

Interactive comment on “Technical Note: Differences in the diurnal pattern of soil respiration under adjacent *Miscanthus x giganteus* and barley crops reveal potential flaws in accepted sampling strategies” by J. Ben Keane and Phil Ineson

Anonymous Referee #4

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Review on "Technical Note: Differences in the diurnal pattern of soil respiration under adjacent *Miscanthus x giganteus* and barley crops reveal potential flaws in accepted sampling strategies"

The abovementioned manuscript presents sound evidence that repeated manual field measurements of soil respiration at an arbitrarily chosen fixed time of day does not only likely fail to reproduce long-term averages of this important flux (which is probably not in conflict with any serious claim in existing literature), but even can cause

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significant errors in a comparison of two nearby treatments (where it is a widely accepted compromise). This demonstration is surely worth publishing in BG, although SC2 indicates that this might not be the first such study, and care should be taken to acknowledge prior ones appropriately. The manuscript is well written and mostly clear, in some points pointed out below, information e.g. on the methodology is missing or too scarce, and the present shortness of the manuscript offers a good opportunity to improve this while still staying concise.

The maybe most important (and only major) flaw is that the two topics presented in sections 3.1 and 3.2 are little connected and differ strongly in terms of originality, soundness of the methods applied, and speculativeness of the discussion. Topic 3.1 is clearly at the heart of the study, as indicated by the title, and the reason why I believe this manuscript should be published in BG. Concerning topic 3.2, I am unsure how much the authors can do with the measured data at hand to improve it up to a point where it can amend existing knowledge, so in my opinion two options - extending it for the analyses and discussions suggested by earlier reviewers and also below, but also cutting it down to what can actually be said, and spending more space on a more elaborate analysis and discussion of the main topic of the study, could both be considered.

Abstract

p1L14: "coincided with levels" - unclear, reword (see also comment on p5L32).

2.1

This section in general: How often and for how long were the chambers closed?

p2L23: ...and *an* infrared gas analyser? p2L24: specify: was it 2 multiplexers (one per ecosystem?) p2L28: inserted 2 cm: It is not mentioned which collar height was chosen (Li-Cor's standard?) and/or how high they protruded above the soil surface. In general, an insertion depth of 2 cm is rather low (possible lateral diffusion in coarse soils) and the resulting large height above the surface should be avoided because of

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its altering effect on insolation, precipitation and wind (probably not so much an issue once the plant canopy is closed). p2L30: Give more details on sensor installation (vertical or through a trench, resulting depth averaging). Note that to gain confidence in the later discussion on (partly lagged) responses to temperature and solar radiation, the temperature would ideally have been measured in several depths.

2.2

p3L6: Licor software and manual sounds a bit odd, maybe "manufacturer"? p3L14: duplicate dot after 80% p3L17-20: Try to secure the reproducibility of the statistical methods not so much (or at least not only) by telling which option of the applied software was chosen, but rather by referring to the name of the test, to literature if necessary, etc., e.g. which test for normality? The result on normality does not seem to be mentioned in the results section (if I didn't overlook it). Note that for soil respiration in general it wouldn't be surprising if it was lognormal rather than normal, where necessary some authors work with log-transformed values.

3.1

p4L3: 9:00 and 20:00: unclear, you mean that instantaneous values close to the daily mean were reached at these times of the day? Reword. p4L18: "...shows that the shift [...] would be totally missed": This type of very straight conclusion would better fit in the following paragraph, where such things are plainly demonstrated.

3.2

p5L1/Fig.5: Make clear that the lag shown in the figure for each months is the one that yielded the optimal R^2 after experimentally testing all lag times in a range from x to y in steps of z (here and/or near p3L25 in 2.2).

p5L3-7: Although this hypothesis is plausible for your case, little is presented to support or falsify it. If radiation data are experimentally shifted to improve R^2 , so should be temperature data to check for the effect of the mentioned lagged response by improper

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temperature measurement depth (ideally it would have been measured at more than 1 depth, see comment on p2L30). The physically most consistent way to do so would be by Fourier analysis, since heat transport in the soil would introduce different delays for temperature variations on different temporal scales (e.g. diurnal cycle vs. slower or faster variations), but if variability in a certain time-window is strongly dominated by the diurnal cycle, a simple shifting might do as well. Also, the sentence is very long. Its 2nd half is unclear to me and should be reworded. It seems that a single case study, where hysteresis in the Rs-T relation could be attributed to photosynthates after comprehensive measurements, is used to infer that the same is true in your case. At the same time, an abundance of literature is ignored which demonstrates that also heat transport and measurement depth effects alone can cause hysteresis (e.g. Pavelka et al., 2007, Plant Soil 292:171 and Graf et al., 2008, Biogeosciences 5:1175 to mention just the earliest systematic studies, many follow-ups have been already mentioned by other reviewers).

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p5L32: Specify what exactly (e.g. the ratio or difference in total respiration between two treatments) can be incorrect by 40 % - the way it is written now suggests that conclusions are, but what would be a 40 % incorrect conclusion?

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