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Comment on acp-2021-279

Anonymous Referee #2

Referee comment on "Radiative energy budget and cloud radiative forcing in the daytime marginal sea ice zone during Arctic spring and summer" by Johannes Stapf et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-279-RC2, 2021

The study analyzes radiometric data collected from aircraft over the MIZ in the Atlantic sector of the Arctic during spring. The analysis begins with 22 hours of measurements and ends with a new model for the role of clouds and tipping points in Arctic Amplification. The interim is a lengthy and meandering discussion on a variety of topics related to the many atmospheric and surface conditions that influence the radiation budget. There are a number of intriguing discussions, but these do not clearly contribute to a central thesis nor do they stand alone, and there are specific problems within the individual analyses as well. By choosing a specific research question well-suited to the core data set and carrying out a thorough analysis, there are probably several promising starts to papers contained within these pages, but as it stands, I don’t feel this manuscript is ready for publication. I will provide a couple examples of my specific concerns next.

As I read the manuscript, I frequently needed to remind myself of the extremely limited sampling that forms the basis for the study. Twenty-two hours of data, spliced from two campaigns in different years, are meant to represent a springtime transition in the surface radiation balance from March through June. This represents < 1% of a single 4-month period, collected largely near solar noon at an arbitrary altitude in a handful of synoptic cycles that were suitable for flight operations. These things are acknowledged, but that is not license to proceed to overly generalizing the results. While I applaud the author’s efforts to link their measurements to other campaigns (e.g., SHEBA, N-ICE2015), such work demands a focus on understanding how these various perspectives complement one another, but this is largely glossed over in favor of a series of overstated assertions of the behavior of the system. I will provide two examples, focusing on Sections 4.1 and 4.2.

In Section 4.1, the modes of the widely-reported bimodal state are compared with surface measurements and some subtle differences are highlighted. These are interesting differences, but I am skeptical that such subtleties can be interpreted robustly. Even neglecting the sample size or the large leap in relating multiyear ice in the western arctic with the eastern arctic MIZ, there is no attempt to demonstrate that the net longwave radiation from 100 m altitude is sufficiently representative of the surface that the comparison is valid in the first place. Indeed, there are suggestions elsewhere in the manuscript that it is not, such as the first full paragraph on page 5. I'm not even certain if the upwelling longwave that is used in this study is from a pyrgeometer for or if it is derived somehow from the KT-15, and neither would be directly comparable to surface measurements.

In Section 4.2 (Fig. 5), a "four mode structure" is discussed. First, I think that Fig. 5 provides the sort of perspective that takes advantage of the strengths of the core data sets (good spatial sampling). But unfortunately, the limited temporal sampling and use of proxy are creating false impressions. The conclusion is that there are four modes, but if you plotted ACLOUD and AFLUX on top of one another, there would be at least 6 modes. More could perhaps be identified if different conditions were sampled. Even the separation of ACLOUD and AFLUX in panels (a) and (b) are used to link the position of the modes to seasons (P15L26), as if these were static features within the time periods plotted. What would happen if you plotted April/May? In actuality, the values where the bimodal state peaks are dependent on surface and cloud conditions and the figure shows snapshots of this. The figure is revealing of the degree of spatial variability within the set of conditions sampled, and this is what I suggest focusing on. Additionally, the use of albedo as a proxy is a bit deceptive. The net LW is not sensitive to albedo at all, but rather to differences in surface temperature, which albedo separates to first order. I'm guessing these four modes are less distinctive when surface temperature replaces albedo.