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Interactive comment on "On the radiation belt location in the 23–24 solar cycles" *by* Alexei V. Dmitriev

Anonymous Referee #1

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The paper addresses the variation in the outer radiation belt location during 23-24 solar cycles (years 2001-2018). The author uses the electron flux data provided by POES (Polar-Orbiting Environmental) satellites. Since outer radiation belt is highly dynamic and undergoes variations under a number of external sources, the author makes special care to exclude most of these. The residual variation of belt location corresponds, in author's opinion, to variation of geomagnetic field and solar activity. Three longitudinal sectors are investigated: 80°W (America), 0°E (Europe) and 100°E (Siberia). The main conclusion is that Siberian sector exhibits anomalous behavior: while American and European equatorial drifts of the outer radiation belt during years 2001-2018 are equal to 1° and are consistent with IGRF-12 geomagnetic field variation, the Siberian drift equals 3° and is 2° larger then predicted by IGRF-12 model. In my

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Discussion paper

opinion the paper addresses an interesting and important question how the satellite data can be used to improve geomagnetic field models. The paper has clearly stated idea, method for data processing and conclusion. However, the main conclusion of the paper seems questionable. The amount of 3° comes from linear fit of data which is too far from straight line. The author attributes the difference to variation of solar activity. In this case he has to introduce some model of solar activity influence on radiation belt position and subtract the corresponding amount from the data prior to applying linear fit. Another way is to treat variation as random, then the uncertainty in linear fit parameters should be calculated, not only their mean values. For example, linear fitting of >300 keV Siberian data for electron flux maximum with Origin 6.0 yields Lat=(-0.166±0.084)·Year+(401±169). (Here I neglected "day-of-year" shift in data, so mean values are slightly different from those in the paper.) It means that the uncertainty in the slope is about 50% and 3° may in fact reduce to 1.5°. In my opinion, the paper should be improved and resubmitted, as its main conclusion is not supported sufficiently by the data used. The author should either account explicitly for solar activity influence or calculate the uncertainty in linear fits and, probably, correct the conclusion of the paper.

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