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Reply on RC1

Dimitry Pokhotelov et al.

Author comment on "Polar tongue of ionisation during geomagnetic superstorm" by Dimitry Pokhotelov et al., Ann. Geophys. Discuss., https://doi.org/10.5194/angeo-2021-19-AC1, 2021

We appreciate the referee comments. Below is the detailed response, with the referee's original comments included in *italic* typeface.

While the topic itself is interesting, I don't think that the research objective and conclusion of this manuscript are new. The main objective of this work is to investigate the role of ExB drift and wind for the vertical uplift of SED. However, a number of recent simulation works have already addressed this topic (Liu et al., 2016, 2017; Dang et al., 2019, Klimenko et al., 2019, Jiang et al., 2020). Those works have shown that the upward component of the ExB drift plays a primary role in formation of the SED and TOI. These works have also discussed the role of wind. I don't think that the current manuscript clearly presents significantly new finding on SED or TOI. The authors should define a unique research objective and establish results that do not overlap the existing works.

The novelty is in the modelling of TOI formation during the extreme (superstorm) event. We agree that the key mechanisms responsible for the plasma uplift, i.e. the vertical component of the ExB drift and the equatorward neutral winds, have been identified in earlier studies. Some numerical studies have been done already in early 2000s to illustrate the interplay between the two mechanisms. These early works are already discussed and referenced in the Introduction. The recent simulation study by Liu et al. 2016 was already referenced in the article and discussed. We have now included the refence to Klimenko et al., 2019. It is highly relevant and we discussed the implications. Dang et al., 2019 work is focused on the splitting of the TOI ("double tongues") that can't be addressed in our work (Dang et al. used experimental setup of TIE-GCM with very high horizontal resolutions of 0.6 x 0.6 deg). We referenced it as well. Jiang et al. 2020 is focused on the mid-latitude SED anomaly, that is only remotely relevant to the topic. All of the mentioned modelling studies simulated relatively moderate geomagnetic storms. The storm of March 2015 (the largest in the 24th solar cycle), modelled by Liu et al. 2016 and Klimenko et al. 2019, had Dst minimum of -226 nT. Such storms are not in the category of "great storms", commonly defined as Dst below ~ -300 nT (Kamide et al., 1997). To our knowledge, the current study is the only attempt so far to model the TOI formation with a physics-based ionospheric model during a great storm (superstorm) event. The magnetosphere-ionosphere interactions in general, and the formation of SED/TOI in particular, are expected to be quantitatively and qualitatively different during great storms (e.g., Kamide et al., 1997; Yin et al., 2006; Pokhotelov et al., 2008). This defines the significance of our study, both scientifically and in the context of space weather modelling. We demonstrated that the ExB uplift is the dominant mechanism during great storms, at least during this particular superstorm (minimum of -422 nT), which is the largest storm recorded by modern instrumentation. In contrast, for moderate storm simulations, neutral wind effects in the polar cap could be noticeable (e.g., Klimenko et al., 2019). We further clarified the novelty of our study in the text.

The simulated TEC does not decay with latitude but has a local peak near noon at 80 degrees latitude at 15 UT. Please discuss the cause of this peak. Is this because of cusp precipitation? How much does precipitation impact the formation of the TOI?

The local peak at 80 degrees noted by the referee is not a permanent feature. In Figure1 attahced we show a sequence of 20 min snapshots before and after 15 UT. The noted peak is probably caused by a non-stationarity of the TOI formation/transport, e.g., due to IMF fluctuations, leading to a local plasma enhancement. It is unlikely to be caused by cusp precipitation, that is to our knowledge not specifically implemented in the TIE-GCM simulation. There are also possible plasma transport problems due to a grid singularity at the pole. We included further discussion in the text.

The authors discussed the local time difference of the simulated and observed SED/TOI. However, the magnitude difference is not discussed. The simulated SED/TOI has much higher TECU and spreads much wider local time. Also the simulated TOI extends to the nightside but the observed TOI disappear near the pole. Please discuss why the simulation overestimated the SED/TOI.

The overestimation of TEC magnitudes by TIE-GCM is already discussed (lines 170-172). The same overestimation of high-latitude positive anomalies is reported by Liu et al. (2016) for moderate storms. The exact reasons for such overestimation are currently unknown and should be addressed in a model validation work. The observed TOI in IGS TEC maps is poorly represented past the north pole due to the lack of ground receivers in the Arctic ocean area, thus we could not definitely compare IGS TEC maps with the simulations in this region. We commented on this in the text.

The simulation runs used statistical input parameters. While this approach is reasonable, the manuscript only compares to the observed TEC and does not evaluate errors of other parameters. It is desired to incorporate convection and precipitating particle observations (such as SuperDARN, DMSP and POES) and discuss errors of the simulation.

We agree with the referee that constraining the simulations by real observations of plasma convection from SuperDARN and satellites could be a fruitful approach. We are planning to do this in the future. Previously, the SuperDARN and DMSP convection data were analysed during the Nov. 2003 superstorm (Foster et al., 2005; Pokhotelov et al., 2008). However, the SuperDARN coverage in 2003 was not optimal and also the DMSP SSIES instruments (ion dynamics) experience problems during great storms (Pokhotelov et al., 2008). We would prefer to do such simulations in the future starting with moderate, as well as more recent, storm events. Adding precipitating particle observations from DMSP or POES should be relatively less important, as the uncertainties in specifying the convection electric fields are generally greater (e.g., Pedatella et al., 2018).

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Please also note the supplement to this comment:
https://angeo.copernicus.org/preprints/angeo-2021-19/angeo-2021-19-AC1-supplement.pdf