Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2018-173-AC2, 2018

Atmospheric Measurement Techniques Discussions

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

Ralf Becker et al.

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Received and published: 12 October 2018

The authors wish to thank the reviewer for his critical comments that provided a constructive guidance to improve the manuscript. Please find our detailed response below.

Ref: 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 terminology will be corrected/streamlined.

Ref: 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 mea-

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surements, as it will provide additional insight into several relevant topics, including tilt correction which I found to be discussed in insufficient detail.

-> The revised manuscript version shall discuss results of research aircraft activities including the tilt correction issue.

Ref: 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: 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.

-> all sensors and receiving systems are synchronized whether they are fast or slow. We define 'no critical issue' that we ignore the sub-second range.

Ref: 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 exam-

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ple, 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

-> We see your point, it is one of the major issues of the other referee, too. Rethinking this aspect, the idea to make of use surface based measured data shall be rejected because of too much uncertainty entering. We'll refer to the correction equation provided by Bannehr & Schwiesow 1992 further on. - The magnetometer of Vaisala meteorological sonde is calibrated onsite before flight. It is assumed that the error in wind direction caused by the deviation between magnetic and geographic north is lower than the uncertainty of measurements itself (5 deg.). This error needs to be regarded in an uncertainty assessment.

Ref: 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.

-> Due to the lack of instantaneous observations the input parameters were selected according to Curry & Ebert (1992). A sensitivity study to estimate the impact of these

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parameters was considered to be beneficial, the same with effective surface albedo (see below)

Ref: 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

-> Albedo cloudy/clear-sky: on average the diffuse albedo profiles show higher values than in clear-sky conditions. From clear-sky to overcast, the drop in shortwave downward radiation is stronger than in shortwave upward. (Figure 6)

Ref: 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.

-> The issue 'variability in surface conditions' implies two main aspects: in case of changing cloudiness conditions like 'cloud-shadow on surface and sun-exposed lifted pyranometer' and vice-versa may happen. This is expected to result in a peak (a drop) in albedo, respectively. You'll never get this with near-surface observations. The other source of surface variability regards surface albedo. Near-surface observation is staring at snow surface whereas lifted radiometers sense a mixed scene composed by snow-covered land and ice-free water. This aspect could be investigated further in the RTM section.

Ref: 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

-> Sect 3.3/L351: discussion regards measurements taken on May 12, 2015, only. Above cloud LWD is about half of LWU, below and in the cloud it differs by only 10 to 20 W/m2. To be reformulated

Ref: 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

-> Transitions shall be marked, preferably in section 4.2. Concerning gradients it doesn't matter

Ref: Line 436: Weak relative to what? Line 437: Stronger relative to what ?

-> L436/437: compared to each other

Ref: 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?

-> Riming can be a problem, and it should be discussed here shortly. We did not observe riming on May 11th and 12th when we got the desired shallow low-level clouds. It takes about 6 minutes for the instrument to descent from the cloud base to the surface, and ice or riming was found neither on the domes nor the instrument bodies. Considered that there was no insolation supporting melting/sublimation we would have seen it then.

Ref: 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

-> 1D vs 3D simulations: possible implications should be mentioned

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

-> A solid uncertainty estimation is needed

Ref: 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?

-> Better explanation of figures. Sigma in Fig 7,8,9 means standard deviation of geometrical height of the balloon (sensor)

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