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Interactive comment on “Global distribution of soil organic carbon, based on the Harmonized World Soil Database – Part 2: Certainty of changes related to land-use and climate” by M. Köchy et al.

Anonymous Referee #1

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General Very important paper which attempts to analyse the uncertainty associated with climate and land use change and their effects on SOC stocks. Importantly, the analysis includes anaerobic soils and land use change which is crucial, difficult to assess due to our limited understanding and lack of supporting data, and is therefore often overlooked. One assumption made in the paper that ‘The first scenario, “limited NPP”, represents a change in productivity caused by temperature and precipitation alone, which could be similar to the net effect of CO₂ fertilization and nutrient-constrained growth’ needs more justification. Otherwise the paper is sound and I have just a few points which need clarification (listed below).

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Abstract Good summary of the paper but a bit more information about the methods should be given in the abstract if possible.

Introduction The limitation of the CO₂ fertilisation affects by nutrient availability is mentioned in the introduction, but limitation due to elevated CO₂ and temperature interactions should also be mentioned in the second paragraph of the introduction. Pg 366 line 4 give some examples of some of the impacts. Pg 366 give the year you are referring to when you say 'present day' to make things easier for the reader in the future.

Methods Somewhere in the methods the uncertainties associated with working at a 5° grid should be acknowledged. What was the rationale behind choosing harvest index as a land use parameter? When was a steady state reached in the organic soils? Page 368 Ln 327 clarify what is meant by 'not too small' values of fmf. Although the reader is referred to the supplement clarification is needed here. Pg 369 mention the possible limitation of the CO₂ fertilisation effect by elevated temperature. The authors say the limited NPP scenario, 'could be similar to the net effect of CO₂ fertilization and nutrient-constrained growth' please explain why you think this. Fig. 2 in Paul et al., 2002 should be reproduced in the supplemental material if possible. Page 372 lines 2-4, does this assumption hold for non-deciduous forests? Results p375 Ln 21 Greatest sensitivity to fmf is a reasonable finding as fmf covers a wide range of parameters. p376 Ln 7-9 interesting finding. End of section 3.2 on page 376, what were the findings for areas of pristine forest converted to grasslands? This is always a controversial land use change when looking at long term SOC stock changes.

Pg 378 lines 15 - 20, this point highlights the caution that should be exercised when making any local inferences from a global analysis. Land use conversion from native land to cropland may show an increase in SOC, but, as the authors point out, this is assuming inputs are higher than under native vegetation and the question arises as to where those inputs come from in hot desert environment. P379, last 3 lines, interesting point. More data is needed on SOC stocks in shrublands.

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Discussion Would have been good to mention in the discussion CO2 fertilisation effects and limitations of this by temperature. Not surprising that the greatest uncertainty is associated with carbon rich soils, presumably because of lack of understanding of the impacts of key variables on anearobic decomposition. End of page 382, linking socio-economic models to vegetation models is still in its infancy in many respects and more work needs to be done on understanding the socio-economic drivers of vegetation change at the global level.

Interactive comment on SOIL Discuss., 1, 363, 2014.

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