We thank the reviewer for their further clarifying comments. Below are our responses.

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.

Figure 2 is meant to simply depict how the model works, demonstrating the setup and what its various outputs are, as applied to a single example. We can clarify this in the text. We do later refer to each example output in its relevant section of the manuscript in contrasting the near- and far-field forms of the solutions, before studying these in more depth through cuts in the later figures which are discussed more in the text. As in our previous response, there is no one single region which is most relevant. Our aim is to characterise the amplitudes and spatial scales across all the ionosphere and ground quantities depicted in one comprehensive piece of work, given these regions are intimately related.

"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.

All these studies concern (or rely upon) time-dependent simulations of the linear MHD equations applied to the magnetospheric domain with a dipole or more generalised magnetic geometry. As we outlined in our introduction, thus far these simulation codes have yet to include surface waves into these geometries. Elsden et al. (2025, https://doi.org/10.1029/2025JA033830) have only recently simulated plasmapause surface waves driven by the model outer boundary in a box model geometry. While in principle the plasmapause surface wave can be modelled in a dipole or more general geometry within these codes, the magnetopause surface wave serves as the outer boundary condition to the simulated magnetospheric domain, so it is unclear how to self-consistently model it in these codes without requiring a priori knowledge of the solution. This is why high-resolution general purpose MHD codes which include the solar wind, magnetosheath, and magnetosphere have been used thus far.

Our aim is to perform a parameter study on how magnetopause surface wave effects may vary with system and wave properties. This is unfeasible for time-dependent simulations due to the computational expense involved, hence why we take a semi-analytic approach. Given analytic solutions to the surface wave do not exist in the geometries the reviewer suggests, this necessitates simplifying the environment. We fully outline the limitations of this in our discussion and suggest further improvements and future work required to test the novel results suggested by our simple model.

At the very least, quantities in the outer magnetosphere should be mapped from their ionospheric values to compare with data.

The surface wave's currents have been scaled from the outer magnetosphere such that their amplitudes are correct at the MI-interface, as mentioned in our previous response. Currents are the only physical quantity that are used in the coupling between the different regions of our model.

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.

In our model x=0 corresponds to the equatorward edge of the projected magnetopause boundary layer, i.e. it is magnetically connected to the closed field lines on the inner edge of the magnetopause. Plasma displacements at the equatorial magnetopause with wavelength and amplitude larger than the boundary layer's thickness leads to a single coupled surface mode across the entire boundary layer rather than distinct modes at the different interfaces (Kivelson & Pu, 1984, https://doi.org/10.1016/0032-0633(84)90077-1), a point we can make clearer in a revision. The Chapman-Ferraro currents associated with this magnetopause surface wave in the outer magnetosphere are converted into field-aligned currents which are able to travel through the cusp region and couple to the ionosphere, as illustrated in Figure 1, which has been demonstrated in high-resolution global MHD simulations (Archer et al., 2023, https://doi.org/10.1029/2022JA031081).

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 RE; however, it is not accurate over the whole field line.

We included the entire field line for the surface wave currents to be self-consistent within the model. However, this little affects the results. Ionospheric current systems only depend on the FACs at the MI-interface. The ground magnetic field is dominated by currents closest to Earth due to the form of the Biot-Savart law. Only 5% of the ground field contribution arises from magnetopause currents above ~0.7-4RE (dependent on wavelength), which we will note in the revision. The corresponding altitudes for 5% of the geoelectric field altitudes are even smaller at ~180-700km. Therefore, in line with the reviewer's comment, this demonstrates that the box model is reasonable for the ionospheric and ground-based effects of interest.

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.

We agree with the reviewer that the WKB approximation is not strictly valid due to the scale of variations in wave speed being shorter than the wavelength. Nonetheless, in practice it has shown to work reasonably well in estimating ULF wave properties, serving as an improvement over calculations using homogeneous conditions with estimated errors from the true (numerically determined) solutions of <20% (e.g. Rickard & Wright, 1995, https://doi.org/10.1029/94JA02935; Wild et al., 2005, https://doi.org/10.1029/2004JA010964; Rankin et al., 2006, https://doi.org/10.1016/j.asr.2005.09.034; Archer & Plaschke, 2015, https://doi.org/10.1002/2014JA020545). Our point invoking the WKB approximation was simply that the phase speed is high close to the ionosphere, so phase variations along the field are expected to be slow.

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.

The magnetopause currents are indeed scaled to be of the correct amplitude, as per our previous response, which the revision will make clearer to readers. Currents are the only physical quantity coupling the different regions within the model.