Review to the manuscript

"A novel infrared imager for studies of hydroxyl and oxygen nightglow emissions in the mesopause above northern Scandinavia"

by P. Dalin et al.

The authors present a new imaging instrument that measures emissions from hydroxyl and molecular oxygen layers in the mesopause region. They describe the technical characteristics of the instrument as well as the derivation of temperatures from these measurements. The new instrument combines the measurements of IR emissions from two different molecules, because the centers of the two emission layers are located at slightly different altitudes. This allows tracing disturbances in the vertical direction additional to the horizontal domain which is enabled due to the imager technique.
Furthermore, they present the first measurements during the winter 2022/23. These measurements were compared with lidar and satellite observations in order to validate the temperatures derived from the imager measurements. Finally, the authors present some small case studies to illustrate the capability of the instrument to monitor temperature changes with time and to detect wave disturbances in both directions vertical and horizontal.
Generally, the manuscript is well structured and written and it addresses scientific questions within the scope of AMT. Thus, I recommend its publication after some minor issues are addressed.

1 General comments:

1. The imager shall be able to trace wave disturbances in the vertical domain. As the two emission layers (OH and O_2) are not located at a constant altitude and the center altitudes vary with time and season, the distance between the two layers is also not constant. Thus, it should be difficult to obtain absolute information on the vertical propagation. Can you comment and discuss this possible limitation of the technique in some more detail.

2. Typically, the contamination of the lines, especially the P1(4) line, by other emissions such as emissions by the OH(4-2) R-branch is corrected during the temperature estimation process (e.g. Schmidt et al., 2013; Pautet et al., 2014). How is this contamination corrected for your measurements?

3. Could you please clarify the name assignment of the different measurements, because it is a little bit confusing to me. In section 3.4 I_{P12} is introduced as the intensity of the P1(2) line, later it is called the raw intensity and and in Fig. 4 this raw intensity is the sum of P1(2) and BG (background). Is I_{P12} the intensity observed in the spectral range of the the P1(2) line and P1(2) (in Fig. 4 and below Eq. 1) is the real line intensity of the line in counts after subtracting the background? And is the dark noise already subtracted from the measurements?
Maybe it is helpful to revise these names in the manuscript to get a clear and consistent name assignment.

2 Specific comments:

1. Eq. 2: Intuitively, I would expect that the background is subtracted from the measurements in each of the spectral ranges of the observed lines separately. Before this subtraction both single measurements (background and intensity in the spectral range of the emission line) should be corrected for the dark noise influence. Here the dark noise is added (with some factor). Can you please explain a little bit more where the equation comes from.

2. Eq. 3 and Eq. 4: Do the different coefficients k_i have their own uncertainties which then should be taken into account during the error estimation or are the uncertainties too small to have an impact on the total error?

3. Fig. 4: Maybe it is useful to also show the OH equivalent temperature which has been calculated from the lidar observation by vertical averaging in the figure.

4. Fig. 6: It could be helpful to change the ranges of the colour bars as in most cases the full range is not present in the observations and some colours are not used then. This would maybe increase the contrast and visibility of the disturbances.