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Discussions

Interactive comment on "The cost of ending groundwater overdraft on the North China Plain" *by* C. Davidsen et al.

Anonymous Referee #2

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The manuscript presents a method consisting in a Stochastic Dynamic Programming (SDP) management model for a system including one reservoir and one aquifer. The aquifer is represented as a box model. The problem is solved with a combination of Genetic Algorithms and Linear Programming (GA-LP) to tackle the non-linearities and non-convexities caused by the head-dependent pumping costs. The framework is applied to the Ziya River system (North China), where groundwater overdraft has led to a significant decrease in the aquifer levels. The results of the SDP are provided in the form of water value tables used as prices in a forward-moving simulation run. The estimated costs given by the model when the aquifer levels reach equilibrium, in comparison with business-as-usual values not considering groundwater overexploitation (previous paper), serve as estimation of the cost associated to a recovery in the

aquifer level.

The provided manuscript refers to a critical problem in many arid and semiarid areas: persistent groundwater overexploitation, which has caused considerable damage in both water quantity and quality across the world. The methodology is well-presented and exposed in the case study. Coupling stochastic programming and groundwater simulation is cumbersome, and new approaches to alleviate its complexity and transform those results into management policies could support the application of those tools in water resources management. For that, this paper has a considerable potential interest for publication in HESS. In addition, it is well-written and well-structured. However, there are some important points that the authors should address in order to enhance the manuscript. General comments

The method strongly simplify the hydrology (just a Budyko model for assessing runoffs, and fixed % of groundwater recharge no justified), as well as the spatial representation of the system (all surface reservoirs lumped into a single one) and the groundwater simulation (a lumped box model with unclear if not missing representation of stream-aquifer interaction). Despite the presentation as a hydroeconomic model, the economics is also highly simplified (constant water demands, constants curtailment cost). These simplifications need to be justified, including an analysis of how realistic these assumptions are. This can be done along the text when the assumptions are presented. Overall, the limitations of the modelling approach should be clearly stated either in the Discussion or the Conclusions.

The paper constantly refers to the previous analysis done by the authors, published in another paper, whose results represent the business as usual situation, not shown in this one with the exception of the total annual cost (Discussion). Thus, the presented paper looks like a second part of the one previously referred, since which it is quite hard to fully understand it without the other one. Maybe the authors could briefly include more description of the method and results for the business as usual situation, or update those at the light of the findings of this paper, in order to facilitate the comparison between both alternatives in this paper.

Comment 2 - Introduction While being successful in presenting the problem, the Introduction seems a little confusing. At first, one would expect some comments about why is important to jointly manage surface and groundwater prior to enumerating the state-of-the-art on conjunctive use optimization. While the division between deterministic and stochastic programming is adequate, the state-of-the-art presented consists in describing several references rather than explaining briefly both approaches supporting both explanations with references. It is said in the paper that "has been addressed widely in the literature" (which is true) but then only 4 references for deterministic and 2 for stochastic are shown. I would prefer to not explain what has been done in a little number of papers, but to discuss the different approaches employed and then enumerate the references. Besides, the review seems to not have moved prior to the 90's, when the topic appeared in the 60's and 70's.

Comments 3 (Case study) p. 5935. It is assumed that the full storage capacity can be managed flexibly without consideration of storage reserved for flood protection or existing management rules. Why ? So how flood protection pools are taken into account? Are you using a realistic useful storage? p. 5935. . . . analysis of dynamic interactions between the groundwater and surface water resources. It seems that the box model that you use for groundwater does not account for any dynamic interaction between groundwater and surface water. Is this correct? If that is the case, groundwater discharges (outflow) and stream-aquifer interaction are not considered . . . Please show that it is correct to neglect this groundwater outflow components. Otherwise, we have an incomplete groundwater balance.

A rainfall-runoff model previously used in the paper of the business-as-usual run. It is unclear if you simply took the resulting inflow values of that study or if you update that model. If it is an update, then the calibration results should be presented. In addition, I do not see the point of developing a daily model and then aggregate the results. It would have been easier to directly develop a monthly model. Besides, it is said

that the recharge is estimated upon the precipitation, using the average precipitation value corresponding to the inflow class as characteristic value. That assumes a perfect correlation between precipitation and inflows, which is uncommon. Would have then possible to be included in the Markov chain? . . . although it would suppose an increase in the curse of dimensionality phenomenon . . .

Comment 4 (2.2. optimization model formulation) There is a variable named "groundwater spill". Does it refer to "groundwater discharge". Where does physically go this discharge? Please give an explanation about what means this spill, and how this is modeled.

Comments 6 (2.4 Solving non-linear and non-convex sub-problems) The non-linearities tackled by your GA-LP algorithm are the decision variables regarding final storages. In an alternative SDP approach, these variables are kept discrete. If you keep them discrete, the problem becomes linear again and there is no need to maintain the time-consuming GA procedure. In fact, that ability to work out non-linearities is one of the main advantages of Dynamic Programming (DP). Why have you not taken the ending groundwater table Vgw,t+1 discrete? It would have saved you a huge amount of time, although with less quality in the results, as you point out. I would think it would have been worth it, specially regarding at the steady water values found in the aquifer.

Comment 7 (2.4 Solving non-linear and non-convex sub-problems) A misunderstanding regarding piecewise linear interpolation is found in this section. You said that, according to Pereira and Pinto, piecewise linear interpolation requires strict convexity. However, Pereira and Pinto used a Benders decomposition, which employs piecewise linear approximations and requires convexity, but it is different from the regular procedure, which does not need the cost-to-go function to be convex. You can fit a linear function between your point and the neighboring ones, as you did when interpolating the future costs with cubic functions. Please correct that.

Comment 8 (3 Results) In the first paragraph of page 5946, it can be read that, at the equilibrium groundwater storage level, the willingness to pay is equal to 2.3 CNY m-3. In Figure 6 user's price for groundwater is always below that threshold if initial groundwater storage is at equilibrium. If the user's price for groundwater is always below the curtailment cost, why is the model curtailing the wheat agriculture? One would expect that pumping would fluctuate according to surface water availability, but without any curtailment, since it is more profitable to pump. Is there any constraint forcing that curtailment? Please elaborate.

Comment 9 (3 Results) Why a reservoir storage evolution plot does not appear in the manuscript? It would be important to see the surface and the groundwater storage in order to identify possible conjunctive use patterns. Please include the surface reservoir storage evolution or explain why it is not necessary.

Comment 10 (4 Discussion) In the first paragraph of page 5948, you say that SDDP only samples around the optimal decisions and, consequently, you will not be able to get the complete set of shadow prices for all state combinations. However, the SDDP sampling procedure actually employs samples that are not subjected to a pre-defined grid and, therefore, the samples are not evenly distributed across space, concentrating in the region located near the optimal decisions. The extrapolation process applied in SDDP covers the whole space but with different levels of accuracy depending in which region you look at. The difference between SDP and SDDP regards to the fact that the SDP results have the same accuracy for the whole space, while the SDDP results' accuracy varies across the space, focusing near the optimal decisions while usually decreasing when moving far from them. With SDDP you will get a complete set of shadow prices as well, but with different accuracy levels: some of them better than SDP and some of them worse. Choosing between them does not regard to having or not shadow prices, but to the degree of accuracy that you can accept on them. Please re-elaborate the comparison between SDP and SDDP.

Comment 11 (3 Results and 4 Discussion) Although a sensitivity analysis was made with regard to the water demands, the curtailment costs and the transmissivity; there

are other sources of uncertainty that must be taken into account. Factors like inflow and storage discretization, assumption of perfect correlation between rainfall and inflow, pumping costs estimation, usage of a lumped model for the aquifer and so on, add a considerable amount of uncertainty to the problem. An explanation about the implications of those sources of uncertainty in the results should be added to the manuscript.

Comment 12 (5 Conclusion) As presented, the conclusions would not attract the reader. They seem to appear as part of the discussion rather than a separate section. It should be re-organized in order to clearly highlight what are the novelties of the study and what conclusions can be extracted from the methodology applied and the results obtained in the case study.

DETAIL COMMENTS

COMMENT 1 (page 5934, line 11) One would expect here references about the water value method, not about the SDP one. In addition, Pereira and Pinto (1991) did not used SDP, but SDDP.

COMMENTS 2 (page 5935) Line 11: upper storage capacity ?. This is storage capacity, what it is represented through a upper bound constraint, but the combination of terms here is unclear. I suggest to remove "upper". Please correct it in all the times this appears in the text. Line 24: Why only the upstream users have a pumping limit?

COMMENT 3 (page 5940, line 21) Replace "the thickness of the aquifer" by "groundwater pumping"

COMMENT 4 (page 5941, line 1) Is it realistic to assume an even distribution of total pumping across all the wells?

COMMENT 6 (page 5943, line 18) Replace "program" by "programming".

COMMENT 7 (page 5944, line 24) I think that, besides the larger storage, one important reason beyond the stability shown by the groundwater values is the fact that the interaction between surface water and groundwater is not reprsented. If some sort of stream-aquifer interaction had been found, the groundwater values would have been affected by surface waters and vice versa.

COMMENT 8 (page 5945, line 1) Rather than decision rules, the water values tables act as pricing policies. In fact, you do that in the Discussion and the Conclusions sections.

COMMENT 9 (page 5947, line 17) You should add “with SDP” after “feasible today”. Other alternatives are able to handle large water resources systems.

COMMENT 10 (page 5947, line 24) Has a simulation model with higher spatial resolution been used? If not, please clearly indicate in the results section (page 5945, line 1) that the forward-moving simulation uses the same system scheme.

COMMENT 11 (page 5949, line 24) I think that the reason beyond the small differences between SDP and DP regard to the inclusion of the aquifer rather than a very good performance of the SDP algorithm (although it is good). If you consider groundwaters in the analysis, their buffer value gives a high robustness to the surface system. This is reflected in the fact that the SDP empties the reservoir almost every year while not doing that if groundwater was not considered: it can always pump so it hedges the reservoir in an aggressive way.

COMMENT 12 (page 5950, line 15) The groundwater results are independent in the recharge as well. It should be added to the list.

COMMENT 13 (page 5951, line 4) I do not understand how the opportunity costs are reduced if electricity prices grow. This would apply exclusively if all the demands could freely pump and all of them had the same pumping head, which is not the case (you have demands that are subjected to pumping quotas while other cannot pump). However, the fact that electricity prices can be used to internalize the groundwater prices is valuable regardless of that.

COMMENT 14 (page 5951, line 7) Rather than opportunity cost pricing (OCP), the

name should be marginal cost pricing (MCP). Please replace this definition hear and in the rest of the document.

COMMENT 15 (page 5951, line 10) The title of the section should be “Conclusions”.

COMMENT 15 (page 5951, line 20) The non-convexity is caused by the head-dependent pumping costs rather than the inclusion of the groundwater reservoir.

COMMENT 16 (page 5958, Table 2) This table has not been cited in the text. Remove it or cite it.

COMMENT 17 (page 5963, Figure 4) In the surface water values part of the Figure, Vgw must be 50% rather than 80%.

COMMENT 18 (page 5965, Figure 6) Do you mean Davidsen et al (2015) rather than Davidsen et al (2014)? If not, please add Davidsen et al (2014) to the reference list.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 5931, 2015.