Second reply to the Anonymous Referee #1

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

The authors have clarified that the main difference from the existing works is the investigation of the superstorm. The usage of the superstorm is indeed mentioned throughout the paper, but the abstract and conclusion do not clearly state the uniqueness of the study. The abstract and conclusion should stress that the past findings are from moderate storms and did not investigate superstorms. The current abstract and conclusion only describe the findings that were also seen in the moderate storms. The abstract and conclusion should also clearly describe what results in the superstorm simulation are different from the moderate storms. The only brief discussion about the differences between the superstorm and moderate storm simulations is made at line 251-254. This discussion should be expanded by detailing differences in TEC, convection and wind between the superstorm and moderate storm. Neutral wind, composition and TEC suppression are briefly mentioned, but the differences from the moderate storms should be stated quantitatively.

The abstract, as well as the discussion and conclusions, have been expanded accordingly to emphasise the results specific to the Nov. 20 superstorm, and to compare with earlier simulations of smaller storms. Since TIEGCM simulations of relatively smaller storms were already published (in particular, Liu et al., 2016, referenced in the paper), we refer to the published results for comparisons. However, it is beyond the scope of this study to provide a detailed quantitative analysis of the differences in ionospheric responses to superstorms vs moderate storms. We believe such quantitative analysis is not possible with the current state of modelling and observations.

One of the reasons why the existing papers of TOI did not simulate superstorms is that it is difficult to know reliable convection pattern in extreme magnetic conditions. The convection model in TIEGCM is not calibrated against superstorms. As the authors mentioned, convection data such as SuperDARN is sparce in superstorms. How reliable is the convection model in TIEGCM in superstorms? How much do errors in convection influence on the results?

As already emphasised in the current paper, the Weimer convection model in TIEGCM underestimates the degree of equatorward expansion of the convection pattern. The actual degree of the expansion was assessed in our early study of Nov. 2003 storm using DMSP satellite passes (Pokhotelov et al., 2008, referenced in the paper). Unfortunately, no suitable instruments operated in 2003 to allow a construction of better global convection models. Since that, the SuperDARN radar network has been expanded to lower latitudes, but no new superstorms have happened. Such an error estimate for the current superstorm of Nov. 2003 would be highly speculative, mainly because the convection data are poor. We will address the topic in future works using the expanded SuperDARN convection data from relatively smaller storms.

Dst = -226 nT storm is not a moderate storm. In Gonzalez et al. [1994]'s widely used definition, Dst < -100 nT storms are intense storms. "intense" storms should be used instead of "moderate" storms. https://doi.org/10.1029/93JA02867

The referee points out correctly that all the storms with Dst < -100 nT are commonly classified as "intense", while the storms with -50 nT < Dst < -100 nT are classified as "moderate" (e.g., Tsurutani and Gonzalez, AGU Monograph 98, 1997, https://doi.org/10.1029/GM098p0077). Accordingly, the storm of March 2015 with the Dst minimum of -226 nT should be classified as “intense”. However, we described the storm as “relatively moderate” in comparison to the much greater superstorm of Nov. 2003. We have now corrected the use of “moderate” throughout the text, by either removing the term or making it clear that we mean a “relatively moderate” event in comparison to great/superstorm events.