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Atmospheric Measurement Techniques Discussions

Interactive comment on "Zeeman effect in atmospheric O_2 measured by ground-based microwave radiometry" *by* F. Navas-Guzmán et al.

Anonymous Referee #3

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This is a generally nice paper showing interesting results from some very careful measurements, I do have two major points which should be addressed and several minor suggestions and corrections. First, while the shapes of the measured and modeled spectra appear to be similar, there seems to be a significant difference between the Brightness Temperature shown in Figure 7 (highest peak ∼64K), which shows the radiation passed through the TEMPERA components as calculated by the model, and that shown in Figure 10 (highest peak ∼54K), which shows the measurements. Perhaps there is just an offset here which I don't understand, but some explanation is required, even if the answer is just that there is a calibration problem which doesn't affect the results (combined with an explanation as to why it does not). It would be very helpful to the reader to see measured and modeled spectra for a few of azimuth angles plotted

on the same figure, with an offset or some other correction added to one or the other if this is appropriate. The main focus of this manuscript is the comparison presented in Figure 11. The results look reasonable, but given that this is essentially the crux of the paper, a little more relevant information is required. First, please write out explicitly the equation for Teff(max)/Tmean(max), including the precise range over which it is calculated (some of the relevant text related to this on page 15 seems to be wrong). Also, it would be nice to see some error bars on the TEMPERA measurements in this figure. I would assume that these uncertainties would come primarily from uncertainties in the tropospheric optical depth, which could perhaps be estimated. Below are some more minor points that should be addressed: The inserts on Figure 5 are really quite small and impossible to read unless expanded to ∼400% on my monitor. Perhaps it would be better to put these on figures. The one on Figure 7 is perhaps large enough. Page 3 – It would be appropriate to point out that Hoppel et al. (Monthly Weather Review, 2013) used the work of Han et al. was used to assimilate SSMIS Upper Atmosphere Sounding channels, together with MLS data, into a NWP model.

Page 6 line 5. I'm curious as to why the calibration discusses only a hot load and noise diode. Figure 1 shows a cold load. How is the noise diode calibrated without a cold load? Is there perhaps an initial calibration with a cold load?

Figure 8 – Should the x-axis be MHz and not GHz?

Page 13 –"In order to correct our measurements for tropospheric effects". Strictly speaking, there is no need to "correct for tropospheric effects". Equation (7) is clearly valid everywhere along the pathlength. The reason (9) is of use here is, I assume (but the author should explicitly state this), because the tropospheric portion of the pathlength provides a relatively spectrally flat signal.

Page 13 –"Since the atmospheric opacity is dominated by the contribution from the troposphere, the cosmic background radiation, Tbg, is in practice used instead of Tb(ztrop) in Eq. (10)." I think that this statement is somewhat misleading. Tb(ztrop) de-

pends upon the optical depth of the stratosphere. It's probably true, that this happens to be small, and perhaps so small that it is dominated by Tbg. But that is not what this sentence states.

Page 15 – "However, differences in the intensity and in the shape are observed in the very narrow range centered on 53.07GHz." Actually, 53.07GHz is at the edge of the plot where there is no data shown. Presumably you the authors mean 53.067 GHz. The exact number here is quite crucial, since it is the basis for the results in Figure 11. I assume that the numbers in Figure 11 come from 53.0665 to 53.0675 GHz, but please state this explicitly.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 1, 2015.