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Hydrology and Earth System Sciences Discussions

**HESSD**

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Interactive Comment

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

**A. Tilmant et al.**

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The authors would like to thank Reviewer 2 for his valuable comments and sugestions. We hope this note together with the revised manuscript will clarify the issues raised by the reviewer.

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?

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 nonlinear 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.

A justification of the use of SDP is now provided. We have also provided more details about the SDP model, especially related to the discretization of the state-space and the approximation of the discrete cost-to-go functions by cubic splines, which was not mentioned in the first version of the manuscript. This approximation removes the distortions found in policy tables obtained with the traditional (fully) discrete approach. Appropriate references are also included.

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

Yes.

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



HESSD

3, S1624–S1633, 2006

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.

The sets of flexible constraints and fuzzy relation are finite because the number of operating objectives/stakeholders/water users is finite. Here there are J water users. Hence, for each alternative r (release decision), there is a profile with J membership grades, one for each water user.

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.

A map is now included in section 5

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

Page 2709, line 5, change “determines” to “determine”;

The manuscript has been modified

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

The manuscript has been modified

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);



HESSD

3, S1624–S1633, 2006

Interactive Comment

The manuscript has been modified

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.

The definitions of feasible, non-feasible and partially feasible solutions have been added in the first and second paragraphs of section 2. The physical constraints, which are imposed through the design of the hydraulic infrastructures, are considered as hard constraints because they are well defined. In a “centralized” management, the services provided by the water resource system are generally considered as operating objectives. In a “decentralized” setting, one water user may have his own objective and consider the other objectives formulated by the other water users as additional constraints. For example, a hydropower company may seek to maximize its revenues over some planning period and may consider the minimum flow requirements as additional constraint because they prevent the hydropower company from reaching its goal.

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

The manuscript has been modified

Page 2715, equation (8), a question: Why is the weight factor wj 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).

Equation (8) is the weighted geometric mean. Equation (6) was selected because it was felt to be more intuitive than the weighted geometric mean. This issue is now discussed in section 3, before Eq. (7).

Page 2716, Section 4: provide pros and cons of using dynamic programming as opposed to other mathematical programming techniques, especially LP and NLP and



HESSD

3, S1624–S1633, 2006

Interactive Comment

explain why it was felt that SDP was the right choice.

The first paragraph of section 4 now explains why SDP was chosen. The most common mathematical programming techniques are linear programming (LP), dynamic programming (DP) and non-linear programming NLP. Deterministic models use a specific sequence of streamflows to determine operating policies. Stochastic models, on the other hand, rely on statistical descriptions of the streamflow and forecast process to obtain operating policies. In this paper, a stochastic dynamic programming SDP formulation is adopted because it can handle stochastic inflows, non-linear objective functions and constraints, and a large number of stages. In addition, since the problem only involves one reservoir, the curse of dimensionality found in SDP is not an issue.

Page 2717, equation (12): this equation does not show how the minimum and maximum releases (rmin and rmax) from storage are set. In particular, does this model take into account that rmin and rmax 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?

When dealing with mid-term planning models (infinite horizon, weekly/monthly time step), these hydraulic constraints are not taken into account because the problem is usually dominated by other aspects (hydrologic uncertainty, hydrologic variability, trade-off between conflicting objectives, etc). However, when dealing with short-term and/or real-time operation, these constraints are becoming more important and are therefore considered. The dispatch of hydropower plants is typically carried out in two steps. The first step corresponds to the mid-term planning and provides, among other things, future water values. Then the second step refines the results of the first step by determining optimal decisions on shorter time steps and horizon. The second step also uses the future water values, i.e. the marginal value of water in the reservoirs, as a boundary condition to arbitrate between the use of water now or saving for future use when it becomes more valuable.

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

The term cost-to-go function is defined earlier (first line, 2nd paragraph, section 4). The cost-to-go function represents the future costs (benefits) associated with the operation of the system.

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.

On page 2717, line 18 (first version of the manuscript) we said that the time span of a stage is one month and the period is one year. Hence, we are dealing with a mid-term reservoir operation planning problem where some hydraulic constraints are usually neglected (see our response concerning the head effect on release). However, the head effect on hydropower generation is taken into account through the membership function of the hydropower objective, which is the production function of hydroelectricity normalized on the unit interval. This function takes into account the reduction in hydropower generation due to decreasing head on turbines. Note that this head effect on turbines can also be neglected when the variation of head is negligible when compared to the normal head, i.e. for high-head power plants with little storage capacity. But this is not the case here. The membership function for hydropower is displayed in a new Figure together with other membership functions.

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

The manuscript has been modified. We have also included a map of the river basin.



HESSD

3, S1624–S1633, 2006

Interactive Comment

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.

We have now included more details on the simulation results. The volumes of water diverted for lateral irrigation are also simulation results. In terms of losses, we are mainly talking about spillage losses (for the hydropower company and lateral irrigators) and evaporation losses (all users). The evaporation losses are given as a function of the volume of water in storage. The parameters of this function vary from month to month.

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.

You are right. We forgot to mention that the flow transition probabilities were derived from a 84 year-long time series of monthly streamflows (1917-1999). The simulation is only carried out for the last 13 years because the reservoir was constructed in the late 80ies and we want to compare simulated with historical storage levels. This is now included in the new version of the manuscript (after Eq 14).

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

The manuscript has been modified.

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 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?

For this case study, irrigation water demands are legally binding constraints for the hydropower company. In other words, the hydropower company can operate as long as it does not negatively affect irrigators prior water rights and that's why monthly volumes must be allocated for lateral and downstream irrigation. These volumes vary within the year but do not change from year to year. Of course, this situation reduces the flexibility of the hydropower company ( its ability to move water in time) and has therefore an opportunity cost.

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 aij 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?

You are right. This aspect of this study was not discussed in the first version of the



HESSD

3, S1624–S1633, 2006

Interactive Comment

manuscript. This is essentially an academic exercise as this reservoir is currently dispatched by the Independent System Operator (ISO) of the SIC (system interconnectado central), one of the two hydrothermal electrical systems in Chile. It is not managed by the water users. We chose to illustrate the concepts using this case study because the first author had audited the operating rules for an investment company and therefore had become familiar with the system. Hence, this reservoir is used for illustrative purposes only. This is now explained in section 5 (Case study).

Again we agree with you that fuzzy set theory and fuzzy logic are not popular amongst reservoir operators. As this is essentially an academic exercise, and because our primary objective is not to convince stakeholders but to show what would be the implications of a "decentralized" management, we think that this should not be a major issue for this paper.

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HESSD

3, S1624–S1633, 2006

Interactive Comment