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Biogeosciences Discussions

Interactive comment on "Measurements of soil respiration and simple models dependent on moisture and temperature for an Amazonian southwest tropical forest" *by* F. B. Zanchi et al.

Anonymous Referee #1

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General Comments

The manuscript describes soil respiration measurements from tropical forests over nearly a whole year. Data on soil respiration from tropical regions are still relatively scarce (compared to temperate and boreal regions), and publication of these results would be a valuable addition to the literature. However, as a strictly observational study, this work does not carry a strong message, as there is no hypothesis under test. The conclusions drawn are relatively weak, with general trends and the confirmation that soil temperature and moisture alone are insufficient to model soil respiration in tropical settings. There appear to be problems with the data (see comments on Fig. 4

below), which need to be clarified. I may of course be wrong, but there seem to be dramatic shifts in measured soil respiration values which appear unrelated to environmental conditions. If this is caused by a technical problem, the overall magnitude of fluxes reported are doubtful. In my view, there are a number of flaws in the present manuscript, and I think some revision is necessary before it is publishable.

The spatial representation of the study is relatively poor. Soil respiration is measured from 5 points at one site only, and this is taken as a representative setting for forests and soils in this wider region. This is a fairly bold assumption and requires data or literature citations to sustain it.

The modelling effort receives most attention in this manuscript, but I did not learn all that much from it. I think for the benefit of the reader, the authors should state more clearly in the introduction what the aim of the study is. The entire focus of the analysis is on soil respiration dependence on soil temperature and soil moisture, but the authors acknowledge themselves that other parameters would be required in order to model the soil respiration dynamics (i.e. autotrophic activity and litterfall are critical).

A major problem for the analysis is the close correlation between soil moisture and soil temperature, which made an independent parameterisation of a model using both moisture and temperature practically impossible. With a temperature depth that is inadequate to resolve diurnal soil temperatur fluctuations (see later comment), the modelling of soil respiration at short time scales (diurnal dynamics) is almost precluded from the start. The fact that longer averaging periods provide better fits is not a surprise, as here only broad seasonal variations in drivers and response variable are considered.

The model parameterisation effort uses a very deep soil temperature for reference (15 cm). As a consequence, temperature fluctuations are very modest, and regressions mostly insignificant. The extremely high Q10 results are almost certainly a result of a highly dampened temperature amplitude, which is not relevant to the dynamics of the soil respiration CO2 flux.

The structure of the text could be improved. The results section is lengthy and covers a fair amount of interpretations that should be moved to the discussion. The literature used for referincing this work with is relatively dated - granted, there are a few studies from 2008 and 2009, but otherwise virtually nothing published after 2004. The field of soil respiration has developed rapidly over the past 5 years, particularly as far as autotrophic influences on total soil CO2 efflux and heterotrophic decomposition (e.g. priming) are concerned. Similarly, modelling of soil CO2 efflux has developed from 5 years ago, and there is by now a clear realisation in the modelling community that exponential temperature relationships are too simplistic to represent soil respiration. These developments are so far only poorly reflected in this manuscript and should be improved in a revised version. The language is generally good, but for any future revisions the text should be proof-read by a native English speaker to smooth out places where the expression is not quite clear.

Detailed comments

6151, 1: Please clarify, are the 20 mm the monthly rainfall?

6152, 3: "units", rather than "unities".

6152, 11: If the chambers were left open at 45°, would this not prevent rain from falling into at least part of the chamber?

6152, 20: For the calculation of the molar density of CO2 in the soil surface efflux, it would be necessary to use the air temperature, not the soil temperature at 15 cm depth (i.e. the temperature of the air in the chamber). Even though the error is bound to be very small indeed, it should be stated correctly here!

6153, 16: It is not clear to me why you check for correlations between chambers. What do you do with this information?

6154, 18-27: Move this to the Discussion.

6155, 6-8: This does not seem accurate. According to Fig. 2, there is a rain event

in mid-August, but the response in respiration is far weaker compared to the dramatic increase you show in Fig. 4.

6155, 15-21: Move to Discussion.

6155, 25: Lloyd and Taylor 1996 is not the correct reference for the Reichstein model!

6156, 12: The variable "REW" is not explained.

6157, 2-5: Or rather, this means that temperature models are not appropriate in these systems!

6157, 18/19: "Simple inspection" is fairly arbitrary. The regression curve in Fig. 5b suggests that the maximum soil respiration is at 0.2 m3 m-3. This range also does not match the optimum range you state in the discussion (see comment below).

6157, 25: The soil moisture increments do not match those in the legend of Fig. 6 - please clarify.

6157, 26-6158, 4: Move the comparison with literature results to the discussion.

6158, 4/5: I agree that the very limited temperature range will have caused the high Q10 estimates. In fact, your Q10 values are more of an artefact than a result, and it should be clearly stated that the Q10 values are not true respiration responses to temperature. I don't think that anything can actually be deduce from these values in terms of the ecophysiological response of these soils.

6161, 3-5: This is an over-simplification. To my knowledge, there are no models in use at present that attempt to predict regional or global soil CO2 flux on such simplistic responses - the field has moved on a fair deal in the past 10 years, and your references are a little dated.

6161, 14: In the results, you state a different optimum range - please clarify this!

Fig. 1: Why no error bars for the soil respiration results?

Fig. 4: The soil respiration dynamics look peculiar. There seem to be two levels of respiration between which the system switches: 0 to 5 umol m-2 s-1, or 12-20 umol m-2 s-1. These do not correlate well with the soil moisture values, and rain events seem to cause either switches up or down-wards. Are these quality checked results, or is there a possibility that the results are caused by failure of individual chambers which affect the average, or possible technical issues? What does the vertical hatched line represent? The shift before and after that line, for example, is not (apparently) related to changes in temperature and moisture. The dates in the legends seem wrong, as the graph shows values between 31st August and 30th September. Also the circles in the graph are not explained.

Fig. 5: The temperature dependence is very weak, given the large scatter. Is the regression at all significant? If it is not, you should leave out the regression line.

Fig. 6: The soil moisture categories on the axes, in the legend, and in the text don't match!

Interactive comment on Biogeosciences Discuss., 6, 6147, 2009.