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# *Interactive comment on* "Optimising hydroxyl airglow retrievals from long-slit astronomical spectroscopic observations" *by* Christoph Franzen et al.

### Anonymous Referee #1

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General comments:

The paper describes a feasibility study for the use of astronomical long-slit spectra from the NOTCam instrument of the Nordic Optical Telescope (La Palma, Spain) for OH airglow investigations. The data properties, data reduction, and analysis in terms of the derivation and interpretation of OH rotational temperatures are described based on a set of three 10.8 s exposures covering the wavelength ranges related to the photometric J, H, and K bands. Furthermore, the paper discusses the available NOTCam data sets and the OH-related scientific questions which can be investigated using such spectra.

The study is interesting since it gives an overview of the challenges and prospects

related to the use of long-slit astronomical spectra for atmospheric research in the mesopause region. However, the paper is fairly short, which makes it difficult to fully understand the discussion. In particular, it is not clear whether the systematics related to the OH rotational temperature measurements are treated in a proper way. In any case, the study neglects the influence of non-LTE effects on the derived temperatures. The following list of specific comments is relatively long for such a short paper. This means that the manuscript has to be significantly modified to allow its final publication in AMT.

Specific comments:

P.1, L.16 and P.2, L.21: It would be better to use the term "spectrograph" instead of "spectrometer" in order to clarify that all covered wavelengths are recorded simultaneously.

P.3, L.3: It is not clear what "standard astronomical observations" means. Is the word "standard" related to "standard star" on L. 9 or does it refer to a certain observing mode? Are there also non-standard observations? Does this classification affect the size of the suitable data sample?

P.3, L.3: It would be good to explicitly state that the three bands are observed independently with a certain time delay. NOTCam offers more than the three mentioned filters, which could be combined in various ways. Why do the authors only list the combination of J-, H-, and K-band observations? Finally, I recommend to describe the wavelength ranges covered by the different modes. There will be readers who are not experienced with astronomical filter bands.

P.3, L.8: I can understand that only a single spectrum is used for the illustration of the data reduction procedure. However, the discussion in almost the entire paper (except for the outlook) is based on a single series of three 10.8 s exposures (each in a different band). If the whole data reduction and analysis is optimised for these spectra, it is not clear how changes in the observing mode (filter band, spectral resolution, expo-

sure time), observing conditions (atmospheric transmission and turbulence), and the calibration data (with a possible impact on the robustness of the reduction) could affect the quality of e.g. the OH rotational temperatures.

P.3, L.9: The given star is probably a telluric standard star, i.e. the corresponding spectra are used to correct atmospheric molecular absorption. Is there a special reason for this selection? In principle, the observations of astronomical science targets or spectrophotometric standard stars could have been used as well.

P.3, L.10: Why is the exposure time of the example only 10.8 s? This causes uncertainties in the line measurements and hence the OH rotational temperatures, which could be avoided by longer exposures. Of course, large time differences can reduce the coupling between the OH intensities in the spectra of different filter bands which are taken consecutively. Nevertheless, observing times distinctly longer than 10.8 s would still be well below the Brunt-Väisälä period. Was the selection by purpose (considering the arguments given above) or were there restrictions with respect to the data that was provided by the NOT for this study?

P.3, L.31: "A Gaussian function with a width of 0.31 nm": What was used for the J- and K-band observations? For constant resolving power, the line width is proportional to the wavelength. Moreover, the resolving power could/should depend on the observing mode.

P.4, L.8 and Fig. 2: The reduced spectrum does not seem to be flux calibrated. Fig. 2 shows ADU as intensity units. It can lead to a significant bias if such a spectrum is used for the derivation of OH rotational temperatures. Flat-fielding is only for the correction of small-scale variations. For the slowly varying intensity variations and the absolute calibration, instrument response curves derived from observations and reference tables of spectrophotometric standard stars are required. Only the effect of atmospheric extinction by scattering can usually be neglected in the near-IR.

P.4, L.11: How were the signal-to-noise ratios estimated? Poisson noise?

P.4, L.17: The continuum underlying the OH emission is removed by applying a Butterworth filter. This is not the most obvious way to separate line from continuum emission. Were therefore also other approaches tested? For example, continuum windows can be defined on both sides of the interesting lines. Afterwards, the continuum at the line positions can be interpolated and subtracted.

P.4, L.19 and Fig. 3: It is stated that the Butterworth filter should have a "minimal impact" on the OH lines. However, Fig. 3 reveals that there are major changes in the line shape. Strong negative residuals occur. Do this complex features reflect the true line intensities in a reliable way? This could be tested using alternative approaches (see comment on P.4, L.17).

P.4, L.20: The resulting spectrum is normalised to 1. However, it varies around 0. Hence, is this approach sufficiently robust?

P.4, L.22: Fitting a model spectrum instead of direct line measurements can cause relatively large systematic errors if the model assumptions in terms of airglow physics and the adaption to the observed data (e.g. line-spread function) are not sufficiently accurate. Was this approach chosen because of the relatively low spectral resolution?

P.4, L.22: "known OH line strengths": The Einstein-A coefficients for OH lines are not that well known. Ratios of Einstein-A coefficients for lines from different OH bands can easily vary by a factor of 2 or more depending on the set used. This study appears to be based on those from the HITRAN database, i.e. Goldman et al. (1998). Have the authors ever considered to also use OH molecular parameters from other publications, e.g. van der Loo & Groenenboom (2007, 2008) or Brooke et al. (2016) (to list relatively recent papers). Since this study combines P-, Q-, and R-branch data, the choice of the coefficients can be critical for comparisons of rotational temperatures from OH bands with different line sets (more critical than in the case of P-branch lines only).

P.4, L.23: The assumption of a Boltzmann distribution with a single temperature is a strong simplification. In fact, any change of the line set will affect the resulting temper-

ature (see Noll et al. 2015). Apart from uncertainties in the Einstein-A coefficients, this is caused by line-specific amounts of non-LTE contributions. They vary depending on vibrational level, rotational level, electronic substate, and observing time. Any OH rotational temperature is a pseudo-temperature (deviating from the kinetic temperature). Whether this is critical for a scientific study is a different question. In any case, one should take OH rotational temperatures and related comparisons with care.

P.4, L.28: The success of the model fit also depends on the accuracy of the instrumental line shape. Are the model spectra calculated using the simple Gaussian with fixed width mentioned at L. 31 of P. 3? Have the authors checked how the results on the OH rotational temperatures depend on the line-spread function (within the estimated uncertainties)?

P.5, L.1: "higher levels may not be thermalized": This is also likely for lower levels (see comment on P.4, L.23).

P.5, L.3: Is "accurate" the correct word to describe the quality of the OH rotational temperatures? As discussed before, the systematic errors related to the resulting temperatures can be relatively large. Uncertainties in the Einstein-A coefficients, non-LTE effects, line-spread function, and also atmospheric absorption (see comment on P.5, L.32) can be critical.

P.5, L.5: An error of 0.56 K is remarkably small. How was it calculated?

P.5, L.6: "may not be completely Gaussian": This is not unexpected. For example, the Gaussian might be convolved with a boxcar, which considers the influence of the entrance slit.

P.5, L.7: "the model used is robust": Note that this statement applies to the spectrum that was used to optimise the analysis. For a different observation, this might not be true anymore (even if the unconsidered systematic model errors are neglected).

P.5, L.11: Fig. 4 shows relatively large errors of the wavelength calibration. In part, the

shifts are higher than a line width. In particular, the spectrum of OH(7-4) does not fit well. Another example is the mismatch of the highest P-branch lines of OH(8-6) and OH(9-7). What is the influence of these wavelength calibration errors on the model fit and the corresponding rotational temperatures?

P.5, L.17: "longer integrations ... would enhance the data quality": The authors assume that the 10.8 s exposure in the J band is too short for reliable temperature measurements. Why was this not tested using a sufficiently long exposure? Only in this way, it is possible to distinguish statistical from systematic errors.

P.5, L.20: Assuming a fixed separation for layers of adjacent vibrational levels of 0.5 km can only be a very rough estimate since this difference is highly variable and also depends on which vibrational levels are compared (e.g., von Savigny et al. 2012; Xu et al. 2012). Hence, Fig. 4 essentially shows OH rotational temperatures as a function of vibrational level, even if the ordinate provides altitudes in kilometres. It would be good to clarify that the resulting temperature gradients are only qualitatively correct.

P.5, L.25: "the altitude [sic] ... were assigned": The fixed altitude step size of 0.5 km could be mentioned here again.

P.5, L.26: The term "atmospheric temperature profile" can be misleading. Apart from the already mentioned uncertainty in the true emission peak altitude (comment on P.5, L.20), it also has to be considered that this profile is strongly smoothed due to the typical emission layer widths of 8 to 9 km. The most critical issue is the fact that the given rotational temperatures are a combination of kinetic temperatures and non-LTE effects. However, the term "atmospheric temperature profile" suggests that a kinetic temperature profile is shown.

P.5, L.27: For a comparison of the NOTCam data and the NRLMSISE model, the latter should be smoothed considering a typical OH emission profile. This should significantly reduce the model temperature gradient (see Noll et al. 2016).

P.5, L.28: In view of all the effects which were not considered for the temperature comparison, I do not think that a safe statement on possible similarities can be made. In principle, there should not be an agreement between the observed and the modelled data due to the contributions of non-LTE effects to the former.

P.5, L.29: It would be better to show the temperatures from the OH(7-4) and OH(8-5) bands. Otherwise one could think that the authors want to hide something. Even if the quality of the rotational temperature measurements for these two bands are lower, they can be plotted if realistic error bars are assumed. There would not be an issue with the signal-to-noise ratio of these observations if longer exposures were taken for this paper (see also comment on P.5, L.17).

P.5, L.30: In view of all the effects which were not considered for the temperature comparison, it cannot be stated that the temperature for OH(8-6) is "anomalously high". Moreover, it is even expected that the values for the 8th vibrational level are the highest (Cosby & Slanger 2007; Noll et al. 2015, 2016).

P.5, L.31: "P(2) and P(4)": The numbers for the electronic substates are not indicated here. What was exactly done to identify that these lines are partially absorbed? The given references suggest that the Cerro Paranal sky model could have been used for this.

P.5, L.32: How were the correction factors for the line intensity decrease in the model estimated? In principle, one needs a very high resolution atmospheric transmission spectrum (resolving power > 10ˆ6) for realistic atmospheric conditions. In particular, the amount of water vapour is highly variable. Therefore, it is important to estimate this amount if selected lines are affected. Apart from data from global weather models, direct measurements using the observed data are most promising. The telluric absorption in the observed stellar spectrum can be investigated with suitable fitting tools. Fortunately, the P-branch lines in the OH(8-6) band are mostly affected $CO_2$, which is much less variable than water vapour. Nevertheless, the high resolution transmis-

sion spectrum and a model of the airglow emission line profiles are needed to derive the correction factors. There are several papers on this topic (e.g., Espy & Hammond 1995; Noll et al. 2015; Chadney et al. 2017). Patrick Espy is even a co-author of this paper.

P.6, L.1: A change of the temperature by 8% is a huge effect, which can cause large uncertainties depending on the quality of the line absorption derivation. More details are needed to evaluate the uncertainties in the resulting OH rotational temperatures.

P.6, L.2: It is not clear what "same technique" means since the procedure for the line absorption correction is not described (see also comment on P.5, L.32).

P.6, L.4: This statement could change (see also comment on P.5, L.28).

P.6, L.5: The authors provide a long list of papers which apparently support their conclusion of decreasing OH rotational temperatures with increasing vibrational level. However, Oberheide et al. (2006), French & Mulligan (2010), and Dyrland et al. (2010) only provide results for a single OH band, which cannot be used for this investigation. In the case of Lübken et al. (1990), the results for OH(8-3) and OH(3-1) are quite similar. However, they are based on two independent instruments with different measurement periods. Sivjee & Hamwey (1987), Phillips et al. (2004), and Wrasse et al. (2004) compare rotational temperatures from the OH(8-3) and OH(6-2) bands. The first two studies find higher temperatures for OH(8-3), which is in agreement with the uncorrected NOTCam results but disagrees with the authors' conclusions. Espy & Hammond (1995) find similiar temperatures for OH(7-4) and OH(3-1), which also does not support the conclusions. Only the northern winter measurements used by Perminov et al. (2007) appear to be in good agreement.

P.6, L.8: The list of publications finding an increase of the rotational temperatures with vibrational level could be complemented by Cosby & Slanger (2007). This investigation was the first one which clearly showed this remarkable temperature dependence with a maximum for the 8th vibrational level, which can only be explained by a significant

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contribution of non-thermalised rotational populations.

P.6, L.12: NOTCam competes with other instruments at the NOT. Nevertheless, an observing time fraction of 15% is relatively small. Moreover, it has to be considered that NOTCam is also an imaging instrument and that there are various spectroscopic modes with different wavelength coverages. Therefore, the number of spectra that cover a given OH band should be relatively small. This means that it is unlikely that a good annual coverage is achieved by the scheduled observations. It appears to be necessary to combine data from the whole period between 2007 and 2016 to perform variability studies. Exceptions could be individual observing nights with a good time coverage. However, this depends on the design of the corresponding astronomical project if archival data are used. Would it possible to perform dedicated airglow observations? I would like to see a more detailed discussion of the data set in terms of such limitations.

P.6, L.13 and Fig.6: The figure lacks information on the total number of spectra for a certain day of year and in which years these observations were performed. One could use more symbols, symbol sizes, and/or colours to provide more details. The figure only shows data taken between November 2007 and June 2016. Were there no suitable observations before this period although the spectroscopy mode started in August 2003 according to P. 2, L. 29?

P.6, L.18: Apart from showing the histograms in Fig. 7, it would be helpful if the total numbers of spectra for the different bands are provided in the text. Since the paper discusses a set of consecutive observations in three bands, it would also be interesting to know how many combined observations of this kind exist in the data archive. As the exposure time seems to be an issue at least in the J band, it would be good to learn more about the distribution of exposure times.

P.6, L.20: I am not sure whether this rather limited data set can be called "extensive".

P.6, L.31: Does the NOTCam archive already include suitable time series with high

temporal resolution or would it be necessary to apply for observing time for a dedicated study?

P.7, L.3: In the case of observations of astronomical objects, the spatial resolution is effectively reduced by the moving telescope. This issue should be discussed. Would it be possible to perform NOTCam observations without telescope tracking? However, if this was possible, there would be possible issues related to the data reduction of 2D spectra with various star traces.

Technical corrections:

P.5, L.25: altitude -> altitudes (see comment on P.5, L.25)

P.6, L.5: It would be better to write "northern winter" (or something similar) instead of only "winter".

P.6, L.8: measurement -> measurements

P.14, Fig.5: In the coordinates for La Palma, the "N" for northern is missing.

P.7, L.2: "large spatial resolution" -> "low spatial resolution"

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