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Atmospheric Measurement Techniques Discussions

Interactive comment on "Modeling the Zeeman effect in high altitude SSMIS channels for numerical weather prediction profiles: comparing a fast model and a line-by-line model" *by* R. Larsson et al.

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Major comments

This paper presents an interesting comparative study between a fast radiative transfer model (RTTOV) and a reference (line by line) model (ARTS) for the high altitude channels 19-22 of SSMIS. The simulations have been performed using globally distributed numerical weather prediction model profiles from MetOffice. One of the main problems present in the study is that these atmospheric profiles only reach altitudes up to 10 Pa, and a constant temperature value is extended to lower pressures. Since the higher altitude channels 19 and 20 are not covered by the altitude levels of the numerical

weather prediction profiles these two channels are not compared with the sensor measurements. The other two lower altitude channels (21 and 22) are compared between the models and also between models and measurements.

The authors state that the agreements between the forward simulations and the corresponding SSMIS measurements is generally good but there are some discrepancies although there are some discrepancies. They recommend that future iterations of numerical weather prediction software starts using versions of RTTOV from version 10 and onwards for the assimilation of SSMIS channels 21 and 22. Moreover, they suggest that model discrepancies for channel 21 would be likely reduced if the model top levels reached higher altitudes. The necessity of higher numerical weather profiles (with top at 100 km) for modeling the channels 19 and 20 have been also proven.

I agree that the study present a reasonable agreement between the two models and also with the sensor measurements for the lower altitude channels. The study also evidences the necessity of work with the RTTOV version which include the Zeeman scheme in order to reduce the uncertainties of the numerical weather prediction profiles at high altitudes. However, I consider that some of the discussion and interpretation of the discrepancies could go a step further. Following I indicate some of the points that should be addressed by the authors:

The authors have found small mean deviations between the two models (RTTOV and ARTS) for the channels 19 and 20 but with an increase in the standard deviations when the three-dimensional magnetic filed is considered in ARTS. The authors state that part of the deviations between the two models could be explained by existing difference in the center of the emission lines for both models. In this sense, it could be clarifying that the authors also show the global distributions of the Tb differences for the ARTS model when the Zeeman effect is and is not considered in the simulation. It could help to identify if some of the patterns observed in these global distributions are due to differences between models or not.

The simulations for the channels 19 and 20 evidence very different results when the models are compared with 2D or the 3D magnetic field for the ARTS simulations. The differences are much smaller when RTTOV is compared with ARTS-2D. It would be interesting to check which is the effect of the Zeeman effect only when 2D magnetic field is considered in ARTS and not the full description how it was calculated (right-most column of Table 1). In this way we could evaluate if the Zeeman effect for this configuration (2D) was also significant.

I consider that the agreement between the models including the Zeeman effect and the measurements has not been sufficiently proven or at least is what I conclude for the current state of the manuscript. Although the agreement between measurements and models are good for channels 22 even better than the results obtained by Han et al. (2007) it seems that Zeeman effect has not a strong influence over the channel 22. It is evidenced for the ARTS simulations when the effect of the Zeeman effect is assessed for this model (very small mean deviation and standard deviation). Moreover, the channel 21 suffers that the weighting functions are shifted upwards under the presence of strong magnetism field, so it is not possible a correct assessment of this channel with realistic atmospheric profiles only up to 10 Pa. The authors should remark the limitations found in the comparison between models and measurements or argue better their conclusions.

Minor comments

- I think it would be clarifying for the reader if the central frequencies of the 19-22 SSMIS channels are specified the first time that the channels are mentioned. - It would be very helpful for the readers if the authors include a figure with the channels weighting functions for the SSMIS channels presented in this study. - Please rewrite the next sentence, it is very confusing in the way that currently is written (Pag. 10193, lines 7-10): "The mean difference between the models should be compared to the size of the Zeeman effect for channel 21 at Tb = -3.1 K, which is the largest average Zeeman effect for all of the channels. From this comparison, the mean difference between

the models is also small." - Some of the discussions about the changes of the global distributions is really hard to follow with the color map. As for example the discussion between models of the channel 21 (page 10192, lines 15-22). I would propose to the discussion in a more quantitative way plotting the deviations for some predetermined latitude and longitude ranges. - Please try to avoid using unnecessary abbreviations, especially at the beginning of sentences. It makes reading less fluid. For examples, page 10192, line 11 (E.g., above . . ..), line 17 (E.g., in the three-dimensional . . ..).

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 10179, 2015.