[detailed] Review of "Response of OH airglow emissions..." by Ghodpage, Hickey, Taori, Siingh, and Patil.

The paper presents the analysis of wave signatures present in OH airglow at Kolhapur in India, namely airglow brightness and (rotational) temperature, from a total of 105 nights of observation, in 2010 and 2011. The resulting values of Krassovsky's ratio scatter widely, but not considerably more than 10 previous literature reports, since the earliest analysis from Svalbard published 27 years ago until results from Hawaii published in 2008. Another comparison is done with respect to model results for long horizontal wavelengths published in the early 90s. However, since all this is shown in the same figure (but different figures for the different parameters), it is very hard to distinguish the symbols and follow the description in the text.

The paper also reports on wave model simulations for the atmospheric background conditions corresponding to the observation site and time of year. These results based on the Hickey et al. model are expected to be comparable to the parameters observed (modulus and phase of Krassovsky's ratio), but it turns out to be successful only for a certain range of small model phase velocities. While the discussion in the text is well done, the corresponding figure suffers from the very wide scale chosen to accommodate some of the model results for short-period waves, and so is not as instructive and easy to read as possible.

No details are given about how rotational temperatures are determined from the two spectral samples available from the airglow photometer, nor any reference to other papers where this may have been described. Neither is there any mention about whether the intensities refer to the whole emission band (as required for meaningful and unbiased values of Krassovsky's ratio).

There is a problem with figure 1 that I hope is only a scaling error. If the relative intensity and temperature amplitudes in Fig 1 are really plotted at the same scale (as the figure makes us believe), then they look too similar to explain eta values much different from the order of one. And indeed, the peak-to-peak distance of 12 mm I measure in Fig. 1a and 8.5 mm in 1b correspond to an eta of 1.4, but not 7 as the text claims, for the principal wave! For the residual wave, the situation does not look better, but it's harder to quantify from the -0.5 to +0.5 scale. The numerical result for the residual wave given in the text (3.7, see details on Line 165 (L165)) is however wrong (it is not even dimensionless but in relative intensity/kelvin).

Figure 1 is also not a convincing example of the quality of the phase information (especially for the secondary wave) that can normally be obtained, and therefore casts doubt on the phases of eta obtained from the observations.

For these reasons (some details are mentioned in the list below), I cannot recommend publication in the present form and think that a major revision (except for the excellent section 4) is needed.

Details (mainly minor technical points but also some explanations of more serious stuff:

L30: I do not understand what is gained by the words "In the present report", since there is no other topic in the abstract with which it could be confused.

L35: delete "the" before "propagating..." because its generic (any gws!).

L37: "ambient" seems to stand for "mean flow", but that's not obvious for the general reader.

L40: Krassovsky (1972) did not include phase in his definition of eta. Better, replace "can be defined as" by "is now defined as".

L42: change "a phase" to "the phase difference".

L68: Hickey et al. 1998 ref is missing. Do you mean Hickey, M.P., Taylor, M.J., Gardner, C.S., and Gibbons, C.R. (1998), Full-wave modeling of small-scale gravity waves using Airborne Lidar and Observations of the Hawaiian Airglow (ALOHA-93) O(1S) images and coincident Na wind/ temperature lidar measurements, J. Geophys. Res. 103, 6439-6453. -?

L74: better, simplify -> "are made with the multispectral..."

L75: I suggest change to read "We analyze the data from... to...".

L76: delete "the availability of" to read "when clear sky conditions prevailed...".

L77: "In particular" sounds as if only details follow, rather than new relevant info about the number of wave signatures found in 2010 and 2011; so, better start with "For 2010, 14 nights out of 45 nights of observation clearly showed....., while in 2011, 30 from 60 nights of data showed wavelike...".

L83: correspondence between wavelengths and emissions is a little confuse; the essential information -the two wavelenghts at which the OH(8-3) band is sampled- is mixed up with the list of red and green atomic oxygen lines (which are not used in this paper).
Better reorganize this sentence!

L91, 92: "This output... processing" is not very informative. The message is simply that the corresponding time series are stored for further processing. And the other reviewer is right when saying that details about the determination of rotational temperatures should be given. The two wavelengths alone, without information on bandwidth and therefore, the rotational band components included in each of the two spectral samples, are insufficient to derive temperatures. In principle, also the spectral background intensity unaffected by the OH emission would be needed for good rotational temperatures (although I think this would be difficult, in the spectral vicinity of the 8-3 band)...

L128, 129: Instrument and satellite names should be capitalized to match acronyms.

L130: "orbital inclination" needs be added to "at 74°"; add missing word "atmosphere"

L135; not clear what is gained by mentioning 2010 and 2011 again; what is meant by "to identify this"?

L136, 137: change to read "(obtained from SABER)" and delete the rest of the sentence (redundant).

L138: replace "representing" by "to represent", because the selected grids do not automatically represent Kolhapur (but are meant to). However, I wonder why the longitude interval is so much greater than the latitude interval, being so relatively close to the equator (where COS(17°)=0.956=~1).

L139-140: the two miss-time criteria just boil down to "night time (excluding twilight)" and do not require this two-item list.

L146, 147: delete "with connecting lines", since that's not informative; only circles can be clearly seen in figure 1.

L149, 150: "mean airglow intensity", "mean temperature" is clear enough (delete "of", "of the").

L153: an even better fit could be obtained if a constant term were included in equation (1).

L150-154: too much information is given simultaneously in this long sentence, just to explain the red lines. Better, start simply: "Also, best-fit cosines are shown (red lines)." And then give details. The reason ("to identify...") is obvious.

L155: then, this sentence ("Note that ... model") should be deleted.

L157: see below (L161-162)!

L158: the argument is that the temperature and intensity oscillations correspond to the same physical wave, in spite of the small nominal difference in period (within combined errors).

L160: delete "(Figure 1c)" after "best-fit model values", because the next sentence is explicit enough. Too explicit, in fact, since the information on the position of the panels ("bottom-left", etc.) is redundant.

L161-162, L165-166: but the correct order (Fig 1c, 1d) is intensity, temperature, respectively.

L164: while the figures 1a-d show percentage amplitudes, the values in L165 are absolute amplitudes.

Most importantly: the relative intensity and temperature amplitudes in Fig 1 look too similar to explain eta values of 7 (principal wave) and 3.7 (residual wave; but see the more serious error, below "L165-166"!).

From the visual impression of figure 1, the principal wave has intensity and temperature nearly in phase, but for the secondary oscillation, visual inspection does not lead to a clear conclusion, because of the ambiguity of relating 3 (nearly 4!) intensity maxima to 2 temperature maxima.

L165-166: the temperature amplitude of 4.1K and intensity amplitude of 15.1 units are insufficient information to arrive at the eta of 3.7 (but 15.1/4.1=3.68 relative units/kelvin; hmmm). I hope that this is not how all the eta values have been determined, because they would be all wrong!

L173: formula (2) is not from Hines' Fundamental Theorem paper of 1997, but can be deduced from
eq 57 and 58 in Tarasick & Hines 1990 (not cited; that is: Tarasick, D.W., and Hines, C.O. (1990), The observable effects of gravity waves on airglow emissions, Planet. Space Sci. 38, 1105-1119); your formula (2) in the form how you cite it (and others have), may have first been given in Reisin & Scheer 1996 (which you cite). This is not an important point in itself. A simpler version (using a numerical factor 22 instead of 2 pi and the gamma terms) can also be obtained from eq 37 of Hines & Tarasick 1987 (which you cite). Note that the sign conventions of H&T87 and T&H90 are opposite to what Reisin & Scheer 1996 and Reisin & Scheer 2001 (not cited, but relevant to the context of your paper: Reisin, E.R., and Scheer, J. (2001), Vertical propagation of gravity waves determined from zenith observations of airglow, Adv. Space Res. 27(10), 1743-1748.) have used. With your formula (2), negative vertical wavelength corresponds to downward phase propagation (i.e., upward energy propagation, as your manuscript mentions only much later), and that means that temperature oscillations precede the intensity oscillations in phase (as, e.g., Takahashi, H., Sahai, Y., and Teixeira, N.R. (1990), Airglow intensity and temperature response to atmospheric wave propagation in the mesopause region, Adv. Space Res. 10, (10)77-(10)81) have shown mostly to be the case).

Since your paper compares with phase shifts from the literature, consistent phase conventions must be used. And since phase difference for waves with different period depends on time, corrections for frequency difference or time reference are needed, in general. Otherwise, considerable statistical errors can arise.

L176: While formula (2) has not been derived for evanescent waves, this does not automatically imply that it is not at least approximately valid for Phi=0, since that leads to infinite vertical wavelength (i.e., constant phase with height), which is not unreasonable. By the way, for phi so close to zero that sign may change, also the sign of VW changes (simply as statistical errors), which is why for large values of VW, sign is meaningless! This is why it does not make sense to choose very wide scale for plots of VW.

L178: importantly, missing "the" before "long period and short period waves", because only the two cases of figure 1 are referred to (and it is not a general statement).

L179: Note that the bias for long-period waves alluded to here could have been removed by simply including a constant term in the fit (eq 1).

L183, 184: "one may note that" and "in the data show" are unnecessary subjective aspects of an objective message ("During 2010... the principal wave components have periods between 5.2 and 10.8 h").

L185: minimum temperature amplitudes of 0.2 K? Such a small amplitude for a "principal wave" must be a chance exception meaning "no wave detected", and not a result to be taken seriously.

L190-198: these numbers are hard to digest without a figure to look at, but unfortunately, Figure 2 has too many different symbols to make the present results stand out clearly. I think, separate plots without the literature comparisons are needed (while including the black model curves would not hurt)!

With respect to the comparisons with other observations, I doubt that the period ranges are all correct (that would be easy to repair by a statement like "also some of the results from other investigations are shown"). Some of the symbols in 2a and 2b differ, making it harder to interpret the plots (symbol 16 for Viereck & Deehr eta, but symbol 7 for Viereck & Deehr phi).

Already from the present figure 2b it appears that Viereck & Deehr had many outliers (but if I remember correctly, several values were derived from the same spectral feature; note that their figure 7 with many strange phases is for O2!). Therefore, choosing such a wide phase angle scale to accomodate these "outliers" does not make sense. However, I can see no evidence for so many phase outliers in Viereck and Deehr's paper (which was, by the way, based on only three consecutive (24-h) days of observation), especially if one ignores periods below 1h; see their figure 4).

L199-205: I insist that comparisons should go to a separate Figure.

Strictly speaking, the different results do not "vary", but each one is constant. However, they do "range from... to", or "fall in the range between... and...". Also in other places, the text abuses "vary", when referring to a range of fixed values.

L215, 216: see my other comments about Viereck&Deehr's phases. I can see no such similarity with respect to the present results.

L225: It is not true that Reisin & Scheer 2004 is for periods of 3000 sec and eta = 5.6. That number (5.6) was derived from mean variances of temperature and intensity averaged over several years, but does not refer to waves of any specific period. However, that paper did state that periods between 1000s and 3h correspond to an eta of 3.47±0.07 (for OH) according to Reisin & Scheer 2001 (which I have mentioned above, and which would make more sense to be cited here).

L235: what is the reason to expect a latitudinal effect on the phases of eta? And, isn't Svalbard (Viereck&Deehr) even higher latitude than Sierra Nevada (Lopez-Gonzalez et al.)?

L237: some word must be missing in this sentence.

L239: Hines's Fundamental Theorem paper (1997; which you cite) uttered a different opinion.

L242: "Winds also affect" - citation needed!

L246: delete "the" before "upward"; also, citation needed about why this is thought to be so.

L263: The Offermann et al. (1981) paper has nothing to do with gravity waves and airglow and therefore must not be cited here. It only discussed the variability of measured atomic oxygen profiles.

L272: this information on propagation direction has already been given. Is this repetition warranted or just an oversight?

L273: typo in "Krassovsky"

L281, 282: Shouldn't the previous results by Ghodpage et al. based on data from other times or places be given more emphasis here?

L288: -> "Full Wave Model results" (?)

L289: what observations are you talking about? Isn't this section about model simulations?

L310: lost single word "show" before "The observed..."

L314: there is something incomplete in this sentence at "are 50-100 m/s". Maybe "at 50 and 100 m/s"?

L321: delete excess "that" (after "that").

L338: let me remark that the same is true around 0°...

L356: orphaned "for ."

L363: please, correct strange "eta"-like font variant.

L367: correct typo in "constituents"; and if subject is "composition", then "were" -> "was"

L369: missing "that" before arises" and missing final "s" in "arises".

my concluding remark to section 4: I wish the authors good luck to elevate the quality of the rest of the manuscript to the (language and argumentation) level of section 4!

L386, 387: this section and the list of 4 items well deserves being called "conclusions"; "concluding remarks" implies that the results obtained are themselves clear enough, so that there is hardly a need to add more text. At any rate, "Following are the concluding remarks" sounds too obvious to be worth the space.

L391: reformulate so that is is clear what is meant by "more magnitude of eta values". (Note that no error bars are given, so that quantitative comparisons are not necessarily conclusive).

L393, 394: do you mean, "On the other hand, the phase values are greater than..."?
Better, limit the use of "we note" to the minimum necessary and better stick to objective conclusions.

L395: according to my impression with the corresponding figure (2b; see your symbols #7), some of the Viereck and Deehr phases differ considerably from your results, and from all the rest. But as stated above, I doubt that V&D really report those phases for OH.

L408: better, delete after "under way", or reformulate to sound more reasonable.

refs: in general, maintain chronological order of references!

L440: missing space between "airglow" and subscript "2" in "O2"

L448: missing hyphen in "T.-Y.", delete Walterscheid initial "L." (paper only has "Richard").

L452: wrong numbers! correct is "148, 266-281, 2000".

L460, 461: remove title capitalization and hyphen in "wave driven".

L494: is first author's name "Pragati Sikha, R." (as mentioned in text) or Sikha Pragati, R. (as in the paper cited)? -it is not unusual that authors' names are spelled wrongly in published papers, so I ask.

L495: delete final "s" in "Current Science"; 98(3).

I did not check all the references, so that's up to the authors.

L552: labeling indicates that (a) and (c) is intensity, (b) and (d) temperature, opposite to what caption (and text; see above) says.

L555: info about meaning of the fits and "data ... Kolhapur" is redundant.

L559: "A close resemblance..." contradicts the visual impression of wide scatter in figure 2b (but maybe the V&D outliers are O2, not OH; see above).

L564, 571: typo in "Guharay".

Each of the sections of figures 2 and 3 contains too much information to be part of a single figure!

L576: Delete "Shown", because it's trivial that a figure shows something; meaning of the sentence is hard to understand. Measured etas are compared with model?

Figure 3a: model values for 100 and 150 m/s very nearly agree, so that the blue symbols are hardly visible. So, why are both ways plotted here?

Figure 3b: most striking feature is that many of the highest phase velocity model phases are unrealistic.

Figure 3c: symbols are here explained in a style that differs from all previous plots. Better, use consistent scheme everywhere. Space for positive vertical wavelengths (downward energy propagation) is kind of "wasted" for the 4 data points that occupy this region.

L586, 587: "are shown in plot" is no information. Where do these emission rate profiles come from? -> "solid lines: 2010, dashed lines: 2011".
