

Interactive comment on “Using O₂ to study the relationships between soil CO₂ efflux and soil respiration” by A. Angert et al.

Anonymous Referee #3

Received and published: 18 February 2015

Overall, I found this paper highly informative and potentially very important. The measurements and their interpretation appear technically solid, and I recommend the paper for publication. If significant amounts of soil CO₂ respiration have indeed been missing from soil chamber and eddy covariance measurements, this would alter the picture of ecosystem carbon balance at many sites worldwide. This is also my biggest concern with the manuscript. The authors need to describe better the potential significance of these findings and also comment on a way forward. Is the oxygen measurement technique they discuss easy (and cheap) enough for others to adopt? It seems that the ARQ numbers they calculate are too variable for others to adopt an average value, though (and there are significant differences across ecosystems).

A few comments: (page, line) 12041, 17: insert ‘hydrological’ before ‘system’ for clarity

C8020

eq. 1: The whole point of the paper is in a sense that eq. 1 is not correct. i.e. that $R(z)$ does not directly translate into surface fluxes of CO₂. Perhaps the derivation could be prefaced by saying that we start with incorrect formulation and modify it appropriately. 12052, 15: Shouldn't the $\delta^{13}\text{C}$ of soil carbonate minerals be around 0 per mil? In either case, providing a reference would be useful.

My other concern with the paper is that the use of English could be improved. I will leave copy-editing to the authors (especially Davidson, who is a native speaker). The paper is understandable, but there are small errors throughout. (some examples: p12040 line 1, comma after respiration is not necessary; 12042 line 13, ‘Roots respired CO₂’; 12055, line 21 ‘This explanations’, etc.)

Interactive comment on Biogeosciences Discuss., 11, 12039, 2014.

C8021