Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-395-AC2, 2017
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Biogeosciences Discussions

Interactive comment on "Ideas and perspectives: Can we use the soil carbon saturation deficit to quantitatively assess the soil carbon storage potential, or should we explore other strategies?" *by* Pierre Barré et al.

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Received and published: 20 October 2017

We were surprised to see that Reviewer 2 seemed to have missed the main messages of our manuscript. We think that the misunderstanding mainly comes from problems of terminology.

We defined in the Introduction what we call the OC storage potential (p2, lines 9-12: "We propose the following definition: the C storage potential of a soil is the maximum gain in soil C stock (kg m-2 or Mg ha-1) attainable at a given timeline (e.g. IPCC default

time period: 20 years), by implementing changes in land management, i.e. land-use, agricultural or forestry practices changes"). Reviewer 2 modified our definition in his/her review ("the definition of a "C storage potential" should refer to a maximum stock which is independent of time and not a mixture of both, stocks and fluxes"). From that stage, as we disagree on the terminology on which all the manuscript is based, the discussion becomes a kind of dialogue of the deaf. For instance, we did not ask in the manuscript the question "whether there is evidence for the existence of a maximum C storage potental of soil of the mineral associated C fraction" which is considered by the reviewer as the most important question in the manuscript. This also explains why the reviewer considers that "it sould be first discussed wether and why such a maximum storage potential exists for total soil carbon", whereas we consider that it is out of the scope of our draft. The reviewer raises interesting research questions but we consider that these are not relevant to the present manuscript which is dedicated to an operational problem needing answers in the coming months or years.

We disagree with the definitions of storage and sequestration proposed by Reviewer 2. We consider following Olson et al. (2014) that Carbon sequestration is "the process of transferring CO2 from the atmosphere into the soil of a land unit, through plants, plant residues and other organic solids which are stored or retained in the unit as part of the soil organic matter with a long residence time", i.e. several decades. Following IPCC (2006), we define Carbon storage as the increase in SOC stocks over time in the soils of a given land unit, with no hypotheses on its residence time, nor on consequences on the CO2 concentration of the atmosphere. We agree that terminology is important and that accepted shared definitions of storage and sequestration would be highly useful for our scientific community. We would therefore be interested in having the references on which the definitions proposed by the reviewer are based.

We agree with Reviewer 2 (and wrote that clearly p6L7) that the data-driven approach has already been explored in a few papers. We call for the development of such an approach based on new data provided by various soil monitoring programs. This would

require a sustained research effort. We also want to highlight the limits of the current methods to achieve this goal. As a result, we do not see why it is inconsistent to write that this approach is an improvement compared to the IPCC default factors but that this approach should be used with care and developed further. Lastly, we did not understand why Reviewer 2 cited Beare et al. (2014) to emphasize that the data-driven approach has already been explored as we just explained that our aim is to estimate the whole-soil OC storage potential, and not just the fine fraction as Beare et al. did in their paper.

We agree with the reviewer that estimating properly belowground C inputs is difficult and is a substantial source of uncertainty for models. This would indeed deserve further discussion. It is in line with our recommendations calling for a joint use of data- and model-driven approaches (P7L12).

In conclusion, we thank the Reviewer for finding that "our paper starts an important discussion on scientifically sounds methods to estimate the potential role of soil C for climate mitigation" but we consider that the major points missing in our manuscript, according to the Reviewer, were not within the scope of our manuscript.

References: Beare, M.H., Mcneill, S.J., Curtin, D., Parfitt, R.L., Jones, H.S., Dodd, M.B., Sharp, J., 2014. Estimating the organic carbon stabilisation capacity and saturation deficit of soils : a New Zealand case study. Biogeochemistry 120, 71-87.

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Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2017-395, 2017.