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Atmospheric Measurement Techniques Discussions

Interactive comment on "Ionosonde measurements in Bayesian statistical ionospheric tomography with incoherent scatter radar validation" *by* J. Norberg et al.

Anonymous Referee #1

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General comments

The submitted paper represents a significant contribution to the field of ionospheric tomography. A key point in ionospheric tomography is how vertical information is obtained. This work uses ionosonde measurements for this purpose, and interprets these data in the form of a prior distribution to be used in a Bayesian inversion. Of course, the problem is still underdetermined, and other regularizing information must be used. Typically, some sort of smoothness constraint is used. In the present work, the authors use a clever techinque to quantify this smoothness in terms of correlation lengths, incorporated in the prior distribution (described in their previous paper, Norberg et al.

[2015]). The technique in the submitted paper is an advance in the state of the art in combining ionosondes with radio beacons for tomography. One advantage is the use of precision matrices, which are sparse, instead of explicit covariance matrices, which allows for fast computation and real-time operation. The description of the methodology is clear, accessible, and succinct. Through experiment, the paper demonstrates the improvement gained by using this method over existing methods for choosing the prior (namely, IRI-based and zero-mean methods).

The main drawback of this work (detailed below) is the seemingly arbitrary nature of choosing free parameters in the inversion. However, as the authors state, such ad-hoc techniques are endemic to any regularization techinque, and in the present work, this ad-hoc nature can be quantified in physical units, unlike other techniques. Nevertheless, the reader is left with the question of how sensitive the reconstruction is to the choice of these free parameters. This question could perhaps be answered by providing a detailed description of the truth model simulation that is hinted at in the text.

Specific comments

The following comments are given in order of decreasing importance:

1. p9823: The authors describe the choice of what appear to be seven free parameters in their method. For the prior mean, the measured ionosonde data is used for the bottomside, and for the topside an exponential distribution with a scale height of 140 km is used. For the prior standard deviation, the authors choose a Chapman profile, with a peak electron density of 40% of the prior mean, a scale height of 200 km above the peak and 60 km below it, and a peak altitude "approximately the same" as in the prior mean. To describe the correlation lengths in the prior, the authors use 200 km in the vertical direction and 2 degrees in the horizontal. The authors state that they exercised various combinations of the free parameters to find the values to be used in subsequent reconstructions. These values are set constant for the first three inversions, but some values are changed for the fourth inversion. It would provide a great benefit to the

perceived robustness of the proposed method to describe the process of choosing these parameters, perhaps with a truth model simulation for better justification. This complexity is the weakest point of this work, which is unfortunate given the beauty and simplicity of the description in Section 2 and the construction of the prior precision matrix as described by Norberg et al (2015).

2. The authors should cite recent work by Chiang and Psiaki (2014) and discuss the relationship with their work.

K.Q.Z. Chiang and M.L. Psiaki, "GPS and Ionosonde Data Fusion for Local Ionospheric Parameterization," Proc. ION GNSS+ 2014, Sept. 9-12, 2014, Tampa, FL, pp. 1163-1172.

3. In Equation (1), is gamma (the unknown receiver bias) a multiple of 2pi, or can it be any real number? Either way, it needs to be made clear what prior is used for these parameters. If it's restricted to a multiple of 2pi, the authors need to describe how this is handled.

4. Is a periodic boundary condition used in this work, as described in Norberg et al (2015)?

5. The authors took care to mention the caveat regarding the use of the UHF data to calibrate the dynasonde measurement. However, on page 9831 line 18, it's not made clear whether the profiles used for validation in this paper were also used for calibration. Please make this clearer, as it substantially affects the interpretation of the results.

6. Please briefly describe how the measurement covariance matrix is constructed, or provide a suitable reference.

7. p9830 line 17: Slightly more detail is needed regarding how negative values are handled. The method that is described seems to me like a justifiable and efficient way of enforcing a non-negativity constraint, but it is not entirely clear. Furthermore, are there any numerical issues with setting such a small variance?

8. For tables 1-4, how is the "true" peak altitude determined? From the ISR (in which case smoothing is needed) or from the ionosonde (in which case it may not be a fair comparison)?

9. p9831 line 3: It may or may not be useful to indicate to the reader how much the measured ionosonde profile varied over 10 min.

10. p9825 line 24. This whole paragraph may benefit from a reference to where the reader may find more information about ionosondes.

11. Higher resolution figures (especially figure 4) are necessary to be suitable for publication.

Technical corrections

The following corrections are minor and should be treated as recommendations only:

A. Eq 1: Replace "dz" with "dl". Since z is a two-element vector, dz doesn't seem right.

B. p9288 line 8: Using the index "t" may be mistaken for time instead of the index of the measurement.

C. p9831 line 1: add the word "is"

D. p9835 lines 1 and 15 are not complete sentences.

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