

Interactive comment on “Soil carbon stocks and their variability across the woodlands of peninsular Spain” by E. Doblas-Miranda et al.

Anonymous Referee #1

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The paper by Doblas-Miranda et al. presents new data for soil organic carbon (SOC) stocks distribution on the Iberian Peninsula. Soil is the largest terrestrial pool of organic carbon, and the dynamics in this pool is highly uncertain. New and more reliable SOC data is essential for both improving the existing SOC dynamics models and gaining insights about SOC dynamics. Authors compiled SOC stocks data from multiple studies and extrapolated SOC stocks across areas with missing data using a generalized linear model (GLM). The authors illustrated that the data in their paper differed from SOC stocks in IGBP-DIS global dataset, and claimed that the data differed from data in other sources, however they compared SOC from top 100 cm to SOC in top 50 cm and 30 cm. In my view, this should be corrected, e.g. by using a paper by Jobbagy and Jackson (2000) to transform the top soil estimates given in other studies to top 100 cm SOC estimates in order to make the comparisons meaningful.

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In their paper Doblas-Miranda et al. also looked for the variables, which would explain the spatial variation of SOC across the Iberian Peninsula. The most significant variables were parent material, consistency, vegetation cover type, temperature, elevation, and precipitation. It was not clear from the methods description whether the interactions between the explanatory variables were tested and included into GLM. To speculate, the elevation and annual precipitation amount are likely positively correlated, hence, if the interactive effect between precipitation and elevation is included, elevation will no longer be a significant explanatory variable. Similarly, temperature and precipitation may be negatively correlated, which could decrease the significance of temperature as an explanatory variable. Vegetation cover is likely correlated to temperature and precipitation (Holdridge life zones), hence, to assess the true effect of land cover type on SOC stocks, the interaction effect between land cover time and climate variables should be subtracted. Because of multiple possibilities of interactions between the explanatory variables, I do not believe that the second portion of authors' results and discussion (controls over spatial distribution of SOC) is in its best shape, as it is not clear whether the effect of the explanatory variable is true or based on its correlation with other explanatory variable. If the authors did test for interactions between the explanatory variables, I would strongly suggest to include that portion in section 2.3.

The manuscripts needs some polishing, especially in the Materials and Methods section. Below are some suggestions: P5L1: please give some examples for the variables P5L15: I think it would better be “Spanish soil science journals” P5L21: “may” should be changed to “might” and “necessarily” should be deleted P7L1-3: I think it would be appropriate to give an example of such visual estimation P7L13: change “for a” to “in the”; change “it can exist” to “there may be” P7L14: change “it” to “this” P7L16: what does “representative profile” mean? How did you choose it? P7L17-19: not clear what the portion in parentheses is supposed to mean P7L19: Please provide more details on the analysis P11L7: “relatively” to what?

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Jobbágy, E. G. and R. B. Jackson (2000). "THE VERTICAL DISTRIBUTION OF SOIL ORGANIC CARBON AND ITS RELATION TO CLIMATE AND VEGETATION." *Ecological Applications* 10(2): 423-436.

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