Hydrol. Earth Syst. Sci. Discuss., 9, C1211–C1219, 2012
www.hydrol-earth-syst-sci-discuss.net/9/C1211/2012/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Potential effects of climate change on inundation patterns in the Amazon Basin" *by* F. Langerwisch et al.

F. Langerwisch et al.

fanny.langerwisch@pik-potsdam.de

Received and published: 30 April 2012

Reply to

Comments on "Potential effects of climate change on inundation patterns in the Amazon basin" (Manuscript # HESSD-9-261-2012) by F. Langerwisch, S. Rost, D. Gerten, B. Poulter, A. Rammig, and W. Cramer

Dear Editors,

Thank you for sending us the reviews of our manuscript. We appreciated the constructive comments which will help us to improve the manuscript. According to your instructions we have now written a reply to the referees comments. We will wait for your response before we change the manuscript accordingly.

Below, we have addressed each of the referee's comments (italics) separately. According to the referee's suggestions, we will state the aims in the revised manuscript more clearly; we will restructure the introduction and parts of the methods and we will remove irrelevant parts from the discussion.

Please contact me if you need any further information.

Yours sincerely, Fanny Langerwisch

Response to Comments of Referee #1

Specific Comments: 1 Comment: 'First of all, I am missing a research hypothesis in this study. It should be mentioned in the introduction as should address the shortcomings of the current state of the art.' Reply: We agree with the reviewer that we have to put more emphasis on our hypotheses. Our hypotheses are stated in the introduction, e.g. P264 'Climate change [...] can potentially lead to changes in flood regime [...]. [...] To understand and quantify the magnitude of impacts of future climate change on the Amazonian water balance, we apply an enhanced process-based model'. To make this clearer, we will restructure the introduction. We will additionally insert a paragraph on the current state of the art of the hydrology models (e.g. the WaterGAP model, Döll and Zhang, 2010) and the advantage of our model as a combination of vegetation dynamics fully coupled with water fluxes.

2 Comment: 'The authors did a quite extensive literature review on the effects of inundation on the ecosystem rather than on the technical details on how to estimate flow velocities in a macro hydrological model (macro HM), which in my opinion is one of the key elements of this paper. After all, the whole method section refer to the calculation of flow velocity and flooding areas. L25 ff P263 can be simplified. Please reconsider the focus of the manuscript in the introduction.' Reply: We will remove the paragraph on P263 L24 to P264 L9 from the introduction and focus more on the current state of

HESSD

9, C1211–C1219, 2012

Interactive Comment

the art of HMs (see also response to your comment #1 and to comment #5 of referee 2).

3 Comment: 'Simulations of fourteen GCMs have been simply taken from the IPCC database and applied in this study. No evaluation of the precipitation product has been carried out at all. This is a critical step because it is well accepted in the scientific comunity that precipitation is one of the variables in which GCMs do a very poor job due to different reasons. If this variable is heavily biased, so are the inundation areas. At least the authors should check whether these models are able to reproduce the climatology of the reference period, i.e. without IPCC emission scenario.' Reply: The reviewer raises an important issue here. Precipitation products from GCMs have been evaluated in several other studies, e.g. Jupp et al. (2010), Li et al. (2006) and Rowell (2011) and is not the focus of our study. We refer to their findings in P271 L11. Indeed, we used results from 24 GCMs and weighted these equally in order to demonstrate the largest range of possible future development – and the resulting uncertainty – given the GCMs/scenarios from the IPCC database. We are familiar with methods to weight the models according to their ability to reproduce observed rainfall (e.g. Jupp et al., 2010) and will mention these in a more detailed way in the revised text ; but we do think that a discussion of these structural climate-model uncertainties is outside the scope of this impacts study. Also note that we used anomalies of temperature, precipitation and cloudiness compared to the average climate simulated for the present rather than the raw GCM outputs, which is expected to yield more or less robust results even if individual GCMs do not match the present climate (P271 L20).

4 Comment: 'The authors emphasize that a new method is presented to estimate velocities on a macro HM. To my surprise, I found the old standard Manning equation usually used in practice by hydraulic engineers for designing channels. The difference is that this empirical equation is applied into a scale at least 100 times larger (from 10e1 to 50e4m) but with the very same parameters found in laboratory. The authors should recognize that at 0.5 x 0.5 deg spatial resolution, there is no more rivers but only flow di-

rections. At large scale, rivers become effective features for which effective parameters should be inferred using observations. In my opinion, it is misleading to use standard text book equations and sell them as a new method in a research paper. I suggest to move all equations to an appendix and to concentrate in the estimation of the effective parameters needed for estimating river velocities, i.e. Mannning-Strickler k, exponents, R. Eq. 1-11 can be found in text books (e.g. GIS, Handbook of Hydrology, Ven T Chow 1964). Authors should look at regionalization procedures to link velocity parameters to morphological features, land cover, slope, actual river length within the 0.5 x 0.5 deg pixel.' Reply: The method is new, insofar it applies the Manning-Strickler formulation to a large-scale basin and that we integrate it into a dynamic hydrology-vegetation-model. We are aware that this method is usually used for small-scale calculation and that it has its constraints on a larger scale. We will add a more detailed discussion the uncertainties of this approach in P273 L5ff. We agree with the reviewer that the equations can be found in textbooks and we will move Eq. 1-11 to the Appendix. See also comment #5 referee 2.

5 Comment: 'Please indicate which relationship links S with I. I could not figure out how these variables are connected. Do you need extra parameters?' Reply: In order to make this more clear for the reader, we will insert a description of how I is calculated from S in P267 L13, as follows: I=tan(S*PI/180).

6 Comment: 'The estimation of inundation areas is also overly simplified. No reference has been made for example to Lettenmaier et al. among others researchers who have work in this topic in the past. Again an ad hoc rule is used here L20 P270: "assume that 25% of the potential floodable area ...". I wonder why 25% and not 31.41592%? The latter is equally good for me. It is extremely important, that the authors carry out a sensitivity analysis of all model parameters, as well as, a robust uncertainty analysis before they attempt to use a model to make future projections. In fact inundation areas in large rivers are governed by dynamic processes, specially on rivers carrying enormous amounts of sediments. If a conceptualization is needed, then one has to

HESSD

9, C1211–C1219, 2012

demonstrate that it is robust. Please justify all your assumptions.' Reply: Our text was probably misleading. The "25% of the potential floodable area..." were directly calculated based on the work of Richey et al. (2002). They estimated that about 16% of the central part of the basin is covered with water during the high water phase and about 4% during the low water phase. This means that a quarter of the high water areas is flooded during low water season. We will clarify this by changing P270 L208. We will add an additional paragraph about further work (e.g. by Tang and Lettenmaier, 2012) to describe more approaches to estimate inundation areas.

7 Comment: 'I do not understand to which summands the authors refer in L9 P269, L11, L14 P268.' Reply: For our calculations of the modified TRMI we use three summands, the classified slope, the classified slope configuration and the classified relative slope position. An overview about these summands is given on P268 L7. The first two summands are explained on P268 L12 and L14. We will make this paragraph clearer for the reader by rearranging it and adding the equation TRMI= Sclass+SConfigclass+RelSPosclass (mTRMI=classified slope+classified slope configuration+classified relative slope position).

8 Comment: 'Section 2.3 is a standard description of river basin delineation. Where is the novelty here?' Reply: The reviewer is right, it is a description of the standard river basin delineation. But in order to fully describe our methods and to enable readers, who are not familiar with this method, to follow our model concept, we think the explanation is necessary. See also comment #5 of referee 2.

9 Comment: 'The results are often qualified with adjectives such as "very good" L.18 P273. What does it mean? Please give a quantitative efficiency measure first.' Reply: We will remove these sentences from the text. We will change P273 L18-20 "The simulated discharge reproduces the river network in the Amazon basin (Fig. 2)."

10 Comment: 'In addition to the efficiency measures provided in L9 P274 ff, please estimate bias, Nash-Suttcliffe efficiency, Pearson correlation coefficient, RMSE, etc.

These efficiencies should be reported for a calibration and evaluation period. Based on the uncertainty analysis, confidence limits for these statistics should be reported. Please include them in Table 5.' Reply: According to the reviewer's suggestion, we will additionally calculate the Pearson correlation coefficient and RMSE and include the results of these statistics. We regard the proposed Nash-Sutcliffe index seems as an unsuitable measure, because it ranges from $-\infty$ to 1 and therefore the distribution is skewed. Therefore, we applied the Willmott's index of agreement and the Error of the Qualitative validation (QualV, as published by Jachner et al., 2007) instead of the suggested Nash-Sutcliffe efficiency coefficient. The Willmott's index of agreement has the advantage to range between 0 and 1 and therefore clearly indicate the range between the points in relation to the maximal possible agreement values. The application of the QualV includes an estimation of the temporal shift and stretch of the simulated data as compared to the observed data. This method is therefore an excellent index to compare the agreement of the temporal patterns of hydrographs. We will clarify this by adding a sentence in P272 L7. Furthermore, we will rephrase this paragraph to make it clearer that we compared the simulated hydrograph with the according observed hydrograph for each site. Concerning your comment on the calibration and evaluation period: We have no calibration period in our model approach. The evaluation period for each site is the period for which measurement data are available for each specific site (see also comments #13 and #14). We will rephrase the according paragraph to make this clearer for the reader.

11 Comment: 'Authors should consider that a small ensemble based on one scenario does not allow to estimate probabilities. At most, these values are conditional probability estimates. No one now the probability of occurrence of a scenario therefore it is not possible to estimate absolute probabilities.' Reply: The reviewer is right. We will rephrase the term 'probability' to 'proportion of models in agreement' etc.

12 Comment: 'Precipitation patterns based on GCMs are extremely uncertain (L20, P276) (Latif et al.) Total uncertainty in this case is related with model, scenario, internal

HESSD

9, C1211–C1219, 2012

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

model uncertainties due to initial conditions and chaotic behavior. Model uncertainty is in turn related with model equations and parametrizations. Authors should ponder these facts when they derive conclusions based on GCM predictions for the next 100 yr! Please change caption of Fig6. "Probability" into something like % model realizations indicating +ve or -ve trends.' Reply: As stated in our response to comment #3, we are aware of the uncertainty in the projections and discuss this in our text. We will change "probability" in the caption of Fig. 6 to "proportion of models in agreement" (see also response to comment #11).

13 Comment: 'Fig 2,3 : no efficiency measures. No calibration, validation periods.' Reply: Correct, we used these figure to give an overview about the spatial and temporal patterns of the river network and the hydrograph. The results of a more sophisticated method to compare observed and simulated discharge is additionally shown in Figure 4, where we plot the Willmott's index of agreement and the Error of the qualitative validation (see also comment #10). Additionally, we will include a calculation of the Pearson Coefficient and the RMSE in the revised manuscript. We will provide a table for all calculated indices and the corresponding sites. Concerning your comment on calibration and validation periods, see comment #10.

14 Comment: 'Fig 4 should be complemented with a table in which the requested statistics are provided. Calibration, validation periods must be mentioned.' Reply: We will add a table with the values for the Willmott's index of agreement and the Error of the qualitative validation. Additionally we will calculate the Pearson Coefficient and the RMSE. We will provide a table for all calculated indices and the corresponding sites. Concerning you comment on calibration and validation period, see comment #10.

Editing Comments: 15 Comment: 'The abstract should be improved. e.g. L2, starting the second sentence with "however", makes no sense because this sentence is not in contradiction to the first one. Quite the contrary, It provided a new piece of information.' Reply: We will change the second sentence in the abstract according to the reviewer's suggestion.

16 Comment: ‘L8: “... floodable areas and inundation.” makes no sense.’ Reply: To clarify this, we will change P262 L8 to “. . . potentially floodable area and monthly inundation.”

17 Comment: ‘L8: “Regarding hygrograph...”, what do you mean here? Its daily dynamics, its statistical characteristics, its flood duration curve?’ Reply: To make the text clearer, we will change P262 L8 to “Simulation results of discharge and inundation area for contemporary conditions compare well against site-level measurements and observations on inundated area.”

References Döll, P. and Zhang, J.: Impact of climate change on freshwater ecosystems: a global-scale analysis of ecologically relevant river flow alterations, Hydrology and Earth System Sciences, 14(5), 783–799, doi:10.5194/hess-14-783-2010, 2010. Jachner, S., van den Boogaart, K. G. and Petzoldt, T.: Statistical methods for the qualitative assessment of dynamic models with time delay (R package qualV), Journal of Statistical Software, 22(8), 2007. Jupp, T. E., Cox, P. M., Rammig, A., Thonicke, K., Lucht, W. and Cramer, W.: Development of probability density functions for future South American rainfall, New Phytologist, 187, 682–693, doi:10.1111/j.1469-8137.2010.03368.x, 2010. Li, W. H., Fu, R. and Dickinson, R. E.: Rainfall and its seasonality over the Amazon in the 21st century as assessed by the coupled models for the IPCC AR4, Journal of Geophysical Research-Atmospheres, 111(D2), 2006. Richey, J. E., Melack, J. M., Aufdenkampe, A. K., Ballester, V. M. and Hess, L. L.: Outgassing from Amazonian rivers and wetlands as a large tropical source of atmospheric CO2, Nature, 416(6881), 617–620, doi:10.1038/416617a, 2002. Rowell, D. P.: Sources of uncertainty in future changes in local precipitation, Climate Dynamics, doi:10.1007/s00382-011-1210-2 [online] Available from: http://www.springerlink.com/index/10.1007/s00382-011-1210-2 (Accessed 7 November 2011), 2011. Tang, Q. and Lettenmaier, D. P.: 21st century runoff sensitivities of major global river basins, Geophysical Research Letters, 39(6), doi:10.1029/2011GL050834 [online] Available from: http://www.agu.org/pubs/crossref/2012/2011GL050834.shtml (Accessed 30 April

2012), 2012.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 261, 2012.