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The review of PhD thesis:
CO_2 efflux in different types of ecosystems
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This review was prepared after letter of appointment from prof. Dr. Ing. Petr Horacek, Dean of the Faculty of Forestry and Wood Technology, Mendel University of Brno.

The exchange of CO_2 between terrestrial ecosystems and the atmosphere is one of the very important problems from the point of view of global carbon balance, especially in the context of climate change. This thesis fits very well to scientific interest of modern ecology, biology, forestry, soil science and global change.

The thesis consists of 6 main chapters:

Introduction,

Aims,

General materials and methods,

Individual experiments design, results and discussion,

Conclusions.

GENERAL REMARKS AND DETAILED COMMENTS TO THE RELEVANT CHAPTERS

Chapter: Aims and Background chapters

This section seems to be clear and all needed basic information, which are necessary to understand the dissertation content are well described. However, one may have an impression that still too less information is given in relation to ecosystem scale soil respiration assessment (based on this the discussion of results should be made). In chapter 3.6 "Grassland" - there is some information about this type of ecosystems and problems related to measurements of CO_2 effluxes. One can feel unsatisfied that there is no description how CO_2 effluxes could differ, or what are the main driving variables, spatial and temporal heterogeneity of CO2 effluxes at other ecosystem types (wetland, forest) investigated in the dissertation. I would expect here some examples of papers where these measurements are described and a table with comparisons of results from similar sites at the same ecosystem types would be very welcome in this section.

Page 8 – It is very nice that all chamber types used for greenhouse gases (GHG) exchange measurements are described, but in the description of closed static chambers PhD candidate should mention not only about chamber based on chemical traps, but also about chambers where the air samples are taken for GC analyses. This issue could me more clear here and more precise.

There are also some detail remarks in this chapter:

Page 2, 2nd paragraph: should be „respiration" instead of „reparation"

Page 2, 2nd paragraph: should be "taken into account" instead of "taken account"

Page 9: "Manual system": as important disadvantage of chamber measurements technique, the needed human power should be mentioned

Page 11, fig 3: it would be better to name y-axis as CO_2 efflux, instead of moisture response

Page 12, 2nd paragraph: should be "which is accumulated" instead of "which accumulated"

Chapter: Materials and methods

The dissertation presents typical experimental work and this chapter should be very precisely described. There are some parts, which, in my opinion, should be added to the description of methodology and sensors used during field measurements.

Pages 19-25. All measuring chamber systems used at different ecosystems to estimate the CO_2 effluxes are described here, which is good. However, description of the measuring systems is not precise enough and not homogenous. From the description it is obvious how the chamber systems were operated (e.g. what was the closure time and how many measurements were taken to calculate fluxes) and what kinds of gas analyzers were used. But although this description is similar in case of automated SAMTOC and SAMTOL systems, in case of ACSEM, this is different. From the description in the text it is not obvious at which sites those systems were applied. Only from photographs one may guess that SAMTOC was applied in spruce forest, SAMTOL in grassland, while ACSEM in beech forest. Also only from photographs one may guess that manual portable systems were used in beech forest and wetland sites. This information should be provided for the reader in this paragraph.

It is not obvious, how the chambers were equipped and therefore questions as follow can arrive to the reader:

- Was there a fan installed in the chamber to mix the chamber headspace? (if yes, what was the flow rate, was there a vent to equilibrate a pressure – how it was designed and installed?),
- Where the chambers tested against leakages?
- Why there was applied a protection net above the ACSEM system (Fig. 12) and how it can influence measurements if a litter was artificially reduced by this procedure?
- What was a collar insertion depth in case of ACSEM system?
- How CO_2 fluxes were calculated – based on linear on exponential functions?

In fact, there are references where the reader can find all the answers: *(Pavelka et al. 2004 &2007 & Pavelka 2009 to support a description of SAMTOC system and Pavelka 2009 in case of SAMTOL, no citations in case of ACSEM)*, but from my point of view, although this solution is acceptable in scientific papers, in a PhD dissertation, the description of measuring systems should be precise and detailed enough to understand the whole measuring procedure. This is especially important that although the dissertation is in English, the Pavelka 2009 is available only in Czech. Only in case of manual systems the procedures how the fluxes are calculated are presented although this information can be found in the LI6200 manual.

Furthermore, in the METHODS paragraph there are missing information and therefore, some issues need explanation:

- How the data were processed and analysed?

- How the fluxes were calculated and how the average flux was calculated? (as in average of 8 repetition in SAMTOC, three in SAMTOL and how many repetitions were considered in case of the other systems?)
- Which quality criteria were applied to filtering wrong fluxes?
- How the cumulated fluxes were calculated?
- How nighttime periods were identified? (some radiation threshold were considered?)

Beside above, one can have also methodological concerns related to the chamber measurements. SAMTOC system was operated 24-hours, at least what can be understood, but SAMTOL system was operated automatically only during the nighttime periods. It is not obvious what was the operation routine of the ACSEM system. The chamber, although similar in shape and constructions have different dimensions/volume (height of SAMTOL chamber is three times bigger than in SAMTOC). Are the nighttime measurements conducted at grassland sites comparable to those measured in forest if we may consider that nighttime chamber measurements of CO_2 fluxes are highly overestimated because of disturbance of highly stratified air layers during the stable atmospheric conditions?

There are also some detail remarks in this chapter:

The vocabulary is not unified. The soil frames are called as "bases" in case on SAMTOL system, and collar in case of portable manual system.

Page 16: what is the difference between mowing and cutting

Page 16: instead of "age of the stand was 108 years in 2011" there should be "age of the stand is 111 years"

Page 17: information about depth of soil in beech forest could be added.

Page 24: in the equation [1] time "t" is missing – it exists the description of symbols used in the equation

Chapter: Individual experiments design, results and discussion

Chapter 5.1.1 (pages 26-27) and 5.2.3. (pages 45-47) should be related/moved to the methods paragraph – some information missing in this previous section (which I pointed out above) are written just here.

Page 30: there is 4 t ha^{-2} , which is wrong: h^{-1} and the unit is wrong (see Tab 10 referred in this sentence – there is µmol m^{-2} s^{-1})

Page 33, Fig. 18. It is not clear which dots (blue or red) refer to CO_2 effluxes before and after rain

Page 34 (Grassland) – there is written that both soil and aboveground biomass CO_2 fluxes were measured. It is not clear enough, as no word was written before that autotrophic respiration of aboveground plants was somehow estimated. If yes, how? Here, the SAMTOL system was applied and the system consists of only 3 chambers. Hence, I cannot imagine, how these two fluxes: soil respiration (heterotrophic + autotrophic respiration of roots) and autotrophic respiration of aboveground biomass were measured separately. I suppose that the measured flux was more related to ecosystem respiration than to soil CO_2 efflux itself, hence it would be difficult to compare those fluxes between different ecosystems. (just at the last paragraph at page 36 there is mentioned about "ecosystem CO_2 efflux" – before one may think that soil CO_2 efflux was measured – this is not very precise.

Beside the above, it is hard to compare measured fluxes as at spruce forest site the measurements were conducted the whole day, while in grassland only nighttime sums of CO_2 fluxes were calculated. What was the reason?

Page 35, table 11 and 12, should be "sums" and not "suma".

In tab. 9-10, the sums of CO_2 effluxes are estimated for 24-hours periods, while in tab. 11-12 only for nighttime periods, however it is not clear from these descriptions. In Tab. 12 there is given the average air temperature (crucial for explanation of the measured ecosystem CO_2 efflux), which is not available for the whole datasets. Description of graphs and tables should be clear and more precise.

Page 43: there is "regression coefficient", it should be "determination coefficient".

Page 43: there is "lower than in 2012", it should be "lower than in 2010".

A lot of methodological details are described in "Spatial heterogeneity" paragraph. They should be moved to methods section. There is a question: Why soil moisture was measured at different depths at the analysed ecosystems (spruce and beech forests, grassland, wetland)?

In grassland, the grass height was measured (as indicator of grass biomass) and was related to the measured fluxes. Why LAI measurements were not conducted? I believe that they will better represent the biomass and better correlate to measured fluxes.

Page 46: 1st line, R10 is calculated not measured.

Page 46: two different symbols are used for standard deviation (1st line and 13th line).

Page 47: there is "in the beach forest", it should be "in the beech forest".

Page 47: there is "The litter thickness of the litter", it should be "The thickness of the litter".

Fig 24 and 26 and 27: why on one figure a coefficient of determination (r2) while on the other coefficient of correlation (r) is used? The figures present the same relationship, but different statistical measures are used to describe those relationships.

Page 49: 3rd line, there is " and A and C", should be "and C".

Page 52 (Wetland), I have doubts related to soil water content (SWC) measurements. From Fig. 29 it is clear that SWC ranged between 65% to 97%. However, at the beginning of measuring period the GWD was +0,4 cm, which means that soil/peat has to be saturated with water. The same should refer to the last day of measurements when after the rain the WTD increased to +4.2 cm. Maybe the measurements conducted with ThetaProbe (delta-T) are biased because this sensor is calibrated for mineral soils?

Page 53: last line, there is "correlation coefficient 0,97", it should be "determination coefficient 0,95" (according to Fig.33)

Page 54: Fig.30, there is "Water table/Temperature", it should be "Water table and Temperature".

Page 54: Fig.31, in the figure equation there is: -10,34 but in the equation [10] at page 53 there is +10,34. Besides, the equation shows 3 parameters: DWT, Q10 and Ts but the picture shows only dependence of DWT on R – this requires explanation.

Page 56: Fig.34, there is "as stated in Tab.12", it should be "as stated in Tab.13".

Page 57: Fig.35, there is "ransom selection", it should be "random selection".

Page 59: there is "1,41 micro CO_2", it should be "1,41 µmol CO_2".

Page 60: last line, there is "1,0m", it should be "0,1m".

Page 61: why not to consider the same length of analysed period as the one considered for assessment of temporal changes of CO_2 fluxes (1st May till 12 of October), while in this paragraph (1st May till 11 of October - maybe it should be 12 of October)?

Chapter 5.3.1. (Pages 61-62) should be a part of method section.

It is not clear why the analyses described in 5.3.1. chapter where performed only for 2008-2009 when data for 2008-2012 were available for this site.

Page 62: there is equation number [8], in consequence order it should be [11] but it is the same equation like [10] at page 53 and [6] at page 27 – it can be just referred to the previous equation.

Page 62: the same remark like on page 43, there is "regression coefficient", it should be "determination coefficient".

Page 63: there is "were lower (Tab.26), but Tab.26 does not exist.

Page 63: there is "7,5 t " but in Table 14 there is 7,2.

Page 64: Tab.14 description, there is "regression coefficient", it should be "determination coefficient".

Again, in the "Drought experiment" paragraph, the first part should be moved to methods paragraph (pages 69-70)

Page 70: first line, there is "Tab.26" but Tab.26 does not exist.

Page 75: 4th line: "difference of CO_2 efflux in the dry variant from CO_2 efflux in the dry variant"... should be "the wet variant".

Page 75: in the text, wrong reference to figures, should be 45, instead of 44, while on the next page 46 instead of 45.

Page 75: Fig.45, for comparison the scales on both figures must be the same and there is "from -20 to 60 and from -20 to 40", additional there is a double date on OX axis "30.07".

Some doubts could be related to designing of the drought experiment – permanently installed roofs changed not only the amount of rainfall reaching the grassland surface, but also the radiation and energy balance of the active surface. The curtains were installed relatively close to the surface (unfortunately it is not written in the text at which height, we may guess only based on photos), but it induced also disturbances in the air movement/mass exchange processes, hence more critical assessment of the results should be conducted, as it seems that other factors which might bias the estimated fluxes, should be considered.

In this paragraph again there is some misunderstanding – which fluxes are measured - there is written about soil CO_2 efflux, whereas ecosystem respiration was measured (not only soil respiration).

Chapter: Conclusions

In PhD dissertation, I rather prefer conclusions structure in separate points, but in this thesis the author used another way and wrote the conclusion as a text similar to the discussion. This solution is of course acceptable and I have to add that all information contained in this section are reasonable and are supported by the research work and the results of this doctorate.

GENERAL CONCLUSION OF THE REWIEV

Even if this PhD thesis has a lot of inexactness and few mistakes (described in details in previous chapter of the review) I think that because of significant benefits and interest finding, I would like to evaluate this PhD thesis as a good job, which proves that the author has chosen appropriate direction of scientific development and is moving toward being an expert in the subject of CO_2 exchange between ecosystems and the atmosphere. The set of references in the dissertation is large (over 130 papers) and gives the impression that the author is well versed in the scientific problems associated with measurements of CO_2 fluxes.

In a synthetic way the main benefits and interesting results are given below.

Benefits and interested findings:

1) It is very worthy that the analysed series of CO_2 effluxes (for spruce forest) were separated/divided for two subsets: before and after rain, and analysed separately

in order to determine Q_{10} values. PhD candidate indicated that wrongly estimated Q_{10} (based on the whole dataset may lead to underestimation of temperature sensitivity of soil CO_2 fluxes, as well as modelled effluxes, in the period after rain and overestimate in the period before rain.

2) Indication that CO_2 effluxes at forest sites depends not only on temperature, but also on soil moisture, and at grassland the respiration processes are driven mainly by temperature
3) Indications that on wetland site the CO_2 effluxes are highly dependent not only on temperature but also on fluctuation of WTD. Development of single empirical model for CO_2 estimation based on T and WTD.
4) Indication the minimum number of positions/repetitions for each ecosystem types which are necessary for estimation of CO_2 effluxes with the maximum deviation of 5%
5) Indication that a litter layer may have pronounced effect on CO_2 effluxes at beech forest, because it accumulates water after rain and induce respiration processes from the surface organic layer
6) Indication that on wetland site, the CO_2 fluxes are negatively correlated with WTD, and that the most respiratory active layer of peat is in 0-3 cm.
7) Indication that the time of the day when the fluxes are measured may have significant impact on calculated values of Q_{10}, and then on seasonal modelled fluxes, if measurements are not performed continuously the whole day and night.
8) Indication that changing distribution of rainfall with a drought in the first part of the growing season may have significant long term effect on ecosystem CO_2 efflux from the grassland ecosystem

My general opinion about this reviewed thesis is positive, even I think that the general idea of this work is a bit too bright. The mass exchange between ecosystem and the atmosphere is a very complicated process, which depends on so many factors that I would prefer a work, which focuses on less problems then in this thesis. From the other side, I appreciate the very ambitious way to study the CO_2 exchange processes on so different ecosystem types. All results shown in this thesis are important for the further development of CO_2 fluxes measurements by Chamber technique.

It is well known that it is extremely difficult to establish a common procedure of measurement and common construction of the used chambers. There are very many research groups working on CO_2 fluxes by the use of chamber techniques and the Czech team is one of the leading research groups in Europe working on this subject. This reviewed dissertation from one side shows how complicated the measurements of CO_2 fluxes can be and on how many parameters the fluxes depend. From the other hand, this dissertation shows solutions how to adopt the measurement systems for different ecosystems and changing conditions (weather, water table etc.). From this point of view this dissertation has a significant input to further development of chamber technique and may have important participation for practice of CO_2 fluxes measurements. I assume that it can have a significant contribution to design sufficient protocol of CO_2 flux measurements.

Even the author made some mistakes, not crucial for the whole work, I am sure that during her PhD studies she significantly developed her knowledge as a young researcher and will be in the future an expert of biometeorology. In the review there were a lot of my comments and remarks, they can be considered and the text can be changed before a scientific publication. A significant number of benefits and findings connected with this thesis urge me to recommend for the next steps of PhD procedure.

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