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Interactive comment on "Large-scale water scarcity assessment under global changes: insights from a hydroeconomic framework" *by* N. Neverre et al.

Anonymous Referee #2

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Summary

This paper presents a generic hydroeconomic modeling framework for basin-scale water allocation and demonstrates its use for a case study in Algeria. As pointed out by the authors, many global-scale water resources models neglect inter-temporal trade-offs due to water storage in regulated multi-reservoir systems, which are important in many river basins. The strength of the approach presented here is that it relies on simple input datasets, which are available worldwide, but still captures economic trade-offs due to multi-reservoir storage. The weakness is a lack of validation and insufficient justification of the various simplifications for specific use cases. I have a number of review comments and details that could be addressed in a revised version of the manuscript.

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Discussion paper

Review Comments

1. Section 2.1, estimating water availability today and in the future: Some more details should be presented here. How good is the climate model in predicting present-day runoff in the region? Is 2050 runoff taken directly from the model or do you use some kind of change factor methodology? What is your assessment of the uncertainty of 2050 water availability? In order to compute future irrigation demands, do you directly use the simulated precipitation from the climate model? How good is the climate model in terms of predicting present precipitation? It would be good to briefly revise the main assumptions, limitations and sources of uncertainty and then refer to Portoghese et al. 2013 for details.

2. It is not entirely clear how the link between water allocation and agricultural yield is simulated: Figure 2 shows a piecewise linear relationship. Does this relationship apply at the time scale of the entire growing season or for individual growth stages? Irrigation agriculture presents the well-known problem of "delayed yields", i.e. the yield is a function of shortages occurring in all growth stages and shortages in one stage cannot be offset by surpluses in the next stage. Does the framework take this into account or is there a constant water value throughout the season, independent of irrigation history?

3. Section 3, network construction: I do understand the rationale behind the chosen approach, i.e. generating the network topology purely from the elevation model. It is attractive because you can generate a model without detailed knowledge about the system, but it is also dangerous, because many links that are outlined by the algorithm may not be there in physical reality and others, that the algorithm cannot find (e.g. South to North Water transfer in China. . .) may be present in reality. However, network topology to a large extent determines spatial and temporal trade-offs. I believe the authors should present more information to validate the network construction algorithm and to elucidate its limitations. If this is used on a new area, how can one establish trust in the outlined network and how can the network be validated?

4. Section 4, reservoir regulation: The optimization problem is solved using a GA. It would be good to report more details on the GA setup: Which are the decision variables (how many are there)? Is it the alpha and beta parameters? What was the computational effort, how was convergence etc.

5. Section 4.3.4 on tree traversal and also the corresponding appendix D are very short. A minimum amount of information should be given enabling the reader to understand how this works. Figs 5 and 6 do not communicate very well, captions need to be expanded.

6. As with all studies using complex modelling chains, uncertainty assessment is a real challenge here. How robust are the headline results reported in tables 2-4? Which of the reported differences are statistically different from zero? What is the largest contribution to uncertainty – future climate or economic valuation? No attempt is made in the paper to address the uncertainty of results. I know it is difficult, but authors must at least discuss the issue qualitatively, quantitative estimates would be much better.

Details

1. P1, Line3: “can they” should be “they can”

2. P2L1: “water use sectors” is more common

3. P3L1: It is not clear what is meant with “mostly quantitative” here. Why is this a limitation of such studies?

4. P6L9: “reservoir system” should replace “reservoirs’ system”

5. P6L11: “valuation” is more common than “valorisation”

6. P7L4: “at best” should be “optimally”

7. Figure 7 should be much improved. Make an inset map showing the location of the area on the planet. Put a scale/coordinate system. Maybe use elevation model as background. . .

8. I believe figs 3 and 4 can be combined into one. Also, from the discussion given in appendix B, it seems that the demand functions should be piecewise horizontal, not piecewise linear. . .

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