Reply on Referee #2 (AMT-2022-259)

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1 Introduction

The comments of the reviewer have been helpful to improve the manuscript. Especially the discussion on the mounting position gave us some new inspirations. And the differentiation of static and dynamic thermal offsets will hopefully make the manuscript easier to understand for the interested readers. The detailed replies on the reviewers comments are given below.
The reviewers comments are given in bold while our replies are written in regular roman letters. Citations from the revised manuscript are given as indented and italic text.

Detailed Replies

The abstract states that the correction function "depends on the mounting position of the radiometer on HALO." Please clarify what is meant by mounting position, and how conclusive this statement should be as several statements were made in the paper related to the impacts of mounting position, some of which seemed fairly conclusive and others more as hypotheses. In section 3.3 (Figure 3), the larger differences in sensor temperatures between the pyranometers than pyrgeometers rather than the upwelling or downwelling instruments is explained as being a matter of "the internal sensor housing" which I took to mean inherent to the differences in the instrument construction rather than how they were mounted on the plane. In section 5, the coefficients of the upper and lower pyranometer are described to differ by a factor of 2, and this is attributed to differing airflow between the two systems given the slight tilt of the plane. Then later in section 5, the up and downlooking pyrgeometers had much similar beta values which was hypothesized to be because "the CGR4 sensors are placed in front of the CMP22s". While there is a difference between sensor temperature agreement and agreement in coefficients for the corrections for dynamic offsets, they are related. I am curious whether the explanations that the authors give for these factors that cause differences in CMP22's and CGR4's thermal responses

(position relative to airflow in section 5, and internal sensor differences in section 3.3) are perhaps related and how important the relative ventilation of the radiometers is thought to be in comparison to sensor differences.

Thank for this discussion! Some of our conclusions and hypothesises were not given precisely and even contradict. You are right, that it is a combination of mounting position and the inherent differences in construction and internal heat transfer between pyranometer and pyrgeometer.

There is an indication, that the front position of the CGR4 sensors leads to a faster response of the sensor temperature (Fig. 3) although we can not rule out, that this is a consequence of a different internal construction of both radiometer. A more solid radiometer body or a different position of the sensor thermometer will also result in differences of the temperature adjustment. Additionally, the thermal offset is a result of the difference of dome temperature (no measured) and sensor temperature, and the dome material. Therefore, the ventilation and response to changes of ambient temperature can not directly be linked to the thermal offset coefficients β. It might be, that the internal ventilation of upper and lower sensor package is similar but the dome temperature adjusts different depending on the position and angle of attack. This may explain the differences of β for the upper and lower CMP22. But also heat conduction from the cabin or fuselage may add to the differences.

Finally, β needs to be determined for each setup (radiometer and aircraft). It is natural to assume, that better ventilation will help to reduce thermal offsets. However, from our analysis we can not quantify to what extend the mounting order (CGR4 in front of CMP22 or vice versa) impacts the thermal offsets. In the revised manuscript we did some corrections to make the reader sensitive to the combination of all potential causes:

> Abstract: *... The parameterization provides a linear correction function (200–500* $\mathrm{W\,m^{-2}\,K^{-1}\,s}$*), that depends on the radiometer type and the mounting position of the radiometer on HALO.*
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> Section 3.3: *This indicates that temperature adjustments are rather a matter of the internal sensor housing of CGR4 and CMP22, their internal heat transfer, and the mounting order (CGR4 mounted in front of CMP22). The ventilation within the fairing is similar in upper and lower sensor package.*
>
> Section 5: *This might be a consequence of the less exposed domes of the CGR4 compared to the CMP22 in combination with the more efficient ventilation of the CGR4 inside the BACARDI sensor mounting where the CGR4 is placed in front of CMP22 with respect to the flight direction.*
>
> Conclusion: *The exact offset correction depends on radiometer type, the mounting position of the radiometer, and the air flow around the aircraft but is independent of the magnitude of irradiance.*

I had a couple of questions about the practicality of the attitude correction method used. It seems to be sufficiently accurate, and the HALO aircraft remarkably level in most flights, so these are minor concerns that the authors shouldn't need to address for the publication of this paper. But I still found myself curious about a few practical details. The

authors state that only the direct beam should be corrected for, so this correction should only be applied to clear sky conditions. I agree with that, however, I didn't understand how the data was determined to be clear or cloudy. In the test case, the profiles could be determined to be clear fairly easily by visual inspection, though I would imagine this would be a harder job for a full field campaign. Was this correction run at all times and it left to users to determine whether to use the corrected or uncorrected data or is some kind of determination made for a best estimate value? Also, as the correction was based on radiative transfer calculations using atmospheric profiles from drop sondes or radiosonde launches—I was curious whether these will always be available for all campaigns where the HALO flies?

You are right, the attitude correction of the solar downward irradiance can only by applied to the direct radiation for which geometry is known. The decision if and when, with what direct fraction to correct the measurements depends a lot on the application of the data and can be done with different degrees of accuracy. A general procedure for a full campaign is challenging. Our approach is the following: published BACARDI data is provided for both conditions: I) cloud-free (correction applied based on the diffuse fraction obtained from simulation of cloud-free conditions. II) cloudy (no attitude correction applied). In addition, the published data set includes the simulated downward irradiance assuming cloud-free conditions and a basic cloud-mask. The decision on cloud-free or cloudy is done by a combination of comparing measurements to simulations of the expected cloud-free irradiance and by analysing the variability of the downward solar irradiance within a 20 s running window. The variability threshold accounts for cirrus, where the irradiance may even exceed cloud-free conditions due to enhanced scattering. It is assumed, that clouds, in particular cirrus, lead to an enhanced variability. It is planned to provide the same data sets also for upcoming BACARDI measurements.

We hope this additional data provides sufficient information to make use of the data for the most applications. In the revised manuscript, we added the following information:

> *A basic cloud mask, that is based on a comparison with the expected cloud-free irradiance and the identification of enhanced variability of the downward solar irradiance within a 20 s running window, is provided int he published data set.*

My primary concern with the methodology is that it wasn't clear to me in the text which thermal offset corrections are applied to the data (that derived in section 2.3 only, or also a correction derived in section 2.2). In Figure 6, after the dynamic linear-fit correction has been applied, there are still negative biases in the downwelling solar irradiance of 5-10 $\mathrm{W\,m^{-2}}$ at night. I don't see an adequate explanation for what this bias is. The reason given in lines 348-349, "caused by different uncertainties such as the radiometric calibration of the pyranometer", seem quite hand-wavey and not satisfying to me compared to the careful work done elsewhere in deriving the corrections. A calibration error is multiplicative so shouldn't give a bias at night. It seems more likely to me from the shape of that bias (larger with higher altitudes) that it is in fact related to a thermal offset (like that derived in section 2.2) that isn't corrected for using the "beta" linear fit. The author's state in lines 325-326 that a more complex multi-variate fit including Tref doesn't improve the correction, and conclude that therefore the dynamic dome effect can't be discriminated from the thermal offset. But they don't show those results, and I still can't help but think that the postcorrection results in Figure 6 look

like they are still impacted by an equilibrium thermal offset. Also, Figure 4 shows downwelling SW offset corrections even in level flights when the temperature doesn't appear to be changing significantly, which implies that the static offset is taken into account in some way. So it was unclear to me whether the dynamic offset (beta) correction was applied to this data or a static offset as derived in section 2.2.

Thanks for making this comment! We need to be more precise in our description of the processing.

What we can and do correct is the dynamic thermal offset effect as described in Section 2.3, which occurs when the ambient temperature changes (e.g., by flight altitude). Data in Fig. 4 was detrended to derive the dynamic offset coefficient, which removed all constant offsets that are present when the ambient temperature does not change. For the solar irradiance, we might obtain the "static" offset following Section 2.2. using the original data and assuming that no radiation is present during night. This obviously will amount to the remaining offsets of $7\,\mathrm{W\,m^{-2}}$ and $4\,\mathrm{W\,m^{-2}}$ that are visible in Fig. 6. However, for two reasons we decided not to follow this approach: A) For the thermal-infrared irradiance this is not possible. B) the "static" offset depends on the difference between dome and sensor temperature, which we don't measure and also can not parameterize. The corrections obtained from the night flight might not be valid for other flights. Contrarily, the dynamic offset depends on the change of temperature only and not on the absolute value of temperatures. This can be quantified during the flights using the sensor temperature.

So it is true, that the remaining deviations shown in Fig. 6 may result from the static thermal offset (Sect. 2.2). In addition, the temperature dependence of the radiometer sensitivity can contribute to the bias. As described in the manuscript, we account for the temperature dependence of the thermopile sensitivities in the data processing. However, the parametrization provided from calibration might not be perfect.

In the revised manuscript, we carefully changed the wording and differentiated between "static" and "dynamic" thermal offset.

> *The remaining bias to $F_{\mathrm{sol}} = 0\,\mathrm{W\,m^{-2}}$ is caused by potential static thermal offsets as described in Section 2.2 and other uncertainties such as the radiometric calibration of the pyranometer.*

The complex multi-variate fit discussed by the reviewer also refers to the "dynamic" thermal offsets as described in Equation 17 and does not include the "static" offset.

Line 6: it would read better as "an efficient new method".

You are right. We changed the sentence as suggested.

Line 30: should it be "which can be measured directly"

Thanks!.

Line 55: should be "Actively stabilized pyranometers"

Thanks! We now tried to consistently use plural throughout the manuscript.

Line 80: Section 6 is not specifically referenced in the paragraph about the structure of the paper. Did you wish to add that?

Thanks! This must have deleted by accident during the writing process. We added the reference to Section 6 in the revised manuscript.

Line 92: should be "In the case of the pyrgeometer"

We changed to plural "pyrgeometers".

Line 108: I think ρ_p should be ρ_d in the $rho_s \cdot \rho_p << 1$ assumption.

Thanks for finding this tricky typo!

Line 157: two the's at end of the line

Thanks!

Line 244: should be "To enable maintenance"

Thanks!

Line 265: What does "one magnitude lower" mean? Does this mean one order of magnitude lower?

Yes, you are right! We changed the sentence as suggested.

Line 313: should be "depends"

Thanks!

Line 416: The wording at the beginning of this line is unclear.

We might have used the wrong wording to express, that the general change of the irradiance is caused by the diurnal cycle and that higher frequency oscillations are added. In the revised manuscript, we changed the sentence to:

> *The uncorrected $F_{\mathrm{sol}}^{\downarrow}$ shows oscillations of different frequency that are superposed to the diurnal cycle.*

Line 508: should be "The data are used by Luebke et al (2022)"

Thanks! We changed the reference.

Section 2.3: This paragraph shows the RT model is used in the data, but the justification and uncertainty of this treatment is not well discussed.

The radiative transfer simulations were not used to replace the measurements, if that is what the reviewer understood. The simulations only provide the relative number of the fraction between direct and solar irradiance, which cannot be measured on the aircraft. This fraction is used to weight the correction of the downward irradiance following the common approach by Bannehr and Schwiesow (1993). The contribution of uncertainties of the direct fraction to the downward radiance strongly depends on solar zenith angle and aircraft attitude. For 60° solar zenith angle, roll and pitch angle of 5°, 5 % uncertainty of the direct fraction amounts to a total uncertainty of less than 1 %.
To make this better understandable we changed the section into:

> *This correction is valid only for the downward direct solar irradiance. Therefore, the relative fractions of direct and diffuse solar radiation in cloud-free conditions are estimated using radiative transfer simulations. ..... For the conditions during ACLOUD, a 5 % uncertainty of the simulated fraction of direct radiation amounts to less than 1 % uncertainty of the corrected downward irradiance.*

This paragraph also assumes "The upward solar radiation as well as the upward and downward terrestrial radiation were assumed to be isotropic". This is not valid for solar radiation. What's the effect of this assumption?

This sentence might have been misleading. The point we wanted to make is that upward solar irradiance was not corrected for the aircraft misalignment. This is common procedure because of two reasons. First, a correction would require knowledge on the exact distribution of the radiation field, which is not measured and is difficult to estimate from simulations. Second, the upward radiation is way less anisotropic as the downward radiation (direct solar radiation) and the effects of the aircraft misalignment are little. A perfect isotropic radiation field would cause no effects at all. But it's true that our argumentation was wrong and misleading.
We rephrased this sentence to avoid any misunderstanding.

> *The upward solar irradiance as well as the upward and downward terrestrial irradiance cannot be corrected for the aircraft attitude. However, these components are characterized by a nearly isotropic radiation field compared to the downward radiation and the effects of a misalignment is minimal for a nearly level sensor (Bucholtz et al. 2008). To limit the remaining uncertainties due to the aircraft movement, measurements with roll and pitch angles exceeding* $\pm 4°$ *were removed from the data set.*