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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.

Anonymous Referee #1

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In this opinion paper the suitability of the C saturation deficit (the so-called Hassink approach) to estimate the soil C storage potential is critically discussed in the context of the 4 per mill initiative and alternative strategies based on reference sites with highest SOC stocks (data driven approach) and model approaches are proposed. The manuscript contains a large part that describes the Hassink approach. The authors want to "explain why, for conceptual reasons, the soil Csat-def is not appropriate, at least in its present form, for assessing quantitatively the whole-soil (total) organic carbon storage potential". The main criticism of the C saturation deficit concept is that

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it is not suitable to estimate the whole C storage potential of soils as expressed by the 4 per mill initiative but only the potential for stable C, referring the C sequestration potential. This is certainly true, the fine fraction-based C saturation does not account for C of coarse fractions, but this is a well-known limitation, which is addressed in the papers published using this approach. There is no evidence in the literature that the Hassink/ Csat-def approach has been used to estimate the total storage potential of a soil and the authors of the current paper did not find one. They refer to 4 papers that have used the Hassink approach to estimate a saturation deficit. I went back to these papers to check what these papers describe exactly. Three of them used the Hassink approach in a clear and differentiated way and did not argue that this would allow for estimating the total soil storage capacity for OC. In detail: -Angers et al. (2011) refer to the "potential for SOC sequestration in a stable form" -Wiesmeier et al. (2014) provide an "estimation of the C saturation deficit corresponding to the long-term C sequestration potential" -Castellano et al. (2015) build on a general concept of C saturation, but they do not use the Hassink equation at all. -McNally et al. (2017) refer to the "capacity of soils to store SOC in a stable form" Another paper referenced also (Wiesmeier et al., 2015) investigates the "ability of soils to stabilize additional OC amounts in the long term" and the authors point out that "our approach only accounted for the OC storage capacity of the fine mineral fraction and did not include the potential of physical OC protection by soil aggregation". So all these authors referred to a sequestration potential and did not claim to estimate the total soil storage potential (i.e., including more labile forms of OC), i.e. these papers use the Csat-def. approach as suggested by the present paper. The authors argue that substantial amounts of C can be present in the coarse fraction of forest and grassland soils, which is not accounted for by this method. However, in cropland soils more than 90% of total SOC are stored in the fraction <20 μm. As the 4 per mill initiative specifically targets improved management of cropland soils, the C saturation deficit approach may also estimate >90% of their C storage potential. The conclusion is that the Hassink approach "may inform on the long-term C sequestration potential" (page 4, line 24). It needs to be investigated in more detail, as the authors seem to acknowledge that it may allow to estimate the potential for OC storage at “a pluridecadal timescale”. But then, the authors write that “this concept is irrelevant alone”. Yes, but this is already well described in the literature. I am confused as several authors of the present paper have proposed in another recent paper (Dignac et al., 2017, referenced in the manuscript) exactly the opposite, as they write about the Hassink approach (among others) “could be developed to improve the prediction of soil organic C stocks, particularly in a context of land use and practice changes“. In the following part of the manuscript, the authors present two alternative approaches, a data driven and a model driven approach. The “data driven approach” is shortly explained and then several limiting factors for this approach are mentioned. The “model driven approach” is described only very shortly and is basically a short discussion of the approach by Lugato and coworkers. Other approaches, e.g., long-term field experiemnts, are not described at all. The reader is left with confusing statements in the conclusions. One is that the proposed “pragmatic approach is not straightforward to implement”, and that it will only “allow little progress in understanding the mechanisms” so that such research “should be carried out in parallel”. Thus the authors finally conclude that “ the soil Csat-def concept remain relevant and may provide fruitful tracks to improve soil C dynamic model formulation”. I am not convinced that this paper is a new contribution to stimulate discussion. I understand the ideas behind, but it presents an oversimplified perception of what has been done up to now with using the Hassink approach, suggests other approaches, but does not explore them in detail. But there will also be no harm inpublishing this paper, as there is limited new ideas presented.

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