We thank the reviewer for their time in reading and providing comments on our manuscript. We have tried to provide comprehensive responses, however, there were some points that we unclear of the reviewers' point and would welcome further clarification during the interactive review stage.

General comments

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.

We are somewhat confused by the reviewer's assertion that our manuscript lacks a specific science question, given that 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 114-116). 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. While we can certainly revise the overall manuscript, in particular the title and abstract, to ensure that the aim is clearer throughout, we would welcome more specific instruction from the reviewer to address their overall comment in light of our response.

Specific comments

The model is highly idealized, containing many questionable assumptions.

In the introduction (lines 103-116) we outlined that global simulation approaches to understanding how magnetopause surface waves scale across the MIG-system are very computationally expensive and not always tuneable, making parameter studies impractical. On the other hand, analytical approaches applied to highly simplified environments have provided useful physical insight, whose results can be further refined later through more complicated modelling or through further development of dedicated linear ULF wave simulations. 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).

The linear MHD wave equations cannot be solved analytically in most geometries. One exception is the rectangular box model, long used in ULF wave research for initial insight, which we use in this work. Thus, analytic solutions to the magnetospheric currents of a surface wave are simply not possible for 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). This necessitates our idealised model setup.

We are well 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. However, we would be happy to add further discussion of any points which the reviewer feels would be required.

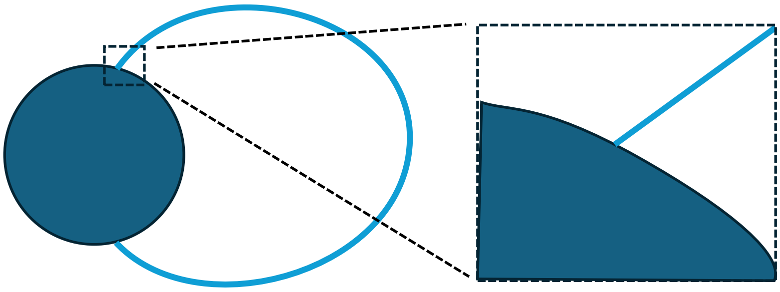
The first confusing point is that the model volume appears to be only in negative z values. It is stated that the northern ground is at $z = 0$, with the northern ionosphere at $z = -h$. That is, the z component decreases with increasing distance from the northern hemisphere. The southern ionosphere 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 we can note in the revised manuscript.

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

Could the reviewer please elaborate upon what they mean by this comment? The coordinate x=0 corresponds to closed magnetic field lines in the inner/equatorward projection of the magnetopause boundary layer's flux tubes (see Figure 1 of the manuscript). As illustrated below, these field lines are expected to close in both ionospheres.



Given the large curvature scales of these high-latitude field lines, when zooming in close to the ionosphere the field lines appear approximately straight. For example, the radius of curvature for a dipole field line at 70deg latitude is 3.6RE at the ionosphere, which is much bigger than the transverse scale of our local domain.

In our model, which requires a geometry that analytic solutions to the MHD wave equations exist, field lines become completely straight and vertical, i.e. a box model setup. This simplifying assumption is often applied when considering the ground and ionosphere at high-latitudes generally (e.g. Untiedt and Baumjohann, 1993; Laundal et al., 2015), with the argument being that due to the inverse-cube dependence on altitude in the Biot-Savart law, the ground magnetic field is most-sensitive to ionospheric current systems and only magnetospheric field-aligned currents very close to the MI-interface. We discussed on lines 743-748 that the curvature and inclination of the field lines should result in a correction term to the ground magnetic field, as detailed by Fukushima (1976), resulting in the non-cancellation of the field-aligned and Pedersen currents, which future work should investigate.

We are happy to expand on any of these points in a revised manuscript and/or address any clarifications on the reviewer's comment.

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. We can certainly make this clearer in a revision.

The reviewer is correct that no inhomogeneities along field lines are included. We can certainly expand discussion of this to the more general discussion of inhomogeneity already present. 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). In a WKB approximation to the inhomogeneous wave problem, solutions vary along the field as $\exp\left(\pm i \int_0^s \frac{\omega}{c(s)} ds\right)$ where $c(s)$ is the phase speed as a function of distance. Thus, when the phase speed is large, variations in wave quantities along the field are expected to be slow.

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 on lines 135-140 that since box models cannot be globally representative, that it was most important to make our model match conditions at the MI-interface. On lines 230-231 we stated that 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 already used the background magnetic field at ionospheric altitude (lines 229-231) together with the displacement amplitude at the equatorial magnetosphere (lines 231-232) in our calculations, the current amplitude at the ionosphere are correct. We will happily include this explicitly in a revision if the reviewer requires.

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. This would necessitate a different study using completely different methods.

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 $\Sigma_H/\Sigma_P \exp(-k_y b)$ (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 494-503. 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 100-200 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 can make this point clearer in the manuscript when discussing the latitudinal scales of the response.

Technical corrections

(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 will add this to the text.

(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 lines 267-268 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 289, 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 will correct this, thank you.

(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 the physical measurement. We still use the complex amplitudes and phases throughout the paper for analysis given the mathematical convenience. We will revise this statement to alleviate confusion.

(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).