26-Aug-2022  
  
Title:ApJ - p-Mode Oscillations in Gravitationally Highly Stratified Magnetic Solar Atmospheres, AAS32345R2  
  
Dear Dr. Griffiths,  
  
On this revised manuscript, I had to move to a second, independent reviewer as the first one could not deliver a report on time due to personal schedule conflicts. This decision was also spurred by the first reviewer's initial assessment of the manuscript's unsuitability for publication. Given these tasks, there was a delay in returning to you with an editorial decision, for which I apologize.  
  
I have now received the report from the second reviewer, and have attached it at the end of this communication.  
  
As you will see, the second reviewer also finds significant problems with your manuscript and recommends against publication.  
  
In view of each of the reviewers' assessment of your manuscript, we will not be able to accept this manuscript for publication in The Astrophysical Journal. I hope that you find the comments of both reviewers helpful.  
  
Sincerely,  
  
Dr. Manolis K. Georgoulis  
AAS Scientific Editor  
----------------------------------------------------------------------  
  
Reviewer:  
  
Referee report  
  
This paper studies wave propagation from the photosphere to the corona of the Sun. For that, it uses numerical simulations in idealized setup and compares the results with observational data. Gaining understanding of the wave propagation in the coupled photosphere-chromosphere-TR-corona system is much needed and it is an active topic in the field of solar physics. However, the paper presents several flaws that make me unable to recommend it for publication. The results lack novelty, the analysis of the simulations and observations is very superficial, the description of the methods is imprecise, making it impossible to evaluate the validity. The simulation setup is not valid for the study. The conclusions derived based on these simulations contain demonstrable mistakes. Less important, the paper lacks revision of appropriate literature on the wave propagation in the magnetized photosphere and chromosphere of the Sun. This literature is rather wast. Several of the recent papers have presented a much better executed models and their detailed analysis, compared to the present work. Finally, the paper is written without caution, there are phrases without an end, language mistakes, typos, figure captions that simply cannot be understood, etc. Unfortunately, I do not see any ways of improving the paper and cannot recommend it for publication.  
  
More specific comments:  
==================  
  
> Abstract: "We conclude that magnetic regions of the solar atmosphere are favourable regions for the propagation of energy by slow magnetosonic modes. The results exhibit a frequency shift, for different values of the magnetic  field."  
  
Articulated this way, this conclusion is not new and is unspecific. The paper fails explaining the physical mechanism behind these frequency shifts.  
  
====  
  
> Introduction, red paragraph starting from line 50. This paragraph discusses mostly coronal literature, and it is rather vague regarding the numerous works on the numerical modeling of waves in the chromosphere and photosphere in different kinds of magnetic structures. From there, I believe, there is the lack of novelty and focus of the present paper. The setup used is very simplistic and it has been considered in other studies. For example, including gravity in the wave simulations, or considering reflections at the transition region, is by far not new, there are works published in the 50-60th, plenty of them in 90th, where this has been already done.  
  
====  
  
> Section 2, first two paragraphs and Figure 1 are too generic and unspecific to appear in a research article. The rest probably belongs to the introduction.  
  
====  
  
> Section 3, around line 160 "Computational MHD simulations of the propagation of waves in 3D solar atmospheres were undertaken by Fedun et al. (2009a)." Apart from Fedun et al 2009, there has been multiple other groups who extensively worked on similar kinds of simulations. At least there have been several recent publications in the last 5 years who studied the wave propagation from the photosphere to the corona in 3D including the cases of flux tube structures, and homogeneous magnetic fields.  
  
====  
  
Section 3, line 168. "In these simulations atmospheric perturbations caused by photospheric global oscillations are represented using drivers located in the photosphere so as to mimic the influence of the solar p-modes."  
  
Solar p-modes are excited below the photosphere. They are already evanescent or nearly evanescent in the photosphere. If a realistic driver to be used, it must be located in the sub-photospheric levels, according to the theory of p-modes excitation. Otherwise, it cannot be claimed that the excitation is produced by p-modes.  
  
====  
  
Section 4. The particular implementation of the open boundary condition is not specified. How well does it allow preventing reflections? If the low boundary is given by the driver from Equation 8, this would represent a node at the bottom of the simulation domain. The waves will reflect from this node. This unphysical reflection would affect the cavity formed by these waves (discussed later in Section 6).  
  
====  
  
> Section 5. The initial magnetic field is shown in Figure 5 and the height distribution of the thermodynamic parameters is shown in Figure 3, but no equations for the equilibrium are provided. The authors cite Griffiths et al 2018 for more details, but unfortunately this paper has even less details than the present one.  
  
The code used in this paper needs MHS equilibrium to be fulfilled, because it is explicitly removed from the equations. If the initial model atmosphere is not in MHS equilibrium, this will create unbalanced forces in the equations for non-linear perturbations. These forces will create waves with unphysical characteristics. However, since the code works in perturbations, the simulation would, of course, not fail, but simply produce unphysical results.  
  
As far as I understand from the current presentation, there is indeed an equilibrium in the vertical direction, but the horizontal magnetic field distribution is not balanced by the the corresponding gas pressure distribution. It is impossible to understand from the current presentation if this is indeed the case. If it is, then there is not sense to read the paper further, the results are simply not valid because the model is not in MHS equilibrium.  
  
Less important, the values of the magnetic field strength used, up to 100 G are far too low for an active region in the photosphere and the chromosphere.  
  
====  
  
> Section 6, first paragraph. Why buffeting motions are discussed here if later the vertical driver is used? Please also refrain from calling the model "realistic".  
  
====  
  
> The vertical velocity driver in the vertical magnetic field can only excite acoustic modes (slow or fast, depending on plasma beta), which propagation speed is entirely independent from the magnetic field. I do not understand how other modes are excited here, except that this is the result of the wrong MHS equilibrium and unbalanced magnetic forces mentioned in the comment above. Then, studying these waves makes no sense to me, because their excitation is a consequence of the erroneous model.  
  
Less important, why standing and not propagating wave driver is used in this work? How are the other quantities driven, are they driven consistently with this vertical velocity driver presented by Equation 8?  
  
How important are the non-linear effects for waves in this work?  
  
====  
  
> it does not seem meaningful dividing the wave fields from Figures 5 and 4 between each other to get Figure 6. The velocity oscillations show positive and negative values and pass by zero, which provides unphysical large values in Figure 6. I do not see a point in doing such an analysis.  
  
Instead, it would make much more sense to me to project the velocity field into the characteristic wave propagation directions to analyze which modes are excited in the system. Somehow, this simple and very informative analysis is not done.  
  
====  
  
> Section 7 around line 254. Three magnetic fields are mentioned: 50, 75 and 100 G. For which value of the magnetic field was the plasma beta evaluated?  
  
====  
  
> Line 256. "It is anticipated that for the region with beta=1, mode conversion occurs with full or partial conversion to magnetohydrodynamic modes."  
  
Vertically propagating acoustic waves in the vertical magnetic field cannot have mode conversion.  
  
====  
  
> Line 269. It is misleading mentioning the frequency shifts obtained by Hindman et al. (1996) because the current work does not have enough frequency resolution to detect such shifts. In addition, the physical mechanisms behind these shifts is not explained in the paper.  
  
====  
  
> Line 279. "The speeds for the 0G field are consistent with the speed of sound in the solar atmosphere, whilst the speeds for the non zero magnetic field are consistent with propagation speeds for magnetosonic modes"  
  
How do the authors define the speeds? The definition must include the knowledge of the wave propagation direction.  
  
====  
  
> Why figures 7 only show a portion of the domain, and not the whole of it extension in height? Showing the whole doming would help understanding if the reflections are produced by the transition region, and not by imperfect upper open boundary condition. The authors discuss wave leakage to corona, but without showing the coronal part of the domain.  
  
====  
  
> Please revise the captions of all the figures, most of them are impossible to understand: unfinished sentences, no details, etc.  
  
====  
  
> Section 9. How long is the time series used in this work? Have the simulations achieve the stationary regime?  
Have the authors taken the part fo the time series in the stationary regime for the Fourier analysis? From what is shown in Figures like 9 and 10, this does not seem to be the case. Therefore, I do not understand how any conclusions about possible frequency shifts can be made from a time series of about 600 duration. This time series fits 2 wave periods, and part of the time is not in the stationary regime.  
  
The frequency shifts may simply arise from the fact that waves, excited in these simulations, reach different heights at different times, since their propagation speeds depend on the magnetic field. Together with the very short time series used, the data simply have not enough information in the frequency domain. There is no reason for such shifts in a simulation with a harmonic driver and no apparent non-linear effects. The physical reason for the shifts is articulated either.  
  
====  
  
> The observational part shown in Section 9 is misleading because the analysis corresponds to a region with much larger field strength than analyzed in the numerical part of the paper. The analyzed oscillations are brightness oscillations in AYA 1600 passband, while Bz and Vz oscillations are analyzed in the numerical part. There is not discussion of computations showing the link between oscillations of these quantities. Oscillations at a single height are analyzed in observations, finding a period around 4 minutes. However, there is no information from the photospheric level about the frequency of their driver, or how do the waves propagate from one layer to another. There is simply no sense to discuss this further, because of the wast amount of observation/simulation comparison analysis that has been done in the past, with much more rigourosity and details. This analysis presented here is weak and brings no new information.  
  
Finally, the role of the cylindrical flux tube on waves has not been discussed at all in the paper.  
  
=====