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Hydrology and Earth System Sciences Discussions

Interactive comment on "Adaptive correction of deterministic models to produce accurate probabilistic forecasts" *by* P. J. Smith et al.

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The authors thank S. Regonda (SR), E.E. Shamir (ER) and the anonymous reviewer (AR) for their comments and suggestions.

Two of the reviewers (AR & SR) request further analysis of the forecast results to support the conclusions drawn in the paper. A number of summaries of the forecast performance (e.g. AR suggests Nash-Sutcliffe performance of deterministic forecast summaries, SR considering in more detail the precision and well as coverage of the probabilistic forecasts) and subdivisions of data (e.g. SR suggests division into rising and falling limbs) are suggested. A revised manuscript will consider a number of the analysis suggested and report in detail on those that offer important diagnostic informa-

tion relating to either: (a) systematic changes in the performance of the adaptive gain in calibration; (b) discrepancy between the performance of the adaptive gain correction in calibration and validation; (c) allow the performance of the corrected forecast to be compared to that of the deterministic model; (d) assessing where the assumptions in the methodology may be being violated.

AR and ER also experienced difficulties with the reproduction and clarity of the figures. This will be addressed in any revised manuscript. Responses to the specific comments of SR and ER not covered in these two general points are listed below:

Response to detailed comments by E.E. Shamir

Title: There are commonly used adjectives that are associated with probabilistic forecast. If the described procedure improved the accuracy of the mean of the forecast you might want to consider adjectives such as skill and/or reliability. Accuracy might be a better fit for a deterministic forecast.
This is an astute observation and the authors will consider revising the title to appropriately reflect the results in any revision.

There is a missing statement in the introduction with respect to the objective/s and the knowledge gap or the specific problem/s that this study desire to solve.
A further paragraph will be added to the introduction outlining in more detail the objectives of the study.

P. 598 – Paragraph 1: You might want to look at...
A large number of papers exist using various combinations of non-linear Kalman filtering techniques. The reviewers provides a citation to one such paper in a hydrological setting that we are happy to site as an example.

...in this manuscript being described as 9 different statistical models. - q is undefined
The parameter q is defined implicitly in the equations at the start of page 601, this will be amended to clarify the status of q as a parameter of the gain model.

P 600 below eqn. 3 - unclear statement "evolved stochastically according to local level or generalized random walk models" - please explain.
Local level or Generalised random walk are two names for the family of stochastic model used in the evolution of the adaptive gain. This will be clarified.

p. 608 bottom of the page – please explain the statement "such biases often exist ... to achieve acceptable forecasting model"
This refers to the artificial selection of an physically unrealistic base flow in the hydraulic model (such as that shown in Fig. 2) to aid in the modelling of river response at higher water levels or discharges.

I am unclear for the purpose of presentation of the upper panes in figures 3 and 4 and their diagnostic inference. You might want to elaborate.
The assumptions of symmetry in the random variables inherent in the Kalman filter imply that, when suitably standardised, the forecast residuals should come from a uni-model, unbounded, symmetric distribution. Clearly this cannot be the case since water levels are bounded below (i.e. they cannot be less then the level of the channel bed) but these panes offer some guidance as to how closely this symmetry is approximated.

Response to detailed comments by S. Regonda

The paper is concise and well organized, however, the content in sections 2.2 and 3 makes it tough to understand technical details and reproduce the technique. Improving these sections in terms of additional details and with a better presentation should improve the readability of the paper and assist if a person wants to reproduce the technique.
Section 2.2 and Section 3.1 provide a concise reproduction of the Kalman filter equations and the corresponding Gaussian maximum likelihood methodology of parameter estimation. Both these methods have previously been used within the hydrological sciences and are standard time series analysis techniques. Section 3.2 present a novel estimation technique based around commonly used statistical summaries. While the

authors are happy to revisit these sections, provide additional references and clarify the points raised in detail below the authors feel little would be added to the paper by providing detailed derivations that could be referenced elsewhere.

3. Page 612, line 11-13: unless detailed evaluation is done, it is considered as an overstated statement.
This will be revised if necessary given the additional analysis.

4. It is suggested to develop plots for adaptive gains with respect to lead time, and draw interpretation of the values of adaptive gains with respect to the deterministic forecast skill. Are these adaptive gains follow a pattern and/or could be changed easily by the forecaster? (Page 610, line 28-29)
Making use of patterns in the adaptive gain beyond those of a temporal nature recognised by the time series models used is undoubtedly an area for further investigation. This will be highlighted within the paper but pursuing such a path is beyond the scope of the current manuscript.

5. There is a clear difference in the performance of various time series techniques, and is suggested to discuss in detail; only the poor performance of IRW and DLLT are mentioned. Why models with smoothing parameters have superior performance only at shorter lead times? And, it is not always consistent.
Additional text will be added to the manuscript to address these comments. Damping parameters imply an a priori assumption that the gain, excluding the random component, will increase or decrease at the next time step. While such an assumption may help in short term forecasts it is unlikely to be useful at the longer lead times where the variations in the model forcing may differ more from the current situation.

6. In general, using of one year for training and three years for validation considered as a stringent validation criterion, however, in this case, it is OK may be due to the year that used for training is a significant event. However, to test the robustness of the approach, it is suggested to present results from the leave-one-year out cross validation *mode; drop a few years, calibrate the technique and then evaluate the method for the dropped period. Repeat the exercise for the entire period, thus all data will dropped and evaluated.*

A leave-one-year out cross validation of the type described would offer some insight into the robustness of the methodology in forecasting a flood event when the calibration data do not contain a flood event data. In the context of this study and type of catchment the methodology used is appropriate and the authors feel a greater insight is offered by a more detailed analysis of the current calibration/validation methodology.

7. Page 609, line 24-25: It is suggested to exclude points that fall outside the Gaussian distribution, but, not mentioned how to deal with these types of forecasts. Additional guidance is needed particularly when these forecasts correspond to large flood events.

It is not suggested to that forecasts that fall outside the Gaussian distribution are excluded. It is stated that significant difference between a Gaussian distribution and the empirical distribution of the forecasts residuals is caused by the prediction of low flow events (where the model has a systematic bias) and that by excluding these (low flow) time steps the empirical distribution becomes approximately Gaussian. This will be clarified in the revised manuscript.

9. Page 611, lines 11-14: Gives impression that the rising limb is always correspond to long lead times. Is it always true?

This sentence (which will be revised for clarity) does not suggest this since the rising limb is a function of the catchment not the model or forecast lead time.

10. Page 611, lines 21-23: Not completely true, did see bias in Figure 5 as well.

Bias is present in both Fig. 5 & elsewhere in Fig. 6. This sentence does not preclude this but simply states that during this time period assimilating erroneous observations introduces a significant additional bias.

11. Page 609, lines 28: 'reasonable approximation' needs to be supported quantitatively

This is supported quantitatively by the figure being discussed.

12. Cite literature related to time varying parameter techniques.

It is unclear as to where it is intended such references should be added, but suitable references will be added at available locations.

13. In the abstract section, it is mentioned that the technique performs better in certain situations but unable to find related information in the manuscript

The abstract will be amended to reflect the manuscript

14. Page 598, line 29-31: is not clear

It is unclear what is unclear.

15. Page 599, line 16-17, what are these robust error models and how these models address the issue, requires more information

Suitable references will be added.

16. Page 600, line 12-17: suggested to provide more information related to "evolved" stochastically according to local level or generalized random walk"

Local level or Generalised random walk are two names for the family of stochastic model used in the evolution of the adaptive gain. This will be clarified.

17. Page 600, line 15-17: Why generalized random walk is selected?

It is a parsimonious and interpretable family of models that has been used widely for such corrections within the engineering community (for example in control systems) that has been used previously with some success in hydrology.

18. Page 601, line 3-7: define symbols, and how one estimates noise variance?

The symbols will be defined. Estimation in explained in section 3.

19. Page 602, line 23-24: Is it OK to assume that xt has similar properties as error? What is Pt?

Errors should more appropriately read stochastic noise terms, this will be altered. Since xt is a linear combination of the stochastic noise terms it will share similar properties.

Whether these assumptions are the basis of a suitable correction scheme is then covered in the later discussion.

20. Page 603, it is not clear how equations (17) through (19) are obtained? How the information from the new observed is utilized?
These are standard Kalman Filter results, reference to there derivation will be given.

21. Page 604, how this initial variance is calculated?
As noted it is a parameter therefore it is estimated using the techniques in Section 3.

22. Page 608, reference for ISIS hydrodynamic model
A reference will be provided.

23. Page 609, line 26: are samples collected at every 0.25 hours, how 28 samples approximate 7hr?
Data are recorded every 15 minutes, this will be clarified in the text.

25. It is not clear why two calibration methodologies are selected.
The two methodologies selected represent a standard statistical analysis and an ad-hoc approach motivated by seeking the 'best' (in a least squares sense) expected forecast value then evaluating the variance of this forecast.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 595, 2012.