

**Author response to comment on “An inversion approach for determining production depth and temperature sensitivity of soil respiration” by R. N. C. Latimer and D. A. Risk  
By P.-E. Jansson (Referee 2)**

**(referee comments in black, author responses in blue)**

I understand this as a well-written paper that is of high interest for inverse modelling and also for understanding how we can potentially understand empirical data by using some of the reliable soil physical principles as a filter.

Thank you.

The only major problem I have with the paper is the authors way to discuss the 2 basic parameters that they can estimate as a try property of high general interest. In my view the 2 parameters are dynamics variables of all ecosystems and none may be of direct interest for the long-term response of environmental changes. The Q10 as a lumped aggregated sensitivity of temperature is in fact also including many other components - especially the moisture response control of CO<sub>2</sub> production is a problem. In most natural ecosystem the moisture is regulating CO<sub>2</sub> production both in the dry and the wet range since microbial processes are regulated strongly by moisture and oxygen. Another issues is the microbial activity and the substrate quality. None of those can be assumed be lumped into a Q10-value. So I think the value of being able to estimate Q10 from simultaneous measured CO<sub>2</sub> conc and surface flux data are limited for understanding CO<sub>2</sub> issues especially on the global scale.

The referee raises a broad and deep discussion for the entire soil science community. The question for us as authors of this manuscript is whether we've solved for the right parameters in this inversion approach? Certainly, we could have chosen to solve for parameters other than Q10 and depth of production. To do so, we would simply need known values of Q10 and depth of production for use as model constraints so that we could focus on solving others. But, we did choose to target these two parameters for several reasons:

*Q10.* First, we totally agree that Q10 is overly simplistic. However, we recognize three things: 1) That temperature sensitivity of soil respiration has been one of the most widely debated topics in soil science for decades, so for that reason alone it seems like a good place to start, in this case by introducing, physical “filters” (a nice compact way to describe our approach – thank you for that). 2) The Q10 seemed like a good focus because there are known problems with field-measured Q10s when we don't use physical filters, which we now know has caused us to misinterpret past field data – and certainly these misinterpretations continue without the availability of physical filters (see Phillips et al 2010 as cited in the manuscript for great examples). 3) The Q10 parameter also forms much of the basis for our understanding of global soil response to warmer climate, because all of our Earth System Models (ESMs) incorporate it, whereas they differ more in what other processes they include. However, I think that we would all share the perspective that a wider debate is needed. From the most cited BG paper in 2014: “most ESMs cannot reproduce grid-scale variation in soil carbon and may be missing key processes. Future work should focus on improving the simulation of driving variables for soil carbon stocks and modifying model structures to include additional processes.”

*Depth of production.* Solving for this parameter actually fits very nicely with the referee's comments below about production profiles. We would personally expect the distribution of root and microbial production would naturally be expected to shift seasonally, as for example trees allocate resources to roots in the spring – which might concentrate some production further down. But, there are very few studies to have really tested these ideas. To some extent, the depth of production parameter provides

opportunities for qualitative partitioning between the root and microbial sources, which could lead to an improved understanding of soil processes.

In the end, it was not necessarily our goal to explain long-term shifts on a global scale, and perhaps the most helpful comment we can make in the manuscript to address the referee's comment is to clarify our overall aims (and also to incorporate some of the points from above). We have chosen to use this comment to expand the conclusion, since the issues are broad, and forward-looking:

“Our modeling is ultimately aimed to help in site- and soil-specific studies, to gain an improved understanding of soil processes. The inversion approach amounts to a physical “filter”, which can be used to improve the interpretations of field datasets, so that soil biogeochemists can feel secure in knowing that their interpretations of biogeochemical response are not overprinted by physical artifacts. Ultimately, our hope is to see our inversion approach couple seamlessly with any of the existing approaches and models which are available to examine the role of soil substrate pools, substrate quality, substrate availability and binding, oxygen limitation, enzymes, moisture response, etc. There are many such models available, of differing complexities. Currently, our most prolific physical-biochemical soil models exist as forward Earth System Models (ESMs), and as described in Todd-Brown et al. (2013), these may either be incorrectly parameterized, or missing key processes. Inversion schemes have a role in helping the soil science community decide which soil parameters are the most important to understand, and to simulate well. But, inversion modeling approaches must be built and validated one step at a time, so that they don't lead to uncertainty. This synthetic study is a first step, and we look forward to applying these physical techniques to real field datasets. While we may in the future hope to invert for parameters aside from Q10 or depth of production, this study shows that physical inversions of soil CO<sub>2</sub> production are indeed achievable, and can help derive information of relevance to biogeochemists.”

Causes of variation in soil carbon simulations from CMIP5 Earth system models and comparison with observations K. E. O. Todd-Brown, J. T. Randerson, W. M. Post, F. M. Hoffman, C. Tarnocai, E. A. G. Schuur, and S. D. Allison *Biogeosciences*, 10, 1717-1736, doi:10.5194/bg-10-1717-2013, 2013.

On top of this I also think that the production depth that here is assumed to follow an exponential decay function can be totally misunderstood. Production depth is not only the decomposition from a substrate with a certain distribution it also originates from autotrophic respiration that cannot be assumed to follow such simple depth distributions.

We thank the author for this comment, and now recognize that this issue was not explained fully in the manuscript. Our position on the topic is a pragmatic one, and does not actually differ from that of the reviewer. To at once address the reviewer comment, and improve the clarity of the manuscript on this issue, we will insert the following text into the methods section of our revised manuscript. This text articulates our assumptions and practices, plus what would need to be done in the future when using real data:

“Linear, linearly-declining, and exponentially-declining production profiles all exist in nature. The actual shape of a total soil CO<sub>2</sub> (autotrophic+heterotrophic) production profile will depend on many factors including the depth and horizonation of the soil, the relative depth of microbial and root activity, soil textural factors, soil hydrology, etc. Even within a small geographic region and under similar vegetation, we have measured both linear and exponentially-declining total CO<sub>2</sub> production profiles (Risk et al. 2002) in soil gradient surveys. What is important for inversion modeling is that the production profile used in the model must reflect that of the data. For the purposes of this study, where our synthetic soil production profiles were created with an exponentially-decreasing production profile

as discussed below, we used a matching model parameterization. If using field data, it would have to match the field data. In the end, any mathematical function could be used to describe the generalized shape of the production profile, so even if a soil were found to have a complex bi-modal production profile, it could easily be parameterized as such. “

Reference: Risk, D., Kellman, L., Beltrami, H. (2002). Soil and CO<sub>2</sub> production and surface flux at four climate observatories in eastern Canada, *Global Biogeochemical Cycles*, 16 (4), 1122, doi:10.1029/2001GB001831

The high accuracy in the suggested method is of high theoretical interest but it should be balanced by the very high uncertainty in some of the assumptions in the conceptual model. However, assuming the conceptual model is valid I understand the suggested inverse model is useful. My suggestion for revision is that the authors make such a discussion. They need to clarify the assumption both in the introduction and also follow this up in the conclusions.

We hope our comments have helped explain our choices in model parameterization, our underlying assumptions, and our expectations as to the role for inverse physical modeling of soil CO<sub>2</sub> production – and particularly how it is a good fit with (but not a replacement for) the array of biochemically-based soil models that are a-physical in nature. We hope to work with the wider community of soil modelers, and if for example they indicate in the future that Q<sub>10</sub> is not the best biological parameter for us to be solving using this physical “filter”, we tweak accordingly. But, this manuscript sets out the synthetic process that we would follow to explore applicability, and validate the approach for a new context.

We thank the referee for his excellent comments, and hope that our additions have helped to both strengthen and nuance our manuscript.