Interactive comment on “Soil respiration in a fire scar chronosequence of Canadian boreal jack pine forest” by D. R. Smith et al.

Anonymous Referee #2

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General comments

The authors present statistical analysis of soil respiration measurements in fire scars with different time since the last fire. Soil state variables temperature and water content were determined simultaneously as well as organic carbon content. Those co-variables were used to normalize and compute average soil respiration fluxes for the scars differing in their fire history.

The manuscript is not written in a precise or well-structured style. The introduction contains a lot of information, which is not really linked closely to this study. A clear goal or rationale is missing in the introduction. The results section contains information, which should have been given in the methods section. Parts of the discussion are simply repeating results. The discussion and the conclusions are rather weak. There is a lot of redundancy in the manuscript and using such a lot of abbreviations makes it hard to read and understand the text.

However, there are also major methodological concerns: (i) on the data handling in the pilot study (ii) on the method used to estimate the number of samples required (iii) on the handling of the soil water contents expressed as water filled pore space (iv) on how organic carbon content was used to normalise soil respiration (v) on not using collars for the first field campaign (vi) on using 3 samples for a two parameter model to link ‘time since fire’ to mean soil respiration. All the methodological concerns are explained in detail in the ‘specific comments’.

Specific comments

8726 9 and 13 This sounds contradictory: "was adjusted for ... and soil moisture" and on the other hand you state "... but no effect of Ms on Rs..." 8726 23-26 The last sentence of the abstract usually contains the most important conclusions. What is stated here is very general. The statement that more detailed measurements are required is rather trivial. Much stronger conclusions should be given here. 8728 11-23 This section should be skipped. There is no direct link to this study. 8729 17-19 This is a lot of references. I suggest to select the 2 or 3 most relevant. 8729 21 Only here a goal is mentioned. I suggest to give at least two precisely formulated goals, which you can refer to in the conclusions. 8729 27-29 Skip; this is weak. 8730 7 I suggest to skip figures 2 to 4. 8730 12-24 This is not really relevant, please shorten significantly 8731 23 skip "voltage" 8731 25 How many many measurements were averaged for water content? Or was the Theta probe only injected at one location for every collar? 8731 25-28 please replace with: "Measured soil water content (cm3/cm3) was normalised with the soil porosity (cm3/cm3) to determine effective saturation (water filled pore space)." 8732 9 You do not clip the grass to minimize autrophic respiration. You intend to measure ecosystem soil respiration, why should you want to exclude autotrophic respiration close to the surface? 8732 10 This does not seem like a sound method to select...
measurement locations. Especially against the background of knowing that there is a
deterministic spatial pattern of respiration in forests, see Fang et al., 1998, Plant and
Soil 205, 135-146. 8733 2 How much variability in Rs is lost due to the adjustment
according to soil temperature? And is the number of samples required still represen-
tative? Because you already removed variability in Rs, which is actually apparent
in the field measurements. You measure Rs not Rs adjusted for temperature. I do not
think this is the most realistic estimate. 8733 8 This approach of Steele and Torrie is
rather old-fashioned and very simple. In statistics there are more sophisticated and
more reliable methods available like for example the Jackknife method, see Confalonieri
et al., 2009, Field Crops Research 113(2), 125-130. 8734 5-6 Only ten collars per fire
scar? According to table 2 you accept an error of almost 30%! 8734 19 This method
of estimating effective saturation (called "volumetric pore moisture") is not appropriate.
Using a constant porosity of 0.38, just because it is a sandy soil, is not adequate. There
is a variability is porosity you have to account for. This is probably also the reason for
the extremely weak correlation between respiration and effective saturation presented
later on. This issue, however, could be solved: You can estimate the soil porosity in de-
pendence of the soil bulk density Bd (g/cm3) you also measured using: Ps=1-Bd/2.65
where 2.65 g/cm3 is the density of the solid phase (quartz mineral). Since you also
measured the organic carbon content Corg (g/g) you should also account for that by:
Pn=1-Bd/(Corg*0.224+(1-Corg)*2.65) where 0.224 g/cm3 is the bulk density of organic
matter (Rawls, 1983, Soil Science 135(2), 123-125). The effective saturation is then
calculated according to Se=theta/Ps. Let me give an example: Assuming a soil bulk
density of 1.5 g/cm3 and an organic carbon content of 0.05 g/g would yield a porosity
of 0.4068. This has to be available for every location you would like to give Se. An
alternative would simply be to use the volumetric water content directly, instead of Se.
8735 4 You have to give the units of the variables. Is Cs given in g/cm3? 8735 16 What
is the unit of Rsc? Rs is given as g C/m^2/s; Cs is g C/m^3? Then Rsc would be given
in m/s. This makes no sense. 1/s would make sense, but then Cs must be given as
g C/m2, which means you have to relate you organic carbon content to a certain soil
depth!? This should be explained in detail. Is there a reference for this normalisation
of Rs using Cs? 8735 16 please give R2 and RMSE of the fit. I wonder whether the
RMSE is larger than the differences between the scars. 8735 22 Eq. 8 should be
skipped, it is identical to Eq. 7 8736 1 Skip; Q10 and T0 are already given in 8735 16
8736 2-5 Now, the reader is completely confused. But Ms has no effect on Rs, see
8738 3! 8737 14 ok, but you only use 10 samples (see 8734 5), equivalent to ~30%
error (Table 2)! 8737 19 df could be omitted; giving N is sufficient 8737 20 here the
Q10 of 2.21 is mentioned for the third time, please skip 8738 17-8739 17 This section
is completely overloaded with abbreviations 8739 18-20 Y ou fit a 2 parameter model
to 2 degrees of freedom. From a statistical point of view this is disputable. From a
scientific point of view I reject this analyses. There should be at least one more degree
of freedom than parameters, not the same number. Particularly for that small numbers
of N. Everything else is dubious. 8740 8-9 "where soil collars were not used"; this
information should have been given in the methods section 8740 17-19 In that case
your measurement method of FC1 is not valid! You could not compare or pool it with
data of FC2. The measurements during FC1 are inappropriate. 8742 I suggest to put
that on context to required sample sizes for other ecosystems like, e.g. Confalonieri
et al., 2009, Field Crops Research 113(2), 125-130 and Herbst et al., 2009, Vadose
Zone Journal 8, 762-771 8742 16-20 Then FC1 could not be compared to FC2, see above! 8746
1-3 If that is the reason for the increase in Rs in time after the fire it should be much
more stressed in your manuscript. 8748 4 The number of replicates is clearly too small
This are not conclusions. This is just what was done. Even if you believe in that, you could not conclude that from this study. An R2 of 0.999 from three data is not convincing. skip, this is trivial please skip the "Future research" section. This is not relevant in a conclusion section. You better give a figure with the Q01 fit.

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