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Interactive comment on "The abiotic contribution to total CO_2 flux for soils in arid zone" *by* J. Ma et al.

Anonymous Referee #2

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Soil CO2 exchange is a major part of the terrestrial carbon cycle and is often considered the sum of biotic heterotrophic and autotrophic soil respiration. However, a growing body of research has shown that abiotic processes can also contribute to soil CO2 exchange. This paper seeks to quantify experimentally the importance of abiotic "respiration" to the total flux across a broad range of cultivated and natural alkaline soils in a developed desert region in China. The results show that abiotic respiration is omnipresent, but its signal and importance is diminished in soils with a strong biotic flux (e.g., in wetter soils with greater amounts of root biomass). In drier, more typical, desert soils, the abiotic respiration was dominant and occurred bi-directionally (into and out of the soil), though the values are quite small and nearly zero out when integrated to daily sums. Across soils, biotic respiration was closely related to temperature (though soil moisture variation was not examined) while abiotic respiration was tightly

linked with the temperature change between measurements.

The paper is well-written and easy to digest (thank you!). The strengths of this paper include: 1) the comprehensive sampling across a range of soils from managed, agricultural ones to the natural desert landscapes, 2) the separation of soil respiration (Rs) into biological and non-biological components, 3) the comprehensive analysis of drivers of the components (either temperature or change in it), and 4) the relationship between total component respiration and soil properties. I do not find any major flaws other than I'm unsure of whether the sterilization process actually left them sterile and did not affect other quantities like soil moisture. It would be helpful for the authors could add a little more text on this treatment to address concerns such as 1) did the treatment sterilize the soil, 2) did it affect the soil moisture, and 3) what would be impact of the soil coring which could potentially cut off plant roots and severely reduced root respiration.

Specific text issues: 1. Title Spell out "CO2" Instead of "Arid Zone", suggest being more specific. Maybe something like "across a broad range of soils and land-use types in a desert region"

2. Abstract L. 5-10. You don't actually describe the methodology at all in the abstract. A sentence or two describing how Rs was partitioned and the measurements would be helpful. L. 6. Suggest changing to something like ". . ..components in soils taken from eight land cover types found in this desert oasis region, including . . ..". That way you're not calling a "alkaline soil" or a "dune crest" a "landscape". L. 16. Change "in most ecosystems" to "in most soils" L. 24-25. The use of "is ubiquitous" is a bit of an over-reach for this study. Perhaps, "wide spread" or "occurs widely" L. 26. Again, "will not" might over-reach. Add some qualifiers "will not change WHEN. . .." or "likely"

3. Introduction P.11220, L2: "uncertainty . . ..has increased" . These science results haven't increased uncertainty; rather they provide more insight into the soil respiration process itself. L. 17. "the character of the abiotic CO2 flux" . Please rephrase. L. 29.

You are not analyzing “over landscapes”. Like the Abstract, I would suggest changing this to something like “..analyzed from soils collected from eight typical land cover types in this region. . .”

P.11221 L. “such as dissolution of CO2 in soil water” . Bringing in this mechanism here, are you suggesting this is the only mechanism that is responsible for abiotic fluxes in your case? L. 5. This last sentence seems to follow from your hypothesis rather than as an addition. Suggest changing “Additionally” to “Accordingly”. L. 17. Not really sure what you mean by “reservoir edge”. Is it regularly inundated, covered with plants, etc? L. 25. “to represent biological activity”. Unclear what you’re saying here.

p. 11222 L.20. Wouldn’t the sterilization procedure severely dry out the soil, such that when putting it back in the field and measuring Rs, these tubes would be much drier than the control samples? L.29. Wouldn’t the extraction of the core also sever roots in the control cores from neighboring plants?

L.11223. L.1. “Furthermore, all measurements. . ..” I would suggest moving this to the end of the next paragraph. L. 15. The use of cumulative fluxes in the regressions of your study strongly limits there applicability to other studies because they are dependent on the length of the time period (in your case, two days). In most cases, I would suggest using mean fluxes instead if that is possible.

P.11224 L. 5-8. Details are missing here about how organic and inorganic fractions were separated. p.11226 L10. Suggest “daily sum of abiotic hourly flux rate”. Also, shouldn’t this be “1/2-hourly”? L.13. Maybe I missed it, but was “deltaT/delta-t” defined previously? If not, please define. Also, what is the justification for the “delta-t”? Since this is a constant (1/2 hr), the regressions would not change.

p.11227 L10-14. I think if you used mean Rbiotic on the x-axis or even mean(Rtotal) here it would make this result much more useful elsewhere. Even, though Rabiotic summed to zero (l.12), this doesn’t prohibit the use of mean Rbiotic in the analysis. Also, not clear how mean Rabiotic/Rtotal was calculated on the y-axis. L. 14. The

following sentence and figure were not clear “From this point of view” and “apparent Rbiotic”. Did you mean, “If one assumed that the Rtotal = Rbiotic, then. . .”?

p.11228 L1-9. You didn’t mean organic or inorganic C as a predictor here. Did they have no explanatory power? Did you consider the ratio of inorganic C to total C?

p. 11229. L. 18-30. This discussion could benefit by including, Hamerlynck et al. (2013, Nocturnal soil CO2 uptake and its relationship to subsurface soil and ecosystem carbon fluxes in a Chihuahuan Desert shrubland, J. Geophys. Res. Biogeosci., 118, 1593–1603) who also show the change in temp relationship controlling negative fluxes and the near zero sum of daily totals and Rolland et al. (2013, Atmospheric turbulence triggers pronounced diel pattern in karst carbonate geochemistry, Biogeosci. Discuss., 10, 5009–5017) who provides a nice mechanistic view of abiotic exchange.) Is your mechanism of dissolved inorganic C in soil solution equivalent to the carbonate precipitation/dissolution? If not, how would future studies get at this mechanism?

p.11230. l.24. “Conditions such as ....high moisture content. . ..” I don’t understand this. This would imply that low moisture content favors Rbiotic more than Rabiotic. Need to more clarification here. It’s important to note that your study really can’t quantify soil moisture controls as soil moisture presumably would have been more or less constant over your two days of measurements.

p.11231 l.10. “. . .when dotted data were gotten” ? Table 1. Add the depth of the sample to the caption, e.g., “0-10 cm” Figure 1. Need to add some details to the caption. These are diel averages from data collected over 2 days, sampled every $\frac{1}{2}$ hour, correct? Fig. 2. Again, I don’t understand the rational for making this delta-T/delta-t. Why not just delta-T? Fig. 4. Again, making the x-axis a mean Rbiotic or, perhaps better, just Rtotal would make this figure easier to use in other studies. Fig. 5. Not clear what you mean by “apparent”. It’s pretty obvious your saying “If one assumed that Rs = Rbiotic), but it needs to be specified.

Interactive comment on Biogeosciences Discuss., 12, 11217, 2015.