Manuscript Number: ASR-D-17-00488  
Section: SI: Waves in Solar Atmosphere  
Article Title: Solar Atmosphere Wave Dynamics Generated by Solar Global Oscillating Eigenmodes  
Advances in Space Research  
  
Dear Dr. Griffiths,  
  
Please confirm receipt of this email by replying to me at [sssrc@msn.com](mailto:sssrc@msn.com)  
  
The reviewers have commented on your above paper. They indicated that it is not acceptable for publication in its present form.  
  
If you feel that you can suitably address the reviewers' comments (included below), I invite you to revise and resubmit your manuscript.  
  
When submitting a revised manuscript, please carefully address the issues raised in the reviewers' comments.  It is essential that you provide a list of changes or a rebuttal against each point which has been raised.  
  
If you feel you cannot satisfactorily revise this manuscript, please withdraw the paper.  
  
The revision due date is Aug 03, 2017.  
  
If you require additional time, please contact me with regard to a possible extension  
  
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I look forward to your response.  
  
Yours sincerely,  
  
Peggy Ann Shea  
Past Editor in Chief  
Advances in Space Research  
  
Reviewers' comments:  
  
  
  
  
  
Reviewer #1: The authors presented a detailed numerical investigation into the response of the lower solar atmosphere to the global p-modes, with the aim being to link p-modes to a variety of oscillatory behavior at different atmospheric levels. This work improves previous studies in that: a realistic equilibrium atmosphere is constructed by combining the VAL and McWhirter models; p-modes are modeled as coherent perturbations to the vertical velocity. The authors examined the consequences of implementing fundamental (0,0) modes with different driving frequencies as well as perturbations with other mode structures as labeled by the combination of mode numbers in the horizontal directions (n, m). The authors interpreted their results in the framework of a Klein-Gordon equation, and linked their results to such recent observations as in Ireland et al. (2015).  
  
To this referee, this comprehensive study itself is pretty interesting, but the presentation needs to be improved substantially for the manuscript to merit publication. The structuring is OK, but the wording is sometimes rather confusing to me.  
  
1)This is a 3D hydrodynamic simulation with the magnetic-free quiet Sun in mind, right? If so, then I suggest that the authors separate Section 2 into two parts.  
  
The first can be a self-contained description of SMAUG itself.  
  
The second part can be used to describe what the authors actually implement and can start with something like "With the magnetic-field-free quiet Sun in mind, we set $\vec{B}=0$ in the MHD equations." A description of the computation box (lengths in the x-, y-, z- directions, setup of the numerical grids) needs to be given here. The boundary conditions, i.e., the part "With the upper boundary of our mode … We used open boundary conditions … induced oscillations" currently on page 7 can also be moved here. Please clearly state that gravitational stratification takes place in the $z$-direction (am I right?). BTW, what are the boundary conditions that you use in the horizontal directions?  
  
2)Section 4.  
Eq.(8) gives the form of the perturbations you actually use, right? If so, then I suggest that you  remove Eq. (7) and the relevant descriptions. After Eq. (8), please offer a description for $T\_s$, $n$, $m$, $L\_x$, $L\_z$, $z\_0$ and $\Delta z$.  
  
Please double check Eq. (9). The sine term should read "$\sin^2\left(..\right)$", and the exponential term should read "\exp\left(-2 (z-z\_0)^2/\Delta z^2)".  
  
3)Section 5.  
The description of different drivers is rather confusing, with Tables 2 to 4 particularly so. What does each column represent in these tables? How are the values calculated? How to relate the computed periods to the driving frequency as given in Eq.(8)?  
  
In Table 1, what does set (b) represent? As I understand, the sound speeds are already fully determined by the equilibrium temperature profile. What parameters are you varying in set (b)?  
  
In my opinion, the part starting with "To determine how wave energy propagation is influenced by  …" (page 11) can be moved to Section 6, because they are relevant for analyzing the computational results. In Eq.(11) and the two equations immediately following it, the magnetic field $\vec{B}$ can be removed if the simulations are hydrodynamic ones.  
  
Tables 5 to 10. What does "energy ratio" mean? The time-averaged energy flux density at $z=5.5$ Mm to that at the lower boundary?  
  
4)Section 6.  
Page 17. Table 11 and relevant parts in the manuscript. How did you compute the values for $a$, $b$ and $c$ in Eq.(12)? Can you show a plot for the power spectrum for the oscillations in the lower corona and show how this spectrum compares with Eq.(12) with your best-fit parameters?  
  
5)Captions to Figs. 3 to 13. Something like "Temporal evolution of the $x$- or $y$- or $z$-distribution of $v\_z$. The profiles are taken where $y=$ and $z=$" reads better.  
  
6)Please go through the manuscript to remove the typos and grammatical errors, e.g.,  
Page 4, last line, "QS quiet sun"  
Page 6, line 37. "Kalkofen et al. considered … they employed …"  
  
  
  
  
  
Reviewer #2: The paper studies the upward propagation of waves driven by the p-modes. The atmosphere is assumed to be field free although realistic temperature and density structures are used for the background equilibrium. Ratios of the energy flux are calculated and show a decrease with increasing height consistent with the predictions for a stratified atmosphere with a cut-off period. The results are also compared with observational estimates.  
  
I believe the manuscript requires improvement before it can be accepted for publication and I would ask the authors to address the following points:  
  
Page 17, Table 11: How many pairs of (P, T) were considered to construct the power law and to obtain the fitted parameters shown in Table 11? The text before equation 12 suggests that only the 300s and 180s periods were used. If that is the case then the parameter values a, b, and c could be calculated more accurately by considering more pairs of (P, T) especially since the value of the fitted parameter a is so small that I wonder if the power law (12) is meaningful at all for the simulated results?  
  
Page 16, line 36: Can you explain why the intensity (amplitude) peaks at 180s? On the same page (lines 39-42), it is mentioned that there is strong signal attenuation for the 30s driver. It has been shown (for example Carlsson & Stein 2002) that acoustic waves can be radiatively damped in the chromosphere but I believe there is no radiation in your model so the damping needs to be explained. The short period waves are unlikely to be evanescent in the atmosphere and may even turn into shocks as they propagate upward but I don't see any evidence of that.  
  
Table 5 and Figure 14 show energy ratios > 1, so that there is more flux in the atmosphere than energy generated at the driver location. Where does the extra energy come from?  
  
Minor:  
  
The colorbars do not show the units and the charsize in the figures is too small. Also 'z' or 'x' instead of 'Distance' would be more useful.  
  
The text is often hard to understand. For example, on page 17: "The results plotted  
in Figure (14) indicate the ratio of the energy flux for models delivering the same quantity of energy to the energy flux for models where the driver amplitude is kept fixed." Please try to make the text more readable.  
  
Page 9, line 48: "Since we are investigating the leakage of energy into the solar atmosphere,  
for consistency, it is necessary to ensure that for the different modes the driver amplitude is set to a value which provides the same total amount of energy over the model cross section and per unit time." The observed p-mode power spectrum peaks for a certain range of frequencies and angular degrees. The authors should address the issue in a couple of sentences if they are trying to reconstruct the observed power spectrum in the corona.  
  
Page 5, line 40: 'frequencies greater than the acoustic cut-off 190 s' ◊ periods shorter than the acoustic cut-off 190 s;  
  
44-49: Why is there more wave energy flux above granules if more waves are detected above intergranules?  
  
Page 6, line 22: 'results are discussed in detail' is unnecessary or should be rephrased.  
  
End of Page 7: I presume v=\tilde{v}? Please clarify.  
  
Page 9: Please define L\_x and L\_y after equation 8.  
  
Page 10, section 5, first paragraph: Second sentence almost repeats the first one.  
  
Line 54: 'To use this relation …' This has already been said at the end of the previous section.  
  
Page 11: It is worth adding 'The corresponding frequencies are' before the expression on line 39.  
  
Page 14, line 49: I am not sure I can see the cut-off period for the isothermal atmosphere. Isn't it constant anyway?  
  
Page 15, lines 49-53: Page 16, lines 37-42, and elsewhere: Please indicate the corresponding figures.  
  
Page 16, line 15: I couldn't see a 30s or a 180s driver in figure 7.  
  
Page 16, paragraph 43-52: I couldn't find the 30s driver case in Figures 8-13.  
  
Page 18, lines 50-52: I disagree that the plasma beta is very large in the low corona.  
  
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Comments from the ASR Editor for Special Issues  
  
A few of the older references do not have inclusive pages listed.  Books need the name of the publisher and city of the publisher.  Articles in books also require inclusive page numbers.  The Lamb reference is incomplete.  These items can all be corrected in a revision.  
  
  
  
  
  
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For further assistance, please visit our customer support site at <http://help.elsevier.com/app/answers/list/p/7923>. Here you can search for solutions on a range of topics, find answers to frequently asked questions and learn more about EES via interactive tutorials. You will also find our 24/7 support contact details should you need any further assistance from one of our customer support representatives.