First of all, I would like to apologize for my late response to the authors' replies. Somehow I missed the fact that the reply had been posted. I am also not so familiar to the interactive discussion process in *Annales Geophysicae*.

General Discussion: I was referring specifically to the many panels in the various figures in the paper. For example, in Figure 2, many different plots are posted without much interpretation as to which ones are most relevant for the authors' purpose. This makes it difficult to the reader to determine what is important here. For example, is it the point to determine the ground magnetic fields due to the surface waves? Or perhaps to compare with radar observations? More specificity would help the reader understand the authors' points here.

"The model is highly idealized": The main point here is that there have been very many numerical studies of ULF wavs in dipole geometry or even more generalized geometries, e. g., Degeling et al., 2010; Lysak et al., 2004, 2020; Ozeke et al., 2009; Wright and Elsden, 2020, to limit it to references cited in the text. At the very least, quantities in the outer magnetosphere should be mapped from their ionospheric values to compare with data.

Negative z values: Ok, as I said before, the authors are free to use any coordinate system they wish. But this seems confusing to me.

Magnetopause boundary at x = 0: The authors seem to miss my point here. Certainly the x=0 field line extends to both ionospheres, but the point was that the regions near the ionosphere are not in direct contact with the solar wind or magnetosheath, rather they may be adjacent to the cusp. I did not comment on the lack of curvature in the field lines (perhaps this is in response to the other reviewer). I would agree that the box model is more reasonable if one is only considering altitudes up to 1 or 2 R_E; however, it is not accurate over the whole field line. Moreover, the WKB approximation is not relevant to ULF waves with wavelengths comparable to the whole field line, since it assumes that the wavelength is much less than the scale length for variation in the wave speed.

Mapping of quantities: The response does give a scaling between the ionosphere and magnetopause currents, scaling with the magnetic field strength as stated. But I do not see that being applied in the manuscript itself. Lines 231-232 correctly note that the ionospheric fields do not change if a dipole scaling is applied, but the corresponding parameters at the magnetopause would in fact need to be scaled.

Spacing of structures: Point taken, for scales not too much less than the ~100 km height of the ionosphere there would still be some signal at the ground.

Technical corrections: I note that I have not seen a revised version of the paper so I can't tell how the authors have modified the paper in response.

Point 3: To be clear, for a plane wave written as a complex exponential, e.g., $Ae^{-i\omega t}$ where A is complex, both real and imaginary parts of A are physical, since the field becomes $\mathrm{Re}\, A\cos\omega t + \mathrm{Im}\, A\sin\omega t$.