO_2, line 313.

L229-230, what is the difference between with or without plants? How do you know your results are better than fast cycling pools? Your study also has large uncertainty.

Thanks to the reviewer for flagging this point. We were not clear. We did not mean to say one is better or to imply any criticism, only that these different system configurations (e.g., w/ or w/o plants) reveal different information. In this case, our experiment is analogous to a laboratory incubation in duration and small physical size, but is different because our plants and soil were relatively undisturbed and the mesocosm (open PVC tube) included a living rhizosphere and

natural climate variation. These could provide more environmental continuity for microbes and root processes compared to laboratory incubations, to get emergent rates. However, the soil-only laboratory incubations might be better for studying mechanisms of heterotrophic response to warming. Neither is designed for examining changes in vegetation growth habits or species cover/composition.
We added text to affirm similarity with incubations: "(We consider our field experiment to be relatively short term as well.)" Line 316-317.

L233-251 please compare your findings with more previous studies about the effects of warming on ER, so potential readers can understand what is the differences between the surface warming and whole warming.

The key references in this paragraph are for the response of ecosystem respiration or of both ecosystem and soil respiration to surface warming and one is for deeper warming. For example, Oberbauer 2007 (surface warming by OTC at multiple ITEX sites; line 327) and Natali 2011 (whole warming by snow fence at permafrost site; line 330) are for ecosystem respiration. Pold 2021 (meta-analysis, multiple sites across the arctic; lines 324, 335) is for both ecosystem respiration and soil respiration, and found that ecosystem respiration had an overall weak (non-significant) response to warming in the arctic.
In lines 323 – 337 we describe the scope of these and other references, and effects on respiration they report.

L260-269 Did you observed the rapid melting of snow and ice during the experiment? What about the effects of melting of snow and ice on ground temperature and soil water content?

Yes, we observed that there was visible melting of snow and thinning of the snow layer near the heater.
The snow layer was shallow (~2-3 cm) at this time, so the snow water equivalent was likely low, but it is a good point that the melted snow water would have been transported downwards. We were not able to measure soil water content after the ground had started freezing up. In the growing season, precipitation can cool the surface and subsurface. When the ground is fully frozen, precipitation can rapidly warm the subsurface. It is a good question what effect this melt would have on subsurface temperatures since the shoulder season conditions are intermediate.

L307-323, the authors used the higher Q10 values for CESM modeling. I think this is not necessary. First, these should be part of results. Second, you simulated the carbon emissions were higher, that is simply related to the model structure. If you want to keep this modelling results, you should use some data, e.g., field observation data such as eddy tower data, to validate the model, and then you assess the accuracy of the simulation, and then you can say that your Q10 values are better.
L353 What is about the time period?

Thank you for this comment. We agree that if we had changed the Q10 parameter within the CESM model and then re-run the model, we would need to validate the model again as you describe, verifying that other benchmark values (e.g., NEE) were still reasonable.
However, the analysis we did is much more limited than that. The objective was to show that the measured temperature response was high enough to matter for carbon cycle projections. To do this, we used the output of CESM and scaled it. Briefly, we calculated the temperature – CO_2 relationship in CESM output for the Arctic. This gave an Arctic respiration over the temperature range predicted for the 21st Century and the emergent (i.e., in model output) apparent temperature Q_{10}. We then substituted the

measured Q_{10} in that relationship and showed (Figure 5) what the new predicted CO_2 efflux would be in the Arctic through 2100 for four different emissions scenarios. The temperature predictions for these scenarios are shown in Extended Data Figure 4. There is more detail in the Online methods section 6, starting at line 669.

Because we are essentially adjusting modeled ecosystem respiration (increasing it) using a much simpler equation compared to the model itself (albeit consistent with the fact that a single global Q10 is imposed in the belowground component of the model), we consider this a back-of-the-envelope calculation for illustrative purposes in the discussion but is not part of the core results of our study. These are some of reasons that we included it in the discussion and not the results section.

L353 What is about the time period?

This is historic climate (1901–2007). I have added this time period and citation to the Methods section, lines 460-461.

END OF REVIEWER COMMENTS AND RESPONSES.

NCOMMS-24-14134A

Barrow Warming Reviews and response to reviews

August 19, 2024

Thank you for this second round of reviews.
We have addressed every point raised by Reviewer #1 and thank Reviewer #3 for the assessment (Reviewer #3 did not call for any response). Below we describe the reply to each specific comment. The comments are in **blue** and our response is in **black**. The line numbers in our replies refer to the line numbers in the currently revised version (the reviewer comments are copied verbatim, including their reference to line numbers in the version they reviewed).

REVIEWER COMMENTS

Reviewer #1 (Remarks to the Author):

Authors did a thorough revision. I have a couple of further comments for finetuning.

Thank you.

My main suggestion is to move Figure 3 into the supplements. This figure shows (a) co2 flux versus soil temperature and seasonal Q10 values in (b) and (c). Having Table 1 with all the substantial Q10 information in the main text already, these additional Q10 details (in fig 3) are not really necessary to be shown in the main part. Seasonal Q10 (Fig. 3) were not used for modelling anyway. The Figure 3 content and caption text are rather complicated to read for a non-expert in this topic and rather distract from the main content. Current Figure 3 could be replaced with current Figure S2 (radiocarbon) and/or Figure S3 (NEE flux), which both would be more interesting/informative for a reader.

We have moved Figures b and c to the supplementary material. Simplifying the main figure (panel a) in this way allowed us to adopt this reviewer's previous suggestion of making it easier to distinguish heated and control values and adding lines showing the Q10 fits. It also simplified the figure caption considerably.
We kept the supplementary figure on NEE and GPP in the supplement.

Figure S3 currently lacks a caption in the version I have downloaded!

Apologies, the caption had slipped to the next page (lines 862 – 866). We have fixed the formatting.

Line comments:

L35: can a model "report" a Q10? It can, but in this case the Q10 is a model input parameter. Hence the sentence should be rephrased.

We have rephrased to: “The observed temperature sensitivity (apparent Q10 of 2.8; n=79) was much higher than that imposed (soil) or emergent (soil and plant) in Earth system models or reported by experiments warming only the soil surface.”

L39: comma after “whole”?

Changed.

L52: I’d delete “respiration” here. Respiration is a ghg flux. You may simply write “natural GHG fluxes” or “ecosystem ghg fluxes”

We agree that respiration is a source of a greenhouse gas flux. We edited these two sentences in response to Reviewer #2, who asked for some description of controls on respiration, so we are inclined to keep that phrase in response to their comments.

L90: please indicate already here how deep the active layer is.

Done. Added to Line 95.

L99: issue as in L35 – can a Q10 in a model be “observed”?

We have changed the text to “higher temperature sensitivity compared to those observed in shallow-warming experiments or imposed (soil) or emergent (soil and plant) in Earth system models”

L142: not really true - during certain warming-off periods CO2 flux was still higher in treatment plots (08.10, 08.13, 09.01). You ma rephrase here.

Thank you for pointing this out, there was a mistake in the shading.
8/10 was not shaded, and we hope the figure format makes this easier to see.
8/13 and 9/01 had an incomplete labeling. On each date, one heating system was off due to malfunctioning microcontroller systems. We did not use that (unheated) block in the treatment average, and therefore should not have had shading on those dates. We have removed the shading from the figure, and noted information in the online methods (lines 523-525).

Figure 2 and Figure 3: In both figures the addition of labels a,b,c into the three panes would be very helpful. You could then in the text more easily refer to e.g. the shoulder season experiment (Fig. 2b) etc. From the current figure caption it is not clear that the “October 2015” is the separate shoulder-season warming experiment.

We have added panel labels to Figures 2 and 4, and reduced Figure 3 to one panel. We have clarified in the caption that October 2015 was a shoulder season experiment. Lines 159-152 and 221-225.

L166-175: based on the text here it would make sense to move the corresponding supplementary figures into the main ms.

This article format has a limit on figures and word count in the main article. Since the findings for these three quantities (radiocarbon, NEE, GPP) are encapsulated in the three sentences here and not covered in the discussion, it was hard to justify cutting other figures and text to stay within the limits.

L200-2001: please refer to Figure 4 here already.

Done. Line 200.

L229: might not be only increased microbial respiration. Root respiration as well typically increases under warming. How many roots were in the soil?

Agreed. We have changed "microbial" to "physiological." Line 232.
We don't know how many roots underlay our plots. Root biomass in this site is quite variable, based on these papers: Iversen et al. 2014, https://doi.org/10.1111/nph.13003, and Zhu et al. 2016, https://doi.org/10.1002/2016JG003554.

L270: "many" might be replaced with "certain"

We appreciate the precision in this comment. We mean to convey "nearly all" whereas "certain" might convey it is a minority.

L272-306 discussion on CH4 fluxes in general: The overall effect of soil warming – namely decreased CH4 emission (fig. 4) somewhat goes under in the discussion. Although the effect was quantitatively smaller and statistically non-significant, it should be at least mentioned. Small effect over long duration also sums up quantitatively. The pulse CH4 during intensive shoulder season warming is impressive, but obviously was not occurring under natural (non-artificial warming) conditions in e.g. spring of the same year when the frozen soil thaws naturally. – or did measurements start too late in the season? Is there any explanation for the high CH4 emissions in September 2016 (fig 4) and the very low CH4 flux of warmed plots at the same time?

We have added more emphasis to this point. Lines 202-206

L522: The model could be better explained – repeated measurement design, etc.?

For assessing significance of treatment and temperature effects, block was the only factor. We opted to use as few explanatory variables as possible to answer the question, which in this case was whether treatment or temperature effects were significant. We did not use repeated analysis methods because the timepoints were unevenly distributed and not at standard intervals. One could use year, month, or sampling date as a random effect but because sampling dates were practically unique, this would result in overfitting. In response to the reviewer question, we re-ran some of these models adding month, year, or day+month as a random effect nested within block or block within time, and treatment and temp effects remain highly significant.

END OF REVIEWER COMMENTS AND RESPONSES.