Review of Angert et al, Using O_2 to study the relationships between soil CO_2 efflux and soil respiration

The authors present profiles of CO_2 and O_2 and use them to calculate apparent respiratory quotients (ARQs) for soils from three Mediterranean and three forest sites. They posit, based on the elemental composition of plants and soils, that RQs should be in the range 0.9 ± 0.1, and they subsequently observe much lower ARQs in field soils. The authors conclude this is due to CO_2 dissolution from the calcareous soil sites, because carbonates produce non-biological CO_2 fluxes. They suggest that in the non-calcareous forest soil site the low ARQs may be due to oxidation of previously-reduced minerals.

In contrast to previous reviewers, I do not think the importance of carbonate-derived DIC fluxes needs to be further justified by the authors. A case for the importance of DIC fluxes has been established in several recent papers, most recently by et al. (2013).

The authors did not mention the basic linkage between redox potential and RQ, which would provide useful context to help the reader understand the metabolic processes responsible for the range in soil RQ. As soil oxygen and redox potential decrease, RQ can approach infinity because CO_2 is produced by anaerobic respiration and no oxygen is consumed. In observing RQs << 1, the authors found RQs that are lower than expected for anaerobic respiration. The two major possible explanations for RQs <<1 are therefore, 1) Dissolution of CO_2 from carbonates. Because soil water can store and release much more CO_2 than O_2, this would tend to lower RQ, and 2) Respiration of substrates that are more reduced than sucrose (e.g. other sugars, lipids, lignin, and phenolics).

I was initially enthused for this paper, because the RQ of soil respiration has not received a lot of attention, and technique development to identify CO2 fluxes from bicarbonate dissolution would make a welcome contribution to the field. Unfortunately, however, I believe this study is based on a flawed premise, which is that the RQ of soil respiration should reflect the stoichiometric ratios of C to O found in organic matter. A more thorough review of the literature for root and soil studies would have revealed that RQ is highly dynamic and often much less than 0.9, depending on respiration substrates and plant stress. The elemental composition of plant and soil organic matter should deviate from RQ because 1) organic matter reflects long term metabolism rather than instantaneous conditions, and 2) not all respiration contributes to formation of organic matter. Plant physiologists distinguish between “growth respiration” and “maintenance respiration”, and in perennial plant systems most root respiration is “maintenance respiration”, i.e. used for nutrient uptake, turgor maintenance, cellular repair, etc. These fluxes contribute to soil respiration without producing new tissue. Because plants carry out these processes even under non-optimal growth conditions the substrates can be expected to vary dynamically with soil environment and photosynthetic conditions. The form of N that is available for plant uptake appears to be another significant factor (Luo, 2006). The authors own incubations of forest soils also produced RQs < 0.7.

It seems to me that the authors need to propose a way to assess the RQ of biologically-produced CO2, before they can use CO2/O2 ratios as a means to correct for carbonate contributions. It seems that the RQ of biologically-produced CO2 can vary too much to make the assumption that it is 0.9 ± 0.1. For instance, Luo and Zhou (2006) provide a nice synthesis of root respiration RQs from a number of studies,

and show a range of 0.39 (this low RQ was from an ozone-stress study) to as much as 1.5 (for sunflower roots). This range for root respiration far exceeds the range for soil respiration proposed by Angert et al, and suggests that their estimates of carbonate contributions could be either far over- or under-estimated.

Can the RQ of biological CO2 production be determined first, through incubations, in order to subsequently apply the partitioning technique? The authors own incubations show that RQs change very quickly with hours into an incubation. By quickly incubating fresh soils, however, such estimations may be robust. For root respiration, Lipp and Anderson (2003) found that incubating detached roots did not alter RQ relative to attached roots. Therefore, including roots in soil incubations may produce an estimate—albeit with limitations, because of disturbance—of biologically-produced soil respiration. In this reviewer's opinion, an incubation-derived RQ would be a more reliable estimate of the biological RQ than the generic assumption of 0.9 ± 0.1, because it would be specific to the substrates found in each soil, the plant functional types in each ecosystem, and the specific environmental conditions encountered at the time of sampling.

I feel using CO_2/O_2 ratios as a method for distinguishing biological CO_2 fluxes from carbonate-derived fluxes is not sufficiently vetted by the experiments described in this paper. In addition to resolving the question of what biologically-respired RQ is, the authors should apply other, independent methods to validate their estimates of carbonate-related CO_2 fluxes, such as ^{13}C partitioning (Stevenson and Verburg, 2006). In addition, there are a number of editorial problems with the manuscript, as described below:

1) The manuscript lacked specific research questions, goals, or hypotheses.
2) Over the last several years a rich literature has developed on distinguishing biological fluxes from carbonate-related fluxes, using modeling approaches (Ma et al., 2013; Wang et al., 2014) and 13C (Stevenson and Verburg, 2006), among other techniques. These references are important to discuss from the methodological standpoint, as they provide alternative approaches to address the same problem. Although the introduction was generally quite good, I thought this methodological context was missing.
3) The purpose of the different methods were not clearly linked to research questions. In particular, the purpose of the laboratory diffusion experiment and the soil incubations were not clearly explained. Furthermore, the results were very brief, and seemed almost like an unfinished outline. They did not provide clear tie-in to research questions.

Citations.

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