Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC1
https://doi.org/10.5194/amt-2021-287-RC1, 2021

Comment on amt-2021-287

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Referee comment on "Hierarchical deconvolution for incoherent scatter radar data" by Snizhana Ross et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-287-RC1, 2021

General comments

I have been following the developments by many of the authors in the area of Gaussian processes and Bayesian deconvolution for a few years now, and I greatly enjoyed this paper as an application of those ideas to deconvolving incoherent scatter radar data. I found the description of this hierarchical convolution technique to be clear and well-organized, and I have high confidence that I could implement the technique based on reading the paper. I think this is an exciting area of development for processing radar data and, in particular, incoherent scatter radar data, and I look forward to future developments. I have some specific comments that follow, but they mainly touch on areas where I think additional information or clarification would improve the paper.

Specific comments

1) In the paragraph containing Equation (7), it is introduced as, "In order to reach resolutions better than the elementary pulse length". I found this slightly confusing on the first read-through because I initially failed to recognize that Equation (7) is a discretization of Equation (5) since my attention had been directed to the resolution issue. The quoted clause implied to me that the form of the following Equation (7) is specialized in order to achieve increased resolution, but in truth the equation would look similar in all cases and is necessary just for discretization. I suggest removing the quoted clause and placing discussion of how to achieve resolutions better than the elementary pulse length to after the description of Equation (7).
2) Using a mean of 0 for the Gaussian Process prior for P is described as a "convenience", and I appreciate from my own experience with GPs that it is indeed such. Are there other justifications you can provide for why that is an appropriate assumption in this case?
3) Similarly, can you provide additional justification for why a Matérn covariance with v=1.5 is chosen? Including a quick statement in the text will help readers who are less familiar with Gaussian Processes so they don't have to turn to one of the references to find the answer.

4) It is noted that L_l is a tridiagonal matrix with reference to Roininen et al. 2014. I suggest adding a quick statement saying why this is the case (e.g. finite differences approximating the derivative) and why it is useful (e.g. efficient computation especially as the problem size scales up). Providing an explicit expression of L_l as a function of l_i here would also be good for clarity, although I do note that it appears in-text later in lines 204 and 205.
5) The Figure 3 labels and text discussing the figure refer to u as the "length-scale function". I think it would be clearer to note that this is the log of the underlying length scale, so that statements like "by factor 3-5 large in smooth parts of the profile" can more easily be associated with the log scale under discussion. Better yet would be to reference the physical units associated with the underlying length scale values.
6) The alpha tuning parameters were optimized to minimize the mean squared error between P and P_hat, and the resulting estimates all underestimate the peak power of the sporadic E layer. Presumably this is because the length scale would need to reach a smaller value at those altitudes in order to permit the large gradient that exists there. Did you test higher values for the alpha parameters, and does that end up fitting the sporadic E peak better? What does that do for the quality of the estimates at other altitudes for the background ionosphere? In other words, if one was more interested in the highest quality estimates of either a narrow feature or the background ionosphere at the expense of the other, how does that affect the decision for setting the alpha parameters?
7) Following on from the previous comment: did you test any other prior distributions (i.e. not Cauchy or TV/Laplace) for the length scale difference that might be better suited to really sharp gradients? If not, can you point to directions for future work in this area?
8) How specifically did you choose the tuning parameter values for the PMWE and PMSE results? (i.e. What "performance" [line 296] is being optimized?)

I look forward to future work that would include non-zero lags of the autocorrelation function for analysis of the full ionospheric incoherent scatter spectrum, and I also look forward to future work that would apply the technique to the data before it has been matched filtered.

Technical corrections

(line 313) "from in TV prior" -> "from the TV prior"?
(line 334) "Cauchy difference TV" -> "Cauchy and difference TV"?