Hydrology and Earth System Sciences Discussions

Interactive
Comment

Interactive comment on "Improving the characterization of initial condition for ensemble streamflow prediction using data assimilation" *by* C. M. DeChant and H. Moradkhani

Anonymous Referee #1

Received and published: 3 September 2011

The paper addresses the question if ensemble streamflow predictions can be improved by assimilation of observed snow water equivalent (SWE) data into a hydrological model. Specifically the paper is concerned with the problem of initial conditions of a forecast model run. Here a case study of the Upper Colorado river basin and the existing method of ensemble streamflow predictions (ESP) is presented. It is aimed to improve ESP by an assessment of the uncertainty in estimating the initial conditions of the hydrological model in use. Thereby the authors make use of SNOTEL observations of snow water equivalent (SWE). They show that there is some improvement compared to the traditional ESP technique and conclude that data assimilation (DA) of SWE data

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

has potential for streamflow predictions. They also point out to the problem of SNOTEL station representativity, which shows to effect the resulting predictions in various ways. However, this apparent and interesting problem is not quantified by the authors.

The question of how additional data sources can be used to improve hydrological forecasts is practically and scientifically interesting and important. However, the approach taken by the authors is not new and the paper may be regarded as another case study. Even though the authors show that on average improvements over the existing method can be made, it is hard to judge these improvements as no other comparisons e.g. to other studies are given. There are already other studies which showed that DA can improve probabilistic predictions. However, the authors touch a generally important question, which is about the representativity of data sources fed into a model. This is however not investigated in detail. Thus I recommend to improve the manuscript along these lines and would be available for reviewing a resubmitted version. Further remarks and comments on the paper follow below.

General Comments

The authors show that the SNOTEL observations may not really be representative for the basin as the observation density in high and low elevations is rather poor. However, with assuming that the SWE states by the SNOTEL observations is correct, they may introduce a bias into the model predictions. Fig. 4 shows that the DA technique has usually lower values of SWE and as stated on P7219L10 -L13 ESP-DA shows smaller ensemble prediction ranges than the traditional ESP, which is counter-intuitive but related to this bias introduced by the low representativity of SNOTEL. I feel that the uncertainty introduced by the different observation densities of SNOTEL observations should be quantified to get more confidence in the resulting predictions. This is important for (i) acceptability of the method itself and (ii) the question which properties of data sources for DA are relevant (such as representativity). Further, it may be worthwhile to constrain the DA technique also with observations of the target variable, i.e. streamflow.

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

P7219L13 and Figure 4: Is there a relation of the prediction uncertainty (deviation, IQR) and the value of SWE in the initial conditions? I think that the paper could be improved by a thorough characterization of the initial conditions (e.g. volume stored as snow vs. seasonal streamflow volumes or the spatial variability in SWE, etc.).

Figures 4-7: Generally I find it tedious to derive general conclusion from these multi-panel figures. First, there are 15 sub-basins introduced. But it is not clear to the reader if the results shown are aggregated for all 15 basins and then how large are the differences of prediction accuracy between these basins. Further, there seems to be some effect on prediction accuracy of (i) season, (ii) individual years (is dataset large enough?) and (iii) lead time. But there is no assessment of these effects in terms of verification measures. Instead the verification presented with Figures 8-9 aggregated almost all data (15 basins * 3 month * 3 years). Further, while Fig.4-7 also shows results for June (has poorest DA results) this month is left out in the verification figures.

P7219L25: it is not clear what reference forecast has been used to compute the RPSS. Section 3.3 suggests that a climatological reference has been used. From my point of view the ESP should be used as a reference forecast.

Results and Discussion: The results obtained are not discussed in the light of existing literature. Thus it is hard for the reader to judge the results and improvements obtained by the approach. Further the Discussion and Conclusion section contains repetitions and the conclusions reached are not very significant.

Minor comments

- P7209L23: Andreadis and Lettenmaier (2006) paper not in references list
- P7215 - 3: why do you use 500 ensemble members, is this recommended?
- References: 9 out of 34 refs are written or co-authored by the authors
- Fig. 1 Some elevation information might be useful, Eventually use a hill-shading.

- Fig. 2 text size and bar widths are not proportional, add elevation levels to xlab annotations

- Fig. 4 display is not very informative

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 7207, 2011.