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Interactive comment on "Moderate topsoil erosion rates constrain the magnitude of the erosion-induced carbon sink and agricultural productivity losses on the Chinese Loess Plateau" *by* J. Zhao et al.

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Reply to comments of referee 2. Please not that page, line, figure and section numbers refer to the revised manuscript with tracked changes.

1. Abstract is too long, please revise to make it shorter, and focus on explaining what you did in this study, major findings, and implications.

Reply: we kindly refer this comments to the response of the question 1 of the short comments which asked a similar question. We shortened the abstract as much as possible without loosing additional information.

2. Abstract, Statement on lines 17-20 presumes that delivery of eroded sediments into Bohai sea leads to no or little loss of eroded C during or after erosional transport from the CLP. It is hard to take that statement at face value without any supporting data.

Reply: this statement was indeed based on the observed data at two gauge stations: one located at the outlet of Chinese loess plateau and named Huayuankou station; another was located at the estuary of yellow river and named Lijin station (Figure 1 and 9 in MS). In 1950s condition, we estimated that soil erosion mobilized, in total, ca. 8.21 $\pm$ 3.44 Tg C yr-1 which consistent with the observation number at huayuankou station (7.95 $\pm$ 1.64 Tg C yr-1). The measured carbon exported by yellow river to bohai sea was 6.96 $\pm$ 1.44 Tg C yr-1. Therefore, comparing of the carbon delivery at these two stations suggested that at 1950s condition a geomorphological equilibrium existed whereby the amount of sediment and carbon exported to the Bohai sea was similar to the amount of sediment and carbon eroded on the CLP. We provided more detail about the number of sediment yield and carbon mobilization at two station in Section 3.4, Section 3.5 and Figure 9 of revised MS.

3. Abstract, Lines 24-27: this statement can have dangerous implications and is wrong. Of course anthropogenically accelerated erosion is a threat to agricultural productivity (and more importantly soil health). Addition of fertilizers to maintain agricultural productivity doesn't eliminate the threat, it just addresses part of the problem.

Reply: While we do agree that the statement can be better formulated, one cannot escape from the conclusion that agricultural productivity on the CLP has dramatically increased, despite a severe erosion problem. The key reason for this is, without any doubt, the use of mineral fertilization. We have now reformulated the sentence to make this more clear (Page 3 line 30-37).

4. Line 26 page 14983 . . . here the authors make a statement (also in abstract) that the maximum of the erosion-induced C sink is set by the amount of SOC mobilized. I

would argue that this is not necessarily always the case. An exception is a case where erosion of topsoil from hillslopes leads to large increases in net primary productivity and hence C input to soils in depositional sites. The magnitude of the increased input of new carbon to the soil in the depositional site does not necessarily have to be set by the amount of C eroded, but rather by the interaction of a range of soil physico-chemical variables and micro-climate in the depositional sites. This is a major point that the authors highlight in this work, and needs to acknowledge that it is not a universal truth. Please see the work of Berhe et al 2007 (Bioscience) for how changes in input of C to the soil pool AND decomposition rates of eroded and in situ C at the eroding and depositional sites determines the magnitude of the erosion-induced C sink.

Reply: We do agree with the comment of the reviewer that the magnitude of the C sink is determined by a combination of processes and that, in principle, it is be possible to have a C sink that is larger than the amount of C mobilized. We now mention this possibility in the text while referring to the paper mentioned above. However, we do believe that such a situation is relatively unlikely, especially under the conditions on the CLP, and have modified our text to briefly explain this. Our reasoning is based on the following. The dynamic replacement of mobilized C at eroding sites is unlikely to be higher than the amount of C removed by erosion. Indeed, one may expect that NPP at eroding sites will be negatively affected by erosion. Furthermore, accelerated erosion leads to lower equilibrium C stock at eroding sites because C is continuously being laterally removed by erosion. Hence dynamic replacement rates are generally estimated to be significantly smaller than mobilization rates (see Dialynas, Yannis G et al., 2016; Van Oost et al., 2007) although full replacement is also possible (Li et al., 2015). When considering the depositional sites, the import of (eroded) C should then not only lead to the full preservation of this C but also to additional NPP so that burial efficiency would exceed 100%. Studies have shown this to be unlikely under conditions of accelerated agricultural erosion (Wang et al., 2014, 2015). Finally, there is the C exported to the sea: a recent publication (Leithold et al., 2016) demonstrates that here C burial efficiency is nearly always below 100%. Of course, there is always the possibility that there is compensation, e.g. that the loss of C due to incomplete dynamic replacement and mineralization in the ocean is more than compensated for at depositional sites or vice versa. However, given the fact that available data suggest that all these processes (dynamic replacement, C burial on land and C burial at sea) generally have an efficiency that is well below 100% in terms of C preservation, such a situation is unlikely to occur.

5. Results and methods: I applaud the authors for compiling such database. But, the justification for up scaling data derived from relatively small plots to an entire region is not well explained. How can we be sure that the extrapolations that are used to arrive at the different estimates are indeed justified? Is it possible that some in the discrepancy of the estimates that they are seeing (discussed in the supplemental files) partly a result of an unjustified up scaling approaches? In addition to presenting better justification for the up scaling approaches the authors are advised to avoid the temptation to over generalize their findings about erosion rates, or contribution of different sediment sources to the regional sediment or carbon budget. Whenever possible, please present limitations of the approaches employed in this study.

Reply: we do agree the review's comments that the up scaling estimation of soil erosion was subject to large uncertainty. However, as described in the text, we did make an honest attempt to quantify these uncertainties as accurately as possible (see section 2.3). The reviewer may wonder why overall uncertainty is not larger than it is: this is due to fact that we do use average (either of a large number of plot years or over a large area): this averaging dramatically reduces the 'random' error component, i.e. the error due to variations in drivers which are not incorporated in our model: we provide references on earlier work where this was demonstrated (Van Rompaey and Govers, 2002; Van Rompaey, 2003). We now integrated this discussion in the main text rather than the supplement so that this is easier to follow. We do accept the point that some bias is still possible but, on the other hand, we do believe that our estimates are the first ones for which (i) uncertainty has been calculated based on the variability of true data and (ii) for which a true validation has been carried out with independent data. The model we used to upscale topsoil erosion rate from the plot scales (erosion plot) to the regional scale was validated by the comparison with 40 independent measures of erosion rata of slopes by using 137Cs (see section 2.4 and Figure 7). We found very acceptable results, with no evidence of any systematic bias (Figure 7). Therefore, the model itself was robust. For the regional scale, we compared our estimated total sediment yield with observed sediment yield at the gauge station located at outlet of CLP. The comparison indicated that our estimation of sediment yield had a good agreement with the observed sediment yield (see section 3.4 and Figure 9).

6. Soil eroded from different landform positions and soil depths not only has different concentration of C, but it also differs in the composition of organic matter, stability and stabilization mechanisms of the eroded organic matter once the sediments arrive at different depositional environments. Moreover, the type of depositional setting that eroded soil organic matter is deposited on has tremendous influence on how erosion can contribute to carbon sequestration. These considerations didn't receive due consideration in this manuscript. The authors are strongly advised to further discuss the implications of source of eroded C and type of depositional landforms (see works of McCorkle et al. 2016 Chemical Geology, Hu et al. 2016 Biogeochemistry, Berhe and Kleber 2013 Earth Surface Processes and Landforms, Berhe et al. 2012 JGR-B)

Reply: We fully agree with the reviewer that this is a valid point and we now address it in the discussion, using the references given above (and some others) (see page 20, line 521-536).

7. The way it is currently presented, the discussion on N and P losses (section 3.5) comes across as an after-thought. If the authors wish to keep this section, they should highlight this issue more in the introduction section.

Reply: We fully agree and adjusted the introduction section, by expanding the section on the relationship between nutrients and agricultural productivity (page 5, line 77-83) and by adding a final sentence stating the evaluation of the effect of erosion on the nutrient balance as one of our objectives (page 7, line146-147).

Reference

Dialynas, Y. G., Bastola, S., Bras, R. L., Billings, S. A., Markewitz, D. and Richter, D. deB.: Topographic variability and the influence of soil erosion on the carbon cycle, Global Biogeochem. Cycles, 30, 644–660, doi:10.1002/2015GB005302.Received, 2016. Leithold, E. L., Blair, N. E. and Wegmann, K. W.: Source to sink sedimentary systems and the global C-cycle: A river runs through it, Earth-Science Rev., 153, 30–42, doi:10.1016/j.earscirev.2015.10.011, 2016. Li, Y., Quine, T. A., Yu, H. Q., Govers, G., Six, J., Gong, D. Z., Wang, Z., Zhang, Y. Z. and Van Oost, K.: Sustained high magnitude erosional forcing generates an organic carbon sink: Test and implications in the Loess Plateau, China, Earth Planet. Sci. Lett., 411, 281–289, doi:10.1016/j.epsl.2014.11.036, 2015. Van Oost, K., Quine, T. A. a, Govers, G., De Gryze, S., Six, J., Harden, J. W., Ritchie, J. C., McCarty, G. W., Heckrath, G., Kosmas, C., Giraldez, J. V, da Silva, J. R. M. and Merckx, R.: The impact of agricultural soil erosion on the global carbon cycle., Science, 318(5850), 626–629, doi:10.1126/science.1145724, 2007. Van Rompaey, A. J. J.: validation of soil erosion estimates at European scale, 2003. Van Rompaey, A. J. J. and Govers, G.: Data quality and model complexity for regional scale soil erosion prediction, Int. J. Geogr. Inf. Sci., 16(7), 663–680, doi:10.1080/13658810210148561, 2002. Wang, Z., Oost, K. Van, Lang, A., Quine, T., Clymans, W., Merckx, R., Notebaert, B. and Govers, G.: The fate of buried organic carbon in colluvial soilsâĂŕ: a long-term perspective, , 873–883, doi:10.5194/bg-11-873-2014, 2014. Wang, Z., Oost, K. Van and Govers, G.: Predicting the long-term fate of buried organic carbon in colluvial soils, Global Biogeochem. Cycles, 29, 65–79, doi:10.1002/2014GB004912.Received, 2015.

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