Reaction to interactive comment, received and published 15 July 2019, on
"Global database and model on dissolved carbon in soil solution", by
Langeveld, J., Bouwman, A. F., van Hoek, W. J., Vilmin, L., Beusen, A. H. W., Mogollón, J. M., and Middelburg, J. J.

We thank the referee for his/her reaction and constructive feedback. The comments will help to improve our paper, especially the suggestion to also use the DOC flux data. Below, we repeat the referee's comments and questions in italics, followed by our response (in normal font). Statements about corrections in the manuscript are printed in bold.

1. The authors have done a lot of work to assemble the data. The database will be a valuable resource. It is disappointing that the flux data are not analysed, since these are arguably more important than concentrations in the context of the carbon cycle. It would be of great interest to know the distribution and statistics of the flux data. I do not think the justification for analysing [DOC] only (page 4, line 22) is convincing. The authors say that fluxes can be obtained as the products of concentration and water flux. What happens if you do this, how do the results compare with the direct flux data? Looking at Figure 5, average fluxes might be > 20 gC/m2/a, which is about twice the value that Buckingham et al reported for non-peat UK soils.

Thank you for your feedback. Indeed, we chose to select only the annual average DOC concentration data for analysis and not the DOC flux data. This has two main reasons: 1. the global coverage of DOC concentration data is much larger as it covers all main climate zones, unlike the DOC flux data. This is important, as we aim to model on a global scale. We aimed to explain this on page 4, line 22, referring to Table 1. 2. The model described in our paper is part of the Integrated Model to Assess the Global Environment (IMAGE) Dynamic Global Nutrient Model (DGNM) (Vilmin et al., in prep.). This model is based on a hydrological framework (Van Beek, Wada and Bierkens, 2011; Sutanudjaja *et al.*, 2018) and thus requires concentrations as an input. However, we agree we could consider the choice for analysing DOC concentrations better than currently done on page 4, line 22.

The aim of our study is to constrain DOC concentrations on a *global* scale. We recognize that a global model with this resolution (30 minutes) should be used with caution when comparing it to individual site-specific data due to scale issues. The lack of good correlations when comparing DOC concentrations with soil parameters obtained from global datasets (see text and response SC1) is an example of these scale issues.

In response to the reviewer, we modelled the DOC fluxes using the database data on climate, soil class, land use, sampling depth and extracted precipitation data from IMAGE. For hydrology, with sampling years and coordinates from the database sites as input, we used the hydrological framework PCR-GLOBWB (Van Beek, Wada and Bierkens, 2011; Sutanudjaja *et al.*, 2018), which also corrects for the impact of surface cover and land use on surface runoff. This method to calculate fluxes using concentrations and hydrology has been previously used in a similar approach on dissolved nitrogen by Beusen *et al.* (2015). We calculated the fluxes depth-specific for the subsoil. This involves a range of assumptions, simplifications and averaging. Thus, because of the spatial heterogeneity of the involved factors we cannot expect a clear correlation between the modelled and measured values. Figure 1 shows the modelled and measured fluxes, with the measured and modelled fluxes in a similar range, and with literature-based fluxes roughly twice as high as the modelled fluxes for a limited number of sites. For a global model this is an acceptable result, especially when realizing that this is a comparison of average large-scale data with point source measurements based on a range of different methods to calculate DOC fluxes (e.g. often but not always correcting for loss of water through surface runoff). We also note that the modelled fluxes generally do not exceed the mentioned 20 g C/m2/y.

In summary, based on reviewer 1 we will improve the text in two ways: 1. we will better explain the choice for using DOC concentration data; 2. we will use the flux data for validating our modelled DOC fluxes (from concentration and water flow).

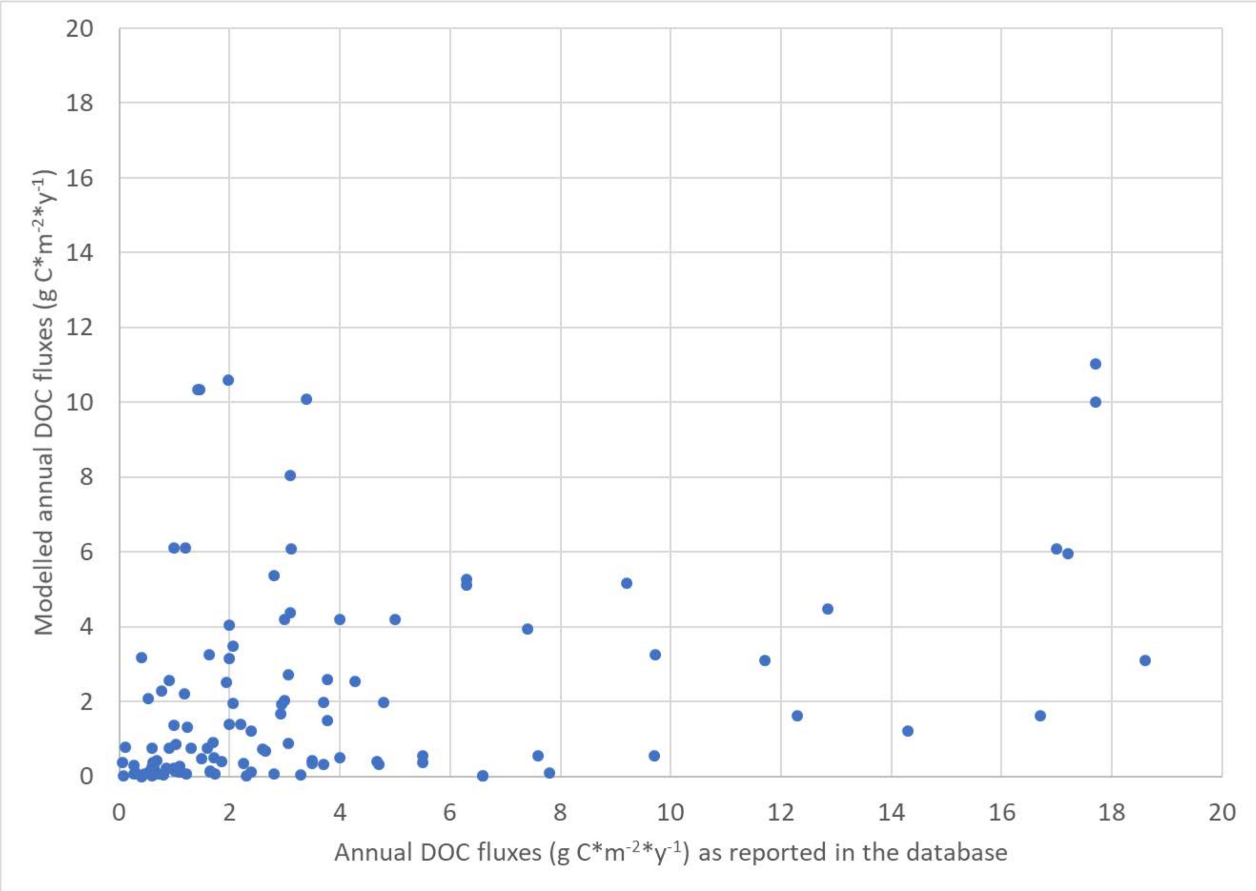

Figure 1: Subsoil annual average DOC fluxes (g C m^{-2} y^{-1}) as reported in the database vs modelled subsoil annual average DOC fluxes (n=114). Five isolated extreme values were excluded. Axes have the same range for comparison purposes.

2. Buckingham et al compared straight average and flux-weighted concentrations, and found not much difference, but this was the UK only. Is it possible for the authors to make such a comparison for their collated data? A discussion of the possible dangers of reporting straight-average annual [DOC], which I assume is what has been done, would be helpful.

Thank you. Much of the data we collected was already reported in studies as an average over several years and as such contains both straight average and flux-weighted concentrations (e.g. annual averages compiled by (Michalzik *et al.*, 2001)). In other cases, we indeed calculated a straight average. We did not make a comparison between the two approaches. We believe it is acceptable to include both, in particular because, as the reviewer already mentioned, in studies like Buckingham *et al.* (2008), the difference is limited. Still, we acknowledge that it is important to mention the existence of the different approaches. In conclusion, **a few sentences on this issue will be added in the manuscript. Furthermore, we will consider to include it as an additional option in a future update of the database.**

3. The model is unimpressive – the predictions do not vary much and so there is strong bias, predictions being too high at low [DOC] and too low at high [DOC]. A problem might arise if high soil moisture promotes DOC production (positive effect) while associated high water throughput causes dilution (negative effect). Again, it would be of interest to know the implications of the modelled data for flux estimation.

For the implications of the modelled data for flux estimation, we refer to comment 1. Although the model is far from perfect, it is the first of its kind at the global scale. We agree with the reviewer's comment on biases, and **will extend our discussion of biases on page 7, line 6 on problems associated with soil moisture and dilution**

4. Can the different averages in Figure 4 be compared for statistical significance?

This is a useful suggestion. We did this analysis, but it was not included in the figure. **We will add this to Figure 4 in the manuscript.**

5. Page 4, line 20. I do not understand this argument – surely there is more decomposition of organic matter near to the soil surface, not with increasing depth? Is it more to do with escape of CO2 as gas, and the dissolved DIC flux, becoming less efficient with depth?

We totally agree (actually, we currently build a DIC model based on this principle). **We will improve this sentence.**

6. The paper is not that well written, it could do with a careful edit for language and grammar. The word "equatorial" is mis-spelled as "equitorial" throughout.

We will carefully edit the manuscript for language and grammar. We corrected the word 'equitorial' which occurred in Table 3.

References

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