Interactive comment on “Does soil moisture overrule temperature dependency of soil respiration in Mediterranean riparian forests?” by C.-T. Chang et al.

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This MS describes a straightforward experiment conducted to show how the soil moisture regime modulates the temperature response of soil respiration, and its heterotrophic component, which was experimentally separated. The presentation is generally clear, and the interpretation of the results is supported by an analysis of relevant literature. The results are of potential significance for carbon cycle modelling: there has been much written about the temperature response of heterotrophic respiration (even if not all of this has been illuminating!) but far less about the impact of soil moisture, and its interaction with the temperature effect.

I have a few suggestions to improve the MS.

1. It should be made clear at the outset (in the Abstract and Introduction) that the first-order control of soil respiration is net primary production (NPP). This is currently not mentioned, so the MS as it stands potentially could contribute to the unfortunate misunderstanding that direct environmental influences (soil temperature and moisture) are the principal controls on soil respiration. This is even stated as a fact, but it is incorrect. When considering major environmental gradients, the principal control on soil autotrophic respiration is below-ground NPP, and the principal control on soil heterotrophic respiration is total NPP – the ultimate source of all soil organic matter (SOM).
I don’t think that neglect of this fact has much influence on the interpretation of the results presented (because they focus on directly observed temperature and soil responses, over a modest range of NPP), but nonetheless, the background should be stated clearly and accurately, and it should be explained why differences in NPP are considered unimportant for this particular study.

2. Exponential responses to temperature are assumed throughout. I have no problem with this simplification for data-analysis purposes; however, it is incorrect to cite e.g. the Lloyd and Taylor paper as authority for an exponential response, when that paper actually shows a better fit to a modified Arrhenius-type response.

3. At the end of the MS (starting p 8005, line 22) a facile statement is made about soil moisture becoming an increasing issue for “most terrestrial ecosystems”. It is not at all clear that this so, as

precipitation also tends to increase (globally) with warming. It is however true for some regions, notably those with mediterranean-type climates, which are experiencing precipitation declines and projected to experience more of the same in the future. This statement should therefore be amended to be more accurate.

Iain Colin Prentice

Interactive comment on Biogeosciences Discuss., 11, 7991, 2014.

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C.-T. Chang et al.

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We greatly appreciate Professor Iain Colin Prentice for his constructive and helpful suggestions on the manuscript. We are now revising the paper and we will amend more clear statements accordingly. We will submit the final revision immediately after the open discussion period is closed.

Interactive comment on Biogeosciences Discuss., 11, 7991, 2014.

For the second comment, we have applied our data into the modified Arrhenius equation. As it is difficult to define the proper optimum temperature, we found no improvement in using modified Arrhenius equation. Therefore we decided to stay with the original van´t Hoff equation.

Interactive comment on “Does soil moisture overrule temperature dependency of soil respiration in Mediterranean riparian forests?” by C.-T. Chang et al.

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General Comments

Chang et al. present an important metric utilizable by the modelling community, in a simple and careful way. As pointed out in the paper, a soil moisture threshold is likely to be site–specific, making this data from a Mediterranean riparian forest both novel and important. The structure is logical, and the figures are clear and precise. There are a few surprising omissions from the references though (see below).

Specific Comments

It was my understanding that long-term global modelling studies do use a soil moisture correction factor, and do not rely solely on a temperature function to derive heterotrophic soil respiration, as stated on P7993, ln18-19. Whilst I agree that the way in which soil moisture is allowed to influence model output can be improved, it does exist.

In fact, there is some debate on the varied methods used to derive the dynamic soil moisture correction factor- parabolic, linear etc. (see Falloon et al., (2011) for a comparison between different soil moisture-respiration functions). Maybe you mean that, currently, there is no consensus on the ‘best’ soil moisture correction factor, or that as this parameter is calculated in the same way over time (see Mayano et al., 2012), there is no predictive power for those systems where changes in precipitation regimes and infiltration affect biogeochemial cycling?

I don’t agree that the manuscript should be re-written to herald NPP as the primary driver of SR, as opposed to temperature and moisture: NPP is in turn controlled by temperature and moisture, so this manuscript is not wrong in its stance. Fairly arbitrary chicken-and-egg paradigm.

What is the error in the SM data? Is each data point an average of multiple readings?
If so, what is the variation?

How much do the PVC tubes (planted 5months in advance) affect normal infiltration of soil water? (even with the two small, mesh-covered holes)

P7992, ln:25- where is the reference for SR being 60-90% of total ecosystem respiration?

P7993, ln:18-19- examples of studies?

The temperature was measured at the same fixed depth at all sites; was the depth temperature gradient the same amongst the different levels, or is it possible that this could also change, akin to the water table depth? This would have implications for the current interpretations.

Technical Corrections
P7995, ln:14- ‘acidic’, rather than ‘acid’.
P8000, ln:12- change ‘till’ to ‘to’ or ‘until’.
P8000, ln:23- change to ‘sharp increase’.
P8002, ln:18- Do you mean ‘the inhibition of diffusion’?
P8003, ln:7- ‘total’ is redundant in this sentence.
P8003, ln:Title- ‘confounding’ should be in the ‘confounded’ tense.

References
Falloon, P., Jones, C.D., Ades, M. and Paul, K. Direct soil moisture controls of the future global soil carbon changes: An important source of uncertainty, Global Biogeochemical Cycles, 25, 2001.
Moyano, F.E., Vasilyeva, N. Bouckaert, L. et al. The moisture response of soil heterotrophic respiration : interaction with soil properties, Biogeosciences, 9, 1173-1182,
2012.

Interactive comment on Biogeosciences Discuss., 11, 7991, 2014.

Interactive comment on “Does soil moisture overrule temperature dependency of soil respiration in Mediterranean riparian forests?” by C.-T. Chang et al.

C.-T. Chang et al.

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We would like to thank Dr. Berridge for the valuable comments. Our responses (AC) to the comments (Ref) are given below.

Ref: It was my understanding that long-term global modelling studies do use a soil moisture correction factor, and do not rely solely on a temperature function to derive heterotrophic soil respiration, as stated on P7993, ln18-19. Whilst I agree that the way in which soil moisture is allowed to influence model output can be improved, it does exist. In fact, there is some debate on the varied methods used to derive the dynamic soil moisture correction factor- parabolic, linear etc. (see Falloon et al., (2011) for a comparison between different soil moisture-respiration functions). Maybe you mean that, currently, there is no consensus on the ‘best’ soil moisture correction factor, or that as this parameter is calculated in the same way over time (see Mayano et al., 2012), there is no predictive power for those systems where changes in precipitation regimes and infiltration affect biogeochemial cycling?
(AC): Thank you for the correction and references, we have rewritten this part.

Ref: What is the error in the SM data? Is each data point an average of multiple readings? If so, what is the variation?
(AC): The error in the SM data is from the three data points of several days reading in each season.

Ref: How much do the PVC tubes (planted 5months in advance) affect normal infiltration of soil water? (even with the two small, mesh-covered holes)
(AC): The highest record of the soil water table level at L1 was 46cm, which was still deeper than the depth of PVC tube, therefore, we assumed that the PVC would only affect the infiltration very weakly.

Ref: P7992, ln:25- where is the reference for SR being 60-90% of total ecosystem respiration?
(AC): We have added the references.

Ref: P7993, ln:18-19- examples of studies?
(AC): We have rewritten this part.

Ref: The temperature was measured at the same fixed depth at all sites; was the depth temperature gradient the same amongst the different levels, or is it possible that this could also change, akin to the water table depth? This would have implications for the current interpretations.
(AC): Unfortunately, we don’t have soil temperature data from different depths. However, we did measure the 30cm integral soil temperature and found very slight differences amongst levels, which might imply that the depth temperature gradient amongst levels, at least within 30cm depth, was very similar.

Ref: Technical Corrections (AC): Corrected.

Interactive comment on Biogeosciences Discuss., 11, 7991, 2014.