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Atmospheric Measurement Techniques Discussions

Interactive comment on "In-situ sounding of radiation flux profiles through the Arctic lower troposphere" *by* Ralf Becker et al.

Anonymous Referee #2

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Review of: "In-situ sounding of radiation flux profiles through the Arctic lower troposphere"

Authors: R. Becker, M. Maturilli, R. Philipona, K. Behrens

General comments: The authors provide a look into recent efforts to make broadband radiation measurements from a tethered-balloon platform. In general, I believe this work to be of great value, as there is a substantial need to conduct more profiling of radiative properties throughout the lower atmosphere. In order for such measurements to truly be useful, it is important to collect profiles over a wide-variety of conditions, and development of instrumentation that can be deployed on a semi-regular (albeit weather condition dependent) manner is useful. Therefore, I applaud the authors in taking steps toward such an ability.

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Discussion paper

Having said this, there are several issues with the current manuscript that require further attention before it is ready to be published. I outline these below, doing my best to divide them between “major” and “minor” issues requiring attention. In general, I believe that this manuscript requires major revisions before it can be considered for final publication.

Major Issues:

- The authors use inconsistent terminology. It is important to distinguish between “flux” and “flux density” or “irradiance”. Please check your units and usage of these terms and make the appropriate changes (including to the title).

- The introduction goes directly from towers to balloons and kites, completely ignoring the work that has been conducted using manned and unmanned research aircraft. I recommend that the authors dig a little deeper into the history of aerial radiation measurements, as it will provide additional insight into several relevant topics, including tilt correction which I found to be discussed in insufficient detail.

- I found discussion of several topics to be lacking or incomplete. As the manuscript reads currently, it seems like a gathering of thoughts more than a thorough scientific paper. For example:

o Section 2.2: Synchronization of logging rates was deemed to not be a critical issue because of the slower response of the radiation sensors. This comment doesn’t make sense to me. Ultimately, being able to match up the radiation sensors with the sensors measuring platform attitude is still critical for tilt correction, developing profiles and more. More information is needed to justify this statement and more details on the logging system would be helpful.

o There is insufficient discussion on the tilt correction algorithm. If I understand correctly, Figure 2 shows the error associated with a 5-degree change in the solar zenith angle from ground-based measurements, but this tells us very little about the error

associated with a sensor that is misaligned by 5 degrees. That is because an actual change in solar zenith angle also changes the pathlength of the sun through the atmosphere, which is part of why there is a chance in the radiative flux density. However, changing the sensor tilt angle at a given path length has a different effect. Additionally, the example provided, while for clear sky, only considers one atmospheric state, and does not account for what happens when there is more or less water vapor present during that shift in sensor tilt. A more rigorous analysis of what the true impact of sensor tilt is needed. Additionally, a much more thorough overview of the tilt-correction algorithms applied is required, along with (particularly for AMT) a more detailed discussion of the instrumentation used to determine sensor attitude (pitch, roll, yaw). For example, is the yaw from a magnetometer? If so, what is the impact on the measurement at high latitudes? Was the magnetometer calibrated to the local declination angle before flight? What are the expected uncertainties associated with the inclinometer in a static (i.e. non-moving) condition? To me, the "calculations" section should really focus on these items, not the much more trivial equations related to radiative flux density.

- In my opinion, the radiative transfer simulations, are inadequate. For example, the microphysical properties of the clouds are assumed to be those reported by Curry and Ebert. There are many other studies that have investigated cloud microphysics in Arctic stratiform clouds in many different locations and seasons. While it is challenging to say which of these studies are most representative of the conditions observed in this case, at the very least some level of sensitivity study should be completed to evaluate how much the microphysical parameters impact the calculated radiative profiles.

- Additionally, the radiative transfer simulations offer an opportunity to conduct some sensitivity studies to parameters implicated in this study. For example, could the authors look at the impact of effective surface albedo and evaluate to what extent this impacts the profile? This would help to assess whether the differences between the measured quantities at the surface or aloft are realistic. Profiles over a range of quantities could be compared directly to the measured quantities in one of the figures.

- In general, the amount of discussion included in the manuscript is lacking, and there are numerous unclear connections made in the text. For example:

o There is reference to the cloudy conditions having higher measured albedo than the clear conditions. I assume this is due to multiple reflections, but there is no discussion on it.

o Multiple times, the variability in surface conditions and increased visibility of this variability is mentioned as the reasoning behind seeing lower surface albedo in the tethered balloon measurements than what is observed at the surface, but there is no discussion on how this is verified. For example, small errors associated with the tilt correction or attitude estimation could also result in increased downwelling irradiance, which would reduce albedo. More detail is required.

o Section 3.3: several comments on the radiative forcing are made, but it is not clear whether these are meant to be generalizations, or just for this specific case. For example, the comments on longwave flux at the end of the first paragraph in the section.

o Line 351: “only about half”: Half of what? I see drop in the LWD of approximately 140 W/m2, and a drop in the LWU of only 50 W/m2. This doesn’t seem like half, but maybe I’m misunderstanding. More detail/discussion is needed.

o There are several comments about something happening as the balloon passes through cloud top, or cloud base. However, there is generally no indication of which direction the instruments are moving during this transition. Is this from within the cloud to outside of the cloud? Or vice versa? Please be clear about these transitions in the text so that the reader doesn’t have to guess at what you mean.

o Line 436: Weak relative to what?

o Line 437: Stronger relative to what?

- There is no discussion on sensor riming within a supercooled cloud layer. I presume that the sensors are not heated, based on the power required. How do the authors

know that riming is not a problem within cloud?

- There is limited discussion on the impact of assuming 1D radiative transfer (vs. 3D) in assessing differences between the simulated radiation and the observed values. There are likely to be implications, especially at a coastal boundary with multiple surface albedos.

- There are no estimates of the uncertainty of these measurements. Ultimately, these are critical for evaluating their value.

- The figures need to be more clearly explained in the captions. Line types and colors should be clearly and consistently explained in the captions. Additionally, maybe I missed it, but what does the sigma represent in the captions for figures 7, 8, and 9?

Minor Issues:

- I believe that “Key and Schwaiger” should be Key and Schweiger. Please check the spelling.

- Section 3.2, second paragraph: Flat terrain or not, it’s the radiative transfer processes that control the measured radiative flux density. Just because the terrain is more complicated, doesn’t mean that it’s not radiative transfer impacting the measurements. . .

- A satellite image/map of the flight area would be useful. The authors could even include some range rings indicating the position of the balloon at a given altitude, assuming a tether tilt angle.

- Was the instrumentation directly below the balloon? Or how far below the balloon were the instruments mounted?

- There are some limited grammatical issues, and I recommend that the manuscript be reviewed for spelling and grammar.

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