

## ***Interactive comment on “Interdependencies between temperature and moisture sensitivities of CO<sub>2</sub> emissions in European land ecosystems” by C. Gritsch et al.***

### **Anonymous Referee #2**

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#### General comments

This paper presents a valuable dataset on the temperature and soil moisture dependencies of soil CO<sub>2</sub> efflux from heterotrophic respiration from a range of field sites across Europe located in different climate zones and subject to different land use. The soil CO<sub>2</sub> efflux at the different temperature (5–25°C) and moisture levels (5, 20, 40, 60, 80 and in one case (peatland) also 100%) was determined in a central laboratory incubation experiment with intact soil cores, which had been collected before the measurements at the different field sites when soil temperatures had reached 8°C (one-week average) in spring of 2012. The data were used to derive temperature and moisture dependencies of soil CO<sub>2</sub> efflux as well as moisture dependencies of temperature

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sensitivities of soil CO<sub>2</sub> efflux and temperature dependencies of moisture sensitivities of soil CO<sub>2</sub> efflux. The former (temperature and moisture dependencies of soil CO<sub>2</sub> efflux) is not new, and the data presented in this paper are in accordance with the literature. However, the data on the moisture dependency of the temperature sensitivity of soil CO<sub>2</sub> efflux and on the temperature dependency of the moisture sensitivity of soil CO<sub>2</sub> efflux are new or at least scarce enough to merit publication. This information is crucial for a more realistic and reliable simulation of the combined effect of soil temperature and moisture change on heterotrophic soil respiration. The paper is generally well written, the experimental design is adequate and the experimental data are statistically sound and properly presented. There are some minor issues mentioned in the specific comments below, also with respect to interpretation of some of the data, which should be addressed before I could finally recommend acceptance of the paper. Furthermore, I have made a number of corrections and suggestions in an annotated pdf file, that should also be taken into account when creating a revised version of this paper.

#### Specific comments

p. 4434, l. 25: What do you mean with “prospective effect”?

p. 4437, ll. 6-8: “. . .expect a positive relationship between temperature sensitivity and moisture content due to the assumption that the effects of soil temperature and moisture are negatively correlated.” I could not follow the reasoning. Isn't it contradictory? Doesn't the assumption that the effects of soil temperature and soil moisture are negatively correlated (that is, for example, if the temperature effect ON a process is high, the moisture effect is low, and vice versa) mean that the temperature sensitivity OF that process is also negatively correlated with soil moisture (i.e. high temperature sensitivity at low soil moisture content, and vice versa)? Or maybe I just didn't get it, at least the wording is complicated then.

p. 4437, ll. 8-9: “Thus, soil moisture would be positively correlated with the temperature

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sensitivity of soil respiration.” This sentence could be deleted, as it conveys the same message as the previous sentence and further complicates the matter as now the variables (independent/dependent) have been switched.

p. 4437, ll. 10-13: The comparison between soil moisture-related and precipitation-related Q10 data is not fully valid, as annual precipitation does not directly translate into soil moisture. You should mention that point here.

p. 4437, ll. 14-18: Provide some more information about that case study. Without it, the reader does not know how Curiel Yuste et al. did the decoupling and where the large differences in Q10 came from.

p. 4437, ll. 19-20: “Peng et al. (2009) even found differences...“ In view of what was stated in the previous sentence, this is not surprising (i.e., the word “even” can be omitted). If you mention this study and the fact that they found differences in Q10 between ecosystem types, you should provide some more information on the range of Q10 found and the types of ecosystems analysed.

p. 4438, ll. 2-4: “Laboratory incubations provide the best and least biased basis for estimating the temperature dependence of SOM decomposition...“ In the natural soil environment of vegetated ecosystems, the priming effect of root exudates on SOM decomposition can play an important role in SOM decomposition and hence on its temperature sensitivity. This should be regarded in the statement here.

p. 4438, l. 23: ÉCLAIRE: provide some basic information on the aims of this project/network for the uninformed reader.

p. 4439, l. 12: “stored at 4 °C”: give some information about storage time before analysis.

p. 4440, l. 7 & l. 10: Give concentrations of KCl and CaCl<sub>2</sub> solutions.

p. 4440, l. 11: ÖN1998: Provide information or delete.

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p. 4441, ll. 3-5: “To investigate how moisture content, temperature and land use influence moisture sensitivity relative CO<sub>2</sub> values (relative to the CO<sub>2</sub> efflux of the lowest moisture content) were calculated to exclude the temperature contribution from the absolute CO<sub>2</sub> values.” This sentence is hard to understand properly. Please rephrase.

p. 4442, ll. 15-18: “As a result mean values of CO<sub>2</sub> fluxes for each of the five temperatures per moisture content and site were calculated to fit the equation. When taking all samples R2 ranged between 0.019 (UA-Pet; 6% WFPS) and 0.958 (NL-Spe; 30% WFPS) due to the variability between soil cores.” Again, these two sentences are hard to understand because it is not really clear what has been done. Why were mean values of CO<sub>2</sub> fluxes of the single soil cores calculated “to fit the equation”? And what exactly did you do with the data from all samples? And why is the moisture level 6% for UA-Pet, I thought the lowest soil moisture level tested was 5% WFPS? Or is this just due to the uncertainty of soil moisture adjustment?

p. 4442, ll. 20-21: “Table 3 shows Q<sub>10</sub> at the lowest and highest temperatures investigated, 5 and 15 °C.” According to the Abstract and the Materials and Methods section, the highest temperature investigated was 25°C. Please clarify or correct.

p. 4442, ll. 25-27: Again, are the odd moisture levels due to the uncertainty of soil moisture adjustment? If so, this should be mentioned in the Materials and Methods section.

p. 4445, ll. 2-3: “. . .for the whole temperature range between 5–15 °C. . .”: Again, the whole temperature range investigated was 5–25°C.

p. 4445, ll. 24-28: Again, check this statement for consistency (like for p. 4437, ll. 6-8).

p. 4445, l. 24 - p. 4446, l. 3: This is more a less a repetition of the introduction. You should decide where to place that information more prominently, but try to avoid literal repetition.

p. 4445, ll. 12-16: First of all, again a sentence very similar to the introduction, and like

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in the introduction you don't provide any information on how they managed to decouple the pure temperature sensitivity from more general seasonal influences. And you don't give info about the ecosystem for which they did that.

p. 4447, ll. 14-15: "We couldn't see any obvious trends of Q10 values among land uses which is in agreement with Wu et al. (2010)." But is that not in contradiction to your statement in the previous paragraph pertaining to the differences in recalcitrance between the different sites, and with recalcitrance being one of the controls of temperature sensitivity of SOM decomposition?

p. 4447, ll. 15-17: "Peng et al. (2009) found differences of Q10 values among ecosystem types but did compare field measurements and different temperatures which both result in different Q10 values. . ." Again hard to get the meaning, please try to reword.

p. 4447, ll. 24-26: "Our results show that significant moisture effects ( $P < 0.05$ ) occurred only at higher temperatures which is in agreement with other studies. . .": But isn't that in disagreement with the statement "that the effects of soil temperature and moisture are negatively correlated" (p. 4437, ll. 7-8 & p. 4445, l. 26)?

p. 4449, ll. 4-6: ". . .indicating an effect of rain events in cold areas on temperature sensitivity." This statement should be made more specific.

#### Technical corrections

See annotated pdf file for detailed corrections and suggestions. Use of abbreviations: moisture sensitivity has been defined as MS, but this acronym has been used only occasionally. In most cases the full term moisture sensitivity has been used. In contrast, for temperature sensitivity no acronym has been defined and used. Please be consistent and use an acronym for both and use it always after definition at first use, or don't use acronyms at all.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/12/C5262/2015/bgd-12-C5262-2015-supplement.pdf>

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