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Comment on amt-2021-81

Anonymous Referee #1

Referee comment on "A compact static birefringent interferometer for the measurement of upper atmospheric winds: concept, design and lab performance" by Tingyu Yan et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-81-RC1, 2021

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.

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.

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

(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.

(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.

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

(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.

(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.

(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.