Review # 1

Author comment: Thank you for your comments and suggestions. Our responses are shown highlighted in blue below.

The following is a review of the manuscript by Yan et al., with the title: "A compact static birefringent interferometer for the measurement of upper atmospheric winds: concept, design and lab performance." The manuscript presents the instrument concept and the design and optimization of a prototype version of the instrument. Characterization of the lab prototype is also presented in addition to the performance of the instrument using a low velocity, laboratory generated, two-dimensional Doppler shifted signal field.

This manuscript documents the theoretical and numerical model calculations, as well as laboratory measurements, confirming nearly shot noise limited performance of a carefully built laboratory instrument. The instrument concept is not new, but the specific measurements, including the imaging observations are. My main concern with this manuscript is that, especially since this is not a new concept, which would warrant a publication, it is not made clear what the advantages of this approach are, which presumably motivate this work. The authors say that "The arrangement provides a similar throughput to that of a field widened Michelson interferometer, albeit constructed without moving parts. Consequently, the instrument provides a compact, lightweight and robust alternative." However, there are several other Michelson-type interferometers that have been used, are in use, or are in the literature, which are also constructed without moving parts and are compact, lightweight and robust. In addition, this concept, according to the authors, might, in practice, not be able to be constructed with the optimal path difference for the atmospheric application (i.e. emission line width). Given the above, it is my opinion that this paper (a) should be motivated much more clearly, i.e. state quantitatively what advantages over the state-of-the-art (Michelson, Fabry-Perot, DASH,...) are expected from this approach, and (b) should be using the results of this work to provide evidence that these advantages can be achieved. This would make it a well-rounded and valuable contribution to the literature.

Author response: You make a valid assessment that the implementation described in the manuscript is an adaptation of a technique that was previously applied to measure high speed (many hundreds of m/s) Doppler winds in plasmas (Hunten et al., 2010). However, the optimization for low speed Doppler wind measurements using airglow emissions is definitely a new (and unpublished) concept. The instrument design described in this paper, especially the birefringent delay plate, has been optimized for the measurements of LOS Doppler winds using isolated airglow emissions. While the optical path difference of the birefringent interferometer cannot be designed with the optimal optical path difference (at the minima in Figure 1), we can realize a version of the instrument with an optical path difference near the minimum (~1 cm). Moving away from the minimum is compensated by constructing an instrument with a large throughput. The resulting interferometer (a field widened delay plate constructed from Lithium Niobate) has a larger path difference and much larger throughput than the instrument described in [Hunten et al., 2010]. Accordingly, many aspects of the optical design have been optimized to accommodate this throughput. Numerous examples of such optimization for specific applications is found throughout the literature in the development life cycle of state of the art techniques, including various versions the field widened Michelson [Langille et al. ,2013, Kristofferson et al., 2017] and SHS [Harlander, 1991, Englert et al., 2020, Kauffman et al., 2020, Langille et al., 2019].

The primary advantage of this technique over state-of-art field widened Michelson, Fabry-Perfot and the DASH instruments is the size, volume and minimal complexity in the construction of the interferometer component. The delay plate component is roughly 10 cm x 5 cm x 5 cm and can be assembled using tools available in most optical labs. The same cannot be said for the state-of-the-art where assembly and construction requires extreme skill and has only be mastered by a handful of industrial players. In fact, industrial involvement in the development of this instrument was primarily motivated by the simplicity of the construction and the ability to by-pass outsourcing the interferometer construction and assembly. We agree that this aspect can be motivated more clearly in the introduction and we have included a paragraph accordingly.

Accepting the simplicity of the interferometer construction as the primary motivation and advantage over the state-of-the art, the goal is to then provide evidence that such an instrument has similar capabilities to the state of the art. Here, we suggest that the quantitative comparisons presented in the Discussion between existing field-widened Michelson interferometers and the BIDWIN instrument provides adequate evidence. More broadly, with the paper more clearly motivated in the introduction, we argue that the material presented in the paper provides a valuable contribution to the literature.

Some specific comments are:

(1) Table 1: please provide references for the projects so the readers can follow-up. Especially for the proposals and studies, which are traditionally hard to find (or leave those out). In addition, there are many other ground based Fabry-Perot wind interferometers, please at least mention that fact.

Author response: Table 1 has been updated with an additional column that includes the specific references.

(2) Table 2: the tangent heights for O1D are missing.

Author response: The tangent height (250 km) has now been included

(3) Line 91: the authors state: “The usual way to send light through an interferometer is to place a telescope in front that defines the field of view and passes a well-defined beam into the interference optics, with an image of the entrance aperture half-way through the interferometer.” It is not clear to me that the statement about the location of the image position is generally true. In addition, this statement does not contribute to the overall message of the paper, so I recommend omitting it.

Author response: This line has been omitted in the revised version

(4) The authors state at several places that knowledge of thermal instrument drift is essential. Please specify if there is a plan to measure the thermal drift simultaneously with the atmospheric measurement (and if so, how), or if it has to be done sequentially.

Author response: We have two options. First, we can, as you suggest, periodically observe a calibration source emitting a spectral line close to that of the target emission. In this case, the requirements on the thermal stability are high. The second option, and the one we plan to implement is to place a dichroic beam splitter in the aft-optics which will produce a copy of the four-quadrant image on a second detector. In this case, light from a calibration source at a slightly different wavelength will illuminate the front optics simultaneously to the scene radiance. This will allow for the thermal drift to be monitored simultaneously to the atmospheric measurement.

We have added the following lines to the Discussion:

“This effect would need to be carefully managed in the case of a practical field instrument where longer integration times are required, by implementing thermal compensation and active thermal control. In this case, the thermal drift will be tracked (and then corrected) by observing a calibration source. This could be done periodically between scene measurements by observing a calibration source emitting a spectral line close to that of the target emission. Or it could be done simultaneously by observing a calibration source emitting a spectral line different from that of the target emission that is separated into a second channel by placing a dichroic filter in the exit optics.”

(5) line 209: “Several additional criteria were also used to constrain the design*of* the field widened birefringent delay plate.”

Author response: Corrected in the text

(6) line 356, 381: Please put the SNR of 1000 (and the precision of 5 m/s) in perspective with expected atmospheric emission rates and realistic instrument parameters. This is especially important, since this result is used generally later in the paper (“Using these

measurements it was shown that wind precisions of < 5 m/s are achievable with the interferometer.") Since the achieved precision is a function of the instrument parameters (etendue, QE, U, V, integration time, etc) and the signal strength, this needs to be expressed more concisely, with these constraints/parameters included.

Author response: As is described in section 2.2, the expressions for the uncertainty in the wind measurement for the Michelson were originally developed by Ward (1988) and Rochon (2001) and general expressions for the sensitivity of Doppler wind measurements are presented by Kristoffersen et al. (2021). In the ideal case, where four samples are obtained with 90-degree phase steps the expression for the standard deviation, σ_w, of the wind measurement is

$$\sigma_w = \frac{c\lambda}{2\sqrt{2}\pi(\mathrm{SNR})UVD} \qquad \text{(RC1-6-1)}$$

According to Table 2 and Table 6 in the manuscript, for a field instrument, we assume UV is ~0.5, OPD is 1cm, when measuring the O^1D red line (630nm), the wind precision of < 5 m/s is achievable with an SNR of ~1000.

The O^1D red line (630nm) has an expected limb intensity of 30kR. In section 2.2, we give the expression for the signal S at the detector:

$$S = \frac{10^6}{4\pi} E_0 A\Omega\tau\eta t \qquad \text{(RC1-6-2)}$$

The etendue AΩ for a field instrument is 0.86 cm^2sr, QE η is 0.9, and τ is 0.1 when account is taken of the transmittance of the filter and lens system. If the spatial resolution is 100*100, we can get an SNR of ~1000 with an integration time of 55 seconds. However, for the ground-based observation, the intensity of red line is only ~100R. Thus, we need to reduce the spatial resolution and increase integration time. Typically, if the spatial resolution is 10*10, we can get an SNR of ~1000 with an integration time of 165 seconds. This calculation combines the SNR of ~1000, the precision of 5m/s, the expected atmospheric emission rates and realistic instrument parameters.

(7) line 440: The authors state: "Comparison of the effectiveness of a field widened birefringent interferometer relative to a field widened Michelson interferometer for the measurement of Doppler winds can be undertaken with respect to the primary instrument design parameters: A, Omega and D." As mentioned above, I think that this is exactly what should be done to motivate this paper, otherwise it is not clear why one would consider this technique for any specific application. Table 6 attempts to do that, but it does not show any compelling improvement over many of the other instruments listed. In addition, the "relative wind precision E" is not a very intuitive metric. Assuming an atmospheric signal

strength, one could give the wind precision in meters per second, which would be much more intuitive.

Author response: The best way to compare the capabilities of different interferometers has been a topic of debate for decades and no agreement exists on the best way to compare instruments. While this metric is not perfect, neither is a direct comparison that assumes a particular atmospheric signal strength. For example, the spatial and temporal sampling of MIADI is chosen to specifically target certain scales of geophysical variability that are not targeted by BIDWIN. Therefore, the spatial binning and measurement cadence also strongly influence the comparison. We sought a simple metric that could be used for comparisons with several state of the art field widened Michelson interferometers while removing the dependence on the scene radiance and detector parameters (Eq.17).

As mentioned above in the response to the general comment, the primary advantage here is the size and simplicity of the construction of the field widened delay plate. The primary finding of this paper is that a Doppler wind imaging birefringent interferometer can be constructed with comparable capabilities to existing state of the art field widened Michelson interferometers. Therefore, the compelling improvement is a smaller, simple to construct, instrument with comparable capabilities.

(8) line 462: The authors state: "In this case, the birefringent interferometer can achieve a throughput of 0.86 cm2sr and is capable of achieving similar wind errors and yet it has the smallest path difference." It is not clear to me why the "smallest path difference" is an advantage. The path difference should be optimized to the width (temperature) of the emission line.

Author response: You are correct, the low path difference is not necessarily an advantage. However, the sentence was more meant to highlight that it is interesting that we can achieve low precision wind errors with such a small path difference interferometer by taking advantage of the large SNR provided by the instrument. The statement has been removed from the text.