Reviewer 1, John Harlander

We thank this reviewer for their careful review of this paper. Our response (in italics) follows each of their comments.

Main issues

1) In section 6 the authors attempt to compare the precision of ERWIN II with other ground-based wind measuring instruments. They correctly state that such comparisons are not straightforward. Comparing instruments with different etendues, measuring different source brightnesses, designed for potentially different objectives, utilizing different observation geometries, and at different stages of development is problematic at best. Although the authors don't state this, my concern is that this section may be misinterpreted as implying that the stepped Michelson technique employed by ERWIN II is superior to the others in their comparison. At a minimum, a fairer comparison would start with source brightness and the etendues of each of the optical measurements which determines how many photons are ultimately detected by each, which something not addressed in the paper. Even that would not address some subtle aspects of a general comparison. For example, when measuring time varying sources or observation geometries, simultaneous rather than scanned techniques are likely to result in smaller systematic errors. Although it is tempting to try to determine the "best technique" to measure atmospheric winds, each measurement problem presents its own subtleties which will determine which measurement technique is best fit to the problem.

There are two ways in which wind measuring optical instruments can be compared. On the one hand, as pointed out by this reviewer, the capabilities can be evaluated theoretically based on consideration of etendue and source brightness. This provides an idea of what is achievable by an instrument technique when all practical issues have been solved. On the other hand, comparison of the capabilities of actual working instruments as published in the literature provides a means of evaluating the degree to which the practical issues have been solved. We have taken the latter course and as stated in the paper consider this to be a "pragmatic" way of comparing instruments.

Our statement that comparisons between instruments are not straightforward was meant in two senses. On one hand because of the varying capabilities of different instruments for different circumstances a comparison is not easy. The other sense that we meant this was that there is not much discussion in the literature of the precision and accuracy of optical wind measurements. While we are not claiming that ERWIN II is in principle the best technique for optical wind measurements we do think that in practice these are the best optical wind measurements using airglow to date. We also hope that our paper stimulates other authors to provide similar information on their observation techniques.

To clarify our intended meaning, we have modified the sentence starting the paragraph on line 28 of page 8293 from "This comparison indicates that ERWIN II is a significantly superior wind measuring instrument than any other ground based airglow wind instrument." to "This comparison indicates that the precision and measurement cadence achieved by ERWIN II is significantly superior to any others reported in the literature." We have also changed the sentence on lines 17-19 on page 8292 from "Few papers have discussed the precision and accuracy of a technique in as much detail and as clearly as has been undertaken in this paper." to "Although theoretical comparisons based on throughput *considerations have been undertaken (see Shepherd, 2003) few papers have discussed the precision and accuracy of a technique in practice in as much detail and as clearly as has been undertaken in this paper." to emphasize the distinction between the theoretical capabilities different techniques have and the pragmatic comparison we are making here. We have also included the reference to Shepherd's monograph where a number of these comparisons are made.*

2) Equations 4 and 14 indicate that the theoretical performance of the wind precision requires knowledge of the source brightness in photons, I0 (equation 4), or I (equation 14). Although figure 7 implies that these values are known they are not indicated anywhere in the paper. A figure showing representative brightness values for the three emissions would be useful.

In our analysis of the performance of the instrument we were using the number of photons detected at the detector per bin per integration time at each step since the observations statistics are directly related to this quantity (as well as the visibility as per equation 4). In particular the closeness of the theoretical and observed standard deviations indicates that intensity and visibility variations during individual measurements were minimal (otherwise the variances would not match).We have added the word "individual" in line 22 , pg 8289 to emphasize this.

In comparing with other wind instruments, we were assuming that the nightglow volume emission rate is generally similar throughout the globe so that we are comparing similar source radiances. We think this is generally true. At the same time we realize that we have not said much about the irradiance measurements which are made simultaneously with the wind measurements (they arise naturally from the algorithm). The radiance response of ERWIN II is 1.7672 R/count/s for green line, 2.0682 R/count/s for O_2 *and 1.0627 R/count/s for OH. We have added a sentence providing this information to the end of the paragraph at line 16 of page 8279.*

To explain the relationship of the standard error to the signal and visibility we have included two more panels in Figure 7. These show the variation of these quantities during the day and clearly show how the standard error decreases when the signal increases. We have also changed the text describing this figure and the caption.

Minor Issues

1) The term irradiance is used for source brightness throughout the paper (e.g. pg 8274 equation 1). From the context provided by equations 4 and 14, this should be "total number of photons detected" rather than irradiance which is an SI unit measured in W/m2.

We have gone through the paper and do not think that irradiance (power detected pre unit area) is inappropriate in general. We do agree however that our use of this term was not particularly rigorous. We have gone through the paper and where we are referring to the airglow emission we now make sure we use volume emission rate, where the general theory of the interference fringes is discussed we use irradiance (since the detector measures power per unit area) and where the observations and the associated uncertainties are discussed we use number of photons detected per integration time per step.

2) One of the roles intended for the calibration lamps was to determine the background phase for each of the emissions. The paper suggests that these data were not sufficient so data from a cloudy night was used for this measurement. It would be useful if the paper discussed potential reasons for the failure of the calibration lamps to provide a suitable background phase (e.g. non-uniform filling?, difference in wavelength? something else?).

This issue was discovered in the field once the data analysis algorithm was suitably developed. As a result a definitive identification of the cause of this difference remains to be determined. However, we suspect that this effect is caused by differences in the optics of the main system and calibration system which result in the irradiance distributions across the aperture of each system being different. This would cause variations in the weighting of any residual path variations in the interferometer between the two arms. It appears that these variations start becoming significant once the instrument precision is pushed to the order of a few meters/sec. We have added the following sentence stating this hypothesis in the paragraph on the background phase. "This effect is thought to be due to differences between the calibration optics and main optics which result in the light distribution across the aperture of each system being different and resulting in a different weighting of any residual path variations in the interferometer." (Line 6 page 8283)

3) Using differences between opposite cardinal point to determine horizontal and vertical winds assumes a non-divergent wind field (see equations 5, 6, 7, 8 and 10). This should be explicitly stated. Also is such an assumption justified?

We thank the reviewer for picking up this lack of precision in our description of aspects of the analysis of the ERWIN II observations. We are aware that gravity waves will cause divergences in the wind fields and in fact this is one of the ways that we are determining gravity wave effects in ERWIN II wind observations (publication in preparation). In fact on line 11-14 on page 8281 (the page on which these equation occur) we note that gravity waves will affect our comparisons.

To clarify things further we have modified the text by changing the sentence (pg8281 lines 4-5) from "The first is to simply use the zenith line-of-sight winds" to "The most precise and accurate is to use the zenith line-of-sight winds", to change lines7-8 on the same page from "Less directly, the vertical winds can be determined using the cardinal direction line-of sight winds," to "The vertical winds can be also be determined indirectly using the cardinal direction line-of-sight winds," and changing lines 10-14 to

"In theory, this provides the means to check the internal consistency of the wind determinations but only for longer temporal scales. In practice, the effects of gravity waves of scales of the same order as the distance between the viewing points (~250 km) in the airglow layer will result in oppositely directed fields of view measuring winds that differ as a result of the aliasing of wind variations associated with these waves. This means that these comparisons can only be undertaken for longer term averages for which the spatial scales are significantly larger than 250 km. At the same time these shorter term differences between opposing fields of view allow observations of gravity wave effects to be undertaken given confidence in the instrument calibration. This will be described in detail in a forthcoming paper.

4) Both degrees and radians are used to indicate instrumental phases and drifts (compare figures 2, 5 and 6). Comparing these would be easier for the reader if either radians or degrees, not both were used throughout.

We have converted to degrees.

5) Page 8288 line 6, “Figure 6 gives” should be “Figure 5 gives”.

Thanks for noticing this error. We have changed it.

6) Grammar issues: Page 8289 line 22 “is” should be “are” and “it was” should be “they were”.

Corrected – thanks.