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Discussions

Interactive comment on "A Framework for Advancing Streamflow and Water Allocation Forecasts in the Elqui Valley, Chile" *by* Justin Delorit et al.

Anonymous Referee #2

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Delorit et al.

This manuscript describes the implementation and evaluation of a streamflow forecasting tool for an arid basin with a high water demand in northern Chile. Three types of forecasting models are used, two of which are referred to as "statistical", and one as "dynamical". These models are then coupled to a simple water allocation model, and evaluated by running them with historical input data in hindcast mode.

The first model uses principal component regression to fit various large-scale meteorological variables against streamflow. The second model is simpler and only uses an ENSO index. A third model uses precipitation data from the NCEP climate forecast system (though it is unclear what lead times are used) and relates these to streamflow

using a quantile mapping approach.

These models are then run with lead times between 1 and 5 months, and linked to water allocation using a water allocation model.

The study finds "mixed success" in the models' capacity to predict streamflow and related water allocation levels.

The manuscript is generally well-presented and well-written and most of the figures are of good quality. However, with regard to the content, I identify many issues of varying severity. But all together I think that they warrant a very serious revision of the study.

First, the scientific contribution is currently unclear. To be blunt, the study currently reads as a consultancy report, with an extensive description of the study case, and a very elaborate description of the model implementation, but very limited scientific contextualization and discussion. A scientific study should be different. Rather than solving a specific issue for a specific location (as I think this study currently does), a case study should be used to gain broader insights in hydrological processes, modelling concepts, and/or improvements of existing modelling tools and methods. Here, the current manuscript falls short in my opinion. This starts with the title. What is meant with a "framework" in this context? Essentially, the approach couples a streamflow forecasting model to a water allocation model. That is a sensible, but not particularly novel approach, and can hardly be considered a specific modelling framework. Similarly, the word "advancing" is probably redundant. I think that a more appropriate title would be "Evaluating model-based seasonal streamflow forecasting for the Elqui Valley, Chile".

Next, the study does not have a clear scientific question or (ideally) hypothesis. As highlighted on p.25 line 28 - 30, it intends to "develop an understanding of the mechanisms contributing to austral summer streamflow in the Elqui Valley, investigate model skill at varied forecast leads, and produce forecast-based water-right allocations". That is of course very broad and vague, and as a result, the study does not really make an impact on any of them. As for the first objective, I do not think that I gained much insight in climatic teleconnections with streamflow beyond what is quite well known. As for investigating model skill, I am not sure what conclusions can be drawn that have relevance beyond the particular case study. And I am surely happy to believe that producing forecast-based water allocations is of great local interest, but again the scientific value is unclear.

Furthermore, many aspects of the modelling approach remain unclear. One reason for this is the description of the data and models is intermingled, making it very hard to follow. I suggest splitting this section up, describing first the data that are used and their characteristics (e.g., coordinates, temporal and spatial resolution, source). Then, a next section can describe the modelling approach and make a clear argumentation as to why those specific models were chosen. This is important, because the selection and design of models is odd. Why, for instance, was a model built that only uses the ENSO index? Clearly this will have limited predictive capacity. If the purpose is to evaluate the predictive capacity of the ENSO index (though I am not sure why one would want to do this) then it is probably better to set up a specific statistical model such as a Generalized Linear Model, which allows for a more rigorous evaluation of statistical significance and predictive power of different predictors.

Also for a problem like this, the most obvious approach to streamflow forecasting would seem to be to route precipitation forecasts or observations (depending on the lead time) through a hydrological model. Seasonal forecasts are globally available at increasing resolution (well above the 250 - 600km mentioned on p.11 l.24 - the reference of Giorgi (1990) is probably out-of date!). Additionally, because of the large time lag between precipitation and streamflow it may well be possible to use observed precipitation, especially for the shorter lead times (August, September predictions). In fact, given that streamflow is so strongly snowmelt-driven in this basin, I wonder whether observations of snow cover and snow depth during the austral winter might be variables with a very strong predictive power. All this to say that the scientific value of trying to make predictions based on large scale climatic predictors is questionable, and at least needs to be

justified much better.

Next, I also had a hard time understanding the context of the water allocation model. Why is the target reservoir volume in February 50%? This would seem very much to me. Also, the fact that any shortfall of this 50% needs to be carried over to the next year (eq. 3) suggests that the reservoir is not replenished during winter. Is this realistic? This would seem to depend strongly on winter precipitation.

As for the "summary and discussion" section, this is very thin and little informative. I suggest elaborating the "discussion" and adding a separate "conclusions" section. This is not only more usual for HESS, but I also think that the lack of conclusions may be somewhat indicative of some of the main problems identified above. If anything, adding a "conclusions" section would be a very useful exercise to think about what particular scientific conclusions can be drawn from the study.

Lastly, I think that the manuscript can be shortened. It contains too much undergraduate text book material that can be removed, e.g., on principal component analysis, Global Circulation Models, performance metrics and similar tools.

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