Comment on "The Zugspitze Radiative Closure Experiment Part II" by A.Reichert et al. (doi: 10.5194/amt-2016-127)

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The work presented by Reichert et al. (2016) is an important contribution to the quantitative use of ground-based sun-pointing spectroscopy. However, we are concerned by their claim that they have a superior calibration to that achieved by Gardiner et al. (2012). As detailed below, the approaches to calibration in the two papers are fundamentally different - Gardiner et al. (2010) perform a full radiometric calibration whereas Reichert et al. (2016) perform a point calibration to an assumed extraterrestrial solar spectrum (ESS). We do not believe the claim that the authors have a superior calibration to Gardiner et al. (2012) is sustainable, and we suggest that they de-emphasise this point in a revised version of their paper. Our comments are not meant to detract from the central point that the technique adopted by the authors is a useful practical method in the field, and would become more so, if uncertainties in ESS were to decrease.

Major comments

1. Fundamentally, the Gardiner et al. (2012) calibration is a full radiometric calibration which is ultimately traceable to a primary standard cryogenic radiometer (see especially Section 2(d)(i) of their paper). By contrast, Riechert et al. (2016) perform a point calibration to an assumed extraterrestrial solar spectrum (ESS), which itself has significant uncertainties, as detailed in the next comment. It is therefore not an equivalent calibration, and the relative level of the uncertainties cannot be compared. We believe a more fundamental calibration could be possible with the authors' system in the future, if they used their blackbody unit as a calibration source (in the present paper, it is used to in-fill between calibration points derived using the Langley method, rather than for calibration itself). The blackbody results offer the potential for a full radiometric assessment of the measurements but this would require an independent assessment of the manufacturer specifications and traceability to an appropriate standard. However the consistency of the methods could be assessed by making a comparison of the Langley and blackbody data at the selected Langley points and see if they agree within the assumed uncertainties in the absolute radiometric values.

2. We question the central assumption that the ESS in the near-infrared (NIR) is so robustly known that it can be used as a calibration point for ground-based measurements, following a Langley extrapolation (at least, for the purposes to which the calibration is put, in Part III). There is a significant recent literature that casts doubt on this assumption and analyses differ by 5-10%.

The main sets of satellite-based measurements of ESS in the NIR are from measurements using various versions of the SOLSPEC instrument dating back to 1983 (see e.g. Thuillier et al. 2015). Reichert et al. use the analysis of Thuillier et al. (2003), which is often referred to as ATLAS 3. Thuillier et al. (2014) present an analysis of more recent SOLSPEC measurements (SOLAR 2) which were about 7-10% lower than ATLAS 3. A subsequent paper by Thullier et al. (2015) reanalysed the data from these measurements (producing SOLAR 2rev), concluding that they are closer than SOLAR 2 to ATLAS 3, and indeed that ATLAS 3 was more reliable. However, that analysis has been challenged by Weber (2015). Part of the issue is that the ESS measured by SOLSPEC used in the SOLAR 2 analysis drifted upwards by about 7% over the period of 2 years from its first deployment in 2008. Thullier et al. (2015) conclude that the later measurements are more reliable, as the drift was due to decontamination, while Weber (2015) contends that the drift was due to instrument degradation. Weber (2015) summarizes other recent satellite and surface-based measurements (including Bolsee et al. (2014) and Menang et al. (2013)) that broadly support the lower value in SOLAR 2. Reichert et al.'s measurements can shed no light on this disagreement, but their error analysis needs to reflect this uncertainty.

We recognise that at 9(22-23) Reichert et al. acknowledge that the closure validation "does not provide information on the accuracy of the used ESS", but we feel that this clear statement comes too late in the paper and does not lead to the logical conclusion that this impacts significantly on the claimed accuracy of their calibration. This is especially so, as at 3(38) the ESS is characterised as "known". We suggest that the revised version of this paper reflects the ongoing debate about the ESS and fully acknowledges the impact of this on their claimed uncertainty. As noted in comment 1, this is one reason why we feel the authors should be much more careful in drawing comparisons between their claimed uncertainty and that of Gardiner et al. (2012) when the calibration methods are so fundamentally different.

A comment on Part III of this series of papers will note that the ESS uncertainty significantly compromises attempts to derive the water vapour continuum absorption by comparing observed radiances (with an uncertain ESS) with modelled radiances using a specified ESS; it is not straightforward to attribute their derived radiance residuals to errors in ESS or to the effect of the continuum absorption.

3. There is significant circularity in the authors' attempt to assess their calibration using a radiative transfer model. They calibrate their ground-based measurements using an assumed ESS, and then use that same ESS in the model to simulate the surface irradiance, and so there is an interdependence between the measurements and the model. The degree of agreement then depends on the ability of the radiative transfer model to simulate the atmospheric transmittance (which is the topic of Part III of this series), which requires knowledge of both the atmospheric state and spectroscopic parameters. Reichert et al. do not test whether this is the case, even though the slope of their Langley analysis would yield optical depth.

We suggest that the wording in Section 5.2 is altered to make clear that this is a relatively weak consistency check and that it does not, in any sense, act as a validation analysis (see e.g. 8(20), 8(33) and 9(27)); the abstract and conclusions should also be altered accordingly.

Further comments (co-ordinate system "page number (line number)")

1(33) "accurate laboratory studies cannot be performed at atmospheric temperatures". It is more accurate to say that they "have not yet been performed". In principle, the cavity ring down systems should have sufficient sensitivity to go to lower temperatures.

2(10 and 12-13) "Gardiner et al. (2012) proposed" and "the installation of ... a ... hot calibration source is highly challenging at remote ... observatories". This misrepresents the Gardiner et al. paper. They did not "propose" but actually implemented the procedure and they used a portable calibration transfer instrument (called TSARS – see especially 2(d)(ii) of their paper) to transfer the laboratory ultra-high temperature black body calibration to the field. This comment is also relevant to the text at 9(37-39).

3(15) Since the blackbody source is not used directly for calibration, but as a method of infilling the Langley-ESS calibration, we suggest that this heading is changed (as is the heading at 5(36)).

3(36-37) The Langley technique is an "extrapolation" not an "interpolation".

3(17) "thin clouds" – provided clouds remain in a single-scattering limit and they are constant in time, they should be removed by the Langley technique. This may not be the case for the high zenith angles adopted here.

4(36) We are curious as to why measurements were made so close to the winter solstice, when the minimum solar zenith angle at Zugspitze will be around 70°, entailing a long Langley extrapolation to zero airmass. Perhaps there were operational reasons for this?

5(1) What was the field of view of the instrument?

5(9-16) We were not sure, ultimately, which of the two ESS were used in this work, as two are mentioned here ("we use").

5(9-16) We presume that the ESS was corrected for the orbital eccentricity (which is especially important here as the measurement time is close to aphelion) but this is not stated.

5(27) We note that many solar lines missing in the Kurucz ESS were presented in Menang et al. (2013).

6(24-25) We do not understand this uncertainty estimate by linear interpolation and perhaps this could be made clearer. If it is a simple interpolation of uncertainty (say between 2 points with 0.5% and 1% uncertainty), then this would imply that the claimed uncertainty at wavenumbers between their Langley points can be lower than at wavenumbers at which they make their Langley calibration.

6(22-26) More details on the low value (less than 0.72%) of the error in the Langley extrapolation would be useful, especially given the high zenith angles employed here (see above – this means the extrapolation is a long one), and the fact that the ESS axis is logarithmic. As we read the text, the quoted error is the error in the individual points on the

Langley plot, rather than the error in the extrapolation to zero air-mass (which is what is important here), but we may be misunderstanding.

6(27-36) We found it difficult to follow this paragraph and how it ultimately led to the statement that the calibration uncertainty due to mirror inhomogeneity is about 0.2%. We suggest that this paragraph could be written much more explicitly. At lines 30-31 the "diurnal variation of the measured signal" is mentioned, but we were not sure what this referred to. If it is the measured diurnal variation in the solar irradiance, then isn't this part of the Langley signal? If it refers to the "outgoing laser beam" in the previous sentence, then more details are needed. Is it a simulation of the diurnal effect, or is this monitored in real time? The variation is interpreted as being due to spatial inhomogeniety whereas it could also be significantly affected by the angular dependence of the mirror reflectivity which we do not think is assessed here (see Gardiner et al. (2012) Section 3(c)). If this angular dependence was a significant contributor, then the authors' "conservative estimate" that converts the 5% change into a 0.2% uncertainty would have to be revisited. We also did not know what the "calibration time interval" (line 35) meant – presumably this means the total period over which measurements contributing to the Langley fit were made.

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