

Interactive comment on “Global distribution of soil organic carbon, based on the Harmonized World Soil Database – Part 1: Masses and frequency distribution of SOC stocks for the tropics, permafrost regions, wetlands, and the world” by M. Köchy et al.

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Received and published: 15 December 2014

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<i>Reviewer's comments</i>	<i>Authors' response</i>
<p><i>General Comments</i> This manuscript provides an interesting examination of different databases that can contribute to the calculation of global soil organic carbon (SOC). The challenges of estimating the extent and characteristics of both wetlands and permafrost areas are known, but the comparison of databases that attempt to address these issues nicely illustrates the current situation. The authors give particular emphasis to the issue of bulk density (BD), which is a problem that deserves greater attention.</p>	

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<p>Although much of the manuscript's content has merit, the effective communication is hindered by the text's organization. A major factor for obscuring the message is the appearance of five different points within the writing: 1) effect on SOC stock estimates from 'correcting' HWSD values for BD, 2) comparison of different databases' estimation of soil depths, 3) comparison of different databases' estimation of permafrost and wetland extents, 4) comparison of different databases' classification of wetland types, and 5) summing of global SOC stocks by latitude and wetland type. Clearly these points are related, but addressing them all in a coherent and focused matter will require careful crafting.</p>	<p>Thank you very much for your careful reading of the manuscript.</p>
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<p><i>Specific Comments</i></p> <p>1. An apparent contradiction for the writing organization is the classification of this paper as a "review," but the text contains a methods section that does not describe the process for reviewing. Instead, this section describes a method for adjusting the BD in the HWSD. One possible solution for addressing this and my general concern about the paper's organization would be to use an outline similar to the following: [...]</p>	<p>We re-organized the revised text by moving parts of the Methods to the Introduction and re-arranging sections within the Methods.</p>
<p>2. Terms and abbreviations need to be used consistently, e.g. 0.5 arc minute v. 0.5', harmonization v. harmonisation (both acceptable spellings, choose one), SOC stocks v. organic C stocks v. organic carbon stocks.</p>	<p>This will be addressed in the revision.</p>

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3. P 326, L 3-6 – This needs elaborated on. Specifically, what constitutes 'relevant'?	We recast the introduction so that our intention becomes clearer.
4. P 332, L 8 – Is it really fair to say that the SOC stock is not underestimated with a reference soil depth of 100 cm? There are several studies showing notable amounts of SOC below 1 m (e.g. Richter and Markewitz, 1995, among others). Both in this manuscript and the published literature the qualifier of "SOC stock in the upper 1 m" is often used, which is an important distinction for what is actually being estimated. Also, later in the manuscript estimations of SOC for depths below 1 m are discussed. The subsequent breakdown of soil depths by soil type is interesting, but I suspect there is a disconnect between the definitions of sampling depth, soil depth, and the depth at which organic carbon can be found. Consideration of these issues should be part of this discussion.	We concur that there are considerable amounts of SOC in greater depths than 1 m. We will phrase the text more exactly in the revised text in this section.

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5. P 332, L 19-22 – These sentences appear contradictory. If WISE and HWSD give the same soil depth for 80% of the area and WISE gives less soil depth for the remaining 19%, how does it work out that in total WISE gives greater depth?	One of the analyses was based on the WISE gridded data set which uses a maximum reference depth of 1 m. We will remove the discussion of differences in soil depth between the databases in the revised text as it goes beyond the scope of the paper.
6. P 333, L 4-6 – Provide the original HWSD 1.1 Pg C calculation as a baseline.	We have added a new Table 2 in the revised text.
7. P 333, L 16 – Should "mean" be inserted before "BD"?	It's the "best estimate" provided by Page et al., this will be clarified in the revised text.
8. P 333, L 24-27 – The difference between 2476 Pg and 1062 Pg (1414 Pg or more than 50%) does not sound "small," but the intended comparison is probably with the 1061 Pg of the modified HWSD 1.1 calculation. Please clarify.	Correct. This will be clarified in the revision.
9. The comparisons of numbers are often difficult to follow. Better organization could help this, but the text at times needs to be more clear about to which number a new calculation is being compared. Tables may be helpful for this.	This will be addressed by the reorganization of the text.

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<p>10. P 336, L 18-20 describes the importance of the spatial mapping's quality for frozen high-latitude soils, but only the attribute accuracy is identified as important for the global carbon mass. The area of an applied attribute is a major multiplier in any calculation of total mass. Some balance is needed to communicate that both spatial and attribute accuracy is important, but different aspects are more of a problem for the current mapping of SOC in certain land use types.</p>	<p>We agree. We will revise the text in this section and the introduction to emphasize this point.</p>
<p>11. P 337, L 1-2 – It appears that the CAMP map is not identifying a separate region, but a unique delineation encompassing many of the same areas as the others. If that is the case, then “a third permafrost region” should be changed to “a third permafrost extent.”</p>	<p>Correct.</p>
<p>12. P 339, L 19 – Is this calculation really based on an “intersection” of the two databases or the ‘union’ of the two? An intersection would be a conservative estimate, but a union seems likely to be closer to reality.</p>	<p>Here (and in line 23) it is an intersection, i.e. the area of the HWSD that is also classified as wetland.</p>

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<p>13. P 341, L 5 – It would be interesting to have the Pg SOC estimation based on the 3.3 Mm² area for comparison with the Pg SOC based on the 10 Mm² area.</p>	<p>We added the information (113 Pg) to the revised text</p>
<p>14. P 341, L 20-22 – Which source are these numbers from?</p>	<p>We calculated them from the overlay of the union of GLWD/GLCC wetland types over the modified HWSD-SOC map.</p>
<p>15. P 342, L 13 – Is this total C or organic C?</p>	<p>The reference to SOC is clarified in the revised text.</p>
<p>16. P 344, L 11-14 – There are many possible references that explore this point specifically; a few of the more recent ones should be cited here.</p>	<p>More references have been added to the revised text</p>
<p>17. P 345, L 10-12 – This statement is not really true for this manuscript, especially considering the focus was on wetland and permafrost areas. The data was broken down by wetland type and by latitude ranges, but not by land-use/land-cover classes in general.</p>	<p>We did not want to claim to cover all LULC classes. We rephrased the sentence in the revised text.</p>
<p>18. P 345, L 20-27 – These last sentences seem to extend beyond the scope of this manuscript.</p>	<p>The position of the sentence "The strong effect of BD . . ." is indeed interrupting the train of thought and obscured our intent to describe the need for better data in C-cycle models. In the revised text we moved the sentence to the start of the paragraph.</p>

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Technical Corrections

These will be corrected in the revised text

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