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Interactive Comment

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

A. Graf et al.

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General

We would like to thank all discussion participants for their encouraging reviews and constructive suggestions on minor changes, which were implemented into the manuscript. We also received some friendly oral comments concerning literature references on lab measurements, root respiration, and soil temperature modeling, which we also included.

Specific

Pavelka, comment SC S641: The potential use of the maximum R^2 method for empirical modeling (e.g. gap-filling) is now mentioned.

Referee 2, RC S769, literature review: We have shortened the text and structured the climate / land use information in the table more regularly.

Referee 2, RC S769, Appendix A: The suggested reference was included; it seems of particular interest not only because of its description of temperature sensitivity relations, but also because of its critical evaluation of those which are most common.

Referee 2, RC S769, Conclusions: We have moved the discussion about consequences to the discussion section.

Referee 2, RC S769, Technical: The lines refer to Appendix C.

All three referees, Fig. 4: Fonts have been enlarged.

Referee 3, RC S773, reference (Davidson et al., 2006): The reference seems to fit in well at p. 1879 bottom, where we mention that the most important other confounding factor (moisture) typically also leads to a Q_{10} underestimation. This alone would have suggested that the "depth-corrected" Q_{10} of 5.9 was still a low estimate. In contrast, the suggested reference offers explanations (e.g. oxygen and other substrate availability) for confounding factors leading also to overestimation.

Referee 3, RC S773, reference (Reichstein et al., 2005b): There is no doubt that the temperature measurement depth issue has been mentioned earlier, and an overview (1994-2006) is already given in the introduction. The suggested reference was added, as it is unique in wo ways: i) It is based on a laboratory incubation, demonstrating that the issue is not only relevant for field studies; ii) it offers another data analysis strategy that might also help reduce errors in field measurement interpretation. We also added a recent overview paper (Reichstein and Beer, 2008) mentioning the issue.

Referee 3, RC S773, tempting to choose period > 200 days or deeper temperature probes: Too convenient practical conclusions by some readers are surely a danger for any publication that demonstrates how things shouldn't be done without being able to give a perfect recipe for how they should be done. We do not encourage such conclusions and have revised the discussions and conclusions section.

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Referee 3, RC S773, assumptions behind the model: The assumption that Corg is a suitable proxy is not part of the model itself, which only needs depth-dependent reference temperature respiration as input. In the validation part, Corg was tested as one of two proxies for this when modeling the field dataset. It was shown to perform slightly worse than a simple horizon assumption, and the reason for this is discussed on p. 1879 top. It seems to us in perfect agreement with the quality argument in your review. The reference (Fontaine et al., 2007) surely adds a worthwile dimension to this consideration. The model is not limited to a depth-invariant input Q_{10}, but the BGD paper only contains such model runs. Depth-variant input Q_{10} produce apparent Q_{10} profiles closely resembling the results obtained with the weighted average over the whole profile, which is an additional reason why "field measurements of CO_2 efflux at the soil surface are not suited to derive the temperature sensitivity of deep buried carbon" as stated on p 1979. A demonstration of this ambiguity (also note the related overfitting argument on p 1875 top) is indeed an interesting addition to the paper. In the revised version, we use a linear change of input Q_{10} between values taken from Boone et al. (1998), i.e. 2.5 for heterotrophic and 4.6 for root-dependent respiration. Increasing, decreasing, and constant (both arithmetic and geometric average Q_{10}) input profiles led to almost the same apparent Q_{10} profiles with a maximum difference of 0.21. We included this instead of Fig. 3 which you suggest to omit.

Referee 3, RC S773, further comment 1: To take up your example of a significant contribution of respiration from deeper layers, this further moves down the measurement depth regaining the input Q_{10}. Errors made in great measurement depths are reduced while those made close to the surface increase. Near the surface, a short measurement period or low annual amplitude aggravates this underestimation (including local minima of $Q_{10} < 1$). With respect to great depths, however, it should be kept in mind that due to the small temperature amplitudes small errors in the measurement and probe location will become increasingly important in practice. We added a random error option to the model but do not use it in the manuscript. It would be difficult to back up its assumptions, and it only changes results for unlikely great temperature

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probe depths. Another example of interacting error sources would be a moderately short measurement period and low thermal diffusivity. The latter may be expected in a dried-out organic soil or a snow cover. Such combinations may explain the extremely high Q_{10} values reported by some authors mentioned in the literature overview.

Referee 3, RC S773, further comment 2: For this reason, we refrained from providing the statistical correlation of these values with depth, which might have been too suggestive. However, as the sample is completely random, other confounding factors (as responsible for the scattering) are very unlikely to artificially introduce, or mask, a tendency such as the one shown in Fig. 2. Maybe even more interesting, these points demonstrate the relative narrowness of measurement depths and Q_{10} values reported from single depth studies. The former is an argument in the consideration that the current global modeling assumption of a Q_{10} of 2 might be biased. The single depth studies can indeed easily be omitted without affecting the main train of thought of the manuscript. Nevertheless, we would prefer to keep them for the above reasons.

Referee 3, RC S773, further comment 3: This reference was added.

Referee 3, RC S773, further comment 4: Figure 3 was omitted. By the way, SRTref is respiration at reference temperature (see above, and Appendix A) and indeed belonged to the the grey line.

Referee 3, RC S773, further comment 5: The table reference was corrected.

Referee 3, RC S773, further comment 6: Yes.

Referee 3, RC S773, further comment 7: Yes, was scaled, and shorter measurement periods (down to 1 day) have been included.

Referee 3, RC S773, further comment 8: Yes (also see above).

Referee 3, RC S773, further comment 9: Our comments on the physical role of vegetation in section 4.1 seem to be too short and are now extended. What we wanted to say is that a vegetation canopy damps the diurnal soil surface temperature cycle more

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than the annual one. In consequence, soils under a canopy exhibit relatively more of the kind of temperature variance that is not so easily dampened with depth, and less steep apparent Q_{10} profiles.

Referee 3, RC S773, further comment 10: A further oral comment has turned our attention towards a paper in press on grassland soil respiration (Bahn et al., 2008). Here, a similar discussion as on p 1878 is held but more elaborate and indeed more critical towards the "phloem time lag" determination. We added a reference to this paper. And yes, the fact that seasonal Q_{10} are wanted to describe phenological effects, but at the same time eliminate much of the annual cycle that is less susceptible to measurement depth problems, does represent an unresolved problem (cf. p 1881 l 8-11). On the long term, it seems more promising to determine parameters such as temperature sensitivity by inverse fitting of a numerical model describing thermal diffusion, pool dynamics, and plant growth (cf. p 1882 l 24 ff.). For the required field data set, the motto 'the longer the better' would hold.

Referee 3, RC S773, further comment 11: 1) The current model version, for purpose of demonstrating the pure effect of the manuscript topic, assumes temperature as the only factor controlling soil respiration. This aim was matched well by measurement conditions (absence of roots and a moderate moisture climate). Other confounding factors, including those driving root respiration and exudation, will not change results if they do not covary with local temperature. If they do so, they will gradually reduce the value of model predictions. As root respiration is also partly controlled by in-situ temperature, only the presence of additional factors covarying with local temperature could lead to model errors. Such a factor might be radiation. Therefore, we stated on p 1878 l 13 not only (in accordance with Bahn et al. (2008)) that the measurement depth effect is a potential source of error in studies determining the "phloem lag", but also that the opposite might occur. Obviously both, authors in search of a phloem lag and those trying to fit a model like ours on a mixed respiration dataset, will need to upgrade their data analysis strategy in an interactive way. 2) The result when a whole

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Interactive Discussion

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year is modelled differs less than 7 % in the upper 30 cm and up to 16 % in 50 cm depth. Given the ability of the model to describe the 10 months period's measured data correctly, we speculate that a full one-year measurement data set would have shown a corresponding deviation.

Referee 3, RC S773, further comment 12: Yes, SR is out of place with respect to the introducing sentence. But as it is common to include the source term, we have changed this misleading sentence.

Oral communication: We owe thanks to W.J. Massman for drawing our attention to some publications on the analytical soil temperature modelling in nonuniform soils, i.e. soils where thermal properties vary considerably with depth (Nassar and Horton, 1989; Massman, 1993; Karam, 2000). Our present manuscript may lead a reader to the conclusion that any depth array of literature-estimated or lab-determined thermal conductivities and heat capacities could be transferred into a depth-dependent thermal diffusivity via Eq. (B1). This is not the case; thermal diffusivity equals the quotient of these two properties only in uniform soils (as in Fig. 4). The determination and subsequent use of an effective depth-dependent diffusivity from measured temperatures in multiple depths can either be done as described in our manuscript (p 1871, 1874) or as described by Nassar and Horton (1989) if the forward model of thermal diffusion is numerical. Note that the effective diffusivity values will differ between these two cases. For an analytical prediction of thermal behaviour of a nonuniform soil before temperature measurements from different depths are available, the temperature model by Karam (2000) could well be combined with our model. It uses the same layer structure, and allows for any vertical profile of thermal properties. Cases of approximate depth-invariance of thermal properties can be modeled straighforward using Eqns. (B1) and (B3) only.

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