Responses to Referee Comments

Referee 1.

Overall I have no problem with the numerical aspect of the study. However, compared to a number of other numerical studies which include more complex fields and physics (e.g. Santamaria et al. 2015, 2016, see also a number of papers from Monash and Tenerife groups), this work does not add any significant or novel result to the field.

Response

We have provided additional justification in the Introduction. The work of Santamaria et al studies local aspects and waveguides into the solar atmosphere. Our goal is to focus on global phenomena of solar interior wave coupling to the global atmosphere. Like helioseismology, we are not addressing the formation of individual structures. The novelty is the search for evidence of the existence of global modes in the atmosphere above the solar surface.

Referee 2.

One may argue that the novelty comes from the comparison with observations, however, I find the observational analysis flawed and lacking any meaningful discussion.

Response

More details about the method have now been provided including a comparison with the hydrodynamic case with zero magnetic field and an increase of the number of pixels for the observational analysis. For more details see our answer to Ref. 8.

Referee 3.

Firstly, the introductory sections lack a certain justification for this study and seems to be void of references (the first two sections have four references). There have been a number of numerical studies that have examined the propagation of waves into the solar atmosphere. Some with more complex fields than the Gaussian studied here and others that include the more realistic case of Alfven wave mode conversion. I encourage the authors to perform a more extensive review and in turn establish the novelty of this paper.

Response

Thank you for the suggestion, we have now added some more references. However, we would like to avoid citing many related studies, instead, we refer to reviews that already summarise these earlier excellent investigations.

However, we would like to point out that we address the wave coupling at global scales, i.e. not that of some individual (e.g. sunspots, pores, spicules) or more complex (e.g. active region) structure. Therefore we do not address physics, such as mode conversion. We model the atmosphere as a homogeneous and continuous medium representing profiles based on the VALIIIC atmosphere. Following the Referee's suggestion, the paper is now extended and discussed in the introduction including a discussion of more foundational references with an emphasis on local effects and building to global phenomena including a review of some recent work.

Referee 4

Furthermore, there are some analytical and semi-analytical studies that have examined the interaction of p-modes with magnetic tubes resulting in the transmission of wave energy upwards (and downwards).

Response

The studies referred to looked at particular localised finescale structures and wave coupling. As suggested above, we perturb the average background and see if global oscillations can propagate through the atmosphere. The individual interaction of flux tubes is outside the scope of the work presented here. We take a step back and investigate if the atmosphere also has global modes i.e. we investigate if these standing modes exist in isolation in the corona. This is undertaken in the spirit of 1990s papers by St Andrews investigating whether p-modes get higher into the atmosphere.

Referee 5

A short discussion on observational results would also be an aid to the introduction. At the moment it is not clear to the reader how this paper presents any significant contribution, when compared to a number of previous work.

Response

We have provided a discussion of references to observational results in the Introduction.

Referee 6

In terms of the method, my concern is with the presentation and interpretation. Whenever examining these kinds of problems, the results should always be directly compared to the quiet Sun (B=0).

Response

Thank you for the suggestion. We have performed the recommended analysis and provided additional comparison between quiet Sun (B=0) and B=100G case.

Referee 7

The authors direct the reader to their previous papers to compare the new MHD results with the hydrodynamical case. However, where am I looking in Griffiths 2018? Those plots are very different and of the order of 1000m/s, but the plots in this paper are of the order of 5m/s. The authors should make a difference plot of the magnetic and non-magnetic vz in this paper. This should be done for Fig3,4,5. Figure 6 spectrum needs to also be compared with the B=0 case. It's well established that pmodes interact with magnetic fields and energy is transported through slow and fast waves, the authors should emphasise the significance/novelty of their results.

Response

Yes, indeed, results for the hydrodynamical cases include velocity magnitudes of order 1000m/s this is because these have been allowed to run for many cycles. We have compared against the 0G case repeated with the same driver used here. Plots have now been added as requested. Reference to the figure in Griffiths 2018 has now been removed.

Referee 8

The most problematic example of not comparing with the quiet sun is the observational analysis. The authors select a single pixel in the 1600A AIA image and compute the temporal FFT. There are systematics and noise in any observation, especially when concerning waves. I would recommend that the authors do the same analysis in 'quiet' regions of the sun and compute a statistical average spectrum before confirming that the FFT from your single pixel is confirmation of your numerical results.

response

Thank you for this comment. To address the issue we have selected a 50-pixel large area based on AIA 1600˚. FFT was performed for each pixel and the average of the spectra is now demonstrated by Figure 12. Using Z-scores and a monte carlo analysis to compute a significance level we are able to identify the significant periods from the FFT analysis. For the purpose of this study it was reasonable to consider a single pore which we discuss in the comment which follows.

Referee 9

Furthermore, select a number of different magnetic pores and see what the average spectrum is also telling you. A number of helioseismic studies have shown that high frequency pmodes can leak into the upper atmosphere (though are evanescent) without magnetic fields. Comparing quiet sun and magnetic observations is vital before you make any connections with the model.

Response

In this paper it was not our intention to put multiple pores (or any other localised structures) as this would be out of scope and would open up many different questions (e.g. mode conversion, as mentioned by the referee themselves). Our aim is to address, in a simple model (VALIIIC) the global wave coupling to the lower solar atmosphere applicable to the Quiet Sun at solar minimum. This way, we may say, there is no structuring, just global uniform inhomogeneity.

Referee 10

Finally, the authors conclude that their spectral analysis shows a larger shift than whats been previously observed by hindman et al. 1996, but then state that this is explained in part by the work of Campbell and Roberts 1989. Campbell and Roberts state very different behavior depending on the radial order n and harmonic degree ℓ. If the observed frequency shift is consistent with what Campbell and Roberts find, the authors should explain why. Is it because your source term emulates certain ell and n modes. What happens with your spectral results when you change the source term. A meaningful discussion is required here if the reader is to gain any insight into the physics at play.

Response

Thank you for pointing out that we were a bit sloppy here. We should not really use the word “shift”, though as an analogy this may be acceptable. We meant here that the magnetic field will cause changes in the oscillation frequencies. In solar magneto-seismology, this is often referred to as frequency shift. What we actually found was the following: the oscillation frequencies of the coupled atmosphere has different values depending on the strength of the magnetic field. In this respect, the analogy does stand with CR89 (and a few follow-up studies led by Roberts).

On the query of what happens with the spectral results if the source term is changed, the answer may be well beyond the scope of our study and may not be so relevant quantitatively. However, qualitatively, our point is (once more): point out that p-modes do have signatures in the lower solar atmosphere. This has been shown locally even for dynamics (see spicule formation, De pointieu et al Nature 2004). We would like to encourage our colleagues to search for these global atmospheric modes and see the Sun as a “star” and not as a highly structured inhomogeneous plasma ball.

Referee 11

Other points, but not an exhaustive list:

- There is an over reliance on the results of their previous paper, if you want readers to compare details, just rerun the simulation and compare. Otherwise make the plots comparable.

- Citep and citet need to be fixed throughout

- Units are not to be italic

- Figure 2 needs to be higher resolution (currently 85 looks like 35)

- Plot the atmospheric model's relevant parameters (cs, rho etc.)

- Plot the beta=1 line on Figure 4, you will be able to make interesting comments from what you see

- Figure 5 has typos in it 100GMm should be 100G

- Page 6 line 202, what am I looking at in Griffiths 2018b

- Page 7 In 211. This reference is wrong. The website describes the non-magnetic case and seems to be for the authors first paper, where are the videos for the current paper?

- AIA has a nyquist of (20mHz), why do you cut at 6mHz in figure 7, especially when the power seems to be growing. Furthermore, the 3sigma line seems too small. Hence, why I recommend comparing to 'quiet' regions

- Also what is going on with Table 2? Why does the flux vary by orders of 10^6 when increasing the field strength by 25G

Response

All of the above issues have been addressed and corrections applied as required.