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Comment on amt-2022-40

Anonymous Referee #2

Referee comment on "An improved vertical correction method for the inter-comparison and inter-validation of integrated water vapour measurements" by Olivier Bock et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2022-40-RC1, 2022

The paper presents a new method for height correction of integrated water vapour (IWV) data. Such a model is needed when comparing IWV measured at different heights, or when one wants to estimate the IWV at one height from measurements at another height. Traditionally, this correction is normally based on an exponential profile of the water vapour density, which might not always be correct. The paper instead proposes a correction method consisting of a slope and an offset parameter. Both these parameters are modelled as fifth degree polynomials. The results show that the new method give better results than the traditional one, and much better results than not using any height correction at all.

One concern I have is that there might be problems related to the high polynomial order of the slope and offset parameters. The polynomial coefficients are derived using data in a specific height range (0 -500m, 0-1000 m was also tested). Thus, there can be errors if the model is applied outside this range. This is especially true if the polynomial order is high. In the paper, all the GPS stations used are at heights below 500 m, so this is not a problem, but in general it can be. I think this issue should at least be discussed.

Are fifth degree polynomials really needed? Of course, the higher the degree, the better the fit to the data. However, there are some other reasons to keep the degree low. One is the above-mentioned problem when applying the model outside its fitted range. It is also easier to handle smaller number of coefficients, and the sensitivity to noise is lower. How much is actually gained in accuracy when using degree 5 instead of degree 2 or 3? Can it be quantified by some numbers?

At the end of Section 3, the parameters derived at three different radiosonde locations, separated by 400-500 km. are compared. It is found that they agree well and thus the parameters obtained from a radiosonde station can be applied to sites within several 100 km distance. I think this could be looked upon more closely. It is hard to draw any conclusions by just comparing the coefficients, it is difficult to know what is good a agreement and what is not. It might be better to look at what error in IWV would results if the model from one radiosonde site is applied at another one? Is it significant?

Also, at the end of section 3 the difference between using monthly and yearly coefficients is investigated. It is found that it is better to use monthly coefficients, e.g., as seen in Fig 5. Can this be quantified by some numbers, i.e., the mean error when using monthly vs. yearly coefficients. Furthermore, if monthly is better than yearly, maybe it is even better going to even higher temporal resolution.

The model coefficient for the traditional method, gamma=10^-4 m^-1, is taken from Bock et al 2007. I think it would be fairer to derive a new value from the radiosonde observations. Maybe even use monthly values, such as in the new method.

For the GPS validation in Sec 4.1 it is not clear what time period was used. In the caption of fig. 7 it says 1 Jan- 29 Feb, 2020. Is this also true for the rest of the investigation in the section? In that case, why was only 2 months used instead of one full year (or a longer period)?

From the GPS validation, two problematic stations were identified, CBE0 and BOUL. In the validation by MWR, it is found that BOUL is no longer problematic. The reason is probably because of changes to this station made in 2020. It would be interesting to investigate this further to validate the assumption. One could look at different months and see for which months there are problems and for which there are not, and check if this agree whit the times changes were made to the station.

If in-situ meteorological measurements (humidity and temperature) are available, there are methods to calculate the height correction using these data. How does the new method compare to such methods?

In the conclusion it is stated that the method can be applied to other regions where high-resolution vertical water vapour profiles are available. What resolution would be needed. Would standard resolution radiosondes or typical resolution of numerical weather models be sufficient?