Reviewer #1

Your comments are in italics:

mmmm .... I think your response actually reinforces my suggestion of a real application that might require treatment of epistemic uncertainties in the structure and inputs.

We fail to see an argument in the follow-up response provided. None of the points we have mentioned in our previous response were discussed.

Epistemic uncertainty is difficult to explore as, by definition, it is not part of the prior parameter probability distribution. Hence all uncertainty methods have difficulties with this issue, and it is a given that the outcomes of any method of uncertainty analysis will be compromised.
Sometimes prior-data conflict is apparent from the failure of model outputs to span field observations when the prior is sampled. Though not noted in our paper, software from both of the PEST and PEST++ suites that work in conjunction with that described in the paper are able to detect whether this is the case based on samples of the prior that are required to build the DSI model.
Another means through which a defective prior parameter probability distribution can be detected is through estimation of parameters that violate the prior. Hence if, in history-matching the DSI surrogate model, a large number of parameters are forced to adopt values that are of low probability for independent Gaussian parameters, this can be construed as prior data conflict. A modeller would be well advised to take note of this.
Both of the above methodologies for detecting epistemic uncertainty are easily implemented in conjunction with workflows that are discussed in our paper. We are happy to add a paragraph to our paper to make this clear, if desired.
However we note that it is uncommon for papers on uncertainty analysis to draw attention to epistemic uncertainty in general, and prior-data conflict in particular, because it is obvious that uncertainty analysis is only as good as the prior – regardless of the method that is used to undertake uncertainty analysis. On the other hand, papers that are dedicated to methodologies that explore and expose prior-data conflict (rightly) focus on this aspect of uncertainty analysis. In short, we do not see that it is incumbent on anyone who wants to discuss an uncertainty analysis methodology to also discuss epistemic uncertainty, for everybody knows that (a) it is an issue and (b) it is unquantifiable but important.

We note that there have been some recent papers in which hyperparameters of highly-parameterized prior probability distributions are estimated at the same time as the parameters themselves. These methodologies do address, to some extent, inadequacy of the prior parameter distribution by treating these inadequacies as uncertainties. This is a worthy undertaking, but is also accompanied by a considerable amount of numerical difficulty. Hence they are worthy topics of publication in their own right.

See, for example:
Oliver, D.S., 2022. Hybrid iterative ensemble smoother for history matching of hierarchical models. Math. Geosci, 54:1289-1313,
Chada, N.K., Iglesias, M.A., Roininen, L. and Stuart, A.M. (2018). Parameterisations for ensemble Kalman inversion. Inverse problems 34 https://doi.org/10.1088/1361-6420/aab6d9

(It is also worthy of note that both of these high-quality papers demonstrate their methodologies using entirely synthetic cases – with less of a resemblance to hydrogeological reality than our own example.)

It is therefore our opinion that our paper covers the topic to which it is dedicated to the extent required to demonstrate the veracity of the method that it employs, in such a way as to allow other modellers to use this method with little difficulty. We make no claims that the methodology that we present is exempt from problems which beset all uncertainty analysis methods. We note this in our paper (and are prepared to note it further), but would rather leave the discussion of these important (but ancillary) issues to other papers.

I was not suggesting that the paper should be any longer - if the proof of concept is moved to a supplement then there is space for a proper application.

We respectfully disagree that this will strengthen the paper. Moving a proof of concept of a new method to the supplementary section is not helpful. A new method needs to be assessed thoroughly and this assessment is an integral part of method development.

The synthetic approach we have chosen allows for the required, thorough assessment. Again, a "real" case will not provide the information required to assess the robustness of the approach. And as already mentioned in the previous response, employing synthetic models is this is a well established, widely used approach- for good reasons.

Note that reviewer 2 also highlights that our synthetic method is well chosen. None of the issues you raise are identified as a concern. See our response on the points raised by reviewer 2 on the choice of the model.

Best,

H. Delottier, P. Brunner and J. Doherty