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Geoscientific Instrumentation Methods and Data Systems Discussions

Interactive comment on "Results from the intercalibration of optical low-light calibration sources 2011" *by* B. U. E. Brändström et al.

Anonymous Referee #1

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Brändström et al., "Results from the intercalibration of optical low-light calibration sources 2011"

The manuscript discusses a very significant and too often neglected topic about cross-calibration of optical instruments used by many different institutes and universities at different locations in several countries. This type of collaboration not only results in improved data quality but also promotes good instrumentation techniques. The journey from raw data to a first-rate data product for geoscience studies is far from trivial. I strongly encourage the authors to continue on their path. Publications of this kind of work is highly recommended.

There are, however, two significant weaknesses in the manuscript that need to be addressed first and a major revision is necessary:

1) I found the treatment of light standards and their tracing to international standards too ambiguous. In theory, any light source that can be traced to a known standard can be compared (intercalibrated) to another light source that is also traced to the same known standard. The concept of national and international standards then makes global comparisons possible (with error estimates). In addition, I was not convinced about the traceability of the prime light source (Fritz-Peak) as the text only mentions its calibrations in 1960's and late 1970's!

2) The novelty in the manuscript is, in my opinion, limited to collecting most if not all earlier results in a form of a report. This is of significant practical value in itself, but I feel that the full potential of a comprehensive consortium of instrumentalists in multiple institutes and nations is not reached. Perhaps, the experts could agree on a common intercalibration procedure that could be recommended for all users. One should also ensure that the most critical information from the physics point of view is captured.

In the following, I will elaborate details of my findings.

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1. Introduction

The introduction (page 93, line 16) does not clarify at all, why absolute measurements are becoming more important. Is this due to new analysis methods or has the use of auroral or airglow data changed recently? This is the motivation for the whole intercalibration effort and should, in my opinion, be elaborated.

The concept of the unit Rayleigh is, for lack of a better word, impenetrable. The references, which seem to be the "classics" in this field, also appear slightly contradicting in their use of terminology. In addition to this, the authors use Ångström to "avoid confusion" (page 94, line 3) while Baker et al. 1976 (and Baker, 1974, referenced therein)

make a conscious effort to convert everything into SI-units.

As the use of Ångström is officially discouraged by several international organisations, I would really prefer the authors to follow Baker's example and use SI-units for wavelengths.

This would also be a perfect opportunity to first clarify and then confirm the common understanding of the unit Rayleigh at the collaborating institutes and universities. An unambiguous reference and calibration instructions for future would be a remarkable contribution to the science community.

Page 94, line 5: perhaps the sentence should clarify that optical instruments for aurora and airglow are calibrated by using the concept of column emission rate?

Page 94, line 9: is there a reference for Michael Gadsden? This is certainly not common knowledge.

Table 1: I propose that the authors would label the calibration workshop in Kiruna and Sodankylä simply 2011a and 2011b (and change the text accordingly including caption of Table 3). Does the label "Sources" refer to number of different light sources?

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2. Calibration sources

The treatment of calibration sources (page 94-96) leaves some critical items wide open. The most severe question is whether the Fritz-Peak international standard can be considered valid any more. The authors refer to calibrations performed more than 20 years ago! No proof is provided to validate the claims of stability since 1980's. Are you really sure that you can rely on the assumed light levels? If there is no proof available such as recent and actual measurements, you have only obtained a relative calibration of light sources. Traceability to a valid national or international standard is to be confirmed. An intercalibration without an absolute reference is still a highly desirable result, but the authors should address the traceability to an international standard with high priority.

Has any of the source owners used their respective national standards to gauge the light output of their equipment? If this is done properly at each institute and university, you should be able to predict the outcome of the intercalibration effort. This would then provide a very useful validation point in addition to recording historical metadata that will assist the scientists in using the final data products.

It would be of great benefit for readers to provide the measured (relative) spectra of all sources. The spectral response and quantum efficiency of the reference photometer should also be provided. In my opinion, this would help in interpreting and would also support discussing the results, especially in the case of the LED-based sources.

Page 95, line 22: In my opinion, regular calibration of LED sources can and do provide long-term stability. For a commercial example, the Instrument Systems have a product (ACS-530 Calibration LED) that can be traced to national standards. Obviously, one needs established practices for regular calibrations, which is something that should be considered in any case for absolute measurements. This approach also makes it possible to replace worn-out equipment as the new light source can still be traced to the same international reference standard.

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3. Intercalibration procedure

A schematic overview would greatly clarify the intercalibration setup details. Perhaps, this could be combined with the photo of the calibration photometer (Fig 1).

What is the purpose of a multimeter (pag 96, line 13), if you are already using a frequency counter for recording the PMT-counts?

The details of the filter bandwidths were hidden in Fig 2. One can assume that you are using bandpass filters with a FWHM from 1.5nm to 4.1nm. No transmittance curves were provided: as we are discussing comparisons, could you perhaps comment on the significance of the lack of this information? Also, how does the quantum efficiency of

the PMT vary in this wavelength range?

I find that recording only three samples for each filter position is not sufficient for estimating the precision. Obviously, this issue cannot be rectified for this manuscript in a reasonable timeframe, but I strongly suggest the authors to increase the number of samples in future. For this manuscript, I would like to see a recommended number of samples with scientific justification based on desired error margins, confidence level etc.

The filter position 8 is "traditionally discarded": does increasing the number of samples still produce a poor signal-to-noise ratio?

How is the intercalibration result calculated (page 96, line 26)?

Is this procedure something that all authors could consider the "standard procedure" for intercalibrating their optical instruments in future?

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4. Results

Page 97, line 5: I find it recommendable that the authors provide the raw data for the science community. However, I would like to see more details about what they consider "unreliable data", which was removed for this study. This is related to my question about number of recorded samples for each filter.

In my opinion, you should include numeric error estimates. Very likely, it is sufficient to provide a single figure or an estimated error range that is common to all measurents. This would then support the discussion in the conclusions.

Figure 3: In my opinion, if the spectra of all sources could be provided, the data in this figure could be presented to show the long-term variation i.e. the year in X-axis (= data from Table 3). As the corresponding author kindly offers the archived data to the scientific community, it should also be possible to provide the error estimates for each

sample point. It is interesting to note that, e.g., the IRF-Lauche-lamp ratios to earlier intercalibrations has first increased and then decreased. Can you offer any reasons?

Table 3: the layout and especially the label of the second column are not clear. Please re-format. Also, I'm not sure whether it would be more useful to choose one of the lamps (which should be the one that can be most accurately traced to the international one) as the reference point (i.e. ratio 1 in case of workshop 2011b).

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5. Conclusions

As mentioned earlier, the measure spectra of all sources should be provided.

Concerning the PGI Chernouss-38AM source: do I really interpret it correctly that there is no power (current) regulation for the LED system when used with batteries? This simply sounds unbelievable from electrical engineering point of view, but if there really is a difference with a regulated power source, then I strongly suggest revising the electronic control of the LED source and making sure there is sufficient regulation also when battery operated.

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