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## ***Interactive comment on “Modelling multiple threats to water security in the Peruvian Amazon using the WaterWorld Policy Support System” by A. J. J. van Soesbergen and M. Mulligan***

**W. Buytaert (Referee)**

w.buytaert@imperial.ac.uk

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This paper reports on a very ambitious attempt to predict the impact of several physical and socio-economic drivers (land-use change, population growth, increase of industrial activities, climate change) on the water resources of the Peruvian Amazon using a global hydrological model (waterworld).

Waterworld is indeed very promising tool to analyse future scenarios of hydrological change in complex environments, in a more interactive process between scientists and stakeholders. There are several very interesting scientific questions here, which I think broadly fall in the following 3 categories:

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- 1) User interaction with model scenarios and simulations;
- 2) Understanding the drivers of hydrological change in complex and data scarce environments;
- 3) Building models that make the best predictions in such environments.

However, the paper seems to hover indecisively between those three foci, and with that fails to make a real impact in any of them. I understand that the presented study is a model application, with the model itself being described in another publication by the senior author. As such, I do not expect this manuscript to focus on question (3). Nevertheless, the conclusions do make this claim (e.g., "combines the best globally available data with a broad and deep understanding of hydrological processes"). At best, the claim is unsubstantiated without a more through description of the model, and at worst a bit grandiloquent. Furthermore, claims that calibration is not necessary and that physical relations will hold in the future (p.570) are extremely controversial in hydrological modelling, and would also need careful justification. Several studies have for instance shown the large uncertainties and potential errors in remotely sensed data for that region (see e.g., Condom et al., 2011, Ward et al., 2011, Clarke et al., 2011). Similarly, representing hydrological processes is fraught with difficulties because of the dominance of poorly known hydrological processes such as groundwater (as also mentioned in the review of Christopher Scott, but see also for instance Zulkafli et al., 2012) and large-scale Amazonian wetlands that provide potentially very large and long-term storage (e.g., Guimberteau et al., 2012). In short, I fully agree that the use of satellite data and global model structures is a very sensible in data scarce regions, and can be useful as a first order estimation of hydrological behaviour. But this of course can entail big uncertainties and risks, which makes that the abovementioned claims need better support. For an introduction in some of the controversies related to the topic see the WRR commentary of Wood et al. (2011) and subsequent discussion initiated by Beven and Cloke (2011)). Again, this may well be beyond the scope of the project, but if so this needs to be reflected in the conclusions.

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Another really interesting aspect, highlighted by the manuscript, is the potential for user interaction provided by a web-based model implementation. However, this emphasized in the conclusions as a possibility, but not really substantiated in the manuscript itself. I concur with the review of Christopher Scott about the main problem being the lack of any user interaction or involvement presented in the manuscript. But even without this, several other aspects could do with further deepening, especially because of the controversial nature of the topic. While some argue that hydrological models are too complex to be exposed directly to non-experts, this reviewer is not one of them (Buytaert et al., 2012). But that same publication highlights some of the many issues of interactive model simulation, with probably the main issue the need to communicate the impact of model assumptions, simplifications and uncertainties. Again, in a data scarce environment, the impact of the latter cannot be overstated (e.g., Beven et al., 2012). One of the major risks of global model implementations is their false impression of data richness, which thus could be an incentive to decrease the pressure on local data collection. This of course would be a perverse effect. On the contrary, using sensitivity and uncertainty analysis in an interactive simulation context can be a powerful tool to guide data collection investments to provide optimal benefit for local decision-making. More in general, decision-makers often have high and unrealistic expectations regarding scientific knowledge and environmental models. The risk exists that they will see them as clear-cut cases for particular policies, or contrarily, they may lack confidence in models and results that they cannot reproduce themselves. All these and more questions come up when reading the manuscript. While of course not all can be solved, I agree with the other 2 reviewers that this needs much more attention. At least, the model being positioned as a policy tool, it should be possible to show how the model results are used to support policy decisions.

Lastly, the tool holds great promise for understanding the potential variations in magnitude and the relative importance of the studied drivers, and as such to inform decision-makers about priorities for policy making. This is what the authors focus most on in their description of methods and presentation of results. However, there is hardly any

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discussion of these results, as if the authors do not feel confident about the produced numbers. Again, I fully support the authors in their attempt to quantify the impact of the different drivers on hydrological change, and agree that such an attempt can be highly informative for policy (see for instance Buytaert and De Bievre, 2012, for a similar attempt to address climate change and population growth impacts). I even perceive an overemphasis on the impact of climate change at the cost of other drivers in current policies and results like these can help in correcting this. So what about for instance a synthesis figure (piecharts or barcharts) that presents the relative contribution of each driver to change for different parts of the basin (e.g., Andean highlands vs. Amazonian lowlands)? But then again, ideally, such figures should include some form of uncertainty estimation. The manuscript mentioned uncertainties several times, so it is really disappointing not to see more effort to quantify any of them. I again agree with the second reviewer that a sensitivity analysis would be a useful first step for this. A major disadvantage of sensitivity analysis is the often arbitrary range of variation. In several of the scenarios, however, the opportunity exists to use existing boundaries. For instance, while I agree that the GCM ensemble mean is a good choice as the best predictor (p 577, l17), it is also become standard practice to use the entire ensemble of GCMs (or at least a subset) to get to grips with the uncertainty of climate projections (e.g., Stainforth et al., 2007). This is especially the case for the tropical Andes, where climate projections show very large uncertainties, and there may be a high risk for systematic biases in the entire ensemble, for instance because of the inadequate representation of the topography (e.g., Buytaert et al., 2011). Given the availability of downscaled projections from CIAT, this should be quite straightforward. Similarly, the SRES scenarios (or the more recent RCPs) give ranges of population growth that could be used to constrain a sensitivity analysis.

To conclude, the manuscript holds great promise, but is currently very descriptive in the presentation of the tool and the scenarios. I think it could do much better in highlighting the scientific relevance. I hope the reflections above will encourage the authors to reflect on the direction of such elaboration, and I very much hope that it will be

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substantial.

Specific comments:

p568, I5. "particularly well suited to heterogeneous environments with little locally available data" -> how do you justify this? See above for some issues on process representation and deep uncertainties.

p568, I18. "See Mulligan et al. (2013) for a similar analysis for the entire Amazon Basin": So how does this manuscript complement the study by Mulligan et al. (2013)?

p568, I26: Is 1ha really justified with the available data? I would be very reluctant to believe such results without some form of local validation. See for instance Buytaert et al., (2011) for an example of where high resolution models may give worse results than coarse resolution models. The discussion of Beven and Cloke (2012) is of course also very relevant here.

p570/ I12: The lack of possibility to calibrate a model highlights the need for some form of evaluation. I would be rather cautious in interpreting results of a model that is neither calibrated nor validated.

p574/16: how is irrigation accounted for?

p576: deforestation: even though the waterworld model is described in another publication, it would be very useful to have some description here of the hydrological parameterisation of the land-use classes. Otherwise it is impossible to interpret the potential impacts. Also, how is the land-use change model implemented? (I.2)?

p578; population growth: as for the land-use change model, it would be very useful to have some details about how the population growth model is implemented, if only to assess the suitability of a (supposedly global) model for this particular region.

p578/20: of course water storage and transfers are already very common in the Andes...

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p581/14: arguably, taking the mean is not a method to limit uncertainties, but only to identify a best estimator. The reference actually advocates the use of a model ensemble.

p582/5: it may be useful to state the main assumptions of the model beforehand (and maybe discuss the impact of those for the studied region as part of a section on model limitations).

p582/11: "hazard preparedness strategies" -> more typically referred to as adaptation strategies

p584/18: Mulligan et al. (2013a): this reference seems wrong and should probably be that on p.586/13, which should be corrected to 2013c instead of 2013.

p584/20: the surname of Waldo is "Lavado Casimiro", so should be corrected.

p586/30: adn -> and

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