

Interactive comment on “Using a spatio-temporal dynamic state-space model with the EM algorithm to patch gaps in daily riverflow series, with examples from the Volta Basin, West Africa” by B. A. Amisigo and N. C. van de Giesen

Anonymous Referee #2

Received and published: 5 May 2005

While several methods have been proposed in the hydrologic literature for filling in gaps in time series, they do not optimally exploit the spatial and temporal information available. This is unfortunate as hydrological variables often exhibit significant correlations in space. In this paper, the authors propose an elaborate spatio-temporal modeling framework for patching gaps in daily riverflow time series. To illustrate the usefulness and applicability of their method the authors use daily runoff data from the Volta Basin in West Africa. By using a linear dynamic state space model in conjunction with the Expectation-Maximization algorithm for parameter estimation, the method is able to generate acceptable forecasts of missing flow values.

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The paper is well written, and I suspect that the proposed method is a significant advance over earlier work. However, I do have a few major concerns:

(1) The authors should compare the performance of the proposed spatio-temporal method with commonly used methods for filling in gaps in time series. Such a comparison is a necessary requirement to indeed confirm the claim of the authors that “the gap-filling method proposed here does make optimal use of all available spatial and temporal information” (see Page 3, Line 3-4).

In the conclusion section the authors state that (Page 15, Line 4-5) “the model works well when there is a good spatial correlation between the runoff series involved”. In this situation, why not using a classic Bayesian joint probability approach? I urge the authors to compare their approach, with the much simpler method set forth in the work by Wang (2001) based on a Bayesian two-site joint probability approach. A comparison with this state-of-the-art method, would better demonstrate the benefits of the proposed spatio-temporal method, and the paper would probably sell much better. For instance, consider Figure 3 with the observed runoff time series of 3 stations. Why is state estimation for the Bui and Bamaboi gauging stations needed, when there is such a good correspondence between their flow values? A simple regression approach would be much simpler to implement and probably generate nearly identical results.

(2) I suspect that the presentation of the proposed method and parameter estimation will be difficult to follow for most readers of HESS. The presentation is very technical with many equations. It took me considerable time to understand the basics of the methodology, and I am still not sure whether I have captured the approach. I am confident that the paper can reach a much larger audience when the methods section is rewritten in a more intelligible manner. Besides excessive adjustments, the clarity of the presentation can simply be improved by using headings for different portions of the manuscript. Moreover, the paper would further benefit when the authors would show more results. Too much emphasis is now on the presentation of the methodology.

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(3) I am still puzzled whether the documented case study of the Volta Basin will best demonstrate the advantages of spatio-temporal modeling. It seems that the relative poor quality of the data does not require such a sophisticated spatio-temporal framework to draw inferences. I note that this case study is interesting from a water management point of view, but I am wondering whether the authors should not use another, more spatially and temporally rich data set to establish confidence in their findings and method. This would require additional calculations and facilitate extension of the results section.

Specific comments

Title Please shorten. I would not explicitly mention the case study.

Abstract - last sentence: “Time series at the same station and spatially correlated stations in the same sub-basin”.

Page 2, third sentence: In principle one could calibrate simple models, and leave time gaps out of objective function.

Page 2, sentence eight: Please give reference to each method.

Page 3, third sentence: Claim is made here, but not proven in paper. See also major comment (1) and (3).

Page 4, third sentence: Remove “to be”?

Page 4 - 10, Section 2: Please use subheadings for various components. This will significantly increase readability.

Page 4, second paragraph: Where do the symbols alpha and beta stand for?

Page 4, second paragraph: Replace work “marks” with “denotes”?

Page 5, fourth sentence: Not a correct sentence. Please rephrase.

Page 5, sixth sentence: Dispersion or uncertainty of the state estimate?

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Page 5, Equation 2: Please distinguish between observed and simulated values. Maybe I am wrong (a common occurrence), but both x_t and y_t are used to indicate modeled as well as measured values. For instance, what does the part of the equation $x_t - Fx_{t-1}$ stand for, taking into consideration the state propagation set forth in Eq. (1). Same holds for the last element of Eq. (2).

Page 5, Equation 3: Here the various system matrices are estimated (denote with $\hat{\cdot}$). However, in all the other equations, they are used without $\hat{\cdot}$.

Page 7, first sentence: Where do the model parameters appear? There is no explicit model presented which contains the parameters. What model are we talking about (first order model)? Please present this, and discuss parameters and their hydrologic meaning.

Page 7, fourth sentence: How does r relate to t ? For me it is unclear whether this parameter estimation is done recursively, after assimilating each new measurements or in batch.

Page 12, Section 5: The authors use runoff values in the state vector. This is very confusing as runoff is not a state, but a flux! Please be careful.

Page 13 - 14, Section 6: The results and discussion section is very short, and does not establish much confidence in the method and findings. The authors should significantly extend their presentation here. Methods and algorithms are useful and needed, but how do these improve our hydrologic understanding.

Page 14, Section 7, second sentence “Parameter estimation was done using $\hat{\cdot}$ ”

Page 15, third sentence: “The model thus works $\hat{\cdot}$ ” Sure, one would certainly expect this. Again, I am wondering how the proposed method compares with available and simpler methods. These will probably also exhibit good performance when significant correlation between gauging stations is present in the data. What is the benefit of using this sophisticated spatio-temporal dynamic approach? (See also major concern 1).

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Interactive comment on Hydrology and Earth System Sciences Discussions, 2, 449, 2005.

HESSD

2, S138–S142, 2005

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