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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.

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1 Reviewer #2 (Comments to Author):

General comments

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 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?

We hope to answer these questions in more detail after the following specific comments. If we understand the question regarding the use of ionosonde measurements in the model matrix correctly, we think we have considered this at the end of Conclusions section:

"As the ionosonde measurements provide relatively accurate measurements of the ionospheric electron density, it would be straightforward to use them also as direct mea- surements above the instrument location. However, the satellite overflight hits rarely at the zenith of the ionosonde site and the electron densities measured by ionosonde and tomographic receiver can vary largely. When 2-D assumption (i.e. small gradients in longitude) is used, the ionosonde measurement error should reflect this discrep- ancy. Hence the information for the projected ionosonde measurement points cannot be modeled as accurately as they are in their actual location and the prior distribution provides substantially the same information. In the 3-D situation the situation will be different as all of the measurements will be modeled in their actual locations."

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.

"Bayesian statistical ionospheric tomography improved by incorporating ionosonde measurements" sounds good for us.

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?***

a): We could change the sentence including the part "statistical clear inversion" to "This results in a computationally efficient tomographic inversion algorithm with clear probabilistic interpretation."

b): Please see the answer to the question 3, where we consider this in detail.

c): It could read: "We find, that in comparison to the alternative prior information sources, ionosonde measurements improve the reconstruction by adding accurate information about the absolute value and the altitude distribution of electron density."

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?

We found this as one of the major points in the comments and we gave it a very careful thought. It is true that the paper could be streamlined by omitting the alternative prior models. However, as this is a new kind of approach for ionospheric tomography, we feel that this kind of comparison is in place. The following paragraph could be added to Introduction to explain the inclusion:

"The IRI model is chosen as it is a well known ionospheric model, and unlike the ionosonde, it provides information also on horizontal electron density gradients. The zero-mean prior is included to demonstrate the performance with simpler and more general prior information. The zero-mean prior carries essentially similar information than the prior model used in Markkanen et al. (1995)."

Maybe even more than being actual worthy alternatives, the inclusion of these models gives information on the overall performance of the algorithm. In the other comments, as well as in the comments by Referee#1, there are justly placed questions about the prior parameters, how they are chosen and about the robustness of the algorithm in respect to changes in these parameters. As a matter of fact, it happens to be that the presented algorithm is rather robust in this sense. Change of the correlation length parameters has very intuitive results in the reconstructions and the changes in some tens of km produce differences that are mostly insignificant in the scale of the model accuracy. The standard deviation affects also very predictably, and by reducing it, the reconstruction follows more strictly the prior mean. When realistically strong altitude gradients are desired, the most probable bias source is the prior altitude profile, especially the peak altitude of the mean profile. We feel that these comparisons give a realistic idea on these caveats.

4. P5, L15-20.

Some of this text, should be moved to a discussion section.

That is true.

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?

In the mentioned sentence it reads: "The prior covariance is given as a squared exponential, i.e. as a Gaussian-shaped function that is defined with its amplitude (variance

scaling factor) and correlation length". In the section 3, where the parameter values are given we continue "The prior standard deviation (SD) is given as a Chapman function for the vertical profile, with approximately the same peak altitude than the prior mean, and the maximum electron density approximately 40% of the corresponding NeXtYZ maximum." So the ionosonde is used to estimate the variance/standard deviation of the electron density. Besides the variance, a Gaussian covariance structure is assumed, and this is controlled with the "correlation length" as explained. Maybe if the part "(variance scaling factor)" would be changed to "(variance/standard deviation)" would make it more clear that when we mention standard deviation later, we are talking about the very same parameter.

The prior covariance approximation is built so that we cannot set the prior standard deviation explicitly to follow the ionosonde profile, but we can control its shape and amplitude so well, that practically the difference is insignificant.

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?

Based on our experience, the model used in NeXtYZ might slightly overestimate the topside ionospheric density. We take this into account with parameter "s", which gives the prior a more sharp peak. The actual value is chosen during the "calibration" trials with Overflight I and then fixed. The same parameter value is then used systematically for all different cases. If the reconstructions are carried out using the topside provided by ionosonde, the results are similar, but the thicker tip of the peak remains in the reconstructions. As the thicker peak allocates more density to peak altitude, this can also result as overestimation for the peak altitude.

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?

As suggested in comment 9, the manuscript would benefit from dividing the present Conclusions section to separate Discussion and Conclusions sections. With respect to this comment we would like Discussion section to contain the following:

"The presented method for ionospheric tomography includes several prior parameters and the selection of the corresponding values might seem arbitrary. The objective of this article is not to optimize all of the prior parameters, but to concentrate on the altitude profiles of the prior mean and the standard deviation. Based on trials with the algorithm and different data, the information on the vertical structures has the most crucial effect on the reconstruction quality. This is also evident in the presented results. When zero mean prior is used, the peak altitude can be found relatively well, but the measurements do not contain enough information to produce steep enough vertical gradients. Then again, when a vertical profile is given within the prior, the reconstruction of peak electron density is improved significantly, but the peak altitude becomes less sensitive to measurements. In horizontal direction, the gradients can be reconstructed rather well regardless of the prior mean in use. Hence, information on horizontal electron density structures (IRI model) is less important if the trade off is the accuracy on the vertical structure.

When accurate vertical electron density profile is provided within the prior, the selection for the values of the other prior parameters is less critical. For all prior parameters the stabilizing effect is also rather intuitive. Decreasing the correlation lengths allow more small scale variation in the reconstructions, however, getting close to the the cor-

responding discretization can result in artefacts. The increment of correlation lengths smoothens the reconstruction, but very long correlation lengths can again produce unexpected behvaiour. With all cases in the previous section, the use of horizontal correlation length values between 1° and 10° and vertical correlation lengths between 20 and 500 km were carried out without drastically unrealistic changes in reconstructions. The peak value of standard deviation was also altered in a range from 20% to 100% with anticipated results.

As mentioned in section 3, the standard deviation profile is parametrized as a Chapman function. Hence, the ionosonde profile cannot be used explicitly, but the choice of the parameter values can be done viably based on the ionosonde measurements. For the three first overflights only the peak standard deviation altitude and density were set according the corresponding ionosonde measurements. With Overflight IV, the ionosonde profiles are significantly different, thus also the shape of the prior standard deviation was changed. Altogether, the results for the overflights II, III and IV could be enhanced by optimizing the parameters through trial and error individually for each case, but the results show that already intuitive realistic choices of these parameters are enough to give reasonable solutions."

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.***

a): Please see the answer for Comment 3.

Table 1. Errors of tomographic profiles compared with EISCAT UHF scans

		Relative mean error of peak density (%)	Mean error of peak altitude (km)
Overflight I	Ionosonde	5	41
	IRI	27	55
	Zero	52	74
Overflight II	Ionosonde	5	17
	IRI	6	58
	Zero	54	15
Overflight III	Ionosonde	4	33
	IRI	6	31
	Zero	60	33
Overflight IV	Ionosonde	5	40
	IRI	12	84
	Zero	61	50

b): Please see Table 1.

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.

We agree. Please see the answer for Comment 7.

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