

Interactive comment on “Altered phenology and temperature sensitivity of invasive annual grasses and forbs changes autotrophic and heterotrophic respiration rates in a semi-arid shrub community” by M. Mauritz and D. L. Lipson

Anonymous Referee #1

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===== General comments =====

This manuscript describes an observational experiment conducted in native shrub and invasive grassland areas measuring soil respiration (R_t is the authors' unusual abbreviation). The carbon cycling consequences of such biotic invasions are poorly understood, and the ecosystem's extreme temperature and water variations are unusual. The text is reasonably well written, discussion interesting, and references appropriate.

There are some significant problems, however. First, I'm quite concerned about the

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authors' many inferences of causality. This study was observational, not manipulative, and conducted in a single very small area with low pseudo-replication. You really can't assert that invasion caused anything; it could be chance, or perhaps the grasses like higher- R_t areas.

Second, the models used to characterize R_t are extremely crude. There's a wide biological literature on this kind of thing, generally and specifically with respect to soil respiration, that should be consulted. In a similar vein, I'd suggest that fitting (VWC) breakpoints 'by eye' really isn't acceptable; do something algorithmic. Again, there's a big literature on this.

Finally, there are a number of equations or methodological points that need clarifying; see comments below.

===== Specific comments =====

1. Page 6336, line 10: “increased” – careful about causality
2. P. 6336, l. 10: specify time period of “cumulative” numbers
3. P. 6337, l. 14: “ R_t ” is an unusual term to use for soil respiration; consider something more standard (R_s , F_s , etc)
4. P. 6337, l. 17: “Högberg” (umlaut over o)
5. P. 6337, l. 17-18: reword. It's not the relative contribution per se affects soil C storage, it's that all these reflect balance of heterotrophs and autotrophs
6. P. 6339, l. 13: specify USA
7. P. 6340, l. 28 (Eq. 1): not $R_a=R_t-R_h$?
8. P. 6342, l. 12: why the conversion to K here?
9. P. 6343, l. 20-21: these polynomials are crude, completely abiotic ways to characterize respiration response to T and VWC; why not at least use a standard Q10-style

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model?

10. P. 6344, l. 1-5: how were these breakpoints determined? Doing so algorithmically (i.e., using a reproducible process) would be good

11. P. 6345, l. 22: “ μmol ”

12. P. 6348, l. 2: “altered” – see comment #1 above; also applies to beginning of discussion and other places

13. P. 6351, l. 5-: move to discussion

14. P. 6351, l. 23: “well outside of” ?

15. P. 6352, l. 1-5: you can't just assert with no evidence that Tang & Baldocchi got the wrong answer because of low sampling frequency (especially since we're talking about season and annual numbers)

16. P. 6354, l. 20: start new paragraph

17. Table 3: seems like this should just be summarized in text

18. Figure 7: connect points for clarity?

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