Review of amt-2021-326: “**Radiative fluxes in the High Arctic region derived from ground-based lidar measurements onboard drifting buoys**” by Lilian Loyer, Jean-Christophe Raut, Claudia Di Biagio, Julia Maillard, Vincent Mariage, and Jacques Pelon.

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Overview

This paper documents an initial evaluation of whether the background noise from the lidars installed on IAOOS drifting buoys in the Arctic Ocean can be used to estimate at least some of the components of the surface radiation budget. The approach relies upon combination of the lidar noise measurement combined with information from a radiative transfer model, along with some input from ERA5 reanalysis fields. The method progresses through a number of steps, some of which depend upon assumptions about conditions. The impact of these assumptions is only explored for some of them, and the uncertainties introduced by the others remain unquantified.

While well written overall, the method described is complicated, and there are a few places where the discussion becomes confusing and would benefit from being clarified – this often results from a term being used before it is defined – these are noted below. There are also a number of grammatical slip-ups and typos, detailed below.

Major comments

While the approach documented appears to work remarkably well, I have some concerns about the potential for self-correlation to give a false sense of its effectiveness a result of the use of the same data points both for calibration and evaluation of the lidar data.

The entire approach rests on the determination of the relationship between the lidar noise, B, and downwelling scattered shortwave radiance for the lidar’s (narrow) field of view, $L_s^{\downarrow}$. This is instrument specific and determined by fitting a function to measurements of B and $L_s^{\downarrow}$, ideally – as noted in the paper – this would be done for clear sky conditions. The authors note that there are very limited periods with clear skies in the data set, and they opt instead to use a slightly less direct approach. They use a value of $L_s^{\downarrow}$ estimated for cloudy conditions, with $L_s^{\downarrow}$ derived from a radiative transfer model, STREAMER, with a cloud optical depth estimated from the directly measured total downwelling shortwave irradiance, $F_{SW}^{\downarrow}$. This is shown in figure 4. On the basis of this figure alone, which shows a very strong linear relationship, I think the proposed technique is almost certainly useful. However, when the full technique is implemented: lidar noise is used to estimate $L_s^{\downarrow}$, which is used with the STREAMER results to estimate cloud optical depth and $F_{SW}^{\downarrow}$, the final results are evaluated by comparison with the same N-ICE data used to calibrate the lidar in the first place, and with the same assumptions in place about other quantities that might affect the results: surface albedo, cloud thickness, and cloud microphysical properties. While it is clear that there is a strong relationship between B and $L_s^{\downarrow}$, if some bias is introduced into the calibration because of an invalid assumption – use of a fixed albedo (perhaps too high or low), cloud microphysical properties, cloud thickness – which might affect either the gradient, K_L, or offset, b, of the linear fit, then that bias CANNOT be identified by the evaluation used here.

The error analysis provided in the appendices deals effectively with random errors – the uncertainty deriving from inherent uncertainty in the measurements or assumed values, but not with any potential mean bias that might arise from the initial calibration.

I appreciate the problem the authors face – they are trying, after the fact, to derive quantities that were never intended to be measured by this instrumentation, and lacking some of the support measurements that one would make if planning this prior to the field campaign. They're done a good job, but could perhaps address the problem of potential calibration bias more effectively.

The available clear sky data is very limited, but it is not zero – May 23 is stated to have 24 hours of clear skies. Do estimates of *B* and $L_s^{\downarrow}$ from clear sky conditions – however limited – fit the function derived from the cloudy cases?

The period used here, is a 44 days, but only 18 discrete lidar measurements are used, each a 10 minute average, from ~4 measurements per day, so about 10% of the 176 total measurements. While the method clearly has merit, does this very sparse data set imply it's operational use would be likely to return similarly sparse data?

Detailed comments

Line 30: the authors state "clouds cover up to 80% of the region at all time and are primarily composed of low-level mixed phase clouds". This true for the summer but not necessarily for the winter season when low level clouds are less frequent.

Line 64: regarding the reference to MOSAiC. Keep an eye on https://online.ucpress.edu/elementa/search-results?fl_SiteID=1000091&page=1&tax=231 – the initial programme overview papers are currently in press, and should be available by the time this paper is accepted.

Line 54: "The surface cloud radiative effect is therefore positive from September to April-May and negative in summer" – it should perhaps be acknowledged that this is also a function of latitude.

Line 159-161: The cloud used for the STREAMER simulations is defined here, and has a fixed altitude and depth. I appreciate that for a given optical depth this probably doesn't greatly affect the results, but I wondered why these weren't height and depth were not taken directly from radiosonde profile – at least for the calibration and to evaluate the range of any impact. Using a better estimate of the actual cloud properties for the calibration of lidar noise against scattered radiance, rather the fixed values, which are then also used for the determination of optical depth from lidar noise, would eliminate one of the closed-loops when validating the method against the same N-ICE data used for the calibration.

Figure 4: It would be useful if on the right hand panel, a line were overplotted showing the location of the peak in L_s as a function of θ - it would help make clear the ambiguity in τ

Figure 5: the caption states that the results here are plotted for 6 values of τ (the different coloured points) and 'various values of θ' – I assume that at a given τ each point corresponds to a different value of θ, but that is not obvious from the figure or caption.

Lines 190-194: this brief description is the closest thing given to a complete description of the method proposed. The various components are covered in more detail in various sections, but it would be useful to give a clear, step by step, breakdown of the full method. Here the description is rather vague, e.g. "…*B* derived from the lidar helps to determine…"

Line 195: “is simply obtained from the equation detailed by Minnis et al. (1993).” – Minnis et al. (1993) has a lot of equations, none of which perfectly match this one. I think you refer to equation 21, but please cite the intended equation explicitly.

Equation 5: the term $\in_c$ is undefined…until 10 lines below. And the term *c* – the cloud mask, isn’t defined for 5 lines. It would help the reader if all terms were defined immediately after the equation, rather than much later in the discussion.

I’m not sure I understand the full reasoning for equation 5 and its relationship with Minnis eqn 21. Two questions:

1) In the cloudy case, eqn 5 gives the downwelling LW irradiance as the sum of those for the cloud (emitted, from cloud at temperature T_c) and the surface (reflected, surface temperature T_s). Minnis eqn 21 looks like the same equation:
$F_{LW} = \in B(T_c) + (1-\in)B(T_s)$, where $B(T) = \sigma T^4$
but it addresses a slightly different situation, the upwelling radiation seen from a satellite, with cloud temperature T_c and T_s the ‘clear scene’ (surface) temperature. Here the radiation from the surface is seen *through* the cloud rather than being reflected. Why the difference for your case?
2) in the clear case you give the downwelling LW radiation simply as that emitted by the lowest level of the atmosphere (at 2m). I don’t understand how that is reasonable, why no contribution from higher levels (or the background of space?) – the atmosphere is largely transparent at the wavelengths concerned here or the measurement of cloud temperature wouldn’t be possible.

Line 253: here it is stated that there are 20 points used to calibrate the lidar noise *B* against the scattered downwelling radiance. I count 18, both on figure 7 and again on figure 6 where the lidar derived cloud optical depth is plotted along with that from N-ICE.

Line 380-381: the authors state that the fact that all LW irradiances were > 230 W m^{-2} suggests that the lidar detected clouds for the entirety of the spring period. Isn’t this forced by the fact that the data used to find the irradiances are selected based on the calculated optical depth?

Line 419: ‘The skin temperature taken from the ERA5 reanalysis may be colder than the actual skin temperature’ – recent evaluation of the ECMWF IFS model in forecast mode, but essentially the same model used to generate ERA5, has a warm bias of ~1K in the skin temperature, at least during the late summer and early autumn. See:

Tjernström, M., G. Svensson, L. Magnusson, I. M. Brooks, J. Prytherch, J. Vüllers, G. Young, 2021: Central Arctic Weather Forecasting: Confronting the ECMWF IFS with observations from the Arctic Ocean 2018 expedition, Quart. J. Roy. Meteorol. Soc. doi:10.1002/qj.3971

Grammar, typos, etc

Line 10: ‘enables to estimate’ -> ‘enables us to estimate’

Line 21: ‘twice as fast then the rest…’ -> ‘twice as fast as the rest…’

Line 27: ‘contribute to regulate the…’ -> ‘contribute to the regulation of…’

Line 37: “The underdetermined knowledge on the thermodynamical and radiative feedbacks..” – awkward phrasing, better: “The limited knowledge of the thermodynamical and radiative feedbacks…”

Line 39: “the radiative budget, which is the primary source in the surface energy…” -> “the radiative budget, which is the primary contribution to the surface energy…”

Line 71: ‘Buoys have also…’ -> ‘Buoys also have…’

Line 75: ‘fluxes from buoys lidar data’ -> ‘fluxes from the buoys’ lidar data’

Line 80: ‘to derive both optical depths and radiative irradiances’ -> ‘to derive both optical depths and irradiances’

Line 82: ‘irradiances measurements at the vicinity of’ -> ‘irradiance measurements in the vicinity of’

Line 91: ‘into the pack ice during several months’ -> ‘into the pack ice for several months’

Line 92: ‘tacked’ -> ‘tracked’

Line 114: ‘at a ice camp’ -> ‘at an ice camp’

Line 121: ‘bandwidths, respectively’ -> ‘bandwidth, respectively’

Line 129: ‘In springtime, Walden et al. (2017); Cohen et al. (2017) indicated’ -> ‘In springtime, Walden et al. (2017) and Cohen et al. (2017) indicated’

Line 134: ‘on 1 minute resolution’ -> ‘at 1 minute resolution’

Line 137: ‘of its emission’ -> ‘of its emission of LW radiation’

Line 139: ‘interpolated at the buoy location’ -> ‘interpolated to the buoy location’

Line 151: ‘same bandwidth as the one of the pyranometer’ -> ‘same bandwidth as that of the pyranometer’

Line 153: ‘by Merkouriadi et al. (2017); Granskog et al. (2018) during’ -> ‘by Merkouriadi et al. (2017) and Granskog et al. (2018) during’

Line 161: ‘water droplets overcoming a 500 m-width cloud layer’ -> ‘water droplets overlying a 500 m-width cloud layer’

Line 169: ‘and decreases afterwards’ -> ‘and decreases above this value’

Line 238: ‘corrected of the Earth-Sun distance’ -> ‘corrected for the Earth-Sun distance’

Line 279: ‘provide a rough information on…’ -> ‘provide a rough idea of’

Line 287: ‘observed the 16 May’ -> ‘observed on the 16 May’

Line 289: ‘whole set N-ICE…’ -> ‘whole set of N-ICE…’

Line 474: ‘A1 Uncertainties on τ’ -> ‘A1 Uncertainties in τ’

Line 496: ‘A2 Uncertainties on…’ -> ‘‘A2 Uncertainties in…’

Line 509: ‘only depends the uncertainty…’ - ‘only depends on the uncertainty…’