

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

Anonymous Referee #2

Received and published: 30 January 2016

The authors present a new approach for 2D ionospheric reconstructions. The methods employed are based on statistical inversion with prior distribution given by its mean and covariance. Such values are obtained from ionosonde measurements. The obtained results are validated against close-by EISCAT incoherent scatter radar measurements. In addition, the proposed approach is compared with results obtained with a zero mean prior and with the prior mean taken from IRI 2007. As indicated by the authors, given the continuous nature of the ionosonde measurements, the presented method enhances the near- real-time ionospheric tomography estimates. The proposed approach is indeed worth to be published. However, the authors need to improve the presentation and motivation. Below I summarize some points that the authors might want to

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consider.

In general, I'm not sure why the authors invest space and time presenting the comparisons with the zero mean prior. I would understand the use of the IRI 2007 prior, but it is not clear why the zero mean prior is considered on all the cases studied. Maybe this is one of the approaches used by other groups?

I think the use of prior distributions is good, but I wonder if using actual standard deviations from ionosonde inverted profiles (including the topside), would be less subjective, than the fixed values used.

Another point that the authors might want to consider or discuss, is the use of the ionosonde measurements in the model matrix (i.e., in X). Have you thought about doing it? if yes, why is not applicable in your approach?

Here is a list of more specific comments/questions:

1. Title. I think the title is misleading, or at least from a first impression. I'm not sure if how is validated should be there, but I think it would be more informative if the title is something like: "Bayesian statistical ionospheric tomography improved by incorporating ionosonde measurements" or something like that.

2. Abstract.

a. L7. What do you mean by "statistical clear inversion"? elegant? simple?

b. L11-12. Do you really need to specify the other approaches (i.e., zero mean, mean from IRI 2007)?

c. L18. Can you summarize in what sense the proposed approach outperforms other methods?

3. P4, L23-25. Is the use of zero mean prior or a prior mean from IRI-2007 known or accepted methodologies? If not, what is the rationale for including them in this study?

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4. P5, L15-20. Some of this text, should be moved to a discussion section.

5. P8, L6-10. Prior covariance. From the abstract, I thought the prior covariance was obtained from ionosonde parameters. Could you elaborate more on how the prior covariance is estimated?

6. P9, L6-9, why is the topside parameterized, instead of using the topside from the ionosonde inferred topside, which is also parameterized? In addition, how is "s" defined? From the bottomside scale height?

7. P10, L1-15. What is the motivation or justification for the values used, e.g., 200 km, 60 km, 140 km, 40%? In addition, how are the prior standard deviation and the prior covariance related?

8. Results

a. In general, do you really need the results from the zero-mean and IRI 2007 priors? If not, I think the results would be easy to present, at least the figures. If the importance is not too much, maybe just mentioned the results in the summary table.

b. I think it would be simpler to present all the summary results in one table instead of four small tables.

9. Discussion and conclusions. I suggest adding a short discussion sections, discussing the pros and cons of the methodology, as well as the discussion on some of the subjective values used. This section could be followed by a simplified, but direct to the point conclusions of the proposed work.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 9823, 2015.