Solid Earth Discussions

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Comment on se-2021-79

Cliff Thurber (Referee)

Referee comment on "Changepoint detection in seismic double-difference data: application of a trans-dimensional algorithm to data-space exploration" by Nicola Piana Agostinetti and Giulia Sgattoni, Solid Earth Discuss., https://doi.org/10.5194/se-2021-79-RC1, 2021

The manuscript by Piana Agostinetti and Sgattoni presents a novel scheme for identifying times in an earthquake arrival time data set in which a change in the pattern of differential times occurred (presumably due to a change in the velocity structure). This can be useful for the purpose of 4-D tomography (3-D plus temporal change) when data from different, relatively stable time periods should be separated. My first suggestion is that the rather vague title be modified to better reflect this key theme of the manuscript.

The authors need to be more careful about the use of the term's error (errors) versus uncertainty (uncertainties). I believe that for the majority of their uses of error (errors), uncertainty (uncertainties) is the correct word to use.

I have difficulty understanding their constructions of the covariance matrices C_e, C_e*, and $C_{e,w}$. They refer to Piana Agostinetti and Malinvervo (2018) for the explanation of $C_{e,w}$, but in my opinion, this needs to be explained here. Where C_e comes from is not at all clear, and then C_e* is the final version of C_e, which comes from Figure 4b, but what is Figure 4b based on? Further confusion is added because Figure 4b is not the least bit diagonal in appearance, but C_e* is assumed to be diagonal. The entire development section needs to be made crystal clear and thereby be reproducible.

I also found the presentation of the weights on page 11 to be overly confusing. One would expect that for a DD datum for which the events cross a change point, the weight would be small. However, the w_{ij} factor that enters the exponent in Equation 9 is a sum of "the weights associated to (sic) each changepoint", giving the impression that the corresponding weight $W_{ij}(m)$ can be large. It confuses things even more by having the variable m be used for two different things in Equation 10.

The discussion of the MCMC sampling could be improved. The four "moves" have specified

probabilities (0.4, 0.4, 0.1, and 0.1, respectively). How is the move to be made actually chosen? Is it the case that some random number between 0 and 1 is drawn, and if it is between 0 and 0.4, move 1 is selected, etc.? Is model fitness used as a factor, as it usually is in MCMC? If so, how? Clarity would be appreciated. I also recommend that the synthetic tests referred to on page 12 be presented to provide confidence in the claimed parsimonious character of the method.

A recent paper by Roecker and coworkers (Double Differencing by Demeaning: Applications to Hypocenter Location and Wavespeed Tomography, BSSA, 2021) shows that a demeaning approach performs as well as, and is mathematically equivalent to, the DD approach. This paper should be cited and the potential application of the new method presented here to their demeaning approach should be addressed, if possible.

In general, a methods paper is more valuable if it is actually applied to something tangible. Here, the authors stop at determining changepoints, which by themselves don't really teach us anything about Katla volcano. Isn't there something worthwhile that can be done with these data and the new method?

My final point is that the manuscript needs major improvements in the writing. I have tried my best to provide suggestions for such improvements in the annotated scan of the paper.

Please also note the supplement to this comment:
https://se.copernicus.org/preprints/se-2021-79/se-2021-79-RC1-supplement.pdf