

Reviewer comments on “Characterising magnetopause surface waves within magnetosphere-ionosphere-ground coupling” by Archer et al.

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.

Specific comments: The model is highly idealized, containing many questionable assumptions. 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 $-z_0$ 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 second point is that the magnetopause boundary is at $x = 0$, evidently extending to both ionospheres. This does not seem physical. 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. 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. 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.

Technical corrections: (1) Line 234 says “The ionospheric altitude is set at the typically used E-region 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.) (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. (3) Typo in line 336: “have” should be “has.” (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. (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. (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 $n_e T_e^{1/2}$, 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)