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Hydrology and Earth System Sciences Discussions

Interactive comment on "Agricultural-to-hydropower water transfers: sharing water and benefits in hydropower-irrigation systems" *by* A. Tilmant et al.

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This paper considers two different ways to allocate water for irrigation and hydropower purposes In particular, it focuses on Stochastic Dual Dynamic Programming, a dynamic management process. Apart form the imperfections in the mathematical notation that the previous reviewer has already highlighted, the mathematical development seems to be rigorous. I think also that the scientific approach is really accurate and that the topic, that it deals with, should be scientifically interesting for HESS; for these reasons I am recommending the publication of the paper after minor revision. I've got the following

few minor remarks:

1) It seems not to be totally clear what the original contribution of this paper is; probably it is partially stated somewhere in the abstract or in the introduction, but I think that it should be better to explicitly stress it. In fact from the introductory paragraph it is understandable that, when the crop irrigation is involved, the easiest and probably more common way to decide the allocation of water is by means of a static allocation process. On the contrary a dynamic allocation process is commonly used in the hydropower sector. Probably the novel idea relies on the fact that it is herein proposed to use a SDDP involving both the hydropower and irrigation sectors. Anyway, Stochastic dual dynamic programming has been repeatedly applied to water resources management (see, for example "The use of PAR(p) model in the Stochastic Dual Dynamic Programming Optimization scheme used in the Operation planning of the Brazilian hydropower System" M.E.P. Maceira, J.M. Damazio 2004). It would be important to better underline the novel concepts that the paper is bringing forward, also given that the application refers to a virtual case study.

2) It would be important to better put in evidence the stochastic nature of the problem. Actually, the authors claim that stochastic dual dynamic programming is being used but actually it seems that they are solving the problem by using 50 realizations of synthetic monthly inflow over a period of 5 years. Does this mean that the authors identify 50 deterministic solutions of the system (by using 50 realizations of inflows treated as deterministic) and infer the statistical properties of the output by analyzing the obtained sample? If so, I would argue that this procedure is not formally stochastic. Stochastic dynamic programming would imply solving the problem by admitting that the input is stochastic. I believe this point requires some more explanation.

3) The dynamic approach leads to a 6% average increase with respect to the static one; but the authors state that the dynamic development always assures higher benefits (also when water is not scarce) probably because the Gap project and the Syrian one have been developed separately. I have got two observations concerning this fact:

a) As the authors state, if both the projects have been planned “together”, probably there would have been not only differences in water allocation but also negligible differences between static and dynamic benefits (at least with wet years). I am wondering if this fact and the final benefit of 6% can justify the additional complication of the problem.

b) The authors admit that cooperation is not possible in their case study. It would be advisable to clarify whether the type of cooperation suggested by the authors is at all possible. Of course cooperation very often leads to better results, but cooperation requires an additional effort (that is to be justified by the additional income) and cooperation is not always possible

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