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Biogeosciences Discussions

Interactive comment on "Abiotic CO_2 sources confound interpretation of temperature responses of in situ respiration in geothermally warmed forest soils of Iceland" *by* Marja Maljanen et al.

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This is an interesting topic and fits into Biogeosciences. However, before acceptance I recommend a few improvements of the paper (rather moderate revisions). The language level is OK, but definitely needs the standard language check done by Biogeosciences.

Although the topic is very interesting, I struggled in some parts with the lack of care in the wording. For example I first did not realize that "soil δ^{13}C" is actually not what I expected (i.e., the δ^{13}C signature of the organic material in the soil), but is jargon for δ^{13}C in soil CO_2, which is a completely different thing.

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Another point is how the author deal with missing data. The justifications are not really convincing. For example, it remained unclear to me why there are no 2014 data from FN+10 presented in Figure 4. The text on lines 255–256 sais that there was no sampling in 2014 because no *Agrostis stolonifera* plants were found. Well, this only relates to the plant tissue δ^{13}C, but it does not explain why there was no soil CO_2 found on which an isotopic analysis could have been carried out.

And the third point is: even if everything is fine, I expected some discussion why the plant tissue in Figure 4 is below the range of the mixing model. To me it appears that only the FN+20, FN+20–40 and FN+40 plant tissue signatures are in the range determined for soil respiration (–26.67 to –28.91‰ according to line 198).

Finally, I have some critique on the experimental design: the authors discuss temporal variabilty of geothermal CO_2 effluxes, but their sampling only presents two snapshots, one from one (!) campaign in June 2014, and one in July 2016. Thus, no variablity can be deduced in a scientifically defensible way from two snapshots. Why did the authors not e.g. add iButton temperature loggers to convince me about the average temperature conditions? It remains obscure how they defined the FN+X classification without such data, and Table 1 actually shows that the deviation of the snapshot temperatures from the planned +X °C temperature class can be substantial.

Thus, let me summarize my critique in the hope that this helps to improve the manuscript in the revisions:

1. **Terminology.** Please say explicitly if you mean air in the soil and not the soil itself. Also when providing mass units, it is essential to know whether this is mass of C or mass of CO_2 (see details below). Also the term "unwarmed" sounds like "cooled" to me. Later in the text you use the term "non-warmed" which I understood probably correctly. Do not use synonyms in scientific texts, this is confusing, and be more specific and clear in your choice of wordings for special terms. Here I definitely would not use the term "unwarmed".

2. **Missing data.** The absence of data points (missing data) is not fully described. I see the aspect of reproducibility in such issues: as a reviewer I need to know whether the authors do not present the data points because they did not like them, or whether they made a sound an serious, objective and reproducible decision about screening out data. Here only partial information is given and what is presented is only partially convincing the reviewer. Please specify all information about missing data. For example, on lines 146–151 you say that there was no *Agrostis stolonifera* found between plots FN+2 to FN+6 (my interpretation of such a statement is that FN+2, and FN+6 had the plants, but between the two plots you did not find this plant, but the presentation in Figure 4 suggest that FN+2 and FN+6 did not have the plant either). But only on line 255 (at the end of the Results section) do you inform me that also FN+10 did not have this plant in 2014, an information that was not provided in the Methods section. I would expect all this information clearly presented in the Methods section, and then in the Discussion section you could discuss how the missing data might have influenced your interpretation. I also expected a statement why there are no soil CO_2 δ^{13}C measurements presented in Figure 4 for FN+0 out and FN+40 out. Moreover, you have FN+1 in Table 1, but in Figure 4 this plot is quietly removed. Why? You must be more clear what you show and what you hide (and why).

3. **Plant tissue isotopic signatures.** In Figure 4 most of your triangles are **below** the lower end-member (soil respiration) of your mixing model. Only the data from FN+20, FN+20–40 and FN+40 seem to be in agreement with 100% soil respiration (which is questionable as well) but I would expect a discussion why all the tissue values are so much lower and cannot be explained with your mixing model.

4. **Experimental design.** Please provide the key information in the Methods section how the plot selection and assignment of the temperature deviations was done. Was this also only based on snapshot data? Is there no way to have a

better statistical information on the longer-term mean temperatures of all plots? Also specify the exact dates of sampling, not only the month, this is essential information if someone wants to consider weather conditions (you do not lose a single word on this) when you did the sampling. I fear that all interpretations are somewhat uncertain since we do not know whether you had comparable weather conditions in June 2014 and July 2016 or whether I as a reviewer should be concerned that one snapshot had much lower air temperatures than the other.

5. **What was the expectation?** You did not phrase any science questions or hypotheses, which makes it difficult for the reader to know what you expected and why I might not have expected the same. For example: Figure 2 shows almost a reversed gradient of the share of geogenic CO_2 flux from what you probably expected (an implicit information deduced from how you present your data). But I wondered: what did you actually expect? When do you think the temperature deviation is high and when is it low? I would expect that if there are no macropores in the soil then the surface soil temperatures might be higher than if there are macropores and cracks where the geogenic heat and CO_2 an reach the soil surface more easily and quickly than under absence of cracks and macropores, and thus the temperature of the topsoil does not pick up as much of that heat as if it were purely molecular heat transfer. Thus I wondered whether the 2016 conditions might reflect a change in macropores – and on line 279 you indeed mention a minor earthquake, but do not follow up on this important information in the discussion. Thus: could it not be that in 2016 FN+10, FN+20 and FN+40 were affected by getting more cracks/macropores, and thus the temperature increase from 2014 to 2016 under presence of cracks and macropores might just reflect a reversed picture? If there is not a good reason for not having monitored the soil temperatures over a longer period (e.g. with iButtons), you should definitely address this flaw of the experimental design in a critical manner in your discussion.

DETAILS (with line numbers)

11: "formed" does not sound correct here. Maybe established, created, resulted, ...

14–15: I assume the mass units relate to CO_2, not C; thus write mg CO_2 m^{-2} h^{-1}. Personally, I am from a community that uses μmol m^{-2} s^{-1}, not mass units, then there is no need to specify such details.

16: it is not a ^{13}C analysis, but an analysis of the ratio of $^{13}C/^{12}C$. Avoid jargon!

17: what does the word "different" exactly refer to? Does it mean that the plot with highest geothermal source had a different source strength in 2014 compared to 2016? Or do you think that it was not the same plot in 2014 and 2016 that had the highest source strength? In the latter case you should not use "the plot" as the subject of the phrase and rather write something like "It was not the same plot that had the highest geothermal source strenght in 2014 and 2016" (but if this is what you want to say: why not say FN+X had the highest ... in 2014, whereas in 2016 the highest ... were observed in FN+Y?).

63: "In 2004-2014." is not a correct sentence. Connect with the following sentence.

97: this confuses me: you write that the sample analysis was done at University of Eastern Finland, but in the acknowledgments you thank Andreas Richter for gas analysis at UNIVIE. What is now the correct information? The two statements are in stark conflict! Also specify how the samples were transported to the (correct) laboratory, and what precautions had be taken to minimize transportation artefacts.

98: Soil temperature measurements: specify what instrument/method you used. This is lacking completely.

125 (and elsewhere): no space between δ and ^{13}C

127: no $\times$1000 needed in the equation. δ values are a ratio which can be presented as a ratio or as a percentage or as permils. But this is not a unit conversion and thus the

$\times$1000 is an error in the equation (although people still use this – if you add a permil sign then it is no longer an error, but not best practice).

135: find an English word for “guarantying”

150: the past of “grind” is “ground”

155: linear regression (if you mean ordinary least-squares regression OLS) is not the correct best practice for Keeling plots. Use orthogonal regression (statistically perfect would be standardized major axis regression, but other orthogonal regressions also tend to be OK for the task). Maybe this solves some issues discussed under point #3 above, OLS is always flatter than orthogonal regressions since OLS does not consider uncertainty in the values ploted on the x-axis).

172: please only use the term standard error as defined in statistics; if you bootstrap as with Phillips and Gregg (2001) then the correct term is “uncertainty”. Note that standard error is only relating to normally distributed data, and it is SD/$\sqrt{n}$, whereas bootstrapping is distribution-independent.

174: wording should rather be “significantly different from zero”

175–179: please reword! The Keeling plot does not violate your assumptions, it is just the other way round: you as authors violate the assumptions made by the Keeling plot! Don’t blame Keeling for this!

175–179: the Phillips and Gregg (2001) model deals with underdetermined systems; why do you not use this approach here instead of the Keeling plot, if you consider three sources/sinks? This is not clear to me.

181–183: you used Pearson’s product-moment correlation. Did you check whether your data are really normally distributed? You never mentioned that you carefully tested your data for model assumptions. If the data are not normally distributed, then you should use Spearman’s correlation coefficient instead.

198–199: clarify that you are not writing about δ^{13}C of the soil, but of δ^{13}C in CO_2 in the air in the soil. This is quite a different thing! (2 occurrences)

Table 1: I prefer manuscripts with tables and figures at the end. If you want to place them in the text then they should appear near the first reference. Table 1 is referred to first on line 84, but you placed it between lines 200 and 206; this neither helps the typesetter nor the reviewer.

Table 1 caption: please specify in Methods why FN+2 was not sampled in 2016. Or did I miss this?

Table 1 heading: say "in soil CO_2" not only "in soil"

214–217: you seem to have changed your font for δ but forgot to export the font to the PDF (3 occurrences). Please rectify (and check your PDF more carefully for errors related to your word processing program)

222: there is a rule to round reported figures to signifcant digits. Here it would be –5 $\pm$ 2‰ or at at best –5.1 $\pm$ 1.8‰ I am however not sure whether the isotope community respects this rule. In any case: make sure you present the same numbers with the same accuracy (and improved rounding) both on line 222 and line 289.

224: this is an error: you're not writing about absolute amounts of CO_2 in what follows, but about **relative** amounts.

238–239: see my comments in major point #5 related to this statement here.

249: when reporting values that are not dimensionless please add the units after the figures.

252: did you check for normal distribution of the data both on the x- and y-axes? If not, please report and if they are not normally distributed, then use the correct statistics for trend testing (Mann-Kendall trend test)

255–256: explanation not satisfactory (see introductory comments above)

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273–276: you use the term “point of maximum outgassing” here, but you have not defined what this means. As it stands, the whole statement remains obscure and not really understandable to me. Also the sentence that follows is inaccessible to me, maybe because of the lack of definition and explanation of “point of maximum outgassing”. This needs some rewording and additional explicit explanation.

295–296: don’t use the term “accuracy” if you do not have an absolute reference; use the term “precision” instead. And I do not agree that there is high confidence, unless you used an orthogonal regression approach (and explain which one you think gives the highest precision of the regression slope in this application). In **R** the lmodel2 package provides major axis and standardized major axis regression and provides references. The Editor in charge (Dan Yakir) might give you an authorative answer, which approach is currently considered best practice here. Get his advice.

306–308: probably your interpretation is OK, but without soil moisture information it is difficult to make a sound judgement. Why did you not also measure soil moisture? Is the soils wet no matter what the temperature is? In most cases respiration at high temperatures decresase because of the negative correlation with soil moisture. Is this what you expect and explain here? Please provide more insight and explicitly state your understanding of the soil moisture aspect. I think that if the trees die then this might be a result of dry soils, but without your explicit information it is blind guessing on my side (at best).

Figure 1: for me it is confusing that you have FN+0, FN+1, FN+3, FN+6, . . . here, but in Table 1 you have FN+2 in place of FN+3. Is this a simple typo or something that must be added to Methods? And why is there no soil temperature available for FN+3? There is no word about this (see my point #2 above). Also it is unclear why no soil temperature was measured at FN+10 in 2014, although in Table 1 you have values for what I assume to be soil temperatures (the caption however does not say what T really is . . .).

Figure 3: please add ‰ after the isotope ratio, or divide them by 1000 (then without ‰).

Figure 4: needs more information in Methods. It remains obscure what FN+0 out and FN+40 out actually show, since you explained that also in the other plots you collected the plants **outsite** the plot. So my assumption is that FN+0 out is more outsite than the FN+0 is, but this is quite fuzzy. Please add the necessary information to your Methods section. And please modify the title of your y-axis at right. It is not any CO_2 that you show here (and it also is not soil CO_2), it seems to be the CO_2 in your flux chamber. My suggestion thus is to label this axis "δ^{13}C in CO_2 efflux".

Signed review: Werner Eugster

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