

***Interactive comment on* “Simulating satellite observations of 100 kHz radio waves from relativistic electron beams above thunderclouds” by M. Füllekrug et al.**

Anonymous Referee #2

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Authors M. Füllekrug et al. in their manuscript "Simulating satellite observations of 100 kHz radio waves from relativistic electron beams above thunderclouds" extend an earlier published first reporting of extremely important novel observations of terrestrial 100 kHz transmitter signals in space by the DEMETER satellite. The physics of naturally occurring relativistic electron beams in the atmosphere is largely unknown and the authors correctly emphasize the significance of their quantitative results, in planning the observational and data analysis schemes of the future satellite mission TARANIS. The paper is clearly written, with thorough discussion of the background, properly referencing other author's works, and it certainly deserves publication due to the scientific value of the results themselves. I have two conceptual concerns with the present form of the

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manuscript, where the authors should add explanations, before publication.

1) I find it inadequate that the reference to the earlier published work based on obviously same data by some of the authors themselves, is left to a statement: "It was only recently reported that terrestrial 100 kHz radio waves can be actually observed in space by DEMETER". Reader is left puzzled because it is not clear how the data analysis is now improved from the earlier paper (Füllekrug et al., 2009). I would recommend publishing the manuscript once the authors clarify the advance of their data analysis compared with their earlier work, by adding proper comparison to their own results already published so that there is a clear description where quantitative data analysis is now advanced. Is the fig. 1 of reference Füllekrug et al. (2009) the same as Fig. 1 in this manuscript? If so, please add reference to the figure caption. Füllekrug et al. (2009) choose to analyse nocturnal transionospheric propagation due to need of selecting data where the instrument is not suffering of electromagnetic interference with sunlit solar panels. In the present manuscript there is no mentioning of such needs, please clarify. The earlier paper states the data to have 1 degree spatial resolution, in the present paper resolution is described to be 3x3 degrees. Does this mean that the data set is now a totally new analysis of the previous data? In the current manuscript there is no comparison shown with ground-based observations, which I think is essential in quantifying the observed transionospheric attenuation. Please add at least a relevant statement on the observations of subionospheric attenuation by some of the authors themselves.

2) The main goal of the manuscript, expressed in the title and mentioned in the text, is to clarify propagation of radio emissions from relativistic electron beams above thunderclouds, thus helping to plan the future observations by TARANIS microsatellite. The authors use the words "simulating satellite observations". I think simulation is a strong word here, the title would be better when somehow expressing that this is a limited experimental simulation only. The results in the paper are strong enough without theoretical simulations, but one should add a source comparison of radio waves from

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electron beams with those emitted by high-power man-made transmitters, and state the limits of the current experimental simulation. Clearer assessment should be done to describe the remote sensing nature of the coming TARANIS observations, and how the new results for 100 kHz help here, specially when electron beams in the atmosphere are known to emit a wide spectrum of frequencies, and the picture of observing such a spectrum in space is fairly complex. Discussion on variability of 100 kHz radio intensities is left rather loose and qualitative, although the variations are well depicted in Fig. 5. We naturally expect the subionospheric propagation, transionospheric propagation and ducting of the waves along the magnetic field lines to be highly variable. As the data shows the variability, a quantitative summary statement at least is needed. If the authors could envisage schemes of using the observed variability in interpreting possible TARANIS results it would add value to the manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 23149, 2010.

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