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Interactive comment

Interactive comment on "The influence of anomalous atmospheric conditions at Ny-Ålesund on clouds and their radiative effect" *by* Tatiana Nomokonova et al.

Anonymous Referee #2

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This paper uses 2.5 years of data collected at Ny Alesund to (a) identify the upper 10% and lowest 10% of the cases by integrated water vapor ("wet" and "dry" anomalies, respectively) and also the upper 10% and lower 10% of the cases by temperature at ∼1500 m MSL ("warm" and "cold" anomalies, respectively) for each season. The differences in the cloud properties (macro- and microphysical) for the anomalous periods relative to the "other 80%" are shown and discussed. The authors then look at the differences in the cloud radiative forcing at the surface for these anomalous conditions, and correlate them with the differences in cloud properties and IWV.

The paper is reasonably well written, the tables and figures are concise and well pre-

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Discussion paper

sented, and the references look reasonable. I have some comments / suggestions that I believe should be considered before the paper is accepted for publication. These are outlined below. The main concern I have is associated with the sampling uncertainty; I speak more about this below.

âĂć Why wasn't the cloud radar included in the instrument list / description section? It is key for the CloudNet products, which are key for the rest of the paper. âĂć Why were NWP thermodynamic profiles used in the CloudNet classification, and not the profiles retrieved from the HATPRO? âĂć Line 138: the RRTMG should be referenced, as it is a critical component to this study âĂć Are the uncertainties in the retrieved cloud properties (and in particular the phase classification) important to this study? âĂć What vertical separation is required to identify multi-layer clouds? âĂć Are the backtrajectories allowed to touch the surface? Or if they touch the surface, do you call that the origin point for the trajectory? âĂć Line 168: The HATPRO period is from 2011 to 2018. This isn't a very long period. Using the longer radiosonde record, how representative is the 2011-2018 period? It is clear that the mean values won't be the same (hence the trends over time), but is the range of variability (e.g., standard deviation of the IWV)) the same for the short period as the longer period? âĂć Ditto for the 2016-2018 period. âĂć I have a lot of questions regarding the sampling uncertainty, especially in the 2016-2018 dataset. The authors themselves hinted at this at line 225 when they say "Such a short period of time would probably not be representative...". Table 1 shows that the number of cases in these "outlier" categories is small (less than 100, often less than 50). This is, by far, the biggest weakness of the paper. The authors must augment their discussion to talk about sampling uncertainties, which includes the two questions above regarding representativeness. âĂć Line 227: you are talking about the "-T -IWV" cases, and stating that the LWC and IWC are larger than in normal conditions. This is indeed counterintuitive. Perhaps the authors could look at the column RH value (computed as IWV / saturated_IWV, where the temperature profile is retrieved from the HATPRO) to see if there are any differences in this value? It would seem that the column RH in the "-T -IWV" case must be larger than the normal conditions for the cloud water paths to be larger. . . âĂć Section around line 249, where the discussion focuses on ice clouds: I think that the authors should consider breaking the analysis into high ice clouds (e.g., cirrus) and “boundary layer” ice clouds. Generally speaking, I would not expect the former to have much of a dependence on IWV, whereas I can see how the BL ice clouds could depend on changes in IWV. âĂć Line 266: To state that there is a 2x increase in LWP is not really clear enough. If the LWP is less than 10 g/m2, then a 2x increase is 20 g/m2 which is still close to the uncertainty in the HATPRO LWP retrievals. Would those retrievals have sensitivity to the atmospheric state, or in other words, is this 2x change in LWP an artifact of the retrieval? âĂć Line 269: again, where the ice clouds are located vertically may be important for this statement. âĂć Line 291: need to add units to the “0 to 85” âĂć Line 464: “excess and shortage” are odd words here. I think this phrase must be changed to be more clear âĂć Line 466: “reduction of LWP and IWP by an order of magnitude” seems to suggest both are decreased by a factor of 10, when I believe you only mean the IWP is changed by a factor of 10. âĂć Line 500: “patterns” is misspelled âĂć Fig 6a: Is the autumn “-T -IWV” bar where it is due to that one 5-day period? I think the answer is yes, and this is a great example indicating that the sampling errors must be better discussed. And a note should be made in the caption here. âĂć Fig 6c: it would be nice to have a horizontal line at CRE = 0.

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