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Atmospheric Measurement Techniques Discussions

Interactive comment on "Ionospheric assimilation of radio occultation and ground-based GPS data using non-stationary background model error covariance" *by* C. Y. Lin et al.

Anonymous Referee #2

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PAPER SUMMARY: This paper is a study of data assimilation approaches for developing nowcasts of ionospheric electron density. The study uses limited time periods (a single day) but several methods for assessing the role of location-dependent versus location-independent covariance. The focus is over the United States sector. Location-dependent covariance is demonstrably better, both in a simulation and in comparisons with independent data from radar. Despite its limitations, this is useful study worth publishing after substantial revision.

REVIEW SUMMARY: The paper adopts a methodical approach to answering the questions, despite a somewhat limited domain being analyzed (one day over the US). The

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main revision required of this paper is the clarification of the approach (is it really a Kalman filter?) and a fuller discussion of location dependent versus independent covariance. If a location independent covariance were used that had a broader correlation distance, would the results look different? Besides these points, the authors develop a useful algorithm and describe it well.

SPECIFIC COMMENTS: p. 2635, Line 5 (2635/5): This set of equations is described as a Kalman Filter, but there is no covariance update. Later, it is stated that the KF forecast step is outside the scope of this study. It should be stated that in this case, strictly speaking, a KF is not being analyzed. This algorithm is closer to optimal interpolation. At the very least, this aspect of the analysis should be more clearly explained. It's not clear to me that the conclusions will not change if the corresponding KF is implemented. The authors should at least consider this point in the discussion.

p. 2638/4: Error in the pseudorange is not simply due to satellites. Ground receiver error sources are dominant such as thermal noise and multipath. Please re-state.

p. 2638/10: This error formula applies to unbiased and uncorrelated errors, ignoring potentially significant errors due to multipath affecting pseudorange. It leads to overly optimistic observation errors. Authors should point this out.

p. 2638/18: Please mention the data rate here.

p. 2638/25: It is not correct to state the multipath error is eliminated. In particular, the formula on line 10 assumes uncorrelated pseudorange error, which is clearly not the case in the presence of multipath. Multipath remains a significant component of the TEC leveling error. How is representativeness error addressed? This paragraph requires revision.

p. 2639/13: State which two months are used, because seasonal effects might not be ignored.

p. 2640/8: is Eqn 2 the correct equation to reference here?

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p. 2640/10: By “sample covariance” what is meant here? Is this viewing the 62 profiles across latitude and longitude as a sample of a single distribution? Please clarify what is mean by “sample”.

p. 2643/10: It should be noted also that in such simulations, difference between simulation truth and background is important. That seems to have been achieved here and should be noted explicitly.

p. 2644/11: There is a great deal of data in Figure 3 from COSMIC. Over what time period are these data accumulated? Please clarify.

p. 2645/12: There is no visible dashed line. It is suggested to use a different color for the GPS only case.

p. 2645/22: How many occultations pass through the region in this time period?

p. 2645/24: It’s not clear the figure shows this conclusion. Most of the time, the assimilated result is similar to background. The RO/no-RO cases look pretty similar. This conclusion should be backed up by statistics, such as mean difference or standard deviation between assimilation result and ISR data.

p. 2646/7: There is an error in Figure 7 (%). Why not also show comparison between Abel and ISR? This should be shown also.

p. 2647/10: It’s not clear that this is a robust conclusion. The error covariance magnitudes and forms are rather different for dependent and independence cases (D and I). Could these results be explained by the different forms of these covariances, rather their location dependence? What if the location independent covariance had a broader Gaussian? Could that significantly affect the results? This must be explored, because a broader Gaussian that is independent is more similar to the dependent case. It’s not clear to this reviewer that the location aspect is key, rather than the rather peaked nature of the independent covariance, that could have a broader horizontal correlation distance if so chosen.

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p. 2647/29: See earlier comments on Figure 3 and how accuracy differs for the different assimilation cases.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 2631, 2014.