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Discussions

Interactive comment on "Towards predictive data-driven simulations of wildfire spread – Part I: Reduced-cost Ensemble Kalman Filter based on a Polynomial Chaos surrogate model for parameter estimation" *by* M. C. Rochoux et al.

Anonymous Referee #1

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I thank the authors for their replies. Several of them are convinving and their addition to the paper will help the readers. Nevertheless I still do not understand two important points in the paper.

Point 1: level set method?

What is this level set method where the algorithm starts from a discontinuous function (with zeros and ones) and selects c=0.5 as the front position? Note that c=0.5 does not even exist at the initial time. There should be a reference included in the paper to

support this approach. At first sight, it looks really unclear whether this approximation should converge to the solution of the propagation problem.

Point 2: filter?

There is a need for justification of the "filter". The algorithm should retain some optimality property to be a proper filter. Current version of the paper, even with the proposed improvements, does not support the use of the described algorithm. I have spent some time trying to understand/prove the correctness of the algorithm, without success. The filter is supposed to be supported by a "standard application of the EnKF" as in Moradhkani et al. (2005) and Durand et al. (2008, 2010). But in Moradhkani et al. (2005), the state is corrected, contrary to what is done in this paper. In Durand et al. (2008), and presumably in Durand et al. (2010), the parameters do not depend on time (which is exactly the Option 2 I mentioned in the review). So these references are not relevant. In addition, they only show applications of filters, not the derivations of the filters.

In the algorithm, the state is somehow hidden in the observation operator. The observation operator depends on the front position (at t-1). Hence it is different from one member of the ensemble to the other. This is a fairly unusual setting, which requires explanations and comments in the paper. Even more crucial is the consequence that the observation error and the parameters error are strongly correlated, which breaks a classical assumption for Kalman filtering. How any optimality property is retained in this setting is a mystery to me. I highly recommend that the authors clarify their method. Otherwise, the reader may be under the impression that the algorithm was simply written by analogy with a proper ensemble filter, without any mathematical justification.

I have to say that the paper is overall well written and that there is a lot of work behind it. So I feel embarrassed to insist on the previous key issues. But a scientific paper cannot rely on unjustified algorithms.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 2, 3289, 2014.