

***Interactive comment on* “Linking economic and social factors to peak flows in an agricultural watershed using socio-hydrologic modeling” by David Dziubanski et al.**

Anonymous Referee #1

Received and published: 15 November 2019

This paper “Linking economic and social factors to peak flows in an agricultural watershed using socio-hydrologic modeling” develops a coupled agent-based model to evaluate the impact of conversion decision on flood reduction in a watershed. I think the scope fits quite well with the journal and the authors explain their goal and method reasonably well. I do have some comments which I hope can further improve the quality of the manuscript. I would recommend a minor to moderate revision.

First, I think the authors can benefit well by enlarging their literature review into the “water resources systems analysis” (WRSA) realm. The study of human-hydrologic cycle interaction strat at the Harvard Water Program in the 1960s. A lot of classic issues

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(including the impact of land use, land cover change) had been addressed extensively in WRSA literature. Compare to “socio-hydrology,” WRSA also have a longer history of incorporating ABM into their modeling framework. I would strongly encourage authors to identify more literature on this aspect.

Second, following my above comment, studies of ABM become more and more popular in the past decade. Methods used to quantify agents’ behavior have been improved a lot as well. Methods proposed by the authors are not entirely new (Section 2.7.2, line 375-385) because it is a Bayesian-based method (the authors even use the terminology: prior and posterior). Authors are encouraged to broaden their literature about ABM that uses Bayesian theory to address behavior uncertainty. The authors should highlight the different settings they used in their ABM compare to other Bayesian-based ABM.

Third, I do have a suggestion about paper structure. Currently, the authors put the ABM calibration in Section 5 which reads weird to me. The purpose of calibration and validation of the model is to demonstrate the credibility, therefore, it should be put before the authors use the model for any scenarios. I would suggest move Section 5 before the results. And add more discussion about ABM validation (beyond line 711-720) because this topic is the most popular issue in the ABM community nowadays.

I have some minor comments below:

Line 71-73: This kind of argument really needs to incorporate the studies of Water Resources Systems Analysis.

Line 137: You mean two “types” of agents?

Line 223-224: This does match with your equation (7) to (9) because I did not see minimize flood damage objective function. Also, why the goal of the city agent is not “minimize the cost = flood damage + contact fee?”

Line 229: I think this is the first time you mention risk-aversion. You need a more

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detailed description of what does it mean in your model.

Line 337: Since FAO has a physically-based crop model, you might want to test the sensitivity of the current crop model on your results given that this will affect farm agents' decisions.

Line 402: How you define "neighbor?"

Line 564-Figure 6d: Why is there a jump in all three curves around 2012? The same question for Figure 8d.

I hope these comments help the authors for their revision.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2019-484>, 2019.

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