Comment on "The Zugspitze Radiative Closure Experiment .... Part III" by A.Reichert and R. Sussman (doi: 10.5194/acp-2016-323)

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Reichert and Sussman (2016) present an important attempt to characterise the water vapour continuum in the near-infrared in atmospheric conditions. Given that relatively few such measurements exist, such work is very welcome.

We have a number of comments on the paper. The major one relates to our comment on Part II of this paper, where the authors calibrate their measurements to an assumed extraterrestrial solar spectrum (ESS); as we note in that comment, there are significant uncertainties in the ESS. This uncertainty has important consequences for the derivation of the continuum, especially in the window regions, which are not taken into account here.

It is our view that this uncertainty renders the continuum derivations here unreliable in window regions; the fact that many of the derived continuum values in the windows are negative and therefore unphysical (as shown in the data in their Supplement but not in the figure in the paper) adds support to the opinion given by Reviewer 1 (10.5194/acp-2016-323-RC1) that the derived continuum values deep in the window are so uncertain that they should not be presented.

Major comments

1. Equations (2) and (3) derive the continuum optical depth from the difference between the observed downward radiance at the surface and the modelled radiance ignoring the continuum. To do this reliably requires that the ESS is well constrained. This is not currently the case, as we explain in our comment in Part II (see e.g. Thuillier et al. (2015) and Weber (2015)). Various derivations from satellite and other observations differ by 5-10%.

The authors' method is essentially to write a radiance residual (their Equation (2)) between observations and model so that

$$\Delta I = S_{actual}\exp\left(-(\tau_g + \tau_{cont} + \tau_{aer})\right) - S_{model}\exp\left(-(\tau_g + \tau_{aer})\right)$$

where τ is the optical depth due to lines of the gases (subscript *g*), water vapour continuum (*cont*) and aerosols (*aer*), and S_{actual} and S_{model} are the actual ESS and the ESS used in the model respectively.

Since S_{actual} is not observed, the authors (in Part II of the paper) perform a Langley analysis on their observations to derive $S_{Langley}$, and then apply a calibration constant (c) to force $S_{Langley}$ to agree with S_{model} (i.e. $S_{model}=cS_{Langley}$) . The authors note in Part II that their "closure validation does not provide information on the accuracy of the used ESS" but here we are concerned about the impact of this on the radiance residual.

τ_{cont} is then derived from the above equation as

$$\tau_{cont} = -\ln\Big(\frac{S_{model}}{S_{actual}}\big(\frac{\Delta I}{S_{model}\exp(-(\tau_g+\tau_{aer}))}+1\big)\Big).$$

If $S_{model}=S_{actual}$ (i.e. if S_{model} is indeed the true value), then this equation reduces to the authors' Equation (3). However, if this is not the case, then any error in the ESS (which would lead to a radiance residual even if τ_{cont} is zero) gets incorrectly attributed to τ_{cont} - the resulting error in τ_{cont} is particularly severe for the low values of optical depth found in the window regions, and even the sign of τ_{cont} is not constrained to be positive.

We believe that it is important to incorporate the effect of errors/uncertainties in the assumed ESS. We expect that such an analysis will lead to the conclusion that the derived values of the continuum in the centres of the windows are too unreliable to be presented.

2. The consistency between the residual method of deriving the optical depth could be compared with the slopes of the Langley plots in part II, as these are quasi-independent derivations of optical depth (and in particular, the Langley method does not require knowledge of S_{actual}).

3. We feel that the summary in the final two sentences of the abstract gives a somewhat misleading picture of the degree of agreement between the new observations and available laboratory measurements. For example, in Figure 1, it is difficult to see that the new measurements are in better agreement with the Bicknell measurements than the FTIR measurements of Ptashnik et al. (2012, 2013). From 5900 to 6600 cm^{-1}, the values derived in this paper, and listed in the Supplement, are almost universally negative, and therefore unphysical. In the 4700 cm^{-1} region, at the wavenumber of Bicknell's measurements (about 4670 cm^{-1}), the author's central estimate appears as close to the Ptashnik estimate as to Bicknell. Even the comparison with the Mondelain et al. (2015) data is inconclusive. At wavenumbers just below 4250 cm^{-1}, where the authors' data have relatively small error bars, the data points tend to go in between the Mondelain et al. and Ptashnik et al. data. It is only at wavenumbers above 4250 cm^{-1} that the new data appear to fit better with Mondelain et al., but at these wavenumbers the new observations have too high uncertainties to allow firm conclusions; the upper error-bars nearly overlap the Ptashnik et al. data.

We feel that there would be greater clarity in the abstract if the situation near the band centre is separated from the situation in the window. In the band centres the disagreements between recent FTIR measurements (see especially Paynter et al. (2009)) and MT_CKD are known to be relatively small, compared to the situation in the windows; these near-band-centre regions constitute much of the “75%” that is referred to at 1(13). In the windows (e.g. 2800-3000 cm^{-1} and 4200-4500 cm^{-1}), it seems hard to sustain an argument that the new measurements are in any better agreement with MT_CKD than they are with the Ptashnik FTIR measurements.

Further comments (co-ordinate system “page number (line number)”)

1(18) and 8(3) We would say “typically a factor of 2-3 times higher”. “5” seems an exaggeration to us.

6(18) It would be useful to more clearly state how the solar absorption lines were defined. We assume these were based on the Kurucz ESS described in Part II. However, as noted by Menang et al. (2013) (using both an analysis of their own ground-based observations and using the ACE space-based measurements of Hase et al. (2010)), the Kurucz ESS does not include a number of solar lines that were detected in these two recent works.

7(1-8) We feel it would be useful to produce a plot that showed k_{cont} using both the linear+constant and the purely constant scaling. At present, the paper has only one figure, and so this could easily be accommodated.

7(29-37) We are unclear why two different temperature dependencies are employed, depending on which laboratory data is used, and what impact it has. Also we were unsure why the MT-CKD temperature dependence was considered more appropriate for some sets than others. It would be useful to see the impact of using a common temperature dependence with all data sets, to establish how much effect this has on the results.

7(40) It may be useful to plot the Paynter and Ramaswamy (2014) data as well as the Baranov and Lafferty (2011) observations.

8(7) There is misleading phrase. There were no “narrow line-like features in the continuum” reported e.g. by Ptashnik et al. (2011); those features were 60 cm^{-1} (FWHM) broad continuum peaks.

8(9-16) We largely agree with the statements here, but we believe it should be added that the assumption that the foreign continuum has no temperature dependence has not been tested at atmospheric temperatures in the laboratory. And it is that foreign continuum which dominates in the wings of water vapour absorption bands (in particular in the 3200-3400 and 4000-4200 cm^{-1} regions) where the large and more certain disagreement with FTIR-based results of Ptashnik et al. (2012) is noted by the authors.

8(31) We think that it should be pointed out that there are regions with rather good agreement with Ptashnik et al. (and better than with MT_CKD), particularly around 3000 cm^{-1}.

13(1) It is rather hard to see the uncertainty bars, especially where they overlap with other data. Perhaps these could be drawn in a bolder format? In addition, we suggest that an additional plot is needed to make clear to the reader that many of the derived values are negative/unphysical; this, of course, cannot be done in a plot with a logarithmic axis.

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