Referee 1. - not complete

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

- study local aspects and waveguides into the atmosphere e.g. studies from tenerrife
- Our goal not to explain how local challenges channel - interested in the global phenomene
- just like helioseismology not addressing formation - f,g not addressing indiviual structures
- but this will locally modify
- looking for existence of global modes

sun is non uniform in the interior - we know this when we study the global modes and information about some internal structures they go far out into the atmosphere

fields

Referee 2 - not complete

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

carfry out similkar analysis to jack ireland. ...

Referee 3 -complete

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

happy to check papers. - have added references more foundational references and including current work

we are not saying there is mode conversion but this occurs in complicated structured medium here we are using a uniform medium

add a couple of space science reviewsa

Referee4 - not complete

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

Response

once more these studies looked at particular localised finescale structures and if they can be coupleed through waves

we perturb average background and see if global oscillations go through

drivber represents

same as ignoring super granular cells i.e. like the helioseismologists

we want to see if these standing modes exist in isolation exist in the corona

we are not interested in the individual interaction of flux tubes this is not what we do one step back the atmosphere has a global mode

approach similar to cambpell and roberts when considering the atmoisphere with no structure

]isothermal atmosphere embedded where the atmosphere was decaying

in the spirit of 1990s papers by st andrews paper wether p-modes get higher into the atmosphere

norbis thesis evidence for some

different time scale

referee 5 - not complete

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

need comments from norbi

repetition norbi look at global periods in the corona

paper from scott macintosh????

if global modes exist we have a cell this kind of cell structure existed in the corona result of SDO

https://opensky.ucar.edu/islandora/object/articles:23940

referee 6 - complete norbi check

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

this is a good point

refer to previous paper

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 emphasize the significance/novelty of their results.

Response

Yes results for the hydrodynamical cases include velocity magnitudes of order 1000m/s this is because these have been allowed to run for may cycles

we have compared against the 0G case repeated with the same driver used here

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

1 pixel, 50 pixell add result to paper... cfreate select 50 pixels compute average fft's

reasonable request from norbi!

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

don't want to make here details will be subject of another paper

global coupling in the quiet sun at solar minimum

the period of 11 year cycle when not much activity

have just come out of grand minimum see a clean sun

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

is it true that we said that shifts larger than hindman

explained in part by work

Cand R had one l there n is raidal directionwe have l m but not n

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.