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Interactive comment on “Measuring and modelling the isotopic composition of soil respiration: insights from a grassland tracer experiment” by U. Gamnitzer et al.

Anonymous Referee #2

Received and published: 22 February 2011

Given the new understanding of NSS diffusion effects and their role in isotopic studies, it is certainly important to extend this understanding to a greater range of studies and ecosystems. In that respect, this is an important work. There are still too few studies where ecological researchers have indeed extended themselves into the arena of physical modeling, but it appears that more will likely have to in order to produce meaningful ecological results with good certainty. In this sense, many researchers will benefit from this study and others like it. I think the remarkable aspects of this particular effort include the water-phase diffusion kinetics, and the application to an interesting overall scenario which involves labeling, two types of chamber measurements, keeling plots, advection, etc. In that way, many of the individual transient impacts documented

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by authors are rolled together into this one study. The moisture diffusion results in particular are very striking, because they appreciably delay the onset of equilibrium in the model world. That in itself is, I think, the most important progress here. In the real world under time-varying conditions, it is likely (given water phase kinetics) that soils can never be at isotopic equilibrium! So, I think this effort is very useful.

The study isn't a demanding read, and is a straightforward approach to physical modeling of a measured system. The actual ecological processes aren't of real concern here - it is primarily the physical processes that are of interest. In that sense, I think the text is sufficient, useful, and appropriately spare. But, the results could more readily digested (and less skepticism generated) if there was more transparency related to aspects of modeling and model performance. Overall, my concerns reduce to two (the second in two parts):

1. The authors should recognize that a 1-D model like this is perhaps somewhat more limited relative to the other models they cite and which have been used by other authors. Many of these others are 3-D models, which allow transient disequilibria to develop more fully. The 1-D models deny lateral escape mechanisms to isotopologues, which would be a natural consequence of chambers footprints and super-ambient internal concentrations etc. So, the 1-D modeling approach used by the authors will probably underestimate the magnitude of transient fractionation. While a larger chamber minimizes edge effects, it still does not reduce entirely to a 1-D system. ACTION: The authors should provide related text in the methods and/or discussion section(s) to clarify these important differences - that results will not likely be exactly comparable to a 3-D model.

2. The authors have the ability to reduce their exposure to methodological criticism by providing more transparency on model performance. There is lots of detail on the model's undercarriage, but not related to performance under known conditions or in the real world. I'd surely trade the latter for the former. In other words, I care less about what goes into a transient fractionation model (most of the equations are well known)

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than how well it does and how stable it is given normal ranges of uncertainty in input parameters. A great many of Reviewer 1's suggestions/concerns would fall into this category. Added information would allow the readers to more thoroughly evaluate the model's performance for themselves. Two types of related information are required:

a) Conformance to known conditions. Does the model generate the right results under known conditions (what are they), or compared to analytical solutions? The authors don't necessarily need to produce related results, but especially since this is a new (not previously used) model they should clearly outline what the validation metrics were, and whether they were met. There is related text (ie p94, line 10 related to impact of dissolution mechanism on model output), but it does not go far enough to say that results were as expected (with clear communication of those expectations). The authors have a good level of experience with this type of modeling so I'm not particularly worried that validation was not done - but it is a legitimate concern I think to say that model performance is somewhat of a black-box. The end result of the modeling exercise fits the data relatively well, and the authors should demonstrate that this is not coincidental. ACTION: The authors should provide related text in the methods section to clearly outline their expectations, the various synthetic tests they did to ensure that data was reasonable, and whether the model passed - either perfectly, or within acceptable limits.

b) Sensitivity to input parameters. While table 1 provides some indication that sensitivity analyses were done, there are no related results. To what input parameters is the model sensitive? What's really important to get right in terms of parameterization data? That way, the authors could address concerns such as lacking pH measurements - for example do small errors in pH even matter? Or, do they matter a lot? I would have liked to see these results, particularly as model is "new", and also because the authors themselves opened the door to the issue of sensitivity (p96, line 3) - which is a very important issue in modeling. The fact that the authors raised the issue suggests to me that they also have the results. Ideally, these would be the first of the results presented

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in the results section - to document the characteristics of the synthetic reality. ACTION: The authors should provide the results of sensitivity analyses, probably with text and/or table/figure.

Interactive comment on Biogeosciences Discuss., 8, 83, 2011.

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8, C92–C95, 2011

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