Response to the comments

The two reviewers' comments were fully incorporated into the revised MS accordingly. The text of the MS was revised by: i) providing more details of the model modifications and the observation methods; ii) reorganizing and rewriting the Materials and methods section; iii) adding the consideration of the productivity in the scenario analysis and the corresponding results and discussion. Furthermore, we recompiled the Supplementary materials by adding Table S1 to present the soil properties of different soil profiles for different land use types and Table S2 to list the information on the observation data applied for model calibration and validation. Last but not least, the results of the simulated effects of no-tillage on soil erosion (i.e., Fig. S5) were added.

Reply on Referee #2

Synopsis

The authors extend the functionality of the existing hydro-biogeochemical land use model CNMM-DNDC with new routines to simulate soil erosion and associated transport of C, N and P in various fractions, which presents a novel enhancement of an existing agro-ecosystem model. The enhanced model is tested for a lysimeter plot and a small catchment outlet showing largely good agreement with measurements. A subsequent scenario analysis reveals sensitivities to changes in precipitation, temperature, and land use whereas expectedly the highest sensitivity is found for the first.

>> Thanks.

General assessment

In principle, the study presents an interesting model enhancement and its evaluation for a case study, combining soil organic matter and nutrient cycling, soil greenhouse gas emissions, and water erosion. However, the manuscript has several shortcomings that render it challenging to assess. First of all, the authors apparently calibrated the model for the study region, but this process is not well recorded in the methods except for few scattered statements, which renders it hard to follow. Also the choice of the erosion model itself is not fully clear as detailed below. Furthermore, it is not clear what capabilities the original and enhanced models have. The authors may consider adding a schematic of key modules included in the existing and enhanced model to provide readers with an overview. Last but not least, the model evaluation methods need to be reassessed and/or very well justified as zero-intercept regressions can be quite misleading. I hence recommend the authors thoroughly revise their manuscript focusing on clear and consistent descriptions of all methods and results.

>> Fully agreed and revised the MS accordingly.

    i)   we reorganized and rewrote the section of 2.2 Model modifications to make clear about the model calibration description. Please see changes in Lines 166−211 in the revised MS.

Specific comments

L37 and other places: The authors use land use change as a scenario to mitigate erosion. What the authors do not consider is the study of field management practices to control erosion. This is the far more common approach to control erosion and implemented in various models such as USLE, RUSLE and their derivatives (see e.g. https://doi.org/10.1016/j.envsci.2015.03.012). The authors need to clearly state why still this erosion model was selected and how they expect management options would affect their results.

>> Revised.

We added some results and discussion related to the tillage scenario. Please see changes in Lines 552−556 in the revised MS and the Text S2 in the revised Supplementary.

More descriptions about the reason why we chosen this erosion model were added. Please see changes in Lines 166−169 in the revised MS and the Text S2 in the revised Supplementary.

L78: To my knowledge, the SWAT model includes USLE and MUSLE not RUSLE. Check again what the cited model can do and consider better describing the state-of-the-art.

>> Revised. Please see changes in Lines 71−72 and 84 in the revised MS.

L143-145: First, is there a reference for this statement? Second, the authors earlier justify their choice of the ROSE model based on the inclusion of the three processes (detachment, transport, sedimentation (L72ff)) in equilibrium but state here that they omit two of them. So why not take another model that as well neglects some processes but may have other advantages such as erosion control management?

>> Revised. We added the reference for the statement what you mentioned. Please see changes in Line 161 in the revised MS. And more descriptions about the reason why we chosen this erosion model were added. Please see changes in Lines 166−169 in the revised MS and the Text S2 in the revised Supplementary.

L158: Unclear what the growing index is. Is this similar to LAI? Why not take LAI directly as is done for natural vegetation? Also, does the growing index vary among

crops? Differences in soil cover can be substantial between a wide row crop such as maize and dense grass-like crops such as wheat. Commonly, erosion models use crop type-specific coefficients that reflect LAI and plant density (e.g. http://dx.doi.org/10.1016/j.landusepol.2015.05.021) or aboveground biomass (e.g. https://doi.org/10.1098/rstb.1990.0184).

>> Revised. We added some explanation about the C_v values of different land use types. Please see changes in Lines 161−169 in the revised MS.

L183: How was the model calibrated? If this was done systematically, e.g. using auto-calibration, the parameter ranges, method, etc. need to be stated. Also, were single parameters calibrated one by one or in combination?

>> Revised. We reorganized and rewrote the section of 2.2 Model modifications to make clear about the model calibration description. Please see changes in Lines 166−211 in the revised MS.

L205: What field management practices are considered and how are they parameterized? Even if these and various other data are the same as in Zhang et al. (2018), they need to be stated here to facilitate understanding of the study. An overview of the models' functionalities would be helpful in this regard.

>> Revised. The inputs of the field management practices for the CNMM-DNDC and the schematic of the existing and upgraded modules in the CNMM-DNDC were added. Please see changes in Lines 257−259 in the revised MS and Fig. S1 in the revised Supplementary.

L228: The scenario section would profit from a table synthesizing the scenario definitions. This may also allow for shortening this quite lengthy section.

>> Revised. Please see changes in Table S1 in the revised MS.

L267: Tillage comes here at some surprise as it is not mentioned earlier to be included in the model. Later, the results are only briefly mentioned. Again, it would be good to have an overview of the scenarios and how they are implemented.

>> We added some results and discussion related to the tillage scenario. Please see changes in Lines 552−556 in the revised MS and the Text S2 in the revised Supplementary.

L277: Why was a zero-intercept regression used and not a regular one? It's for most cases strongly discouraged to force a regression through zero, which may also substantially affect the calculation of the correlation coefficient.

>> Revised. The regular linear regressions were applied to conduct the model evaluation instead of the original zero-intercept linear regressions. Please see changes in subsection 2.5 and 3.2 and Table 1 in the revised MS.

L292ff: This seems to be part of the calibration methods and should accordingly go to the methods section, ideally a new sub-section focusing on calibration.

>> Revised. We reorganized and rewrote the section of 2.2 Model modifications to make clear about the model calibration description. Please see changes in Lines 166−211 and 343−344 in the revised MS.

L367: It's surprising that residential areas are a major source of soil erosion. The authors should explain and show more thoroughly why this is the case here.
>> Revised. We added some explanation about the residential areas acting as a major source of soil erosion. Please see changes in Lines 506−509 in the revised MS and the Fig. S6 in the revised Supplementary.

L486: Earlier you mention tillage scenarios; here you state that tillage was not considered. So which of the two is correct?
>>Corrected. Please see changes in Lines 545−549 in the revised MS.

Table 1: Columns Operation and Size require explanation.
>> Revised. Please see changes in the footnotes of Table 1 in the revised MS.

Figure 4: It's unclear why the third panel (PN) is connected with lines to March in the central panel. If this is just about showing that the third panel refers to PN, it would be better to remove the lines and simply mention this in the caption.
>> Revised. Please see changes in Fig. 4 in the revised MS.

L909: I'm not familiar with the term "humad". Do you mean litter? Please make sure to only use terms that are common in the related literature.
>> Response and revised. We usually considered the word "humad" as the labile or resistant humus. To improve readability of the MS, the term "humad" was changed to the common one "labile or resistant humus" according to the reviewer's suggestion. Please see changes in Lines 210−211, 372 and 938 in the revised MS.

Figure 5: Top right x-axis unit [ha^{-1}] is confusing at first sight. Consider adding the numerator unit as well.
>> Revised. Please see changes in Fig. 5 in the revised MS.

Figure 6: Unclear what the line colors (green and violet) refer to. Seems like they overlap while the regressions should be mutually exclusive?
>> Revised. We added the explanation about the green and violet line. Please see changes in the footnote of Fig. 6 in the revised MS.

Figure 7: The authors explain what the circles in the top right half mean (L937) but not what is shown in the corresponding scatter plots of the bottom left. Do these show the data points relating to the regression coefficients? Please include in the caption.
>> Revised. Please see changes in the footnote of Fig. 7 in the revised MS.

>> Response and revised. We also double-checked the whole MS thoroughly and adjusted the inconsistent expressions and terms throughout the revised MS.

L21 "subsequent" should be "associated"
>> Revised. We changed the mentioned word "subsequent" to "associated" throughout the MS. Please see changes in 21, 46, 87, 104, 106, 113, 147, 236, 473, 500, 549, 568, 570, 573, 575 and 577 in the revised MS.

L26: The model name needs to be spelled out
>> Revised. Please see changes in Line 26 in the revised MS.

L31 and various other places: "credible" is not a scientific term.
>> Revised. The unscientific term "credible" was changed to the scientific term "acceptable". Please see changes in Lines 32, 328 and 341 in the revised MS.

L33: "larger" should be "higher"
>> Revised. Please see changes in Line 33 in the revised MS.

L40: "may become a" should be "renders it a potential"
>> Revised. Please see changes Line 42 in the revised MS.

L50: "deteriorate" does not fit here
>> Revised. We rewrote the mentioned sentence. Please see changes in Line 52 in the revised MS.

L62: "water-reduced" should be "water-induced"?
>> Revised. Please see changes in Line 64 in the revised MS.

L91 and various other places: "complicated" should better be something like "complex"
>> Revised. Please see changes in Lines 88, 93, 98 and 111 in the revised MS.

L191: "reliable" should be "labile"?
>> Revised. Please see changes in Lines 209−210 in the revised MS.

L192: "humads" is not in the dictionary. Is this "litter"?
>> Response and revised. We usually considered the word "humad" as the labile or resistant humus. To improve readability of the MS, the term "humad" was changed to the common one "labile or resistant humus" according to the reviewer's suggestion. Please see changes in Lines 210−211, 372 and 938 in the

revised MS.