Discussion of "Impact of Winter Warming on CO_2 Fluxes in Evergreen Needle-Leaf Forests"

(Old title: Divergent response of evergreen needle-leaf forests in Europe to the 2020 warm winter)

Reviewer comments are highlighted in black, while author responses are marked in blue. Line numbers correspond to the revised document with tracked changes.

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We would like to thank the editor and the referee for the additional round of feedback and would like to take this opportunity to address the remaining comments.

Response to associate editor comments

Associate editor comments:

Dear authors,

The remaining reviewer requests several revisions mainly about the clarity of the results and the tests. For example, they question the significance of the decline in NEP in colder sites, which did relate to my comments in the previous round of revisions. They also mention that they were unclear about the hypothesis and wonder about more simplified display of results (scatterplots), recommending adding them to the supplemental information.

Based on the relevant questions about the fundamental results of the paper and its clarity of presentation, I do think this requires another round of revisions by the authors (to be considered only by editors) given that I think it would lead to strengthening the manuscript. Please carefully reply to each comment and consider improving the clarity of results for each comment.

Sincerely,
Andrew Feldman

Response: Thank you for your feedback and for clarifying the points raised by the Reviewer. We understand that the hypothesis as we had phrased it deserves the clarification that the reviewer is asking for. We have now made sure this aspect is clear and provided additional scatterplots that the reviewer is asking for and made adjustments in the text. We hope that all points raised by the reviewer are now clarified. Please see below our response to each comment.

Reviewer comments

I am happy to review this paper again. Unfortunately for all parties involved, my main point of confusion still remains, despite the well-appreciated efforts by the authors to clarify things further. Indeed, one major point still requires further clarification for me: the significance of the decline in NEP in colder sites. This issue is mentioned in several parts of the manuscript, but the specific statistical tests and results underpinning the conclusion remain fuzzy.
For example, in line 464, you state: "In colder regions, NEP showed a significant decline in response to winter warming, reflecting this heightened sensitivity ($r = 0.66$, $p < 0.05$)." However, it is unclear which test in the results section this refers to. I searched for "0.66" in the manuscript but could not locate it in this context. Could you explicitly connect this statement to the corresponding analysis in the results section?

Response: here we had reported the result of a correlation test. However, this result is removed now (and text is adjusted) because in the test we had included delta NEP across all sites, while the hypothesis is about sites where NEP declined. Thus, we should have tested the correlation across sites with significant negative delta NEP. Please see more details below where we explain how we have addressed this point.

Additionally, what exactly are you testing here? Are you analyzing whether the average ΔNEP across all cold sites (with a clear definition of what constitutes a "cold site") is significantly lower than zero? Or is the analysis limited to the subset of sites that already show a significant change, as implied by lines 383–384?

Response: this was the test of correlation between delta NEP and site mean temperature.

Are you testing the results in Fig. 9b? If so, why do they visually not match this trend?

Response: because it was not testing the results in Fig. 9b.

If it is a test only on the significant sites, wouldn't that risk cherry-picking — analyzing significant changes only for sites that already exhibit them? Or is it a regression through those six sites as a function of winter warming, showing that delta NEP gets lower as delta T gets higher? A clearer explanation of the testing procedure and rationale still feels needed. Since the hypothesis is about effect of warming, first it is important to test the sites where the warming was indeed significant.

Response: since we are discussing sites where NEP changed significantly (as mentioned in line 340) we had to restrict this test to the sites that exhibited a significant change.

I understand that you have opted to present more complex figures in the manuscript, which is a reasonable choice. However, this makes the absence of simpler scatterplots, such as one showing ΔNEP as a function of temperature and/or winter warming, more noticeable. Especially the latter plot, ideally with separate trend lines for cold versus warm regions, would greatly strengthen your conclusion that NEP declined significantly in colder regions with increasing deltaT and not in warmer regions. (UPDATE: I understand now from your response to my question that you thought that 'the hypothesis 'Our hypothesis was that warming in winter will lead to a larger negative effect on net ecosystem productivity (i.e., higher CO2 emissions) across colder forests due to increased ecosystem respiration.' can be answered through a linear model with the interaction between mean annual temperature (to identify colder forests) and deltaT (to identify warming).' was asking for a linear model of the relationship between temperature and deltaT. This was a miscommunication, I wanted one of the relationship between delta NEP and the interaction between temperature and deltaT).

If you have deliberately excluded such simpler scatterplots to safe space, would you at least share them with the reviewers? Adding them to the supplementary materials — where space is not a constraint — could significantly enhance the clarity of your analysis. Simple plots often help bridge understanding and provide essential context for interpreting more complex figures. If these scatterplots support your conclusions, they would greatly strengthen your argument. Conversely, if they do not support the conclusion, it raises important questions about the validity of the statistical test referenced in line 464.

This remains important, I believe, as it remains unclear on what the conclusion that NEP is significantly declining in cold regions in response to winter warming is based on. First, I still not entirely agree that there is more decline in NEP in cold than in warm regions (even though the mean of the one positive and two negative NEP changes for the significantly changing warm regions is positive). When taking the 6 coldest regions, we see there are 3 significant negative ones, one significant positive ones, and two non-significant positive ones. Very similarly, for the 6 warmest regions, on the other hand, we have again 3 significant negative ones and one significant positive one, and one non-significant positive and negative one (Fig. 4, daily).

Second, is then the analysis of the relationship of delta NEP with delta T in cold regions based on the six coldest regions? Or only those that show significant changes in delta NEP?

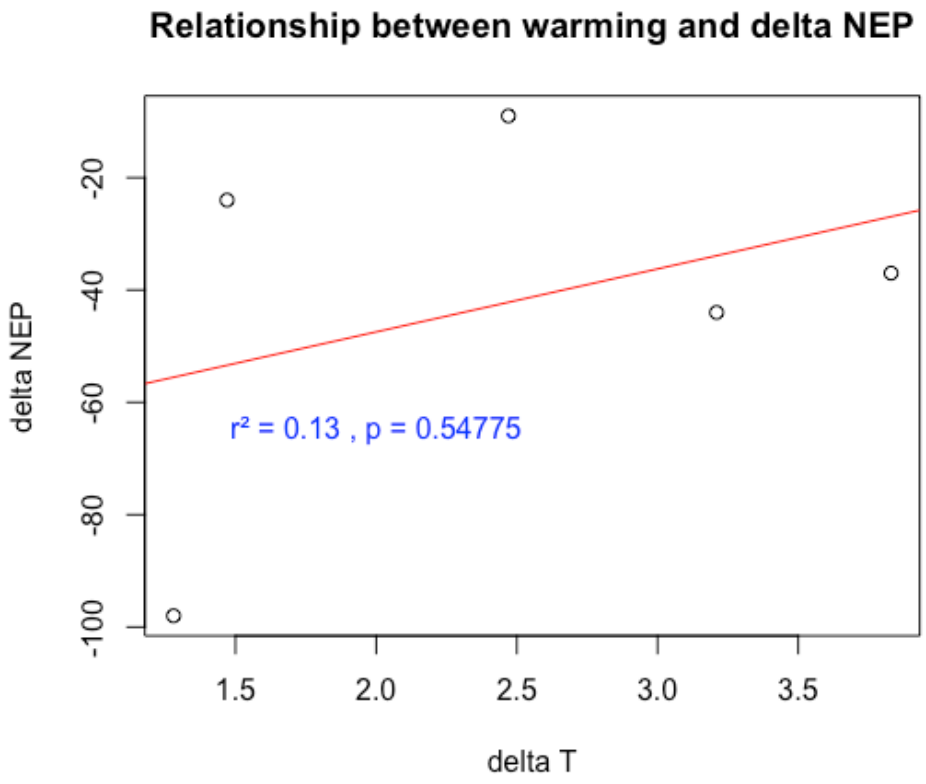
Is this than a regression based on four points? This could be enough if the trends are clear, but to me it remains important to show. Third, if NEP should be declining with winter warming, shouldn't that mean a negative slope, while in Fig. 9b the slopes of the coldest sites are all positive? Are we supposed to see all this in Fig. 9b?

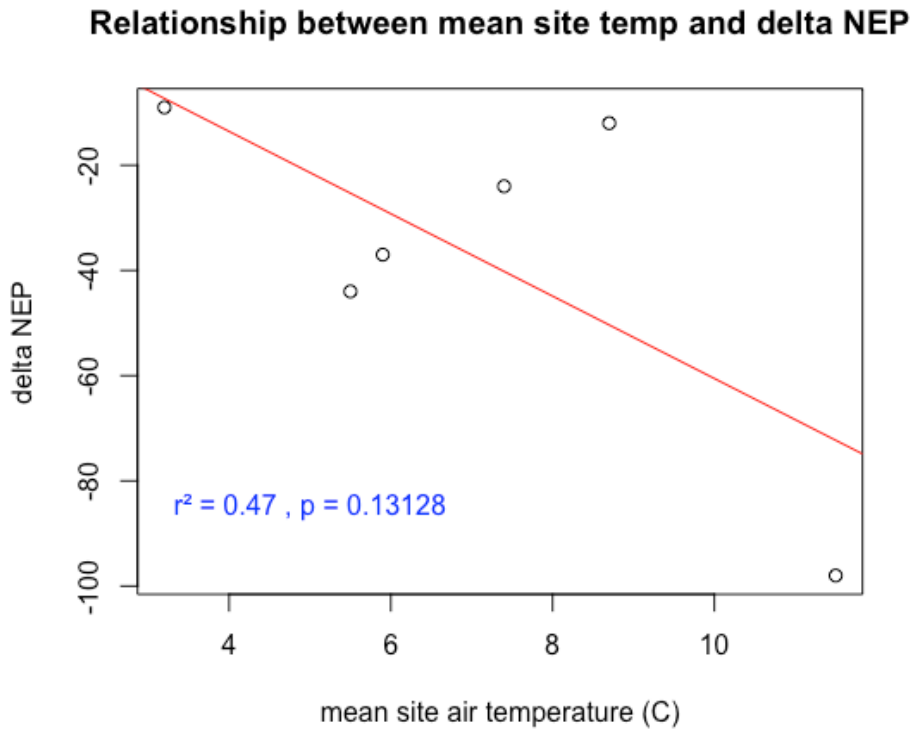
It could also simply be the case – as you mention in your response to the previous review – that the across-region patterns are not sufficiently strong to make many conclusions about them. But then this should really be stated more explicitly throughout the manuscript, so the reviewer refrains from trying to make sense of these across-region patterns as I'm doing here.

Response: In the previous version of the manuscript, we had looked at the relationship between delta NEP and mean site T across all sites where NEP changed significantly (as implied in line

340). The reported $r = 0.66$, $p < 0.05$ belonged to this test. However, the hypothesis is about "higher emissions" meaning that only sites where delta NEP is negative should have been tested.

Our hypothesis - as mentioned in line 35- is that warming in winter leads to higher emissions across colder sites. We looked into the relationship between magnitude of warming (significant delta T, 5 sites), and site mean temperature, with delta NEP at 6 sites where NEP declined significantly. This is shown in the following scatterplots as the reviewer requested:





However, the relationships were not strong enough to draw a conclusion. In fact, we see the opposite pattern (but not significant) in the relationship between mean site temperature and delta NEP compared to what we had expected. Hence in the revised version of the manuscript we revised all statements where a conclusion about larger decline in colder sites was drawn. This includes: The statement in the abstract lines (Line 45-46). The statement in lines 406-407. The statement in the Conclusions lines 477-478.

We have also added to the Discussion section Lines 399-404:

“Our hypothesis was that warming in winter will lead to a larger negative effect on net ecosystem productivity (i.e., higher CO2 emissions) across colder forests. While we observed that 1) across most sites winter emissions increased during the warm winter, 2) and that generally emissions in winter increase in response to increase in soil temperature (observed at 7 sites, Figure 7), however we did not find a link between warming of the air and increased emissions that would confirm this general hypothesis.”

While this hypothesis was not confirmed, the main body of our Discussion remains unchanged as we discussed study objectives that were directly supported by our results. These objectives were to: 1) evaluate the relative change in air and soil temperature and incoming radiation during the winter 2019-2020, compared to a 6-year reference period of 2014-2019, 2) quantify the relative changes in the winter CO2 fluxes across coniferous sites with available ecosystem-level CO2 flux measurements, 3) teasing apart the contribution of air temperature versus soil temperature versus

solar radiation to changes in CO2 fluxes during the warm winter, 4) test the sensitivity of CO2 fluxes to each of the climatic drivers, and 5) test if the sensitivity of CO2 fluxes to temperature changed during the warmer winter compared to previous years.
We have reported on the results on each of these objectives and shaped the Discussion accordingly.

Some remaining minor comments:
Regarding the hypothesis stated in lines 35–36: It explicitly focuses on ecosystem respiration (ER), but the results and conclusions in the abstract predominantly discuss net ecosystem productivity (NEP). It would be helpful to make the link between ER and NEP explicit. For instance, it is possible that the decline in NEP is driven by changes in GPP rather than ER remaining constant. Clarifying this connection would align the hypothesis with the presented results and enhance the overall coherence of the manuscript.

Response: Given that the tests mentioned above were not significant, our hypothesis was not confirmed and thus this statement does not hold any longer. We have made changes in the text to avoid confusion (see responses above please).

You state in your review that: 'Additionally, we have included new lines (124-130) that discuss how the impacts of soil temperature and air temperature on CO2 fluxes differ'. However, these lines don't mention soil and air temperature separately, just talking about temperature. UPDATE: you seem to describe what you say on L157-161, so that looks fine!

Response: OK, thank you.

L381-382: I mentioned in my previous review that I found the mean of three sites, one positive, one negative, not super meaningful. The fact that this mean is positive seems to get quite some weight by the way it is written here. Can it not be removed? I think it partially contributes to my remaining confusion described above.

Response: Our responses to the previous comments address this point now.

Is Supplementary Figure S10 mentioned anywhere in the text?

Response: Yes, it was mentioned in line 433.