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Hydrology and Earth System Sciences Discussions

Interactive comment on "Effect of flow forecasting quality on benefits of reservoir operation – a case study for the Geheyan reservoir (China)" *by* X. Dong et al.

Anonymous Referee #2

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General Comments:

As an economist I am not well able to judge the hydrological aspects of the paper; I do have some knowledge, however, of optimization theory. From this perspective, the paper seems to carry out the analysis in a workmanlike fashion. The focus of the paper is on the constraints - the flows - and the optimization procedure. From the economics perspective, the objective function is key, and this (on p. 3786) is not well explained. I elaborate in the specific comments below. The second issue that is central, but seem not to be adequately elaborated, is the key role played by uncertainty. My

basic conclusion is that the paper is a useful step in understanding reservoir flows, but lacks too many details to be a useful tool in understanding any particular reservoir.

Specific Comments:

1. The objective function.

The objective function is described over three pages, and frankly I cannot claim to be that much the wiser after reading it several times. The mixing of details (physical constants) with discussion of the meaning of the objective function is confusing. The main part of the objective seems to be the product of the level of water released times a change in water levels. Why is this the right objective? To this is later added a penalty function - the key coefficient of which is then calibrated to meet a certain objective. Would it not be better to add the constraint directly, and treat the coefficient as a Lagrange multiplier? From the perspective of an economist, it seems unlikely that the benefit of power generation and flows is time invariant. Surely demand is higher, and so power more valuable, in some seasons that others. Similarly, I imagine that the benefit of water releases depends on the planting cycle. Absent discussion of these key economic variables, it seems necessary to regard the model as a "toy" model rather than a serious attempt to understand the optimal policy.

2. Uncertainty

Uncertainty enters the model in several places. First, the model is based on a single year of data. Given that there was a single flooding event that year - presumably quite significant for the optimal policy - is it not necessary to use more years of data so the arrival process of flooding can be properly understood? Was is the mean annual number of floods? The peak over twenty years? How often does it occur. I didn't see these questions clearly addressed.

Second, uncertainty enters into the forecasting through the coefficient ϕ. It isn't clear to me how this was calibrated. More to the point, one expects that the longer the

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horizon of the forecast the less the accuracy. So one expects a tradeoff between longer forecasts and more accurate shorter forecasts. I would have thought this played a key role in the optimization, but was unable to find a portion of the paper that came to grips with it. Again, this is along the lines of the model being a "toy."

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 3771, 2006.