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Interactive comment

Interactive comment on "An integrated modeling framework for coevolution and feedback loops of nexus across economy, ecology and food systems based on the sustainable development of water resources" *by* Yaogeng Tan et al.

Anonymous Referee #1

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The first problem of developing integrated models is that you need to sacrifice detail somewhere. And in this model, the economic module is clearly not adequate. For example, farmers maximize yield instead of profit/utility, and moreover appear in the food and not the economic module. This means that farmer's response has no economic basis/rationale. As noted in the paper, the goal is simply to maximize Crop yield and meat production. This is by no means the objective of farmers in real life. It is admissible to assume farmers maximize profit (not really, but let it pass). But assuming farmers maximize crop yield will lead to misleading simulation results and policy

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Discussion paper

recommendations, because they simply don't do that. In my opinion this assumption already makes the model deeply flawed. Agriculture is the largest water user worldwide and you are assuming an objective function that is unrealistic. For example if you have water scarcity in the future you may expect farmers to shift to less water intensive crops / deficit irrigation / rainfed while maximizing profit; which may conflict with the way crop maximizing farmers respond.

The second problem is that in order to combine the models into a single optimization problem / target / goal, the conflicting objectives of each module need to be simplified and this requires some additional assumptions that may not work well; in this case for example, the overall objective of sustainable development looks to me rather normative, which does not really fit into the positive nature of some of the disciplines considered. As an example, authors note that non-linearities are important to accurately represent the EEF nexus but then, within each one of the modules, the objective function is linear, contradicting elementary theory. Assuming economic agents will increase their demand along with population and quotas independently of quality of life, for example, is a simplistic assumption. This may be acceptable as long as household demand is not very much significant in terms of overall demand. But in the agricultural sector, a linear demand that disregards economic incentives and simply looks at the surface of crops and measures ET to assess demand is likely to be inaccurate. What is the rationale for having a mixed portfolio of crops if this is the objective? Or for prioritizing one crop over another one? This is not done in a kg/ha basis, but on a profit basis. The upshot is that while the model may represent well the observed behavior in the year 0, projections are difficult to believe, simply because the key behavioral drivers identified by theory (profit, risk aversion) are missing in the model.

The dynamics between systems have some merit, but they build on a conceptual approach that is inherently wrong. I commend authors to read the paper by (Pindyck, 2015), who warns against over-ambitious and overcomplicated integrated models that for the sake of reproducing complex interactions need to simplify or straightaway ignore some basic principles and therefore may become “useless as a policy tool”.

I am well aware this sort of simplifications and objective functions are often assumed in several models, even if they disregard basic theory in several disciplines. But if the authors want to use this approach it has to be much better justified, and the caveats above need to be highlighted and acknowledged as a critical limitation of the model. To me the useful contribution of the model is the way feedbacks are approached and represented, but the modules are flawed. Therefore any forecast should be done with this important limitation in mind. Right now authors do not warn about this, which may be misleading. If the paper is accepted, such limitations need to be acknowledged.

Connected to the conceptual framework adopted, authors do not deal with the issue of modeling uncertainty. As noted above, authors had to do many several assumptions on model design (and therefore on real life interactions within and between systems) that may be (and in some cases are) wrong. This means the model, and not only scenarios, may be responsible of sim and prediction errors. I’m not talking about calibration residuals, which in the normative model adopted are missing; I’m referring to the error inherent to the model choice. See e.g. (Herman et al., 2015; Marchau et al., 2019)

Minor comments

Please check the English grammar, there are some problems throughout the text. For example, the first statement (“As global warming causedby climate change and growing population, the world isfacing the disequilibrium between natural resources sustainabilityand human wellbeing”) appears incomplete.

Herman, J.D., Reed, Zeff, H.B., Characklis, G.W., 2015. How Should Robustness Be Defined for Water Systems Planning under Change? J. Water Resour. Plan. Manag. 141, 04015012. https://doi.org/10.1061/(ASCE)WR.1943-5452.0000509 Marchau, V.A.W.J., Walker, W.E., Bloemen, P., Popper, S.W., 2019. Decision Making under Deep Uncertainty: From Theory to Practice, 2019th ed. Springer, Cham, Switzerland. Pindyck, R.S., 2015. The Use and Misuse of Models for Climate Policy (Working Paper

No. 21097). National Bureau of Economic Research.

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