**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.**

Anonymous Referee #1

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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

22 March 2012

1   General Comments

In this manuscript, the authors describe a procedure to estimate inundation areas in the Amazon Basin as well as potential changes of its spatial distribution due to climate change. To that end, 24 GCMs outputs and a procedure to estimate flooding areas within a macro scale hydrologic model (LPJmL) is presented. The state-of-the-art in flood routing and estimation of inundation areas is far from satisfactory in large scale hydrologic model. The authors made several remarks regarding the novelty of their approach. In my opinion publications of relevant research in this subject should be encouraged in HESS. In the present manuscript, however, several key issues should be clarified before publication.

2 Specific Comments

This manuscript has the following technical shortcomings:

- 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.

- 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.

- 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.

- 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 directions. 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.

- Please indicate which relationship links S with I. I could not figure out how these variables are connected. Do you need extra parameters?

- The estimation of inundation areas is also overly simplified. No reference has been made for example to Lettemaier 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 demonstrate that it is robust. Please justify all your assumptions.

- I do not understand to which summands the authors refer in L9 P269, L11, L14 P268.

- Section 2.3 is a standard description of river basin delineation. Where is the novelty here?

- 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.

- 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.

- 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.

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

- Fig 2,3 : no efficiency measures. No calibration, validation periods.

- Fig 4 should be complemented with a table in which the requested statistics are provided. Calibration, validation periods must be mentioned.

3 Editing Comments

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.

L8: "... floodable areas and inundation." makes no sense.

L8: "Regarding hygrograph...", what do you mean here? Its daily dynamics, its statistical characteristics, its flood duration curve?

4 Final Remarks

Based on previous comments and bearing in mind the HESS publishing standards for a research article, I recommend to return this manuscript to authors for major revisions.