Authors response to on-line comments on the manuscript

Bruce Forgan
Dec 2022

All typographic and grammar corrections from reviewer comments have been acknowledged and have been either been incorporated into the revised document or in the case of grammar, the sentences have been rewritten to make the original intent clear.

As a result of comments from one of the reviewers, two sections now also include uncertainty analysis tables. This excellent suggestion was simple to as and enabled removal of a significant amount of explanatory and exploratory text, but required explanation of the tables.

Another reviewer's suggestion (Stefan) that the paper clearly summarize the future work required, meant the total text lines of the revised manuscript is now just marginally shorter than the original.

One review (Laurent) suggested (see below) the removal of the section using an estimated solar calibration. However, as it is a key element for future utility of the ACP, and essential in demonstrating the original equation assumptions need to be questioned – it remains but with reduced text.

The only suggested rejected was from a co-author (Ibrahim Reda) who was keen to incorporate international comparison work demonstrating the success of the original equation. My response was accepted by the co-author.

As a result of the review process the reporting of results in the manuscript have been strengthened.

Below, for the editors' information, I have provided additional comments – these are highlighted by sentences/paragraphs starting with ***.

Initial Comment from Nozomu

Dear Editor,

This manuscript provides a new valuable equation for the Absolute Cavity Pyrgeometer (ACP). The authors present theoretical derivation of the new equation and evaluate the caluculation results by the new equation and the exiting one very carefully. The authors also point out the problems to be solved in the equation in the future, and the proposed new equation is expected quite useful for calculation of the reference terrestrial irradiance from the ACP measurements.

I would recommend it for acceptance after the very minor points listed below.

line 1: The word in the title of "Radiometer" would be better to be changed as "Pyrgeometer".

line 28: Same as above.

line 72: The word "Trb(t)" in the Eq. (7) should be written as "Tb(t)".

line 80: "×" should be added in the description of S value as "7.044×10-4" and the unit for S should be added as "K/μV" after the value.

line 102: In the numerator of the first term on the right side of the Eq. (13), there is one missing parenthesis of ")" before "K".

line 104: Same as above in the Eq. (14).

line 116: Description of the equation number should have parenthesis as "Eq. (11)" the same as the other equations.

line 304: The ACP thermopile responsivity should be written as "Csolar" instead of "Cs" the same as in the other description.

line 317: "WIRIS2" in the second parenthesis should be written as "WIRIS4".

line 330: The order of parameters description should be the same as that in the Figure 2.

line 484: The word "cooling" is written twice, but either one of them is considered as "heating".

line 581: "×" should be added in the description of S value as "7.044×10-4".

I hope that my comment is very useful for the improvement of the article.

Thank you.

Best regards,

Nozomu

*** all suggested corrections have been used including the replacement of ‘radiometer’ with ‘pyrgeometer’ in the paper title.

Comments to Authors:

Dear Authors,

I think this study provides a new valuable equation for the Absolute Cavity Pyrgeometer (ACP). The theoretical derivation of the new equation and evaluation of the calculation results by the new equation and the exiting one are described very carefully. And the problems to be solved in the equation in the future are precisely pointed out, the proposed new equation is expected quite useful for calculation of the reference terrestrial irradiance from the ACP measurements.

I would recommend the very minor points below before its publication.

line 1: The word in the title of "Radiometer" would be better to be changed as "Pyrgeometer".

line 28: Same as above.

line 72: The word "Trb(t)" in the Eq. (7) should be written as "Tb(t)".

line 80: "×" should be added in the description of S value as "7.044×10-4" and the unit for S should be added as "K/μV" after the value.

line 102: In the numerator of the first term on the right side of the Eq. (13), there is one missing parenthesis of ")" before "K".

line 104: Same as above in the Eq. (14).

line 116: Description of the equation number should have parenthesis as "Eq. (11)" the same as the other equations.

line 304: The ACP thermopile responsivity should be written as "Csolar" instead of "Cs" the same as in the other description.

line 317: "WIRIS2" in the second parenthesis should be written as "WIRIS4".

line 330: The order of parameters description should be the same as that in the Figure 2.

line 484: The word "cooling" is written twice, but either one of them is considered as "heating".

line 581: "×" should be added in the description of S value as "7.044×10-4".

I hope that my comment is very useful for the improvement of the article. Thank you for your attention.

Response to Nozomu

Many thanks Nozomu for your comments.

I appreciate the detail in your comments and finding the typographical errors.

I have no issue with changing the title, that is changing 'radiometer' to 'pyrgeometer. That said, I deliberately chose 'radiometer' as a general description as I believe the ACP could also be used for other measurements not only as a pyrgeometer. However, as you implicitly point out using 'pyrgeometer' does provide the continuity to the Reda et. al. (2012), hence I support the suggested change.

The correction of the typographical errors you (and I in the meantime) have identified in the manuscript will be as you recommend.

Best regards, Bruce

Initial comment from Laurent Vulumier

Overview and general recommendation

This manuscript describes the inference of equations depicting how the electric signal of the Absolute Cavity Pyrgeometer (ACP) can be related to the incoming irradiance at the entrance of the instrument considering the different fluxes within the instrument. The ACP was first described by Reda et al. (2012) and is considered as one of two types of instrument that can be used for providing the World primary reference for terrestrial radiation in the future. The description given by Reda et al. (2012) included assumptions that strongly restricted the conditions when the instrument can be used. In any case, the ACP having no dome, it can only be used during fair weather nights, which is already a restriction, but the further added restrictions by Reda et al. (2012) complicated its use. Thus, this manuscript is an important contribution to the description of an instrument that may participate in the definition of the next primary reference for terrestrial radiation and its scope is appropriate for publication in Atmospheric Measurement Techniques.

I find the manuscript of good quality and most of my comments concern minor points where I think that clarifications are needed or I find some formulation unclear where I usually suggest reformulation that I hope to be clearer. I have two more important comments, the first being that I believe all elements are present in the manuscript for computing a full uncertainty budget, which I recommend to add. The second is that the manuscript is overly long, and I recommend suppressing one part that I find can be omitted. I think the manuscript can be published after my main comments are addressed and the minor points are resolved.

I detail my comments below starting with my two most important comments followed by minor points. Some of the minor points still emphasize questions that need clarification, while some other are just corrections of typos.

Major Comments

1. Because the ACP is considered as a potential instrument for a group providing the future primary reference for terrestrial radiation, it is important that its uncertainty budget is well defined. The manuscript provides improved equations for describing the physics of the ACP. With these, it should be possible to better determine the uncertainty budget than was possible for Reda et al. (2012), and the resulting uncertainty for the ACP may also be smaller than the one given by Reda et al. In addition, sections 7.1, 9.1 and 9.3 already contain most of the elements that should be considered for the uncertainty budget. I am aware that the authors recommend further work, especially concerning the influence of the convection term when it varies, for instance, when humidity is high, but I believe the authors can already give estimates. I strongly recommend including a full uncertainty budget computation just before the conclusion. I think this would not be too overwhelming an effort given how valuable it would be.

   *** Two uncertainty budgets have now been incorporated in the paper, which address their concens regards the convection coefficient uncertainty.

2. While the manuscript is extremely thorough, it is also very long. It is valuable to compare several methods for determining the characteristics of the ACP, especially detailing how the new determinations differ from those of Reda et al. (2012). This partially explains the length of the manuscript. However, I do not find the part describing the calibration using solar irradiance very helpful, especially since generic values had to be used for the thermopile calibration because no solar calibration of an ACP was performed. I would recommend suppressing the parts of the manuscript referring to the calibration using solar irradiance and eventually let it for a further publication when such calibrations could actually be performed.

   *** The solar section was not removed, rather it was used as an example to complement the initial uncertainty analysis.

Minor Points

Note: assuming the authors used \LaTeX to format their manuscript, I used \LaTeX formatting for the minor comments allowing me to use the corresponding mathematical typesetting. I also include the PDF file (available as supplement) obtained by processing the \LaTeX document to facilitate reading the mathematical formulas.

\begin{enumerate}

\item Eq. 5 (line 67) should be $ KV = \varepsilon_r \left(W - W_r \right) $. The different equation in the manuscript is most likely a typo because the equation given here is consistent with both the previous and next equation in the manuscript.

\item Eq. 7 (line 72). The first term of the right-hand side should be $ T_{b}(t) $, not $ T_{rb}(t) $.

\item Lines 90-91: is the concentrator absorptance a consequence of the scattering on the walls because some radiation is again scattered toward the concentrator wall where it can again be absorbed? Please, clarify.

\item Line 92: Replace “at one end of the symmetrical concentrator” with “at the lower end of the symmetrical concentrator affixed to the body part of the ACP pyrgeometer", so that there is no possible confusion.

\item Lines 98-100 and derivation from Eq. (11) to Eq. (12): Eq. (11) is the same as Eq. (4), and it can be suppressed while simply mentioning that Eq. (4) is also valid for the outgoing flux from the receiver when the concentrator is present. It may be more useful to replace Eq. 11 from the manuscript with an equation that results from solving Eq. (4) and Eq. (10):

\begin{equation} F_{\downarrow} = \frac{ \tau W + \varepsilon_c W_c + \beta \varepsilon_r W_r }{ 1 - \beta (1-\varepsilon_r) } \end{equation}

Which then easily leads to Eq. (12) in the manuscript.

\item Line 112: Replace “… dependent on several factors such as water vapur content” with “… dependent on several factors such as water vapor content."

\item Line 130, citation of Jinan et al. (2010): I think Jinan is the given name and Zeng is the surname, which is a rather common Chinese surname. Thus, I believe this reference should be Zeng et al. (2010) and not Jinang et al., (2010). This also applies at lines 134, 139, 144, 215, 265, 548, 555-556, 675, and at line 691 where the reference is given that should be “Zeng, J., Hanssen L., Reda I., Scheuch J.” and not “Jinan, Z., Hanssen L., Reda I., Scheuch J.”

\item Eqs. 19 and 20 (line 131-138): The derivation by Zeng et al. (2010) does not use the same terminology as this manuscript. I believe it is assumed in the manuscript that Zeng et al. described the relationship between the thermopile signal and a reference irradiance signal $ S $ when the concentrator is not present as $ K_1 V_0 = S_0 - W_{r0} $, while the relationship with the concentrator present was taken as $ K_1 V_c = \tau S_c - W_{rc} $. Dividing the latter by the former allows obtaining Eq. 19. If these assumptions were made, it would help the reader stating them in the manuscript. I also believe this manuscript includes corrections for convection and emission by the concentrator wall in these two relationships as $ K_1 V_0 = S_0 - W_{r0} + \gamma (T_{air0} - T_{r0})$ (without concentrator) and $ K_1 V_c = \tau S_c - W_{rc} + \varepsilon_c W_{cc} + \gamma (T_{airc} - T_{rc}) $. Again, dividing the latter by the former allows obtaining Eq. 20. It would also help stating it in the manuscript.

\item Eqs. 19 and 20 (line 131-138): I wonder if other subscripts than “0” and “c” could be used for the situation with and without concentrator. Subscript 'c' is also used for describing characteristics of the concentrator itself such as its emissivity, temperature, thermal emission, etc. Also using it to denote measurements taken with and without concentrator makes it confusing for the reader: in this case, W_{rc} is the thermal emission from the receiver (not the concentrator), when the concentrator is in place.

\item Lines 183-184: I am not sure what the authors meant with “Using Eq. (22) these results implied data selection generates...” The authors may have meant “Using Eq. (22), the data selection implied by these results generates $ \langle K_1 \rangle $ that are approximately 6\% less than the Reda et al. (2012) implementation.”

\item Line 185: Replace “… the difference between the results from Reda et al., 2012 and the use Eq. (18) could be…” with “… the difference between the results from Reda et al. (2012) and results using Eq. (18) could be…”

\item Line 190: Replace “the base temperature of the pyrgeometer, and its dome, and the blackbody output irradiance are changed…” with “the base and dome temperatures of the pyrgeometer, and the blackbody output irradiance are changed…”

\item Lines 206-207: I am not sure how to understand the end of the sentence. With “α_c is not required but given Kirchhoff’s law indicates it would not be independent of ε_c” did the authors mean “Kirchhoff’s law allows relating α_c to ε_c, reducing the number of concentrator properties to determine”?

\item Line 211: “For ACP95 the concentrator emissivity … was found to be 0.0225…” is already mentioned at the beginning of section 5.

\item Line 225: A range for $ \gamma $ is given at the end of the paragraph. In addition, there is an estimation of the impact of γ uncertainty on $ W_{atm} $ in the third paragraph of section 8 (starting at line 250). I would suggest to move this estimation here after line 225 (section 7.1 is about impact of uncertainties on $ W_{atm} $) and refer to it later in section 8.

\item Lines 247-249: “but α_c the absorption fraction…” It is unclear to me if α_c is different from α in eq. 9. By analogy with ϵ_c (that I understand as the emissivity of the concentrator

walls), α_c may be the absorptivity of the walls and not the concentrator as a whole. If they are the same, this sentence only expresses Kirchhoff's law in words or is a rewording of the fact that the backscatter is assumed insignificant. Please clarify.

\item Lines 252-253: Replace "convection coefficient γ divided by concentrator transmission of…" with "convection coefficient divided by concentrator transmission ($ \gamma / \tau $) of…"

\item Lines 276-278: Replace "… and for ACP96 a 1 s measurement sequence every 10 seconds" with "… and a 1 s measurement sequence every 10 seconds for ACP96."

\item Line 282: Replace "… instrument expanded (k=2) of 2 Wm^{-2}…" with "… instrument expanded uncertainty (k=2) of 2 Wm^{-2}…"

\item Line 303: Replace "One we will assume…" with "We will assume…"

\item Line 314: "Using the new equation…" Do the authors mean "Using Eq. 18 with τ estimated with Eq. 20? Please be more specific.

\item Line 355: Replace "… the average differences were much smaller in magnitude" with "… the average differences were smaller in magnitude when using the data from both IRIS instruments to derive single C and τ values."

\item Line 371: "in the new equation…" Again, I think the authors are referring to Eq. 18. Please refer to it by its number.

\item Eq. 26 (line 383): To strictly follow Eq. 18, the term $ \langle K_1 \rangle V(t) $ should be preceded by a minus sign.

\item Lines 413-421: It is not clear whether there are three or four conditions. One condition states that a cooling sequence does not start before $ \left(T_r - T_c \right) \left(t_i \right) - \left(T_r - T_c \right) \left(t_{i+1} \right) < 0.04$ or that it ends when this condition is not anymore fulfilled. On the other hand, there is also a condition that $ \left(T_r - T_c \right) \left(t_i \right) - \left(T_r - T_c \right) \left(t_{i+1} \right) < 0.02$. Does this mean that, within a cooling sequence, only measurements satisfying the latter condition are kept? Is there also a condition on the minimum number of valid point in a cooling sequence? Please clarify. I also think it will help the reader if the paragraph started with a general statement on the conditions then listed the conditions explicitly. In case the authors have three conditions (adapting to four conditions is straightforward), I suggest introducing the paragraph as follows. "Three conditions were used to select measurements sequences acceptable for the LSQ calibration, limiting the voltage difference between two successive measurements, the change in the temperature difference between the receiver and the concentrator between two successive measurements, and the total voltage increase over a full cooling sequence. Explicitly, the conditions were:" After this introduction, the authors can include a numbered list with the conditions explicitly detailed.

\item Lines 415-416: Replace "… the voltage difference between two consecutive voltage was least than or equal to +3.5 µV" with "… the voltage difference between two consecutive voltage was less than or equal to +3.5 µV."

\item Lines 425-426: "… the slopes for $ (T_r - T_c) $, and $ \langle A_{dT} \rangle $, shows an upwards shift between days 200 and 255 that recovers when data collection recommenced on day 312." It is difficult to see such an upwards shift on Figure 4. There are three groups of points between days 200 and 255. For these groups, the green points have extremely similar values, except maybe for the last day in the period where there is some dispersion, but it is not larger than the dispersion in the group of point between day 0 and 30. I am not sure this statement can be upheld and I would suppress it.

\item Line 433: Only a single short sentence mentions Figure 6, just saying what parameter is displayed on the figure. If this figure is not discussed, it should be suppressed as well as this sentence. I think Tables 5 and 6 are sufficient for the discussion.

\item Table 5 caption (lines 441-443): The caption of Table 5 is confusing for me. I think the authors meant, and should indicate as caption “ACP $ \langle C \rangle $ determined using linear LSQ calibrations and using IRIS measurement as reference irradiance (section 8.3). LSQ calibrations use all 244 periods regardless of the stability of $ W_{atm} $. When IRIS measurement are used as reference, values of $ \epsilon_c = 0.0225 $, $ \gamma = 6.5 $ and $ \tau = 0.977 $ are chosen and only periods with a standard deviation of $ W_{atm} $ from the IRIS less than 0.4 Wm${-2}$ are used, including 115 periods for IRIS2 and 63 periods for IRIS4”.

\item Table 6 caption (lines 445-447): In relation to the change suggested for Table 5 caption, I suggest to replace the caption of Table 6 with “Same as Table 5 for the determination with IRIS as reference, but with values of $ \epsilon_c = 0.0225 $, $ \gamma = 8.4 $ and $ \tau = 0.977 $”.

\item Lines 449-450: Replace “… are shown in Figure 7 with the convection coefficient used value is 6.5” with “… are shown in Figure 7 (computations use a convection coefficient $ \gamma = 6.5 $”

\item Line 453: I do not understand what the authors meant with "give consistent < C > values about a mean".

\item Lines 474-475: Replace “… is a consequence of net irradiance based on the temperature difference of the base of the thermopile to the top of the thermopile” with “… is a consequence of net irradiance inducing a temperature difference between the base and the top of the thermopile.”

\item Lines 505-506: Replace “… over the cooling and heating period was below 0.6 Wm${-2}$” with “… over the cooling and heating period to be below 0.6 Wm${-2}$.”

\item Lines 514-515: Replace “… provided irradiances compared well” with “… provided irradiances that compared well.”

\item Line 580: Replace “The Reda et al., 2012 and the new equation are dependent…” with “The equation from Reda et al., 2012 and the new equation are dependent…”

\item Line 582: Replace “… provided the other coefficients in new equation are known” with “… provided the other coefficients in the new equation are known."

\item Line 615: Replace “… this is complicated by needing to divide by the transmission” with “… this is complicated by the need to divide by the transmission.”

\item Lines 653-654: Replace “Further work is required to ensure that confirms the ACP…” with “Further work is required to confirm the ACP…”

\item Lines 668-669: Replace “… were reduced but not eliminate” with “… were reduced but not eliminated.”

\end{enumerate}

*** All the typographical errors and grammatical stype have been corrected as per Laurent’s comments. Where a sentence in the manuscript added to confusion not insight, it has been either removed or modified to make its intent clear.

Response to Laurent

Hi Laurent

Many thanks for your comments - I sent a long reply but it seems to have been rejected.

I noted your indications of typographical comments - and will be incorporating those.

As to your main issues:

1. Uncertainty analysis: based on your suggestion I will put in two (a) in the solar section, and (b) a new sub-section after the last LSQ method. Both will be short so not add to the paper.

2. Re deleting the solar section: I have a number of reasons for putting in this section and am loath to remove it. I will reword this sub-section to highlight why it is important - even as a guess.

Sorry my long reply did not make it.

Cheers, Bruce

Final comment from Laurent

Hi Bruce,

Thanks for your plan to add full uncertainty estimates; it will be really helpful. Concerning the deletion of the solar calibration-related points, I proposed this to make the paper shorter since it is already quite long and I proposed the uncertainty estimates in addition. If you have good arguments that it is useful, I can certainly be convinced. On the other hand, I would really not encourage adding another subject such as long-term comparisons between IRIS, ACP, as proposed by Reda in another comment. I think it would be a good idea for another paper, not this one.

Concerning Reda's comment that the method described in (Reda et al., 2012) is independent from IRIS or blackbody calorimeter characterization, it is correct. However, did I not understand correctly that the manuscript proposes an update of the LSQ method using Eqs. 18-20 that *is also independent* if Eq. 20 uses a determination similar to that of Zeng et al. (2010)? If I am correct, it may be a good idea emphasizing it in the manuscript, so that it is clear.

Cheers,

Laurent

** I in full agreement with Laurent's comment that intercomparison material suggested by Reda is not the subject of this paper but is suitable for future papers.

1st Comment from Ibrahim Reda

It would be much informative and good idea to compare the irradiance results between the proposed method and my method, note that based on 6 IRIS&ACP95F3 comparisons the avergae difference is less than 2 w/m2 using my method as shown in Table 2 below.

Reda

Table 2. U_{95} of the irradiance measured by ACP95F3 with respect to SI units.

ACP95F3-IRIS	Difference, W/m^2	Standard Deviation , W/m^2
PMOD February 2013	0.10	0.83
PMOD IPgC-II October 2015	-0.57	0.31
SGP phase 1 October 2017	0.86	0.78
SGP phase 2 November 2017	-1.05	0.85
PMOD July 2019	0.75	1.11

PMOD IPgC-III October 2021	0.10	1.20
Average	0.03	0.85

Response to Ibrahim Reda

Hi Reda

There is already some disquiet on the length of the current paper and international comparisons isn't the focus of the paper.

The current paper is about introducing a new equation like the foundational Reda et al (2021) paper introducing the ACP and the LSQ method. It's just a different equation and 3+ calibration method suggestions with examples.

The idea of comparison type papers is a very good one. As there is for example now plenty (4 years) of data at Davos with ACP96 and IRIS. And as there is now evidence that a good calibration of an ACP is possible using a static BB at room temperature some other work should be in our work pipeline.

As we have discussed previously there are several ways ahead and all very interesting - but off line from this review process - I will be emailing you, Julian, Stefan and Nozomu on some potential ways ahead.

Cheers, Bruce

Further comment from Ibrahim Reda

agree with potential characterization which is a good idea to explore how the ACP is used in different applications, yet the Beauty of using my method to run the ACP is its independance from any Blackbody charcterization, since it is using the source which is the atmospheric longwave irradiance which we are trying to measure, note that the atmoseric emittance is ~ 0.7; +/- 0.15 based on atmosheric contents, which is not a blackbody with emittance 0f > 0.999 and to establish a world reference the two radiometers must be independant and have different methods of charcterization/measurement-methods, hope this helps clear my thoughts about the imortance of showing the difference in the measured irradiance using all methods. which would add value to the article.

*** Two out of three co-authors believe the irradiance comparisons Reda is suggesting should be the subject of another paper. Reda is a co-author of this paper.

Comments Stefan Wacker

General comments:

The manuscript presents a modified equation for the Absolute Cavity Pyrgeometer (Reda et al., 2012) and four new/revised calibration techniques for the operation of the radiometer. The ACP is an Eppley PIR pyrgeometer with its dome removed and a dual compound parabolic gold-plated gold concentrator placed above the thermopile. Reda et al. (2012) proposed a calibration technique to determine the ACP's responsivity under cloud-free night skies by cooling down the pyrgeometers case over a set of periods and calculating the rate of change of the thermopile output voltage versus the changing net irradiance and thus termed it as absolute. The ACP has become a potential candidate to contribute to a new primary reference for terrestrial radiation which is intended to be realized in the upcoming years. However, the original radiometer equation is incomplete as for instance convection in the gold concentrator is not considered explicitly. In addition, the authors of the manuscript showed that the Kirchoff's law is violated by the equation in its original form. As a result, larger uncertainties than the stated +/- 3.96 Wm-2 by Reda et al. (2012) can be observed under specific atmospheric conditions. The methods described in the manuscript help to explain, better understand and

reduce these discrepancies as shown in the observations taken with an ACP and two IRIS radiometers (Gröbner, 2012) between January and December 2020 at Davos. The methods to characterize key parameters of the ACP are thoroughly described and quantified. The manuscript is an important contribution in regard to the realization of a new terrestrial World reference and thus should be published in AMT.

Specific comments:

Conclusions: In my opinion the authors could give clearer information about the most relevant actions to be conducted as next steps. The authors indicated and proposed some further work. However, which action(s) would they prioritize (if any)? I got the impression that there is a bunch of work to be done. So, it might be helpful the most relevant next actions to be identified.

Technical corrections:

- Line 26: may use "…convection and other issues…" (I think not only the determination of the convection (coefficient) needs to be solved)
- Line 71: may add "Wr, is given by σT_r^4 with σ as the Stefan–Boltzmann constant and T_r as the surface temperature of the thermopile receiver".
- Line 105: Philipona et al., 1995
- Line 192: I am not familiar with the term "dome irradiance coefficient" but it may be commonly used. Otherwise may replace with "the coefficients k_1, k_2 and k_3 for domed pyrgeometers… (Gröbner and Wacker, 2012)" or similar.
- Lines 196-198: I find the sentence difficult to read; is there a verb missing? E.g., "… with the latter implying an improvement…"
- Line 201: May replace "However" by "In addition"
- Line 216: May rephrase: "Using the Jinan et al., 2010 transmission measurements with the new equation suggests…
- Line 650: "…parameters at different…"

Apologize for the delay

Stefan

*** All typographical and grammar issues have been addressed. The excellent suggestion of a recommended list of work to be done is now highlighted in a clear and single paragraph in the Discussion section of the paper. It also enabled removing a small but significant amount of words in the revised manuscript. The recommendations were listed in my responses to Stefan prior to being incorporated in the revised draft

Reply t Stefan Wacker

Hi Stefan

Thanks for you comments, and your timing was more than Ok.

Based on you excellent comment I will list future work explicitly rather than the way currently drafted.

That said ....I will list them for you so you don't have to wait....

Since the article was drafted more work has been done, and has emphasised the work suggested in the conclusions, but the key ways ahead as indicated in the paper's discussions and conclusions are:

(a) a method for determining the convection coefficient - recent work has provided a method to give an approximation for a value dependent on temperature - but that is another paper work.

(b) related to (a) a literature based approximation of the convection coefficient based on the Nussebt coefficient - also being investigated.

(c) Determine if an ACP can be calibrated in a BB environment (also suggested in the current paper - if it isn't it needs to be so I will check). I beleive the answer is yes regards BB calibrations but in conditions not normally used for a domed pyrgeometer or IRIS - am working on that now with Julian's help.

(d) A solar cal (with the concentrator replaced by a normal pyrheliometer tube) - in the paper - to provide an initial SI traceable estimate of C if the pyrheliometer is also referenced to SI without the problem of the convection coefficient.

(e) Using the heating cycle of the LSQ for calibration - also being done with the Davos data and in the draft paper. However, it does require continuous monitoring of the signal so some data collection methods need to be updated, but the Davos method is fine.

(f) Higher frequency measurements to confirm the phase difference between the base of the instrument and the thermopile (i.e. moving to 5 or less second sampling). I will add that to the paper. With now over 4 years of ACP96 data I beleive the phase difference is between 9 and 15 s.

Again many thanks for you very helpful and direct comments.

Cheers, Bruce

Reply Stefan Wacker
Hi Bruce,

thank you very much for your reply. This is very clear and sufficient.

cheers, stefan