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Biogeosciences Discussions

Interactive comment on "Measurement depth effects on the apparent temperature sensitivity of soil respiration in field studies" *by* A. Graf et al.

Anonymous Referee #3

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As soil CO2 emissions constitute a major component of the global C cycle their sensitivity to predicted changes in climate have become an issue of increasing concern. Besides the ongoing discussion on the temperature sensitivity of SOM decomposition, as based on incubation studies, there is still a lack of conceptual clarity as concerns the assessment and interpretation of the temperature sensitivity of soil respiration measured in the field, where multiple potentially relevant factors are confounded (cf. also the review by Davidson et al. 2006, Global Change Biology, which the authors might like to refer to). In this context the study by Graf et al. is an important contribution in that it systematically analyses the effect of an often overlooked factor that may greatly influence apparent Q10 values, namely the measurement depth of soil temperature (note that this issue has already been pointed out earlier, e.g. by Reichstein et al. 2005,

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Global Change Biology). The study by Graf et al. is comprehensive in that it combines a literature survey with own measurements on a bare soil and a number of simulations using a numerical model, which has been validated against the own set of measurements. The authors demonstrate that the measurement depth of soil temperature is a parameter that may greatly influence apparent Q10 values. It becomes evident that this critical depth is difficult to determine accurately, as it 1) rarely corresponds to the layer where the depth of measurements is most closely related to soil respiration (i.e. R2 of the correlation is highest) and 2) its effect may further be strongly influenced a.o. by the thickness of the respiring layer, the duration of the study and frequency of measurements, as well as the annual amplitude of soil temperature. The authors suggest that based on their simulations most measurement depths used by earlier studies have likely led to an underestimation of Q10.

The study by Graf et al. is well conceived and well written and will likely be an important reference for future work. It is therefore important to emphasize that their model simulations are based on a number of assumptions that need to be accounted for carefully when transferring some of their observations and conclusions to future field studies. It might e.g. be tempting to take from the study that a measurement period of 180-200 d should suffice for determining Q10, or that temperature should best be measured in deeper soil layers than is normally done. As far as I can see the model was applied with the assumptions that 1) 'real' (=input) Q10 is constant across the soil profile , 2) soil organic C is a suitable proxy for source strength contribution and 3) soil moisture does not influence Q10. While 3) might perhaps be neglected for a range of non-droughted and non-waterlogged ecosystems, 1) and 2) may likely oversimplify the situation in most ecosystems. It has been shown that SOM quality rather than quantity, but also the amount of fresh C (cf. eg. Fontaine et al. 2007, Nature) available may determine the rate at which SOM is decomposed, and that this rate per unit SOM changes with soil depth. It should also be noted that root and mycorrhizal distribution and related autotrophic respiration (incl. the rhizosphere) may be variable across the soil profile. Autotrophic respiration plays a significant role in most ecosystems and may exhibit a

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different temperature sensitivity than heterotrophic respiration (e.g Boone et al 1998, Nature). For these reasons it would be highly instructive to see also model simulations showing how measurement depth influences apparent Q10 when vertical gradients of Q10 are assumed.

Further comments:

- It would be interesting/ important to show and discuss also the combined effects of the more sensitive parameters? Eg. the thickness of the respiring layer may be larger than 50 cm at many sites, how would this affect the sensitivity to the length of measurement period or annual temperature amplitude?
- Results: p 1874 l. 21-22 and Fig. 2. omit single value studies, as effects of depth and other factors on Q10 are confounded across sites- these values do not add any further insight.
- Table 1 should refer to Fig 2.
- Omit Fig. 3 1) for the reasons outlined above, 2) as Corg does not add to the explanatory power of the model (cf. eg. Table 2). In case the authors insist on keeping Fig. 3 is they should define the variables. What is SR Tref (AU)? Does it correspond to the grey line?
- Fig. 2 refers to Table 1 (not 2!). 9 is not defined in Table 1.
- Indicate in the legend of Fig. 4 that input Q10 = 2.5.
- If inter-annual variations in temperature are neglected (text p. 1876) measurement period length in Fig. 4b should be scaled to 365 d.
- Fonts are too small in Fig. 4 and in inset text of Fig. 2

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- Effects of soil depth and annual –and daily – variation of soil temperature are confounded. They thus not only reflect variation in climate or vegetation (shading of soil).
- Discussion: The authors discuss potential short-term influences of plant photosynthesis on soil respiration by referring to a study that has inferred such a relationship on the basis of a hysteresis in the soil temperature-respiration relationship that may as well have been caused by shifts in phase and amplitude of the two parameters, as demonstrated by Graf et al. themselves. In view of their own results the authors should thus be more critical. Generally, I wonder whether seasonal changes in SR as related to phenology would not influence Q10 values derived from an annual dataset more importantly than diel variations in a Tsoil-SR relationship.
- The model has been validated against a set data obtained for 1) bare, rootless soil (cf. comments above), where 2) two winter months (December and January) were discarded, thus less than 365 d were available for validation. Discuss how may this have altered the result?
- Appendix C: p. 1888 eq. (C1): why is SR needed here?

Interactive comment on Biogeosciences Discuss., 5, 1867, 2008.