

Interactive comment on “Vegetation heterogeneity and landscape position exert strong controls on soil CO₂ efflux in a moist, Appalachian watershed” by J. W. Atkins et al.

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General comments

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This ms reports three years of soil surface CO₂ flux (F_{soil}) measurements, at a number of different elevations and vegetation types, in an eastern US temperate watershed. This is an interesting subject, as such controls on F_{soil} are poorly understood and important, and very appropriate for Biogeosciences. The ms is generally well written, with appropriate references, and the authors do a good job of covering relevant issues.

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There are a number of significant problems, however. First, this is an observational study, and the authors need to be much more careful about (not) inferring causality—starting, for example, with the ms title! You have no proof that vegetation and position “exert strong controls” on Fsoil, only that they’re correlated with it. In a similar vein, the landscape position “treatment” (see note #6 below) seems to be unreplicated (one high area, one mid area, one low area), so need to be doubly careful with this.

Second, a number of the statistical and analytical choices are unclear or weak. Much of the analysis hinges around breaking the Fsoil at 11 °C, for example, but it’s unclear why this point was selected—this really needs to be done in a reproducible, i.e algorithmic, way. In addition, I don’t understand why Fsoil wasn’t tested *directly* against bulk density, PAI, etc., rather than simply against the elevation and vegetation categories.

Finally, both the figures and writing could be improved; see comments below.

In summary, there are many points of interest here, but the current ms needs significant revisions in many areas.

Specific comments

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1. Page 17632, line 9-10: no need to give data source in abstract
2. P. 17632, l. 15: don’t define Fsoil again
3. P. 17632, l. 23-26: odd sentence, feels out of place
4. P. 17633, l. 1-10: confusing start. Why define separate Rsoil and Fsoil terms? What is your definition of Rsoil, exactly, if different from Fsoil?
5. Introduction generally – long and wanders a bit. I suggest you focus more tightly and concisely on relevant issues
6. P. 17637, l. 19 and throughout: I’m quite uncomfortable referring to these as “treat-

ments” – this is, as far as I can tell, a purely observational study

7. P. 17639, l. 8: “cm-3”

8. P. 17641, l. 1-10: when were these measurements performed?

9. P. 17641, l. 16-: what’s your justification for breaking the data this way, at 11 °C? It seems like this was done by judgment, as opposed to reproducibly. There are established statistical procedures for determining optimum change-points

10. Statistics generally – why this mix of SAS and R? Would be better to stick with one (ideally R) if possible

11. P. 17641, l. 17: what test is being performed here, exactly? Just because there’s a difference at 11 C doesn’t mean this is the correct breakpoint; see comment #9 above

12. P. 17648, l. 19: “shrub effect” – you’re inferring causality without any basis to do so (see comment #6 above)

13. P. 17649, l. 17-: very long paragraph. Break apart for readability

14. P. 17652, l. 24: I applaud posting the data! Ideally, post the code that produces the statistical results presented, too

15. Figure 2: is the line in panel (a) linear, or nonlinear? Note also that overplotting is a significant problem in these panels, and it’s hard to see what’s going on

16. Figures generally: take advantage of color, rather than only grayscale

Interactive comment on Biogeosciences Discuss., 11, 17631, 2014.

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