

Nat. Hazards Earth Syst. Sci. Discuss., referee comment RC2
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Comment on nhess-2021-290

Anonymous Referee #2

Referee comment on "A dynamic hierarchical Bayesian approach for forecasting vegetation condition" by Edward E. Salakpi et al., Nat. Hazards Earth Syst. Sci. Discuss.,
<https://doi.org/10.5194/nhess-2021-290-RC2>, 2022

General comments

The paper has objectives similar to those of the paper by Salakpi et al., NHESD, "Forecasting vegetation conditions with a Bayesian auto-regressive distributed lags (BARDL) model (<https://doi.org/10.5194/nhess-2021-223>). The aim is to forecast vegetation conditions leading to agricultural droughts. The paper presents a new method, as compared to the previous paper, based on a dynamic Hierarchical Bayesian Model (HBM). The data used in the two papers are the same, and the method for assessing the forecasts are also similar. The HBM model has the advantage of providing differentiated forecasts within the same region, as compared to BARDL that provides homogeneous forecasts within each region. The BARDL model is used as benchmark for assessing the improvements provided by the HBM model. There are therefore lots of repetitions between the two papers. The present one refers to the previous one to present the data used in the study. This is not acceptable, as a paper should be self-contained and a minimum of information should be provided about data. The problem would be solved if the two papers were merged in a unique paper.

Concerning the present paper, I find it is well written and the analysis is sounded and honest. I only have a few minor comments listed below.

Specific comments

1/ P.1 line 16: the number of affected persons is different than in the previous paper

2/P.5. Section 2.1 should be developed

3/ p. 6 line 116: ROI is not defined

4/ p.7 line 135: the authors mention that seasonal means are subtracted, but how are the seasons defined?

5/ p.9 lines 172-175: this section is not clear

6/ Figure 4: I do not understand how to read this figure

7/ p.10 eq.(3). This is strange to present the equation of the model after the presentation of the HBM model that refers to the parameters listed in Eq. (3).

8/ p.12 line 253. The BARDL model based on a "no-pooling" is the same as the model presented in the previous paper, isn't it? If this is true, this should be stated.

9/ p.12 line 261: the differences between the two models in Figure 5 are not so large. So the claim that HBM model is more accurate than BARDL is not fully justified.

10/ Revise Figures 6, 7, 9, 10, 13 as the full line and dotted lines cannot be distinguished.

11/ p. 20 section test transfer learning. From Figure 2, it is not easy to see which counties are used for calibration and for validation.