

Response to referee comments and changes in the manuscript

Manuscript title: *Total power calibration in FTIR emission spectroscopy for finite interferograms*

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The responses here are the same as already published in the open discussion on December 15, 2025. A description of changes in the revised manuscript is added here. The line numbers of the changes refer to the latexdiff file.

Referee 1 – Comments, Responses and Changes

Comment 1: In my opinion, the most significant improvement which may be made is that this work does not adequately present the scope of this work, the wider implications of this calibration technique, and how this new technique compares to the usual method. The introduction, in its current state, is brief and does not adequately introduce the utility of the work presented.

Response 1: We acknowledge that the manuscript in its original state is too brief and we extend the introduction and the conclusion in the revised version in order to more clearly present the scope of this work and its implications on the application of total power calibration in FTIR emission spectroscopy.

We now emphasize more explicitly that there is an incompleteness in the paper by Revercomb et al. (1988) as the finiteness of real-world interferograms and its implications on the total power calibration are not considered. As such, the main motivation for this work is theoretical in nature at first but in principle relevant for all real-world measurements where Revercomb's total power calibration method is applied.

We explain more clearly the line of thought of our work: We present the mathematical description of this issue. We show that the problem of unequal-sided interferogram truncation can always be reduced to the problem of equal-sided interferogram truncation by means of a phase correction step within the calibration routine. This is important because the error introduced by the application of total power calibration with unequal-sided finite interferograms is generally larger than the error introduced by equal-sided finite interferograms. This means that the usage of unequal-sided interferograms to obtain higher spectral resolution poses no greater fundamental limitation than that of equal-sided interferograms of the same resolution. As an example, we perform a case study for the NYAEM-FTS which is operated by our group in Ny-Ålesund. We present quantitative differences between the optimal estimate on the spectrum from truncated interferograms as described by Sakai et al. (1968), the original Revercomb method applied to unequal-sided interferograms and the new method including a phase correction step, which is equivalent to applying the standard method to equal-sided interferograms. In our case, we find that errors are small in spectral regions of interest for trace gas retrievals. This result is reassuring for our application and might be generalizable to others, as major effects occur where water vapor or carbon dioxide absorption lines are very strong. In those spectral regions, however, the information content on the atmospheric state above the lowest layer is strongly limited because of saturation effects and is therefore of limited practical use. However, the quantitative relevance of the limitations of total power calibration with finite interferograms needs to be investigated

for each application individually, since it depends on the instrument, the measurement site, and the intended use of the spectra. We therefore recommend in the revised conclusion that such an analysis be performed for each individual use case in order to estimate the radiance error arising from finite interferograms, to decide on the necessity of applying the additional phase correction step for unequal-sided interferograms, and, for example, to inform retrieval window selection.

Changes 1: The introduction and the conclusion are expanded with this rationale (l. 98–121, 380–409). Also the expanded quantitative analysis of the case study (Sect. 5.1 including Table 1) as suggested by referee 1 and agreed by the authors in comments and responses 14–17 should substantially improve the presentation of the scope, utility and implications of this work and directly reflect the theoretical considerations from the response above. The results from this analysis were also included in the abstract and the conclusion where appropriate.

Overall, we shifted the focus of our paper from a more theoretical point of view to the direct application with our new instrument, the NYAEM-FTS, which makes the utility and implications of this work more explicit. The detailed instrument description is found in the new Sect. 2. In order to clearly separate the theoretical considerations from the application in the paper, we put the theoretical work in Sect. 4 and added Sect. 5 which contains the application to our instrument, including the expanded case study with detailed quantitative results as well as detector linearity and phase stability tests relevant for the calibration procedure with the NYAEM-FTS.

Comment 2: The number of references to primary sources is also lacking, and some suggestions are provided in the following comments.

Response 2: We thank the reviewer for these suggestions and provide the references in the revised manuscript.

Changes 2: Citations in the introduction (l. 28, 29, 36, 38).

Comment 3: Two instruments are introduced, the AERI and NYAEM-FTS, but measurements from either of these instruments are not strongly featured in the discussion.

Response 3: We agree and would like to emphasize that this paper is more conceptual in nature. The analysis of the NYAEM-FTS serves as an illustrative example and to assess the impact for our instrument deployed in Ny-Ålesund. But in general, we recommend that such an analysis be performed for each individual use case.

Changes 3: As described in Changes 1, we decided to shift the focus of the paper to the application to the new instrument NYAEM-FTS, see mainly the expanded Sect. 5.

Comment 4: Given that the authors have specified improvements to trace gas retrievals as one of the motivating factors for higher resolution emission FTIR spectra, there should be some introduction on exactly why current instrument resolution (e.g., AERI) is insufficient, and why higher resolution spectra would improve retrievals.

Response 4: We agree and now cite Hung et al. (2025), which demonstrates limited vertical resolution in profile trace gas retrievals from current resolution FTIR emission spectra. We explain that the vertical profile resolution can be expected to increase with increasing spectral resolution as more information on the line shape increases the sensitivity of the retrieval to different altitudes along the observed light path.

Changes 4: Added l. 36–38.

Comment 5: It would also make sense for Section 2 to be moved to the introduction, as that is background material, and for the main results (that is, Figures 2 and 3 and associated text) should be expanded, possibly showcasing wider applicability of this calibration method. Specific comments and technical suggestions are given below.

Response 5: We move the content of Section 2 into the introduction to clearly separate the background material from the new developments in this work.

Changes 5: Section 2 moved to introduction (l. 62). Main results expanded (Sect. 5.1 and Table 1) more clearly showcasing the utility and improvements of the new calibration method.

Comment 6: Lines 17-18: Emission FTIR instruments have been used to retrieve cloud microphysical properties (e.g., Turner et al., 2005; Rowe et al., 2019; Richter et al., 2022), any of which would be a better reference for cloud radiative properties. Turner and Blumberg (2019) also use AERI spectra for thermodynamic profiling of the atmosphere. Along that line, an updated (and much longer time series of) AERI trace gas retrievals is available at Hung et al., (2025).

Response 6: We thank the reviewer for these helpful suggestions and will include the references in the revised manuscript.

Changes 6: Citations in the introduction (l. 28, 29, 36, 38).

Comment 7: You could point to the limited AERI spectral resolution being one of the limiting factors for vertical sensitivity to trace gases in that work to motivate the need for improvements to emission FTIR spectral resolution for trace gas measurements. The Mariani et al., (2013) reference retrieves column amounts of gases, and so the retrieval may be (presumably) less sensitive to spectral resolution.

Response 7: We strongly agree and make this point in the revised manuscript.

Changes 7: Added l. 36–38.

Comment 8: Line 21: “In order to achieve higher resolution, which is especially important for trace gas retrievals” should include a reference. There are many suitable references among the solar FTIR community, such as through NDACC-IRWG or TCCON, which discuss fitting various line broadening effects.

Response 8: We agree and provide such a reference in the revised manuscript.

Changes 8: Added citation l. 38.

Comment 9: I would also suggest adding some additional text to motivate why one would want to use (specifically) FTIR emission spectroscopy to measure atmosphere composition (e.g., do not need sunlight, better equipped to monitor diurnal cycles, workable during polar night, etc).

Response 9: We agree and add such a comment in the manuscript.

Changes 9: Added l. 30f.

Comment 10: Line 25: “trace gas retrievals are currently being developed”. Is it possible to include an example of this in your results? I appreciate that this may be a significant amount of

effort, so I do not believe that this is mandatory for this work, but I believe it would strengthen this paper to include an example/case study of such a retrieval (possibly on simulated spectra) when this new calibration technique is applied, especially for gases which have retrieval windows coinciding with some water lines.

Response 10: We agree that this would strengthen the paper but consider this beyond the scope of this work and more appropriate for a future study on trace gas retrievals with the NYAEM-FTS.

Changes 10: No changes.

Comment 11: Line 27: “most information” is ambiguous. Does “information” here refer to the spectra or retrievals of interesting quantities from the spectra such as the previously mentioned trace gases or aerosols.

Response 11: We were referring to most of the interesting quantities that can be retrieved from the spectra and clarify this in the revised manuscript.

Changes 11: This statement was made twice, in the introduction and the subsequent section on total power calibration. We thus removed the first instance and kept the second, where it is formulated unambiguously (l. 63 ff.).

Comment 12: Figure 1: Please define the y-axis unit “a. u.”.

Response 12: We chose “radiance in arbitrary units” since the quantity shown is technically the Fourier transform of the electric signal detected after the interferometer. This corresponds to radiance but is not given in the usual units of radiance ($\text{W m}^{-2} \text{sr}^{-1} (\text{cm}^{-1})^{-1}$) because of the (wavenumber dependent) responsivity of the instrument and the possible offset from internal emission. The spectrum is given in the usual radiance units only after performing the total power calibration.

Changes 12: No changes.

Comment 13: Lines 105-106: This sentence is confusing. If I am reading it correctly, it may be rephrased as “...i.e., there is a phase shift $\phi_i(\nu)$ relative to the source signal (external) $\phi_s(\nu)$ and therefore the amplitude of the spectra. This means the spectra cannot be used because of artifacts due to the complex contribution”. Please clarify accordingly.

Response 13: This is not what we meant to say, but rather: “...i.e., there is a phase shift $\phi_i(\nu)$ relative to the source signal (external) $\phi_s(\nu)$. In the absence of such a phase shift, we can obtain the measured real spectrum (responsivity times the sum of the source spectrum and the internal emission) from the magnitude of the complex signal. However, this is not possible if a phase shift is present because cross terms arise.”

Changes 13: Added clarification in l. 211–214.

Comment 14: Lines 162-163: Provide some more detail on “We use Arctic atmospheric composition and thermodynamic profiles”. For example, specifying the precipitable water vapour is important for putting your simulated spectra in context. Depending on your location, the Arctic humidity can vary one or two orders of magnitude between summer and winter, so one spectra would likely not be representative of the range of conditions found in the region. It may also be useful to look at two spectra to capture this range, one for low humidity (e.g., 0.1 cm precipitable water) and one for high humidity (e.g., 2 cm).

Response 14: We agree and provide two spectra, representative of summer and winter conditions in Ny-Ålesund with substantial differences in precipitable water vapor, and provide the corresponding atmospheric profiles.

Changes 14: Added Figure 2. with summer and winter profiles. Added Figures 4 and 5 with spectra from both profiles. Added Table 1 with statistical analysis of both spectra.

Comment 15: Figures 2 and 3: Given that one of your main motivations is to improve spectral line fitting for trace gas retrievals, including similar plots for the windows used to perform those retrievals is important (e.g., the windows used for AERI retrievals in the Mariani et al. (2013) and Hung et al. (2025) references), especially when those windows contain some water lines.

Response 15: We provide the entire mid-infrared spectral range and evaluate particularly the windows used for trace gas retrievals in the mentioned publications.

Changes 15: Extended spectral range in Figures 4 and 5.

Comment 16: Lines 186-194: My understanding is that the particular window in Fig. 2 was chosen due to the strong water line, where the modified calibration should provide the best performance gain. The authors say the relative differences are much less when the water vapor absorption is weak - it would be good to quantify this by including the exact numbers for spectral regions of interest. Again, windows used for trace gas retrievals are suggested, especially for evaluating suitability of higher resolution spectra for such retrievals.

Response 16: We add quantitative values for the spectral regions of interest in the revised version of the manuscript.

Changes 16: We added Table 1 with the quantitative analysis and discussed it in the text l. 329ff.

Comment 17: Lines 202-205: Add the exact quantification for specific claims in the conclusion. E.g., for the statements “We have quantified the deviations from the best estimate...” and “...provides a lower limit of the error introduced by water vapor lines...”.

Response 17: We add quantitative values to the revised conclusions.

Changes 17: See l. 399, 407f.

The technical corrections are applied in the revised manuscript.

Referee 2 – Comments, Responses and Changes

Comment 1: I have a major problem with this paper: It does not become clear, why the calibration method described by Revercomb et al. should not work for NYAEM-FTS, since they propose a method which is also valid for one-sided interferograms (page 2315, left column, last paragraph). This is basically represented in eq. 15 of the present paper. The argumentation of the authors, why this equation should not be valid for finite interferograms, is not convincing to me.

Response 1: We think that the claim made by Revercomb et al. (1988), that the presented

method is also valid for one-sided interferograms, is incorrect. That is because removing the negative axis of the interferogram adds a contribution from the Hilbert transform of the spectra in the denominator and the numerator, which do not cancel in the quotient. The resulting spectra from Revercomb’s method thus differ, depending on whether the interferograms are double- or single-sided. While our paper contains the theoretical treatment, we include a toy model at the end of this document, which consists of a quotient of two Gaussian lines with same peak position but different amplitude and width to demonstrate this. We think it may be helpful to include this as an appendix to the manuscript as well.

Further, it is not the same if we truncate the interferograms of the input spectra and then take the quotient, which corresponds to the convolution of the input spectra with the Fourier transform of the apodization function, which we call the scanning function, or if we convolve the final calibrated spectrum with the scanning function. This is also illustrated with the Gaussian line toy model at the end of this document. This is important because the former corresponds to what happens if we apply Revercomb’s method to real-world finite interferograms. The latter is what we usually want as an acceptable spectral estimate following Sakai et al. (1968).

Those two points are the main motivation for our paper.

Changes 1: Added appendix to the manuscript.

Comment 2: It is extremely hard to follow the argumentation, because the symmetric and asymmetric truncation functions are not given explicitly. . .

Response 2: We do not give an explicit truncation function since the argument is independent of the specific truncation function and valid for any such function.

Changes 2: No changes.

Comment 3: . . . and thus, also the results shown in Fig. 2 are not traceable.

Response 3: We specify "boxcar apodization" in the revised manuscript to make clear which form of truncation is used in the example.

Changes 3: L. 310.

Comment 4: I have the impression that some of the deviations between the different methods may stem from approximations (e.g., taking only the real part as stated in line 147) which are maybe not justified.

Response 4: This is not the case. Taking the real part is not an approximation. Since S_a is a real quantity, the real part of $S_a * \tilde{A}$ is indeed exactly the convolution of S_a with the cosine transform of the apodization function as stated in Eq. 20. And this is the desired spectral estimate following Sakai et al. (1968).

Changes 4: No changes.

Comment 5: Overall, the results are not convincing and the need for a modification of the Revercomb calibration approach is not clear (I don’t see why a correct application of the Revercomb approach should not work).

Response 5: Revercomb et al. (1988) do not discuss the impact of finite interferograms in their calibration method. In general, in the case of finite real-world interferograms, we accept as an estimate of the measured spectrum the convolution of the actual spectrum with the cosine

transform of the apodization function as presented in Sakai et al. (1968). Since the calibration method by Revercomb includes the quotient of two measured spectra, we need to take into account that we do not get as final spectrum the atmospheric spectrum convolved with the cosine transform of the apodization function, since taking the ratio and the convolution do not commute.

Changes 5: No changes.

Comment 6: One motivation of the authors for this paper is that a quantitative error analysis is needed for the NYAEM-FTS. Such an error analysis would be a valuable publication, and the aspects of phase issues induced by beamsplitter emission and water vapour lines in the calibration data could be addressed there - preferably supported with real data.

Response 6: Such an error analysis is interesting but beyond the scope of this paper. This paper is thought of as a theoretical investigation of general interest on the limiting assumptions in Revercomb's calibration method, which is in principle relevant to all real-world measurements. To which extent this is quantitatively relevant needs to be addressed for each instrument/measurement condition individually. In addition to the theoretical considerations, we show an example case for our instrument, the NYAEM-FTS, as during the preparation of an error estimate there, we encountered the effect discussed here.

Changes 6: As described in Changes 1 to the comments by Referee 1, we shifted the focus of the paper towards the application to the NYAEM-FTS. We also included a test of detector linearity and phase stability relevant for the calibration of the new instrument.

Comment 7: In the first part, theoretical considerations are presented, which basically repeat the calculations already given by Revercomb et al.

Response 7: Yes, to provide the complete picture and we extend the consideration to the case of absorption lines in the black body spectra, which is not done in Revercomb et al. (1988). This is important for the main results of the paper in Section 4 and 5.

Changes 7: No changes.

Comment 8: Section 2 repeats the calculations already given by Revercomb et al.

Response 8: We do this to make the difference between Revercomb's considerations and our extension clear. As a side-effect, this makes the paper self-contained. We will shift the content of Section 2 into the introduction to separate clearly the background material from the new developments of this work.

Changes 8: We put the content of Sect. 2 into the introduction as also recommended by Referee 1.

Comment 9: Section 3 discusses the effect of spectral absorption lines in blackbody measurements and basically states that the effect cancels out in the instrument setup of NYAEM-FTS, because the radiance from the atmosphere and the radiance from the blackbodies passes the same air masses. This is important when estimating possible error sources in the calibration setup. It should be noted, however, that this is an aspect which is rather independent of the calibration method by Revercomb et al.

Response 9: As we show, the cancellation only works for ideal infinite interferograms. In real-world measurements, the absorption lines do not exactly cancel because of the effects discussed

in our paper, and the error needs to be quantified. This problem arises because of the way Revercomb's calibration method works in combination with absorption lines in the black body spectra as discussed in Section 5.

Changes 9: No changes.

Comment 10: Section 4 deals with the the problem that in FTIR emission spectroscopy, the different phases of the instrument contributions have to be taken into account, such that simply using the magnitude of the spectra as input to the calibration formula does not work. This again is a repetition of the work presented by Revercomb et al.

Response 10: We also clarify how the method is extended to existing absorption lines in the black body spectra, which Revercomb et al. did not consider in their original paper.

Changes 10: No changes.

Comment 11: At the end of the paragraph the authors state that "it is only true for infinite interferograms that the transmittance cancels in the quotient" without explaining why this should be the case. As long as the spectral resolution of all types of measurements (atmosphere and blackbodies) stays the same, I don't see why the transmittance should not cancel.

Response 11: As we state in the second part of this sentence, we give the explanation in Section 5 which contains the main rationale of our paper in which we show mathematically why the transmittance does not cancel for finite interferograms.

Changes 11: No changes.

Comment 12: In Section 5, the effect of a truncation function, which describes the finite interferogram measurement, is discussed. Unfortunately, neither a symmetric, nor an asymmetric truncation function is given explicitly, such that it is difficult to follow the argumentation.

Response 12: Our argument is independent of any specific form of truncation function. Specifying one would unnecessarily restrict the generality of the argument.

Changes 12: No changes.

Comment 13: Next, I don't understand the intension of eq. 19. It tells me that the calibrated spectrum is the input radiance, convolved with the truncation function, but it does not make use of any calibration measurements anymore.

Response 13: The input for this equation are the three input spectra, i.e., the three calibration measurements. To make analytic calculations we insert expressions that represent the requirements used in the Revercomb method. The intention of Eq. 19 is to show that in the case of no absorption lines in the black body spectra, we obtain the spectral estimate that is desired following Sakai et al. (1968) after calibration. We show in the subsequent part of the paper why this is not the case in presence of absorption lines in the black body spectra.

Changes 13: No changes.

Comment 14: In eq. 20, only the real part is taken, although the truncation function is asymmetric. If I understand Sakai et al. correctly, the assumption of eq. 20 is only valid if the asymmetry is small compared to the interferogram length which is not the case for NYAEM-FTS. So, again, what is the use of this equation? How valid is this approximation for NYAEM-FTS?

Response 14: To our understanding, this is not the claim made by Sakai et al. They state that in the case of a symmetric interferogram, the truncation needs to be symmetric if we want the magnitude of the resulting spectrum to be the desired spectral estimate i.e., the atmospheric spectrum convolved with the cosine transform of the apodization function. They also state that for a not too asymmetric interferogram, the truncation must be symmetric around the point of stationary phase if we want the phase of the spectrum from the finite interferogram to be the phase of the spectrum from the infinite interferogram. They also show that for a symmetric interferogram, the real part of the Fourier transform of the finite interferogram always corresponds to the atmospheric spectrum convolved with the cosine transform of the truncation function, no matter the asymmetry of the truncation function. This is the same argument we use in Eq. 20.

Changes 14: No changes.

Comment 15: For the simulated dataset, which is described in lines 160 ff, I am missing a description of how the instrument self-emission, including beamsplitter emission with a phase of 90 degrees is included. If such instrument contributions are not added, you cannot demonstrate that they are handled correctly during calibration.

Response 15: While this is interesting on its own in the error analysis of a specific instrument, this is not the point of our paper. For our argument it suffices to make the same assumptions for the instrument self-emission as Revercomb et al. (1988) did, i.e., that they cancel in the difference of the complex spectra or the interferograms.

Changes 15: No changes.

Comment 16: Concerning the discussion of the convolution effect, it is not clear to me, what exactly is done. The main point is that the truncation is not explained, especially the difference between the symmetric and asymmetric truncation. The stronger the asymmetry, the more the result of eq. 20 will change compared to the case of the symmetric truncation.

Response 16: We hope to have clarified this above.

Changes 16: No changes.

Comment 17: Since it is not well explained what exactly is done for calculating the best estimate, the equal sided Revercomb and modified unequal sided Revercomb (which seem to give identical results) and unequal sided Revercomb, respectively, I also cannot follow the results shown in Fig. 2.

Response 17: We will extend the manuscript to include that the best estimate is obtained by transforming the real atmospheric spectrum S_a to an interferogram, performing a symmetric boxcar truncation and transforming back into the spectral domain. This is equivalent to a convolution of the atmospheric spectrum with a sinc scanning function. The equal-sided Revercomb is obtained as in described in l. 160 ff. as follows: Create artificial difference spectra $S_a - S_c$ and $S_h - S_c$, multiply an artificial phase function $e^{i\phi_s}$ to it, transform into the interferogram domain, truncate with an equal-sided boxcar at $L = 11.25 \text{ cm}^{-1}$, transform back into the spectral domain and insert into the Revercomb formula. The modeling of the internal emission can be left out, since this contribution cancels in the difference. This is the basic assumption of Revercomb's method. To test its validity by testing the phase stability of a specific instrument is important in the error analysis for each measurement setup but not the point of this paper.

The modified unequal-sided Revercomb method is equivalent as shown in Eq. 23. The unequal-sided Revercomb is obtained by performing the same procedure but with asymmetric boxcar interferogram truncation at -1 cm^{-1} and $+11.25\text{ cm}^{-1}$ around the ZPD. In order to account for the information doubling around the ZPD, a linear ramp function is applied from -1 cm^{-1} to $+1\text{ cm}^{-1}$ around the ZPD as is usually done for the Fourier transform of unequal-sided interferograms.

Changes 17: Added l. 316ff.

Comment 18: When it comes to the effect of water vapour lines in the blackbody spectra and the truncation of interferograms, it is not clear why these have an impact on the calibration quality (in the frame of this theoretical study). As long as the interferogram truncation is identical for the blackbody and the atmospheric interferograms, the line shapes should be affected in the same way, such that no error is introduced.

Response 18: As we hope to have clarified, this is not the case as truncation and taking a quotient are operations that do not commute.

Changes 18: No changes.

Comment 19: In real life, I would still expect an effect if the lab conditions are not perfectly stable and humidity and/or temperature of the lab air vary over time.

Response 19: This is an interesting question on its own and will be addressed in a paper about the NYAEM-FTS instrumental setup and error analysis. This is however beyond the scope of this paper.

Changes 19: No changes.