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Interactive comment on "Magnetic local time asymmetries in electron and proton precipitation with and without substorm activity" *by* Olesya Yakovchuk and Jan Maik Wissing

Anonymous Referee #2

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The paper describes electron and proton differential number flux 1/(mˆ2-s-sr-MeV – according to the figure labels) measured in various energy channels by detectors on POES and METOP from 2001-2008 in the radial direction away from Earth for electrons and 9 degrees from that in protons. It presents the results of these by field line mapped latitude at 110 km altitude and by magnetic local time (MLT) for non-sub-storm times and isolated sub-storms. It also makes mention of comparisons to geomagnetic activity (Kp), but does not present any data in this regard.

The data in the paper could have potential contributions to the field with additional work and resubmission.

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Discussion paper

The data itself is a contribution to the field providing the same type of measurements and analysis over a wide energy range for both electrons and protons. However, the difference in scales for the different channels and the substantial difference in energy widths of the channels makes inter-comparison between the channels difficult other than relative locations of peaks and troughs and differentials, which are the focus of the paper results. Multiplying by the geometric mean of the channel energy ranges and dividing by the channel energy width would give a reasonable proxy for energy flux, which could be put on the same scale and would allow direct comparison of levels between the energy channels, which would add to the contribution from the presentation of the data.

The data would be a contribution (and could be improved as indicated), but a good amount of additional is required to other facets of the paper.

In particular, there are two major components that need to be significantly improved. One is the need to put the data in appropriate context of previous work, empirical and physical theories. The results also would need to be appropriately put into context. The second is a loose use of language in important ways that causes misrepresentation of the data and focus of the paper.

In particular, the has been a substantial amount of work done of the latitude and MLT distributions of electron (and proton) precipitation, particularly at auroral latitudes and at the lower (TED equivalent) energies, such as Cattell et al, 2006, Newell et al, 2009, and more recently, Dombeck et al, 2018 and the references within these. There is also a substantial number of papers on the MLT and latitude dependence and relationship between Alfvén waves and electron precipitation, by the likes of Keiling, Chaston and Hatch. Although these later (Alfvén wave papers) only relate to one small energy band and one part of the precipitation in the channel, so comparison to the presented results would be indirect, so perhaps could not be included in detail. However, the Cattell, Newell and Dombeck papers all show latitude versus MLT plots of precipitation that can and should be compared to the data and results presented in this paper.

To get precipitation (other than drift lose cone affects in the SAA, which the authors are pointedly not trying to investigate). acceleration and/or wave scattering into the loss cone is necessary. These mechanisms have been discussed and investigated in the literature for decades, and they are dramatically different for the different species and energy channels discussed. All of this has direct relevance to the presented data and the proper and useful interpretation of the results from those data, and the data and results should be discussed from firmly within that context.

For the second major issue, the loose use of language, the most egregious issue is the use of the loose use of the word precipitation. In particular, the title and abstract indicates that the paper is about precipitation, but even in the best case this is only half of the presented results, the highest latitude results. As the authors state, the lower latitudes are primarily measuring trapped populations. Therefore the title does not properly describe the paper. Even at the higher latitudes, what is being measured is the downgoing population (centered at various pitch angles depending on latitude, population, and relative potion of the spacecraft to Earth's magnetic pole). While this is still mostly "downward", downward does not necessarily mean precipitating, as the authors point out in relation to their discussion of the SAA. The authors need to be clear what it is that the data presented are. The title and discussion should be adjusted accordingly.

Additional loose uses of language include discussions of "source" particles, and "qualitative" and "quantitative" results. In particular, there are really only two sources of particles that are being detected: the solar wind/sun, and the ionosphere/Earth's atmosphere. One could argue that populations that are trapped in the ring current, plasmasphere, radiation belts, plasmasheet, etc, are different pools of particles that are "sources" for the particles measured in study. However, that argument has not been made in the paper. The authors also appear to using the word "source" to allude to both populations pool that the measured particles came from/belong to, and for the mechanisms that cause them to precipitate/be observed in the data. This really needs to be clearly described in the paper.

The authors also make a pointed distinction between "quantitative" and "qualitative" results in the paper. In nearly all cases, however, the results described in the paper are qualitative descriptions of the quantitative presented data. The only except to this is the minimum to maximum differences by MLT, which is purely quantitative. Regardless, the use of "qualitative" and "quantitative" as descriptors for the results is unnecessary and as used highly confusing (and inaccurate). As such they should likely just be removed.

At a very minimum these two major issues need to be addressed: the data and results need to be presented within the proper context (a useful review of the TED energy range precipitation is in by Frey, 2007), and a more precise description of what is presented/covered in the paper is required.

Until the paper is reworked into the proper context, it is difficult to determine whether the conclusions are significant (currently they are not), how important the contribution is, or if the length of the paper is adequate. The language is mostly fluent, but does need to be made more precise in ways, as mentioned.

The figures are of adequate size, although the text in them is too small in some cases. The presentation is clear and organized, although missing the context for appropriate organization.

Several other things that should be addressed in the next version of the paper, include the following:

A figure showing coverage by MLT and latitude would be very helpful.

A thorough discussion of the minimum count levels of the instruments, and how the noise associated with 1 count levels, interpretation of zero counts, and differences between instruments and channels with regard to this are addressed should be included.

The field of view of the detectors should be discussed as well as the effects this has in interpretation of the results, from both the perspective of mirroring particles as well as potential anisotropic distributions.

The word "moves" is used to described the differences between energy channel results. This is not the appropriate word, they are different populations, the features are not "moving" in any sense.

The "precipitation zone" is not at lower latitude with higher energy in general as indicated in the paper. The peak latitude appears identical for TED 8, 11, and 14, for example. The peak "precipitation zone" latitude is related to where particles of these energies reside (radiation belts, etc.) and/or where the acceleration mechanisms that cause those energies occur. This will be clarified once the word is put into appropriate context. It is unclear that there is a physical meaning to the particle energy to latitude of peak flux, so perhaps this relation should be omitted. If it is included, it should be demonstrated with a statistical plot of energy versus peak flux latitude, or some such, from the data.

The plume has no relevance to the discussion of the 9-10 MLT hotspot, and should not be included in that discussion. This hotspot has been observed before in the Cattell, Newell and Dombeck et al, papers, for example, as well as tangentially addressed in Frey, and should be discussed within the context.

Other details that should be addressed include:

Page 1, Line 8: The sentence ending with "how" should be reworded.

Page 1, Line 13: "main link" should be replaces with something like "a primary link". Solar UV input has much more affect on atmospheric chemistry than particle precipitation, and even affects the precipitation.

Page 1, Line 16: The statement the auroral particle precipitation *is due to* . . . does not make physical sense. This sentence needs to be reworded.

Page 2, Line 22: "over" a wide energy range, rather than "on" a wide energy range.

Page 6, Line 25: "Of course", rather than "Of cause". Although that is somewhat colloquial, and isn't really required in the sentence/

Page 8, Line 19: "A potential explanation", rather than "An explanation".

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