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Interactive comment on “Sodankylä ionospheric tomography dataset 2003–2014” by J. Norberg et al.

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Authors thank the associate editor and anonymous referees for giving valuable comments on the manuscript. The referee comments are listed here with bold text type. After each comment authors' response is given with normal text type.

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1 Comments

The paper gives a good description of tomographic methods and in particular those used on a dataset between 2003-2014. The specific objective of the paper is to investigate the solar cycle variations in the vertical total electron content (VTEC) from this method, which is done well. The other result is the comparison as reference with VTEC derived from the IRI-2012 model, sunspot number and solar flux. The tomographic estimates correspond well with the model results in the way that they vary with the solar cycle, which is an interesting and worthwhile result. However, there is a discrepancy between the two in magnitude, with the estimates based on the model being the higher by 40% on average. The implication is that there is a systematic error, but little discussion is provided on what is the most likely cause, and whether it is in the model or the tomography method, or data. Further studies are suggested, which is indeed a sensible route to take, but more detail is needed on what these studies might be.

We agree that the differences between the IRI and tomographic results needs to be discussed more. As written also in the answers for anonymous referees #1 and #2, we would like to add in the manuscript:

The overestimation of high latitude Ne has been widely reported for different versions of IRI model. Zhang et al. (2006) reported that IRI-2001 overestimates Ne at the peak altitude and above, especially in winter time compared to ISR measurements.

One of the main improvements for IRI-2007 was the topside Ne modeling (Bilitza & Reinisch 2008). Lühr and Xiong (2010) compared the IRI-2007 model results to orbital averages of CHAMP and GRACE satellite measurements from 2000-2009, with the satellite height range from 300 to 500 km. Especially during the solar minimum

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period the overestimation was up to 60%. The overestimation was concentrated on the lower latitudes, but in Xiong et al. (2011) a 20% overestimation also for trough area was reported. Xiong et al. (2011) utilized CHAMP and GRACE satellite based Ne measurements from 2005 to 2010.

These studies then suggest that despite the development, the modelling of F peak and topside Ne still carries some problems. The improvements for IRI-2012 were made for the thickness and the shape of the bottom-side F2 layer, as well as for the description of storm effects in the auroral E region (Bilitza et al. 2014).

We have found it difficult to find out a comprehensive account on the different measurements used in the IRI model. In Altadill et al. (2008) a network of 27 ionosondes were used for the enhanced bottom-side modelling of Ne. The closest ionosonde measurements to Fennoscandia in the network were from Chilton, UK. In all, the network comprises two high latitude ionosondes, both located in Greenland.

The Sodankylä tomographic set-up employs numerous measurements from the high-latitude area which is poorly populated by ionosonde stations. However, ionospheric tomographic inversion is well-known to be an unstable inverse problem, and its performance, especially in small scale details in vertical structures, can be argued. The Bayesian approach (Markkanen et al. 1995.) utilized here assumes zero electron density a priori, and variations from zero background are then controlled with a Chapman profile shaped standard deviation. The approach is hence more likely to underestimate than overestimate the electron densities. Then again, as mentioned in the Introduction of our manuscript, improved inversion methods for this measurement concept are under development. The upgrading work includes also better methods to estimate the quality of inversion results. Therefore, we believe that beacon-based tomography could be used more intensively in future research, perhaps even in IRI-validation and upgrading.

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The figure numbers are strangely organised, or at least the references to them. Figs 3 and 4 are referred to early in Section 2. I can find no specific discussion of Fig 4. Then follows a detailed discussion of Fig 1 then Fig 2. If Fig 4 is important for this paper it should have more than a link to a webpage.

We are sorry for the inconvenience with the ordering of the figures and will of course change that. Figure 2 should be moved to 4 figure. The motive for Figure 4 was that it would help to understand the nature of individual tomographic results, but it can be left out.

When the detailed discussion of Figs 5–8 is given, it would be helpful to refer to the exact figure that is relevant. For example, when mentioning the summer results, please refer to Fig. 6 in the text. The second paragraph of Section 3 could be ordered better to aid the reader. Maybe it would help if the figures were combined into one figure with 4 rows. It is also worth considering if the results would be easier to interpret in a two colour format, since the gradations between blues and greens are very subtle. But mainly the text needs to be more specific and clearer. Another example is the sentence at the top of page 391 - does this refer to all seasons? In the sentence "At the magnetic local night time the differences are in general somewhat smaller and in both directions" the colours are certainly not very easy to interpret as values. An estimate of the differences would be useful, and better worded as "the differences are both positive and negative". Regarding "especially at the higher latitudes", the statement at line 8 that the difference at equinox and winter is likely associated with auroral activity should be expanded if important. There are differences in the summer plot across latitudes as well. In describing Figs 9 and 10, it states that differences

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between stations are "almost indistinguishable" in this type of plot. It would be better to state that the large-scale features are the same for all stations. Figure 9 again suffers from the problem of colour differentiation.

We tried plotting with different colour scales, but we honestly think the present version is clearer.

We could change the part in the text as follows: "To characterize the data, it is first presented in Figs. 5–8 as averaged VTEC values in magnetic latitude and magnetic local time (MLT) coordinate system. This is done separately for the complete and seasonal datasets from summer, equinox and winter. Winter is defined as one third of a year centred around the winter solstice. Summer starts one third of a year after winter solstice and lasts for one third of a year. Everything else is defined as Equinox.

In Figs. 5–8 first the data for tomographic then for IRI-2012 VTEC values are shown. Third image illustrates the differences between these two. In all Figs. 5–8 the relative diurnal behaviour in VTEC values within different seasons are relatively comparable between tomographic and IRI-2012 data. Both approaches show in dayside VTEC values a dawn-dusk asymmetry with higher values in the dusk side. This asymmetry is pronounced particularly during summer time in Fig. 6, where according to IRI-2012 enhanced VTEC values extend to pre-midnight hours. In the tomography results a similar trend is visible but the extension of high VTEC to night time hours beyond 18 MLT is missing. In all seasons the electron densities are systematically higher in the IRI-2012 data, with the maximum difference close to 5 TECU. The difference plots show that the differences in summer, in Fig. 6, are slightly smaller than in equinox and winter seasons in Figs. 7 and 8. In all Figs. 5–8, at the magnetic local night time the differences are in general somewhat smaller and both positive and negative. Figs. 7



and 8 indicate that in equinox and winter at magnetic local night time, the tomographic VTEC values at higher latitudes are larger than the corresponding values from the IRI-2012 model."

Also, if 2003 is indeed a special year of extra magnetic activity, then it is important to consider in more detail what might be happening to make the results different. To this end it would be better to display the data so that direct comparisons can be seen by the reader, perhaps as line plots over MLT. How different are the midnight hours in 2014?

We agree with the Referee that more detailed discussion would be useful here. We suggest the following modification:

Geomagnetic activity is strong particularly during 2-3 years after the solar maxima (Nevalinna and Pulkkinen, 1998). Both 2003 and 2014 are such years. Geomagnetic activity is caused by processes in the night side magnetosphere which generate also enhanced electron and proton precipitation into the ionosphere. The impact of this precipitation is mostly visible in the E-layer densities. Our results suggest that IRI-2012, as a statistical model, cannot describe these special situations accurately, while the tomography inversion manages to catch at least partly the altitude integrated impact from this precipitation.

In the conclusions, the question arises at page 392 line 13 whether "error" is the correct word, unless more can be provided as a discussion of what the source of the error might be.

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In conclusions the "error" should indeed be changed to "difference".

There is certainly an almost systematic difference, but as has been pointed out there are exceptions, and these events may well be clues to what is happening. One suggested rationale for the method is to assess the changes to the thermosphere from possible cooling effects, such as increases in methane and carbon dioxide. The tomography measurements are suggested as having a "crucial role in refining" a previous result (Lean et al., 2011) in the high latitude region, known to be a region where GPS measurements are less accurate. Such a claim should be assessed in the light of the results presented.

Having a "crucial role in refining" may be a bit too bold statement here. We notice the fact that further studies are still needed to get better understanding on the reliability of tomography measurements, but on the other hand we want to point out that most likely also GPS-based TEC-timeseries would show some differences when compared with the IRI-models.

What we want to say in our paper is that the TEC values recorded at low and mid-latitudes have a dominant role in the results of Lean et al., simply because most of global TEC comes from those regions. Like earlier studies with ISR and ionosonde data have shown, at high latitudes the picture is more complicated as regional differences in the trends are likely to appear. Tomography measurements, when properly validated against ISR data, could serve as a convenient way to monitor such regional trends. They are a cost efficient way to expand the field of view of ISRs and ionosondes

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at high latitudes and they do not suffer from low-elevation ray paths similarly as GPS measurements do.

2 Technical corrections by page and line number

386 12 perhaps "descending" and "ascending" would help on first usage, at least

Thank you for pointing this out. We could add for example "descending, i.e. southward"

18 model results are on average 40% higher than those of. . .

387 12 consists of

Thank you for pointing these out.

388 3 in the ionosphere

5 statistics (or a statistic)

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9 too far south

11 "also" is not needed as implied in "include"

15 over the global ocean, but what does this mean?

16 comma not needed after "paper"

20 the data

Thank you for pointing these out. Instead of "global ocean" we could use "areas above ocean".

389 15 compared to a strictly polar orbit

16 could put "descending" here, and "ascending" at line 19

We don't quite get the comment for the line 16.

390 23 In the tomography results

Thank you for pointing this out.

C315

391 6 "and in both directions" but this expression needs clarifying.

We could say: "differences are in general somewhat smaller and both positive and negative"

392 21 consists of

Thank you for pointing this out.

Interactive comment on Geosci. Instrum. Method. Data Syst. Discuss., 5, 385, 2015.

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