

Interactive comment on “A Bayesian Approach to Infer Nitrogen Loading Rates from Crop and Landuse Types Surrounding Private Wells in the Central Valley, California” by Katherine M. Ransom et al.

Anonymous Referee #2

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As stated by the authors, the manuscript is keyed to the assessment of the type of information that available groundwater quality data can provide towards the identification of nitrate loading rates from the various crop types spread across the Central Valley in California. The authors resort to a Bayesian analysis framework. The manuscript is a resubmission of a previously assessed work and I can see the relevance of the study to HESS.

The manuscript is well written and up-to-the-point, so I see no particular reason to delay its acceptance. There are only a few minor points which I think should be clarified

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before acceptance and doing so would require only a set of minor revisions.

A point is related to the statement about the degree of innovation. The authors state that they develop “an innovative statistical framework”. Then, they state that they “are not aware of any study that has employed Bayesian methods to estimate nitrate loading rates to groundwater.”. They then conclude by saying that “The novel Bayesian tool developed here ...”. I understand that it might be semantics, but I do think that the work is cast in the statistical framework of Bayesian analysis. As such, the authors do not develop an innovative statistical framework. Otherwise, they employ an existing framework and take advantage of it to solve a problem in an interesting way. Indeed, they then propose a new model within this general theoretical framework. This is my view, which seems to be in line with authors’ concluding remarks, and can be as debatable as the view of the authors, of course. I simply think the terminology should be clarified.

The authors state that “Records in the database collected between the years 2000 to 2015 were selected. Locations with data collected in multiple years were assigned the median nitrate value of all the recorded measurements in order to prevent multiple samples of the same well and associated landuse.”. I do concur with the approach. I am not sure if this course of action is somehow masking temporal dynamics of concentrations in the groundwater system. I am not sure there is an easy answer to this and I would just like to have the authors’ idea on this aspect.

I am not sure if the authors are explicitly considering measurement data uncertainty in their Bayesian framework, but I might have missed the information. In any case, I think that some information about this could be provided.

I understand the reasoning behind the use of Student t-distribution (or log-normal distribution for potential recharge rates) priors and I do agree with it. Did the authors try to consider other formats of priors and obtained the same results?

I understand that the authors finally employ “a total of 1,000 realizations per parameter”. Is the shape of the resulting target densities (or their key moments) depending

strongly on the number of realizations retained? It also seems that the number of realizations retained is relatively high with respect to the total number of realizations produced and some comments on this could be beneficial, to provide guidance to future users in other settings.

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