

Interactive comment on “Small scale spatial heterogeneity of soil respiration in an old growth temperate deciduous forest” by A. Jordan et al.

Anonymous Referee #1

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General comments The authors describe an attempt to characterise the spatial pattern in soil respiration using repeated measurements with a closed chamber system. Generally, the manuscript is well-structured. However, there are many deficiencies. The overall scientific construction of the manuscript is weak. The two hypotheses mentioned in the introduction are rather common and are basically already known from literature. Relevant literature like e.g. the paper of Kosugi et al. is missing. There are major methodological concerns. Basic principles of geostatistics are not fully understood by the authors and are thus applied in a strange way. Terminology is used falsely. Many relevant steps in the data handling are only roughly described or completely missing. Results are discussed, which were not presented properly in the results section, see 'specific comments'. The written English does clearly not meet international publication standards. I made some suggestions. Since I am not a native speaker, too, I suggest

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some serious editing by a native speaker.

Specific comments

9978 2 the abstract should not begin with "large scale spatial heterogeneity" when the focus is on small scale heterogeneity 9978 3 replace "comparably little" with "less" 9978 4-6 but all these factors are not focus of this manuscript. 9978 7 how could "heterogeneity" be "evaluated", better use "investigated"; skip "To do so," 9978 21 the term "extrapolation" is falsely used throughout the whole manuscript. "interpolation" would have been the correct term. 9978 26 replace "permits" with "allows" 9978 27 replace "reliable" with "reliably"

9979 12 there are much more and newer references like e.g. Rayment and Jarvis 2000, Soil Biol Biochem., Herbst et al. 2009, Vadose Zone Journal 9979 13 replace "high" with "large"; replace "total annual values of site scale" with "annual averages of site-specific" 9979 18 give a reference to soil organic matter content, for example Fang et al. 1998, Plant and Soil 9979 21-22 I do not agree to this statement. "Spatial heterogeneity" describes any variability in space not just between sites. 9979 27 "extremely rare" I strongly disagree. There is a number of relevant publications, which are obviously unknown to the authors.: Kosugi et al. 2007, Agricultural and Forest Meteorology, Fang et al., 1998, Plant and Soil, Xu & Qi 2001, Global Change Biology.... Further, there are references cited within this manuscript explicitly dealing with the spatial heterogeneity in old forests like Saiz et al. (2006) and Soe & Buchmann (2005).

9980 5 skip "Having said this," 9980 5-6 but this is not investigated within this study. You just work univariate, only for respiration you try to investigate spatial autocorrelation. 9980 this is impossible. If I get it correctly, you want to improve the average by using kriging? Kriging always reproduces the average of the sampling values. That is one of the main features of kriging, best linear unbiased estimator (BLUE).... Look for it in a text book on geostatistics, e.g. the one written by M. Armstrong, Basic Linear Geostatistics 9980 14-19 These are rather weak hypotheses, which are known from literature 9980

23-25 please skip this sentence, not relevant

9981 measurement locations should not be called "plots". This is simply inappropriate wording. 9981 6-23 I do not understand why you used 43 random locations (in a grid) and additionally 81 completely randomized locations. There is a bunch of literature how sampling schemes should be designed, especially for geostatistical purposes. I am pretty sure the nested approach of Oliver and Webster 1987, J. Soil Sci. would have been much more efficient. In particular against the background that autocorrelation at different spatial scales should be investigated. Small sampling distances are only given for two cells of the grid. And you can never tell whether their small-scale variability is representative for the whole site.

9982 9 skip that sentence 9982 14 "were seen as reliable..." was that a question? Why do you compare measurement devices here? 9982 21 But those sensors are only available at one point in space!

9983 1 How did you convert the gravimetric water content into volumetric, required in Eq. 3? You would have needed bulk density (g/cm³) measurements at every sampling location. Was that the case? 9983 17 please give the units of k and a 9983 24 In Eq. 3 you assume an exponential relationship between respiration and soil water content, similar to the arrhenius approach. I understand, that makes it easier. However, first you have to prove this functional relation. Plot the residuals of the fit of Eq. 1 against water content...

9984 21 was the standard deviation computed from R_{standard} or from $R_{\text{deltaTdelta_theta}}$? However, was no skewness detectable in the data? Kolmogoroff-Smirnoff...., see above in the manuscript. That would significantly hamper the usefulness of Eq. 6 as it is. Usually a log-transform solves that problem.

9985 3 replace "construction" with "determination" 9985 4-6 What was the criterion for this optimisation? 9985 13 "Fig. 6" The figure numbering should be in accordance to the appearance in the text. So, this should be Fig. 2 I guess. 9985 19-21 Show plots

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of the regressions, R^2 and so on.... 9985 25-26 Again, kriging always reproduces the average of the sampling points....

9986 8 skip the "x" in the units 9986 14 $Q_{10}=2.3$, Q_{10} amounts to 3.9???? What is true now? 9986 20-21 "at a given measurement date"? I suggest to compute coefficients of variation (=mean/standard deviation) and to investigate this systematically for every single measurement campaign. 9986 25 " R^2 increased" but what did you use for $R^2=0.6$? Temperature only?

9987 3-5 That is called first order stationarity. However, this does not need to be mentioned explicitly. If that would have not been the case geostatistics could have not been applied. 9987 17-24 ????? You could not infer kriging parameters from varying sampling density. This is completely strange.

9988 2-3 because average sampling distance is so much larger than the range! What makes all this ambiguous... 9988 10-22 trivial 9989 2 show regression and R^2 9989 3-6 but the predicted standard error is only smaller because you removed the variability from the data. This tells you nothing! The average computed from the non-standardized respiration has a much higher variability. This approach is not valid! 9989 4 Which RMSE?

9992 1-11 this paragraph complete lacks from a link to the results of this study 9992 15 If outlier detection is that relevant, why was it not applied?

9993 25 30 cm!!

9994 7-8 "show little autocorrelation" This result is not presented at all within this manuscript. 9994 3-5 I strongly disagree. There is a suite of widely applied geostatistical methods that allows multivariate co-regionalisation, like external drift kriging (Ahmed & DeMarsily 1987, Water Resources Research) or regression kriging (Odeh et al. 1995, Geoderma). Something like that should have been applied instead of all the analyses presented here.

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