03. November 2016

Response to ‘Editor's comment on

F. Langerwisch, A. Walz, A. Rammig, B. Tietjen, K. Thonicke, and W. Cramer.
“Deforestation in Amazonia impacts riverine carbon dynamics”
submitted to Earth System Dynamics, esd-2015-66.’

Dear Prof. Christian Reick,

Thank you very much for your very detailed and constructive comments and suggestions. In the following we will describe how we addressed your comments and questions (replies to your remarks are inserted in bold after each comment or question) and how we changed the manuscript accordingly, providing line numbers. The respective changes are highlighted in green in the revised manuscript text to separate them from the text in yellow which mark the previous revision.
We hope to have adequately addressed your concerns for the benefit of the manuscript. Should further information be missing or something being unclear, please do not hesitate to contact us, we would be happy to address them to work towards a successful finalization of the review process.

Best wishes,
F. Langerwisch on behalf of all co-authors.

Dear Mrs. Langerwisch,

thank you for your revised manuscript. The referee's comments are well accounted for. Nevertheless, I think that the scientific relevance of the paper could be improved.

Currently the paper reads like an engineering study, where one is only interested in the outcome, but not in improvement of understanding. Such papers can be published in ESD, but as an editor such papers are not my first choice. Modeling terrigenous carbon losses from inundation is a quite new subject. Hence there are large uncertainties involved in how to model the relevant processes and one cannot expect that with your study the last word has been spoken, particularly with respect to the numbers obtained from your simulations. But your insights in understanding the relevant processes may well survive future developments of the subject. Therefore, I would very much appreciate if this process insight is well explained in the paper so that your study stays worth reading even after new studies have appeared that may revise your numbers.

Adding more explanation would be helpful in two directions. First, the new aspect beyond the contents of your just published study in ESD is deforestation. To model this, you developed quite some process understanding how deforestation may affect the terrigenous carbon losses. It would be very helpful, if your process understanding could be made explicit in the paper. Currently there is no **introductory passage in the paper explaining how you model deforestation and why you model it that way**
Please see our Reply 1, below.

There is one remark in lines 244-246 where you say that you assume **15% soy bean and 85% pasture**
Please see our Reply 2, below for beef production on the deforested area, but it is left to the reader to imagine what this means for your study. And in section 4.1 "Shortcomings" you add some hints that would have been useful to be introduced early (Reply 2, below) in the paper, and not as shortcomings, but as features of your model.
Reply 1: We added more information on how deforestation is implemented to LPJmL to the section 2.1.1. For more details see reply below 'What is specific about agricultural lands for terrigenous carbon losses into rivers compared to natural grasslands or forests?' and 'What happens to the carbon cleared upon deforestation?'
Reply 2: We explained it in more detail in the reply to 'How do the particular types of agriculture that you assume (soy bean farming, ranching) enter your calculations?'

For me as a reader of your paper it would be very helpful to add answers to the following questions early in the paper when introducing your model:

- What is specific about agricultural lands for terrigenous carbon losses into rivers compared to natural grasslands or forests?

Reply: Deforestation reduces living biomass when land-cover changes and over the course of several years less litter and soil carbon is stored on agricultural land (Bondeau et al., 2007) that can then be exported. On agricultural land occurs a constant removal of organic material during harvest. In contrast to forests, carbon exported from natural grasslands is comparable to pasture. LPJmL simulates the same type of grass on both vegetation types (C4 grass). The effects on the carbon balance only differ in the sense that on pasture 50% of the aboveground living biomass is removed regularly, while on the natural grasslands this biomass remains on site. We added more detailed information describing the processes on agricultural land in LPJmL to the Methods Section 2.1.1 (ll. 161).

- How do the particular types of agriculture that you assume (soy bean farming, ranching) enter your calculations?

Reply: To estimate the effects of deforestation and land-use change on the amount of carbon that is exported to the river it is crucial to first assess the amount of carbon that is lost when forest is replaced by managed grassland or annual agriculture and second to simulate carbon storage and fluxes of the new land-use types. Soy bean farming and pasture leave different amounts of litter carbon on the site. In the LPJmL model, during soy harvest a maximum of 30% of the aboveground soy biomass, representing the beans, are removed as harvest every year. The remaining aboveground biomass as well as all belowground biomass is left on site and enters the litter pool. Managed grasslands are harvested annually as well, but always 50% of the aboveground biomass is removed. The remaining aboveground biomass and the total belowground biomass enter the litter pool. Therefore, harvest and the low amount of carbon stored in the crops and grasses explain why less carbon can potentially be exported to the river. In each cell a common litter and soil pool exists, which contains the carbon from the natural vegetation stand and from the agricultural stands. The amount of exported carbon during inundation is calculated based on the inundated area and the amount of available carbon in this common pool. Therefore, the export of litter and soil carbon to the river during inundation does not differ between natural vegetation and agricultural land.

• In line 390 you talk about "deforestation-induced increase in discharge" -- how is this modeled?

Reply: The increase of discharge which follows deforestation (as reported by Costa et al., 2003) is included in the model via the following approach/function: Forests are able to deplete the available water in the soils more effectively, compared to grasses or crops, because they have a higher amount of (belowground) biomass. Therefore, an increase in cropland at the expense of forest cover leaves more water in the soil. The calculation of discharge in LPJmL takes the water saturation of the soils into account. This is used to calculate which amount of water is not able to enter the soil anymore and therewith generates runoff, which builds the discharge (Rost et al., 2008). In LPJmL the plant types 'tropical broadleaved evergreen tree' and 'soy' have their roots in approximately the same layer of soil. But since the tree has far more biomass in this soil layer it is able to take up more of the available water. The soy plants take up less water and leave therefore more water for runoff and discharge. If plants leave more water in the soil, the discharge is therewith increased. LPJmL simulates these related processes.

• What happens to the carbon cleared upon deforestation?

Reply: The land-use scenario which is an external input to the LPJmL model defines for each grid cell in each year the proportion of land use. Natural vegetation is then simulated on the remaining proportion of the grid cell. If the share of land-use is increasing from one year to the next, the biomass stored in natural vegetation on the deforested proportion of the grid cell is removed. The removed biomass enters the litter carbon pool. The size of the cropland and the managed grassland stand is adjusted accordingly. As soon the vegetation carbon enters the litter carbon pool it 'decomposes', in model terms it means that a fixed fraction (per time step) of the litter carbon enters the soil carbon pool. The soil carbon pool is reduced by fixed ratios as well (one fast and one slow fraction) and after this 'respiration' the carbon is released to the atmosphere (Sitch et al., 2003). The same procedures and fractions are applied for both the natural vegetation and the agricultural land. We have added this information and now provide a more detailed description in section 2.1.1, ll.161.

• There is a huge amount of literature on water erosion and it is well known that agriculture and overgrazing enhances soil losses to neighboring grounds and rivers permanently (see e.g. [1,2,3]). This is different from the temporary effect of deforestation that you discuss in lines 488-491. How do such processes affect the results of your study? That you consider only the effects of inundation must be made explicit already early in your study.

Reply: The model is not able to estimate the losses in soils or its content (soil carbon) caused by erosion. The function included in RivCM, which removes litter and soil carbon during inundation is the only function included assessing lateral losses. We now state in the methods section 2.1.1 (ll.170) that erosion is not included in the model. Any reduction of soil carbon is only caused by the respiration of organic carbon and the production of CO_2, which is implemented in the model as described in the reply above ('What happens to the carbon cleared upon deforestation?'). A consideration of erosion would change the results in a way that the amount of exported carbon not only depends on the amount of available carbon in litter and soil, but also on the coverage with

vegetation in general. It would probably lead to a faster depletion of the litter and soil carbon pools as compared to the current version, which does not include erosion. We now clearly state in the manuscript that erosion and related loss of soil carbon is not considered in the model in methods section 2.1.1 (ll.183).

- Generally I would expect that deforestation for farming happens outside the areas subject to inundation. What are your assumptions on the overlap of deforestated and inundated areas? Would it be helpful to add a map showing this overlap?

Reply: You are right in assuming that the conversion of annually inundated forest to cropland is not likely. This holds true, especially if you consider processes on a high spatial resolution. Our study was conducted on a resolution of 0.5° lat /long, which gives us the possibility to assess large-scale consequences of climate and land-use change. It has on the other hand the disadvantage that small-scale processes have to be simplified. For the inundation we calculated the area that is close to the river in a flat area and is therewith prone to flooding. For the deforestation we used a land-use scenario for the Amazon basin by Soares-Filho et al. (2006). At our spatial scale we did not explicitly exclude fractions of the cell from inundation, because we don't know where in the cell the deforestation occurs. LPJmL calculates the natural vegetation, the crops and the inundation in fractions of the cell, without considering the actual position (not spatially explicit within the grid cell). This general approach enables us to assess and quantify interactions and effects at the basin scale.

Second, your results and discussion sections are mostly descriptive, without much explanation why things happen the way you see them in your simulations. Here are some suggestions to improve the paper in this respect:

- The effect of deforestation must be related to the overlap of deforested an inundated areas in your model. Hence shifting the original Fig. 2 showing the distribution of deforestation into the supplement (Fig. S1) is in my opinion not a good idea. I understand that you did this in reaction to a remark by referee #1, but he wanted only a different way of presenting the deforestation data (which is disputable). That the deforestation information is already published elsewhere doesn't matter, since having Figs. S1 (formerly Fig 2) and 2 (formerly Fig 3) alongside has much value for the readability of your paper because it nicely demonstrates that the deforestation pattern explains e.g. the pattern of outgassing CO2. It also makes very obvious important differences in the intensity of deforestation in the selected regions. But: to which scenario refers Fig S1? It would probably be good to see the structure of deforestation also for the other scenario – or are the spatial structures so identical? And I think it would also be helpful for the interpretation of Fig. 5 to see how deforestation develops in time.

Reply: Thank you for sharing your opinion on how to display our results in the best way. Following your suggestion, we have now moved the figure, which shows the deforestation patterns (formerly Figure S1, now Figure 2), back to the main text. We added also the map of deforestation for the GOV scenario (Panel A in Figure 2) as well as time lines for each scenario to the spatial patterns (maps in panels A and B in Figure 2) and the temporal development (timelines in panels A and B in Figure 2).

- In Fig. 2 the POC pattern also largely follows the deforestation pattern shown in Fig S1. But there are many white spots in the eastern part where deforestation is strong – what does this mean?

Reply: Thank you for spotting this missing information in our figure captions and legends. White areas within the Amazon basin represent cells where changes are not significant (p-value >0.05). We added this information to the figure caption.

- In Fig 2 inorganic carbon (IC) is affected only at the margins of Amazonia, but not in inner Amazonia. How can this be explained? Why does the IC pattern not follow the deforestation pattern?

Reply: The inorganic carbon in the water is only marginally affected by deforestation because the amount of IC that remains in the water depends on how much the water is already saturated with IC. This water saturation with IC depends on the water temperature and the atmospheric CO_2 concentration. Deforestation directly affects only the organic carbon pools in the water. Figure 3 shows the change in the amount of IC in the cell and not the concentration. There are small changes in the IC amount in the whole basin, which are caused by changes in the water amount in the cell. In some cells at the edge of the basin this change is largest (+10 to +15%), but in comparison with changes in POC (-80%) or in outgassed carbon (-90%) these changes are comparably small. As already discussed, deforestation also alters the discharge. This effect can be seen especially in the cell at the very edge in the south of the basin (Fig 3E, F). We added some more information on the reason for the insensitivity of IC to the deforestation to the discussion section (ll. 456).

- Interpreting what you say in section 3.2, I guess that Fig. 5 shows the combined effect of deforestation and climate change (E_CCDefor). Since your paper is primarily about deforestation it would be good to see also a similar plot on the temporal development of E_Defor and discuss differences in the temporal behaviour with and without climate change.

Reply: Thank you for pointing this out. The focus of our manuscript lies on the effect of deforestation and the figures should support this focus. Figure 5 shows the solely effect of deforestation. We now understood that the caption of Figure 5 was confusing and we therefore changed it. We added another figure to the supplementary material (Figure S2) to show the combined effects of climate change and deforestation in the same style as in Figure 5. This figure shows the relative changes in the considered carbon pools compared to the non-deforested past ($E_{CCDefor}$), comparing the amount of carbon in the reference period (1971-2000) calculated in the NatVeg scenario with the amount of carbon in the future period (2070-2099) in both deforestation scenarios (BAU and GOV). We put Figure S2 in the supplementary because it is expected to give some further insights, but is not of central importance to the manuscript. We reference this figure together with Figure 6 in the results section (lines 380).

- I cannot really follow your explanation for the drop in POC and outgassed C in R2 and R3, seen in Fig. 5 happening only in the BAU scenario (your lines 477-483). Why do the scenarios behave so differently? And in region R3 this drop already starts before 2050 – is this also explained by your "shortcoming"? Also your explanation to Reviewer #1 on this point is not comprehensible to me. Moreover, if this drop after 2050 is really an artefact of the artificial scenario extensions, why at all then showing results after 2050 (this concerns all figures)? And why then mentioning results for times beyond 2050 in the abstract?

Reply: Thanks for this remark. It shows that we have to elaborate in more detail on that point in the manuscript. If in LPJmL a fraction of a cell is deforested the living above- and belowground biomass enters the litter carbon pool. The litter pool fills the soil carbon pool and both pools provide organic carbon that can be exported by the river. The proportion of litter carbon entering the soil carbon pool is fixed to 0.3 yr^{-1} (each year 30% of the litter carbon enter the soil carbon pool), the proportion of soil carbon being decomposed (conversion to CO_2) is either 0.03 yr^{-1} for the fast soil carbon pool or 0.001 yr^{-1} for the slow soil pool. The consequence of this is that after some years without further deforestation (no large input to the litter carbon pool) the litter and soil carbon pools are depleted. The timelines in Figure 5 showing results of the GOV scenario don't depict this sharp drop because there are no cells completely deforested (see also Figure 2A). Under the GOV scenario litter is constantly provided by the natural vegetation and small-scale land-use and therefore fills up the litter and soil carbon pools, which are responsible for the POC and the outgassed carbon. There is a much clearer drop in the BAU scenario, where a larger fraction of the cell is subject to deforestation; partly 100% of the cell area is deforested in this scenario. In areas where the drop already starts before 2050 (e.g. Figure 6K and L, showing the results for R3) the deforestation in parts of the area already reached 100% before 2050 (also compare with timelines in Fig 2B). In these cells there is a drastically reduced influx of carbon to the litter pool (only from crops) and therefore we already see the drop earlier than in other areas (e.g. R1). We added a more thorough explanation of the patterns also discussing the differences to the manuscript (ll. 435)

- If I understand Figs. 2 and 4 right, they show according to Eqs. (3) and (4) the effect for 2070-2099. Hence this includes the artificial stop of deforestation in 2050. Wouldn't it be more interesting to see the effects in 2050 because the scenarios you use are maybe of regional political interest?

Reply: Analyzing the effects between 2070 and 2099 allows investigating combined effects of intense climate change and the deforestation maximum. We show the results for the end of the century for both, changes only induced by deforestation as well as changes induced by climate change and deforestation together. For Figure 3 (formerly Figure 2) it would not make a difference if we would show the results for 2050, because there is no additional change after that year if we exclude the climate change effect. Figure 5 (formerly Figure 4) shows the effect of deforestation and climate change, of which the latter continues. To see the combined effects for 2050 would be interesting as well. But we think that the results become clearer (because climate change gets more intense) if we use the end of the century as our 'future period', which we used for comparison. We made this clearer in the manuscript (section 2.2.1, ll.232)

Further remarks:

- Figs. 2 and 4: What do the white grid cells mean? Outside Amazonia its clear, but not inside Amazonia.

Reply: Thank you for pointing to this missing information. This is now added in the figure caption of Figure 2 and Figure 5: White areas within the Amazon basin represent cells where changes are not significant (p-value >0.05).

• Grammar error in line 36.
Reply: We corrected the error.

• You say in line 189 that only "relative changes in in the carbon can be assessed by the model". What means "relative" here? I fear that since total carbon mass is conserved, even relative numbers may not be trustable, because an overestimation of a relative change in one carbon variable may imply an underestimation in the relative change in another carbon variable.

Reply: There are still large uncertainties in the process understanding of riverine carbon processes that translates to uncertainty in the parameter estimation. Therefore, a respective model like we have applied here can currently only reproduce broad estimations of exported CO_2 (outgassing) and exported organic carbon (discharge). Both amounts are either underestimated (-70% basin wide outgassing) or overestimated (+60% for POC/DOC to the Atlantic, +30% within the catchment). However, the concentration of organic carbon in the water lies within observations (Langerwisch et al., 2016). In general the model reaction to climate change alone and in combination with deforestation and land-use change is as expected (e.g. reduction of organic carbon due to deforestation, increase of inorganic carbon due to climate change). Therefore, we think it is reasonable to use our model to estimate changes in process relations and general trends. Further data-model comparison and improved parameterization are still required to allow assessing the simulated absolute numbers model. This is also mentioned in the methods section 2.1.2 (ll. 217).

• I do not really understand what you want to say with lines 437-447: Why do you start with global NPP in line 437? Has it any relevance for your question? The sentence starting in line 438 is also not clear: I guess that you get the 6 PgC/yr NPP by multiplying the per squarekm NPP value from the next line with the area of Amazonia. If so, the 0.25 PgC/yr exported to the Atlantic ocean make only 4% of Amazonian NPP – this is much less than any accuracy in NPP that can be obtained for Amazonia and thus is unimportant for any assessment in contrast to what you say in lines 445-447.

Reply: Thanks for mentioning this. You are right in assuming that the relevance of this statement (lines 437-439) for our study is only minimal. We tried to set the carbon fluxes in Amazonia in relation to the global ones, although it is sufficient to mention the Amazonian values to understand how much climate change and deforestation might alter the past state. We removed the sentence mentioning the global NPP. We see that discussing the regional numbers of outgassing and export together with global NPP is confusing. Our aim was to stress the importance of including the riverine fluxes into large-scale carbon assessments. We changed the paragraph (ll. 467) in a way that we state more clearly now how climate change alone and in combination with deforestation alters the outgassing and the export to the ocean and therewith the regional carbon fluxes. We are aware that the amount of carbon exported to the ocean (observed 36×10^{12} g C yr^{-1} (Moreira-Turcq et al., 2003)) is relatively small in comparison of the standing biomass (23×10^{9} g C km^{-2} (Malhi et al., 2009), appr. 138×10^{15} g C in the basin) and the errors in these estimates might be large, but we think that including these fluxes and considering the changes should be a part of regional carbon assessments.

- In lines 225-226 you explain how you handle deforestation after 2050. Do I understand this right that you stop deforestation in 2051? I find this sentence a bit confusing, first because of the word "while" which indicates some temporal parallelism which is probably not meant here, and second because you say that you "neglect" further deforestation as if you would know what happens after 2050.

Reply: We changed this confusing sentence to make clearer that we keep the deforested area constant and only use the climate change as driver for changes after 2050 (l. 225/226). This assumes that deforestation is halted in 2050 and gives us the potential to look at climate-induced changes when climate change impacts intensify later in the century. The sentences now read: "After 2050 the fraction of deforested area is kept constant. From 2051 until the end of the century the only driver of change is the continuing climate change."

- In lines 225-226 you note as reason for the artificial extension of the deforestation scenarios "to show the long term effects of deforestation". But in the remainder of the paper you do not discuss these "long term effects". So, what are the "long term effects"?

Reply: With long-term effects we mean delayed changes in the carbon pools and fluxes after further deforestation is stopped. Especially the soil carbon pools need time to show reactions to changes in the vegetation. Halting the deforestation and continuing with climate change was supposed to show effects of the deforestation later on in the century. We expected some lagged effects after 2050, especially in areas where several grid cell have been deforested completely. For POC and outgassed carbon we could not see these lagged effects. We only see the effect of the depletion of the litter and soil carbon. We also see that the pools stabilize after 2060 and that at least for carbon exported to the river that deforestation has no drastic additional long-term effect.

- I guess the nice new insets to Fig. 4 are computed on the basis of the D-measures introduced in (5) and (6)? If so this should be mentioned in the caption.

Reply: Thanks for pointing to this missing information. We added the information which indicator (DCC and DDefor) is shown in the caption of the figure.

- Line 382: changeS --> change. **Reply: We corrected the word.**
- Line 482: What do you mean by "as it is"? **Reply: We removed this irritating phrase.**

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