**Hydrology and Earth System Sciences Discussions**

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# *Interactive comment on* "Modeling and analysis of collective management of water resources" *by* A. Tilmant et al.

**Anonymous Referee #2**

Received and published: 10 October 2006

Summary: this paper addresses an emerging and interesting topic related to previous inability to mathematically define objectives in river basin management. The paper suggests a combined use of fuzzy sets and stochastic dynamic programming as tools of choice to overcome these difficulties. Although the ideas are clear in general, some important details have been left out. The paper could be improved by offering more information and clarity related to some of the crucial steps in the proposed process.

1) Does the paper address relevant scientific questions within the scope of HESS?

Yes.

2) Does the paper present novel concepts, ideas, tools, or data?

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The concept is new, however the tools that are used are already known.

3) Are substantial conclusions reached?

No. Conclusions are in line with expectations.

4) Are the scientific methods and assumptions valid and clearly outlined?

Yes, except for a few issues addressed below.

5) Are the results sufficient to support the interpretations and conclusions?

Yes.

6) Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

No. This paper does not provide a full input dataset of runoff sequences, storage capacity curves and all other data that would be required to replicate the results. However, an effort to include all the necessary data may exceed the limits of the paper.

7) Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

Yes. The authors have not justified the use of Stochastic Dynamic Programming (SDP) as opposed to other mathematical programming approaches, such as linear or non-linear programming for example, and they have not provided a discussion related to pros and cons of their choice. This is essential since SDP solutions are limited to a set of pre-defined discrete states (12 grids of 110 points are mentioned in the paper on page 2717). It is not clear how this crude representation affects the accuracy of the solutions obtained in this study, and some previous references in this regard (if available) would be useful, along with justification for making this choice.

8) Does the title clearly reflect the contents of the paper?

Yes.

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9) Does the abstract provide a concise and complete summary?

Yes.

10) Is the overall presentation well structured and clear?

Yes, although the paper could undergo improvements.

11) Is the language fluent and precise?

There are grammatical and spelling errors in the paper that should be corrected.

12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

The paper could improve as far as the notation is concerned. Symbols used on line 1 and 2 on page 2712 are unclear – expected are some letter of the Greek alphabet but these are illegible (the symbols used for the set of flexible constraints and the set of fuzzy relations). Also unclear is why these sets are finite – I assume it is because of the choice of the problem representation required by SDP, but this is mentioned in the paper. Otherwise, a set of numbers between 0 and 1 is infinite, so both of the above mentioned sets should strictly speaking be infinite.

13) Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

Yes. Please see below.

14) Are the number and quality of references appropriate?

Yes.

15) Is the amount and quality of supplementary material appropriate?

The paper could be improved by providing a map with the Maule river basin and with the major structures that were modeled.

**HESSD**

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The following are spelling / style corrections that would improve the paper:

Page 2709, line 5, change "determines" to "determine";

Page 2709, line 8, change "the aggregated satisfaction of the different water users" to "aggregated satisfaction of different water users";

Page 2710, line 1, change "associated to" to "associated with" (use 'global search and replace' for this since this is a common phrase used repeatedly in the paper);

Page 2710, line 16, the term "partially feasible" solution is used without previously defining what it means, especially since later in this paper on page 2718 line 2 & 3 it is stated that "all physical constraints are modeled as hard constraints" and as such they must be met.

Page 2713, line 5, replace "it lacks of discrimination power . . . " with "it lacks discriminating power . . . "

Page 2715, equation (8), a question: Why is the weight factor $w_j$ printed as an exponent (i.e. in superscript font)? Should it not be a multiplier in the sum-product? The same question holds for equation (10).

Page 2716, Section 4: provide pros and cons of using dynamic programming as opposed to other mathematical programming techniques, especially LP and NLP and explain why it was felt that SDP was the right choice.

Page 2717, equation (12): this equation does not show how the minimum and maximum releases ($r_{min}$ and $r_{max}$) from storage are set. In particular, does this model take into account that $r_{min}$ and $r_{max}$ are in fact functions of the available storage, and if not, why? Ignoring the fact that storage release is limited by the available head can lead to serious errors, yet the paper does not provide any information as to how these hydraulic constraints were built into the SDP model, nor does it mention if they are taken into account at all. Also, how is net evaporation on reservoirs modeled?

Page 2717, line 16, the term "cost-to-go function" is used without previously defining what it means.

Page 2718, line 2 & 3: the statement that "physical constraints are still modeled by hard constraints" should be explained. How are the flow limits related to the inflow into the irrigation canal or flow through the turbine modeled properly knowing that they are a function of storage which changes dynamically over each simulated time step? Also, the authors should mention what kind of time step is used in the model (monthly, weekly) and explain the pros and cons of selecting their choice for the time step length.

Page 2719, section 5, reference to Table 3 is missing in the text.

Page 2721, line 4 & 5 mentions "the losses" as one of the output variables, without explaining what kind of losses are meant? Head loss of the hydro power plant, leakage loss or evaporation loss from storage, loss to leakage from irrigation canals? This should be explained. Also, why is there no irrigation supply in the list of output variables? Without irrigation supply, it would be impossible to evaluate reliability of its performance which is mentioned in Section 6.

Page 2721, line 16 states that this study relied on the use of a series of 13 years of historic flows. This is a rather short period for developing long term basin allocation strategy. These hydrologic years may not be representative of the long term runoff conditions in the basin. Authors should explain why their series was so short, and how they evaluated the risk of poor input data representation on their results and conclusions.

Page 2722, line 22 replace "waters users" with "water users".

Additional comments:

The paper does not explain if this model was solving optimally for all time steps simultaneously, or if it progressed from one time step to another in a sequential form. In particular, if the irrigation deficits are inevitable due to lack of runoff and water in storage, is the model able to optimally balance the deficits over time, thus avoiding a

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situation with full supply for the first two months of irrigation season by drawing down storage, and then losing the crop in the third month due to insufficient storage and runoff. What mechanism does the model use to avoid this situation?

My main criticism of this paper is that it hardly deals with the admittedly most important aspect of this paper. On page 2711 the authors admit that "The methodology described in this paper heavily relies on preferences formulated by water users". There is no information how to select coefficients $a_{ij}$ on page 2714, line 14, which is of critical importance to the presented methodology. Neither is there a review of how responsive the stakeholders were to the selections made in the case study presented in this paper, nor how well they understood and accepted the process. Fuzzy logic has always been a hard sell for storage operators and other stakeholders. Why is the user participation in formulating the weight factors not even mentioned in the case study? Table 3, which should represent outcome of this process, is not even referenced in the text. Was there full stakeholder participation during the case study to justify this approach, or was the case study de facto an academic exercise? Has this paper changed anything in the way the stakeholders manage the Colbun reservoir nowadays? If not, how do the authors propose to change this in order to make their work applicable in practice?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 2707, 2006.

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