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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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Anonymous Referee #1	Authors' comments
Soils stores a large fraction of terrestrial organic carbon. The soil organic carbon plays an important role in the biosphere. A reliable data set is the prerequisite to accurately represent the pools and fluxes of carbon. There are several estimation works based on global soil mapping (observation derived) and earth system models (simulation), and given the terrestrial organic carbon in a large range from 500 to 3000 Pg or higher. This manuscript gave us another number and geographic distribution. It is also valuable to improve our understanding on carbon cycle as a reference or benchmark data set. This manuscript gave us another estimation of soil carbon based on HWSD with adjusting the bulk density of Histosols, the definition of wetland, and incorporating more detailed estimates for permafrost from the Northern Circumpolar Soil Carbon Data Base. Though there is no anything original, it still is an important approach and valuable, and could be accepted after revision.	Thank you very much for taking the time to review our paper.

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General comments: Soil depth matters in the SOC stock estimate, especially for deep soils. Though this paper corrected SOC of depth >1m in the peatlands, most of the true soil depth are not known in soil profile observations. Though we may lack data or method (extrapolation?) to reduce the uncertainty caused by the assumed soil depth, we need to keep this in mind. This uncertainty is not only for the <1m soil but also especially for the deeper soils. As a result, the uncertainty should be emphasized in some parts of the paper, such as page 338, line 8-17 and the conclusion section.	We agree that SOC stocks of deep soils are associated with great uncertainty. We emphasize in our paper the stock in the top 1 m and associated uncertainties. Stocks of deeper soils are mentioned to complement existing estimates but are not our main objective. We point out the associated uncertainty (p 338, ll 10-17, p 343, l14) and will additionally be mentioned at the end of section 3.5 in the revised text.
Consider describe the correction of frozen soils ..., and tropic peatland (Page et al.) in the method section, since the combination of the three dataset is more reliable.	Although the correction makes the estimate of global SOC mass more complete, it is not spatially explicit and would not allow the calculation of percentiles and masses within categories. We intended to reduce confusion by reporting in the Methods section the changes applied directly to the spatially explicit HWSD and in the Results section additional corrections considering the mass.

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Consider change the order of section 3.3 and 3.4, i.e. correcting first and then overlaying with the wetland data. With the above two modifications, corresponding tables and figures need to be redrew or added.	Both sections can be considered separately, we do not apply any corrections within either of the sections.
Consider delete “based on the Harmonized World Soil Database” in the title.	We will reconsider the wording of the title.
The part 2 of the paper only uses the HWSD to calibrate a SOC model, which is not very close to the part 1. It is better to treat these two parts as independent papers.	We will discuss the suggestion with the editor.
Specific comments: Page 328, line 15: 1325Pg, to be consistent with the number in the conclusion.	This will be changed.
Page 330, line 6-8: WISE(v.2) was once publicly available. But it is now replaced by the WISE (V.3.1), which is available online and includes all profiles of previous versions.	Thank you for the clarification.

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Page 331, line 17-20: The description is not precise. Shangguan et al. (2014) used three soil profile database directly, i.e. China, WISE(V.3.1) and NCSS of US, and they also used estimates (produced by others) based on local soil profiles and soil maps from ESDB (Europe), SOTWISE (various regions), GSM (US), SLC (Canada) and ASRIS (Australia).	We agree, the description will be revised.
Page 331, line 17, line 23: change “Shanguan et al.” to “Shangguan et al.”	The typo will be corrected.
Page 332, line7: It should be “equal to or smaller than”.	Correct.
Page 332, line9-29: Almost all the soil profiles in WISE do not have a real soil depth (or depth to the bedrock or R horizon), but have the observation depth. These soils are very likely much deeper than the recording depth in WISE. Only 189 profiles in WISE have an R horizon (some have a SOC great than 0, which seem to be errors). As a result, the overestimation for Cryosols, Podsols, and Umbrisols might not happen, especially for Cryosols and Podsols.	Good point. We will include this aspect in the revision.

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Another point is that the soil depths in both HWSD and ISRIC-WISE are an underestimation of the true soil depth in almost all cases. It should not be named as “soil depth” in the paper. You may use the term “effective” soil depth in ISRIC-WISE (v3.0), or use the term “reference soil depth” in the HWSD.	We agree and will change the wording in the revision.
Page 333, line7 and et al. : kg cm^{-3}	The numbers given are correct in kg/dm^{-3}.
Page 333, line9: Why these regressions and the R2 are different from the authors previous report, i.e. Hiederer and Kochy (2011)? They are both based on WISE3.1. The difference of BD is 0.139 for the topsoil using the regressions when OC = 12%.	Based on your comment we reviewed our documentation. The regressions in this paper are in fact based on the SPADE/M2 database. Although the different equations produce divergent global mass results, the following correction of BD of Histosols, diminishes the difference to 1Pg. This will be mentioned in the revision.
Page 333, line 9: ln(Corg *100)	Actually, $C_{org} \cdot 10$ was used, i.e. expressed in g/100 g soil or %. This will be indicated in the revised text.
Page 335, line 13-17: It lacks a soil profile database with WRB classification information to develop a WRB based soil property maps. Taxonomy reference between WRB and FAO will increase the uncertainty.	We appreciate the clarification and will add the information to the text.

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Page 335, line 23-24: the stock was estimated based on the polygon based soil map except Australia, not after rasterization.	The text will be corrected in the revision.
Page 336, line 23: 13.4 Mm2	The text refers to the soil area, so 13.1 Mm^2 is correct in this sentence.
Page 338, line 6-7: in the other regional stocks and the stocks of soils deeper than 1 m.	The phrasing will be corrected in the revision.
Table 1: The authors used some definition which is not consistent with most literatures and may bring some confusion to the readers. I suggest using the general meanings of a terminology in the literature, instead of creating some new terms. Like the following: Content: organic carbon mass/soil dry mass; ??: organic carbon mass/soil volume (I do not see any use of this term in the paper); SOC density of a layer: organic carbon mass/soil volume$\times$depth$\times$(1-fractional volume of rocks, coarse roots, and ice); SOC density of all layers:areal density of fine soil integrated over all layers to a specified depth: SOC stock: stock integrated over a specified area.	We expect the paper to be of use in the soil science community but also in the carbon cycle and climate modelling community. We therefore chose to use terminology that is unambiguous and close to basic physical definitions. We understand that this decision is not optimal for both scientific communities. The definition of "content" will be deleted in the revised version as it is not used.

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Table 3: Tables should be self-explainable. Please explain what is the hist/soil and it is not explained in the text.	Hist/soil: fraction of soil area covered by Histosols. This will be added in the revised text.
Table 3: What do you want to show with so many figures in table 3, while you only mentioned the total numbers in the text? This table needs further interpretation or you may delete it.	The percentiles provide information about the distribution of C in different categories of permafrost. The reason for giving the percentiles is explained in section 4.2.
Table 5: It is better to show the percentage of the overlapping. Maybe use the overlap area/(GLWD +GLCC), and 50% indicate completely identical.	Both approaches have their merit. We present absolute values to emphasize the significance of the overlap. In the revised text we will add the areas of each category so readers can calculate the percentage of overlap if desired.

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