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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.

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Reviewer 1: Reviewer's comment	**Authors' comments** - page and line numbers refer to the reviewed version
	Thank you for the review.
Good summary of the paper but a bit more information about the methods should be given in the abstract if possible.	We will add a sentence after line 8: "In principle, controlling factors were discretized into classes, each class associated with a probability and linked to an output variable. This creates a network of links that are ultimately linked to a set of equations for carbon input and output to and from soil C pools."
'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.	This seems to be a misunderstanding. The model behind the 'limited NPP scenario' does not include any CO2 effect, so any increase of NPP is only due to an increase in temperature or precipitation. This absence of a CO2-fertilization could be similar to a limitation by nutrients.
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.	There may be a misunderstanding (see above). Limitation due to elevated CO2 and temperature interaction is addressed in the text beginning page 365-line 8 ("The large variation . . . ").
Pg 366 line 4 give some examples of some of the impacts.	These are events like fire, and erosion, but also insect outbreaks, erosion, landslides, windthrow and flooding.

Pg 366 give the year you are referring to when you say 'present day' to make things easier for the reader in the future.	We take 2010 as the base year for calculations. Clarification will be inserted in the revised text, line 25.
Somewhere in the methods the uncertainties associated with working at a 5° grid should be acknowledged.	We use a 30 arc minute grid (0.5°), p367-line4. The sub-pixel variation is now addressed in text inserted after p367-line 5 and page 369-line19: "We acknowledge that variation in soil, vegetation, environmental, land use and other factors controlling SOC decomposition exists within a pixel. This variation is partly included in our analysis but we cannot quantify its contribution to overall uncertainty."
What was the rationale behind choosing harvest index as a land use parameter?	This is a typo in page 367-line 4. The 'harvest factor' is the closest relationship between land use and the C cycle in our opinion and, as explained in page 370-line 25, is practical for estimating the effects of future land use on NPP.
When was a steady state reached in the organic soils?	We never explicitly calculated when a steady state was reached - we changed the text in page 367-line 23 to prevent a possible misunderstanding. Before we implemented the formula in the model, we applied it in a spreadsheet and, picking reasonable values, we observed that a steady-state ($\|\Delta C\|$ < 0.1 kg/m^2/yr) was reached within 75 years in most cases.

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Page 368 ln 27 clarify what is meant by 'not too small' values of fmf. Although the reader is referred to the supplement clarification is needed here.	We can draw the line at fmf=0.1.
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' [page 370, line 5] please explain why you think this.	This seems to be a misunderstanding. The model behind the 'limited NPP scenario' does not include any CO2 effect, so any increase of NPP is only due to an increase in temperature or precipitation. This absence of a CO2-fertilization could be similar to a limitation by nutrients.
Fig. 2 in Paul et al., 2002 should be reproduced in the supplemental material if possible.	The relevant part of the figure is also available in this online document (Paul 2001): http://www.kirschbaum.id.au/NEE_Workshop_Proceedings.pdf. Maybe this URL would be a sufficient hint.
Page 372 lines 2-4, ["that leaf litter and fine root litter on the one hand and dead wood (above- and belowground) on the other hand contribute on average equal proportions to litter input entering forest soils"] does this assumption hold for non-deciduous forests?	At a global scale and given the heterogeneous methods used in the reports cited in the literature, we believe that our generalisation is backed by observations and also apply, grosso modo, to coniferous forests.
Results p375 ln 21 Greatest sensitivity to fmf is a reasonable finding as fmf covers a wide range of parameters.	(no comment)

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p376 Ln 7-9 interesting finding.	(no comment)
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.	On the "global scale" the assumed probability of transition from tropical pristine forest to pasture was set to a low value (0.01), hence we did not want to draw conclusions from a small sample. In individual pixels the response was more drastic and is referred to on page 380-line 5. The sentence applies not only to cropland but also pastures (see Supplement 5, last lines of Table S5.2) and will be corrected.
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.	(no comment)
P379, last 3 lines, interesting point. More data is needed on SOC stocks in shrublands. Would have been good to mention in the discussion CO2 fertilisation effects and limitations of this by temperature.	(no comment) (see our comment above)

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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.	(no comment)
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.	We agree.

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