Response to Reviewers

‘Characterising mesoscale magnetopause surface waves within magnetosphere–ionosphere–ground coupling’

We thank both reviewers for the engagement throughout the interactive discussion. Here we reiterate our responses, making note of the changes made to the revised manuscript. Line numbers correspond to the tracked changes version.

Reviewer 1

This manuscript presents many calculations from a quasi-static model of magnetosphere-ionosphere-ground coupling driven by boundary surface waves at the magnetopause. This is an important problem, worthy of investigation. However, the overall impression of this work is that it gives many calculations of different quantities without a real main focus. What physics points are meant to be illustrated by these calculations? As it stands, the manuscript reads more like a chapter from a Ph.D. thesis covering many aspects of this problem rather than addressing a specific physical issue. It would be preferable for the main text of the manuscript to answer specific scientific questions with other aspects of the problem put into supplementary information.

The last paragraph of our introduction outlines the main problem surrounding the generalisation of simulation results on magnetopause surface waves and the purpose of this work – namely “to advance understanding of how magnetopause surface waves’ effects within magnetosphere–ionosphere–ground (MIG) coupling might scale spatially and in amplitude across key wave and system parameters” (lines 120-124). We later outline and justify the specific wave and system parameters that are of greatest interest to vary (section 2.2). All results and figures serve this aim, though they do cover several different regions of the magnetosphere–ionosphere–ground coupling, namely the ionospheric currents, ground magnetic fields, and geoelectric fields. While one could feasibly focus on only one of these leaving others to separate manuscripts, given the quantities are all intimately related we found it more instructive to include them all together in one more comprehensive piece of work. We have revised the manuscript throughout to ensure that the aim is clearer, in particular we added this more clearly to the abstract (lines 5-6).

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 have now clarified this in the text (lines 145-146). 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. 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, containing many questionable assumptions.

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.

Currently, in terms of time-dependent simulations, only general-purpose global MHD codes which include the solar wind, magnetosheath, and magnetosphere can self-consistently reproduce magnetopause surface waves. However, these need to be run at high resolution to correctly resolve the physics, making them computationally expensive. Furthermore, given some of the wave and system properties are not always tuneable makes these codes impractical for parameter studies. These points are made on lines 109-116.

Dedicated linear ULF wave simulations with dipole or more generalised geometries also exist which are more configurable, which include the references the reviewer mentions. Thus far these time-dependent 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. We now mention these points more explicitly on lines 117-120.

Our aim is to perform a parameter study on how magnetopause surface wave effects may vary with system and wave properties. Given this is unfeasible for time-dependent simulations motivates our semi-analytic approach. However, the linear MHD wave equations cannot be solved analytically in most realistic geometries – even for a dipole the equations cannot be fully solved requiring numerous approximations which yield highly complicated results (Leonovich & Kozlov, 2019, https://doi.org/10.1029/2019JA026842). Solutions do exist for the rectangular box model, long used in ULF wave research for initial insight, which we use in this work. This is an approach with a long history in ULF wave research which continues to be employed even to date (e.g. Elsden et al., 2025, https://doi.org/10.1029/2025JA033830). These points are made on lines 133-137.

We are aware of the simplifying assumptions behind this idealised model, hence why we included a dedicated section discussing their limitations (section 5.2). There we outline to what degree results may likely be affected by the more questionable assumptions behind the model and how further refinements could be made in the future. We therefore feel that we have justified our simple approach and been transparent in which aspects of the results would likely be modified by incorporating more sophisticated modelling.

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 (see lines 261-268). Currents are the only physical quantity that are used in the coupling between the different regions of our model.

The first confusing point is that the model volume appears to be only in negative z values. It is stated that the northern ground is at z = 0, with the northern ionosphere at z = −h. That is, the z component decreases with increasing distance from the northern hemisphere. The southern ionosphere is a distance −z0 from the northern ionosphere, and evidently the ground signatures in the southern hemisphere are not considered. While the authors are of course free to use any coordinates they wish, it seems unusual to work with negative values of the coordinates.

A coordinate system where the ground corresponds to z=0 with z pointing downwards is quite common in geophysics, particular in induction problems within MIG-coupling (e.g. Thomson & Weaver, 1975, https://doi.org/10.1029/JA080i001p00123; Pirjola & Viljanen, 1998, https://doi.org/10.1007/s00585-998-1434-6). Coordinate systems where the background magnetic field is aligned with z are also common in ULF wave research (e.g. Southwood, 1974, https://doi.org/10.1016/0032-0633(74)90078-6). Furthermore, many ground magnetometers use a coordinate system where z points downwards (e.g. Laundal & Gjerloev, 2014, https://doi.org/10.1002/2014JA020484).

By symmetry, ground signatures in the southern hemisphere would be mirror reflections of those in the northern hemisphere, which is now noted on lines 212-213.

A second point is that the magnetopause boundary is at x = 0, evidently extending to both ionospheres. This does not seem physical.

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). 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). These points are made clearer on lines 31-40 & 173-174.

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). 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. We now mention these points on lines 149-150 & 796-798.

The model is also quasi-static in that the propagation of the current along the field lines in the form of Alfvén waves is not considered, nor are any inhomogeneities in the plasma parameters along the background field.

MHD waves in the magnetosphere quickly form standing modes between boundaries, such as along field lines' conjugate ionospheric footpoints (Wright & Mann, 2006, https://doi.org/10.1029/169GM06), due to counter-propagating waves. This is also the case here, where a fundamental surface mode along the field is considered. This is now mentioned on lines 57 & 164-166.

The reviewer is correct that no inhomogeneities along field lines are included. Given the magnetic field strength is largest near the Earth, surface wave phase speeds are also much larger here than out in the equatorial magnetosphere (e.g. see Figure 2 of Archer & Plaschke, 2015, https://doi.org/10.1002/2014JA020545). Therefore, phase variations along the field are expected to be slow. Since our results showed little dependence on current variations along the field in our simple model, it is likely this effect will be very small. This is now discussed on lines 793-798.

One consequence of this is that the current amplitude is not mapped along the field as would be the case in the dipole magnetosphere. For example, it is stated that the displacement of the surface wave is one Earth radius and equation (3) calculates the current based on that value. This is also assumed to be the current amplitude at the ionosphere, which would not really be the case. Thus, while the calculations appear to be correct within the model assumptions (at least as far as I can tell), the model cannot produce realistic model results.

We argued that since box models cannot be globally representative, that it was most important to make our model match conditions at the MI-interface. Current amplitudes incident on the ionosphere are unchanged when typical magnetospheric field and scales are used, with dipole flux tube scaling subsequently applied. This is a result of the insensitivity of the surface wave current to wavenumber, which we will demonstrate to the reviewer here. Consider a box model that covers only the outer magnetosphere. The amplitude of surface wave currents in this model are, as per equation 3,

$$J_{msp} = \frac{\xi_{msp} B_{0,msp} k_z}{\mu_0}$$

Here the `msp' subscript refers to the outer magnetosphere. Applying flux tube scaling of the currents from the boundary of the magnetospheric box model to the ionosphere, we get that the current amplitude incident on the ionosphere is

$$J_{isp} = J_{msp} \frac{B_{0,isp}}{B_{0,msp}} = \frac{\xi_{msp} B_{0,msp} k_z}{\mu_0} \frac{B_{0,isp}}{B_{0,msp}} = \frac{\xi_{msp} B_{0,isp} k_z}{\mu_0}$$

Here `isp' corresponds to conditions at the MI-interface. Given that we used the background magnetic field at ionospheric altitude together with the displacement amplitude at the equatorial magnetosphere in our calculations, the current amplitude at the ionosphere are correct. This has been made clearer on lines 260-270.

It is not possible to self-consistently incorporate variations in current amplitudes along field lines due to flux tube scaling, as this necessarily implies a magnetic geometry (such as a dipole) for which the MHD equations cannot be solved analytically. Note, that to first approximation it is the change in location of the flux tubes that will affect the Biot-Savart integration most, rather than the varying current density given that jdA is invariant along the flux tubes. We now mention this on lines 787-805, where we also note the correction term outlined by Fukushima (1976) to account for a different magnetic geometry.

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, see lines 260-270. Currents are the only physical quantity coupling the different regions within the model.

As a final point, the authors argue that surface waves may have scales smaller than the typical 100-200 km spacing of ground magnetometers; however, it has long been known (e.g., Hughes and Southwood, 1976) that perturbations with scales less than the ionospheric height are exponentially attenuated before reaching the ground.

Our results are not in contradiction with the work of Hughes & Southwood (1976, https://doi.org/10.1029/JA081i019p03234). Their seminal work demonstrated that plane Alfven waves' ground magnetic signatures are screened by the ionosphere by approximate factor $\Sigma_H/\Sigma_P \exp(-kh)$ from that above the ionosphere, where h is the ionospheric altitude. Given the incident plane wave assumption from the magnetosphere, all physical quantities follow the same plane wave form in the ionosphere and on the ground. They also showed that altitude-dependent phase variations also occur to the magnetic field components. Overall, the assertion is that magnetospheric perturbations with scales less than the ionospheric height are significantly screened from reaching the ground.

As Hughes & Southwood point out, the general problem for an arbitrary magnetic field distribution requires decomposing the incident wave into each Fourier mode, since the ionospheric screening effect applies to each individually. A surface wave modifies the spatial structure of the incident magnetospheric waves' currents to a plane wave in y multiplied by a delta function in x. Thus there is a single wavenumber along y, but a broad spectrum of wavenumbers along x. We showed that the amplitudes of magnetic perturbations on the ground directly underneath the magnetopause boundary layer follow a similar exponential screening effect $\Sigma_H/\Sigma_P \exp\left(-k_y b\right)$ (Figure 8a). This is in fact is more attenuating than the Hughes & Southwood result since the characteristic scale, b, is larger than h for each component of the field. This agrees well with Hughes & Southwood when considering the average overall wavenumber $k = \sqrt{k_x^2 + k_y^2}$, as we explained on lines XXX. When wavelengths in y are around 200km (the shortest scale surface wave we consider, larger than the ionospheric altitude), the ground magnetic amplitudes remain measurable at tens of nanotesla.

Our point about the scales of the ground magnetic response to surface waves being smaller than the typical 200-300 km spacing of ground magnetometers applies only latitudinally. The ground magnetic response is the interference pattern of all the attenuated incident Fourier modes, which in general will be complicated. Due to the interference, the latitudinal scales of the response are not the same as those incident, unlike the plane wave case. Indeed, Figure 8b shows that latitudinal scales of surface waves on the ground are larger than those in the magnetosphere and ionosphere. Nonetheless, the latitudinal scales of the response need not be restricted to being longer than the ionospheric altitude given the longitudinal variation also exists. This can roughly be understood thusly, given incident longitudinal wavelengths larger than h, any additional scales latitudinally still result in overall scales $\sqrt{x^2 + y^2}$ larger than h, which are then not expected to be significantly screened by the ionosphere. We have clarified these points on lines 567-570.

(1) Line 234 says "The ionospheric altitude is set at the typically used Eregion value." What value do the authors think that this is? (It's stated in the table, but it would be better to say it in the text.)

We have added this to the text (line 270).

(2) In the equations on lines 267 and 268, the factor in the denominator should be k_y^2, not k_y. This is corrected on line 289.

The reviewer is incorrect. Firstly, this can be demonstrated through dimensional analysis. The near-field ionospheric potential in the equations on line 304-305 must have units of Volts. The surface current J_0 in the numerator has units of Amperes per metre (see Table 1) and the conductance in the denominator has units of Siemens (equivalent to Amperes per Volt). All other factors apart from the wavenumber are dimensionless. This means an additional factor with units of reciprocal metres is required in the denominator, which is only achieved by k_y and not k_y^2. Secondly, one can use the identity

$$\frac{d^2}{dx^2}(\exp(-|kx|)) = k^2 \exp(-|kx|) - 2k\delta(x)$$

to verify our equation is correct.

In the equation on line 326, which here concerns the far-field, there is an additional factor of perpendicular radius, R, in the denominator. Thus, the required overall dimensions of reciprocal metres are only achieved by k_y^2 multiplied by R.

(3) Typo in line 336: "have" should be "has."

We have corrected this, thank you (line 373).

(3) Line 371: “only the imaginary parts are treated as physical” but usually the complex amplitude gives information about phase shifts, i.e., values at other points in the traveling wave.

Complex numbers are used in wave physics to simplify the mathematics of sinusoidal oscillations, representing both amplitude and phase in a single compact expression. In these cases, it is appreciated that the physically measured quantity is, however, a real number, e.g. the real part. Our point here was that, due to the discontinuities present at the wave packet edges in the real part, one should consider the imaginary part to be more representative of a physical measurement. We still use the complex amplitudes and phases throughout the paper for analysis given the mathematical convenience. We have revised this statement to alleviate confusion (line 194-196).

(4) Figure 4: the caption says that the multiple curves are for “various y-values” but it should be stated what those values are. A similar problem applies to Figures 5-7.

The specific values are denoted by the respective colour bars displayed in each of these figures, hence we do not think it is required, or indeed helpful, to list the values used in the captions.

(5) Lines 765-766: it is stated that the Knight parameter is “relatively insensitive to magnetospheric electron densities and temperatures”; however, this parameter scales like neTe 1/2, where these parameters should be taken at the top of the acceleration region (Boström, JGR, 2003), which could be at various altitudes in different cases. Thus, the parallel electric field can have a significant effect on the ground signature of boundary layer flow (e.g., Lotko et al., JGR, 1987).

We take the reviewer’s point that the resistive-coupling length scale from combining the Knight parameter with the Pedersen conductance can, in general, vary. While it is typically smaller than the wavelengths of surface waves at the MI-interface, in cases when they are comparable then the field-aligned current amplitudes of the waves may become modified. We already provided an approximate modification of the linear current amplitudes due to this effect. We agree that parallel electric fields, in general, play a significant effect within MI-coupling. We have revised our statement (lines 825-832).

Reviewer 2

The authors also indicated that the Cartesian box model is simplified for local mesoscale waves (200-1600 km in the ionosphere, 1-9 Re on the magnetopause). At what wavelength does the Cartesian approximation begin to break down, especially when taking into account the field line curvature in a realistic dipole like geometry?

The reviewer raises an interesting point that we had overlooked mentioning in the text. Our focus on the applicable wave scales for the Cartesian box model was based on the curvature of the Earth/ionosphere. To assess this, we considered when the line-of-sight vector between a ground point and an ionospheric current source, as applicable in e.g. the Biot-Savart law, was tangent to the surface of the Earth. This leads to roughly a 1250km distance from the origin (in any direction) for which the box model is appropriate. Taking a similar approach when considering instead the curvature of magnetospheric field lines, the distance over which the ground tangent intersects its overhead field line will be at least of order the field lines' radius of curvature at the ionosphere and more likely comparable to the L-shell value. For the high-latitudes applicable to the magnetopause these are considerably larger values, e.g. a dipole field-line at 70deg latitude has a 3.6RE radius of curvature and L-shell of 8.7RE. These are clearly much larger than the transverse scales in the ionosphere considered. We now mention this on lines 25-251. A full treatment of magnetic geometry would involve computing the correction term outlined by Fukushima (1976), which we mention in the discussion (lines 803-805) and leave to future work given the large parameter space to explore with this.

The box model seems to neglect the contribution from the background FAC, i.e., classical Region 0/1/2 FAC pairs that exist in realistic magnetosphere-ionosphere.

We only incorporate current systems due to the surface wave in this model. The Region 0/1/2 FAC systems and their ionospheric counterparts serve as a time-constant (though spatially varying) background to the linear equations solved. These do not affect, and can easily be separated from, the time-varying wave effects. Choosing to omit the background renders our results more general and independent of any specific background current system used. We now note this on lines 142-144.

Some minor notes:

Line 61: need a space between Re and "in size".

Line 733: ionophere -> "ionosphere".

Thank you for spotting these, we have corrected these.