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Hydrology and Earth System Sciences
Discussions



# *Interactive comment on* "Review article: Quantifying the human impact on water resources: a critical review of the water footprint concept" *by* J. Chenoweth et al.

**Anonymous Referee #1**

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I am impressed by the extended response of the authors to the 4 discussants and the Editor. It is clear from the comments of the referees and the authors response that the manuscript is too long; highlighting the need to either tabulate references and/or to cite not exhaustively but bit more wisely. However, inspite of the authors detailed response and their suggestion to tabulate the references by topic, I fail to see how it can still be called a critical review. Perhaps exhaustive but then do we really need scientists for exhaustive compilation of references (though I appreciate its value to broad scientific community) or whether a simple Google search (or another smart algorithm to mine literature database) is sufficient!



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What I, as a reader of a reputed journal such as HESS, would expect is a 'critical' meta-analysis of the cited references. The authors in their response mentioned that I did not provide them with any guideline on how to do so and proposed a tabulation of citations. This may be a good start but for the paper to be a critical review, a further exploration/discussion/comparison of assumptions underlying dominant ideas/methods within the WF literature is a must (in my opinion). The idea is to explain the similarities or differences between dominant methods based on these assumptions and to explore whether the dominant methods are 'fundamentally' different or whether they are merely a result of some tweaking of certain assumptions.

For example: Why is LCA analysis different from others? – why is it fundamentally different from accounting the water use in crop production? Afterall, all such methods calculate the use of water through a cycle, be it a crop growth cycle or the life cycle through the supply chain of a good. For me, the accounting is fundamentally the same- the differences lie in context, for eg. the scale of activities under consideration. Though there may be 'minor' differences in calculating the water use, which can be explained by underlining the differences in assumptions made on the bio-physical (etc.) processes of water use. Here I donot see the need to cite every possible article on the planet but dominant ones (I prefer the earliest one and not the one most cited) or a set of those. How/when then, once all major assumptions that are common across a class of WF methods have been explicated, these methods are suitable for policy analysis. This is where detailing of assumptions underlying the calculation of water use would help and help assuage the criticism of neoclassical economists. If the policy objective is to save water by directly regulating the use of water (and not indirectly by using economic instruments or incentives) then the accounting of water use of various activities and the knowledge of how an activity is affected by changes in its water use with other inputs being fixed are sufficient ingredients to support policy analysis. Under such a policy setting, the neoclassical criticism of Wichelns and others donot hold (though the debate on robustness of policy innovated based on water accounting may remain if there is huge uncertainty about estimation of water use but then that would be revealed



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by the meta-analysis of the assumptions underlying different methods).

I am, as a reader of a 'critical' review, not just interested in different ways of accounting use of water in production activities but I am also interested in some insights into which method should be used when, given that there is a plethora of literature on 'the hot topic'. Such insights cannot be provided without any explicit discussion of the underlying assumptions of various methods. It will also filter out many publications from the 'long' list of citations to a more critical set of citations and distinguish fundamental publications from other publications.

There is another reason why I emphasize on such a meta-analysis. Unless we crystalize our approach to WF analysis (to which a 'critical' review can immensely contribute), a definite response to neoclassical (economic) criticism can never be formulated and it would take longer to adapt WF concept in response to it.

I believe I am now sufficiently clear on what I mean by the meta-analysis of cited literature. It is upto the scholarly creativity of the authors to provide such an analysis. Without such a meta-analysis, i.e. comparing and contrasting the assumptions that lead to conclusions on fundamental similarity or difference between the various approaches, the review is not at all critical. It is also not clear which methods are more resilient to neoclassical or other dominant criticism of the WF concept(which may be important for policy analysts or makers). I would suggest the authors to revisit the neoclassical criticism of Wichelns in order to formulate the set of assumptions. The authors may suggest that the tabulation of references is enough for it to be deemed a 'critical' review. In my opinion it still would not be (it may perhaps be called an exhaustive review).

Finally, I donot find much use in readers being engaged in exact words of certain publications as the authors attempted in their rebuttal. But I understand the need of the authors to do so.

I welcome the authors to use this online discussion forum to rebut my suggestions



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above and/or debate/discuss further the details of a meta-analysis and/or why/whether it (meta-analysis) is necessary for a review to be critical. I would promptly respond next time :).

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 9389, 2013.