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Earth System Science Data Discussions

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Interactive Comment

Interactive comment on "Climatology and time series of surface meteorology in Ny-Ålesund, Svalbard" *by* M. Maturilli et al.

Anonymous Referee #3

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Overview: This paper presents basic, near-surface meteorological data from the Ny Ålesund, Svalbard, site run by the Alfred Wegener Institute. The tables and figures presented illustrate that the data are of reasonable quality, though some additional analyses could be done to show this better (see comments related to data quality). The authors mention that this data set is different from the longer-term data at the nearby Norwegian Meteorological Institute because of its higher temporal resolution, but fail to support this point and illustrate it with an analysis that utilizes this temporal resolution. Hence, they don't motivate why this data set provides anything different than already available (see Comments related to the uniqueness of this data set).

A major concern regarding the data and this paper is the representativeness of the data for the surrounding area. Observations in complex terrain are notorious for only representing a very small area near that particular site. While I don't expect this data paper to go into the analysis to sort out the complex physical processes occurring at Ny Ålesund, I feel that the authors need to mention the great uncertainty of how representative the measurements from this site are for the surrounding area, and encourage such studies to be done. This paper needs to include more discussion on this topic. If the authors want to retain their claim that the data "surely indicates Arctic variability and change when looking at synoptic time scales", they need to show more evidence of this by doing substantially more analysis. I have my doubts as to the veracity of this claim. (see Comments related to the representativity of the data)

Also, because this is a data paper and does not need to go into detailed diagnostics to achieve understanding of the complex processes in this unique environment, only a few analyses and analysis interpretations are needed to illustrate the quality of the data and a few of the processes captured by the data. For instance, this data set would certainly be useful in performing the process studies mentioned previously, especially because of its high temporal resolution. It would also be very useful as representing one site for mapping out the spatial variability of atmospheric conditions in the Svalbard region, and for use as validation of any modeling studies of the area. These aspects could be mentioned by the authors, and one or two such analyses done to illustrate the usefulness of the data. Furthermore, the authors also need to make sure that the data interpretations offered are physically sound. The comments related to data interpretation mention some concerns of the interpretations.

These major concerns are described below with additional details, as are some more minor suggestions.

Major Comments: 1) Comments related to the data quality:

a) A table can be provided showing the instruments used and the changes for each parameter. This would be more useful than trying to describe it in the text, and would shorten the text. In the text, mention only the instrument changes that directly impact

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the interpretation of the data, trends, etc.

b) bottom of pg. 1060 and top of pg. 1061: Were there any calibration of the temperature sensors? If so, have these been applied? If not, what is the likely error and/or drift in the data?

c) pg. 1061, l. 24-29: You could use the differences of the temperatures at the two heights to plot the variability of low-level stability, assuming that the calibration and errors in the data set allows this. If a physically reasonable stability estimate is obtained, it would demonstrate the accuracy of the data set. Furthermore, if the temperature calibrations are good so the stability is representative, an estimate of the sensible heat flux might be made using the winds and a bulk method. This would also improve the data completeness and usefulness.

d) section 2.2: You should also show relative humidity with respect to ice. Studies over Arctic sea ice have shown that this is near saturation year-round (e.g., Andreas et al 2002; JGR). How does it vary at a fjord coastal site like Ny Ålesund? Does airflow from the interior of Svalbard produce periods of much subsaturated air? This has implications for clouds, fog, and instrument riming. This calculation will also illustrate the quality of the humidity measurements.

2) Comments related to the uniqueness of this data set:

a) pg. 1066, l. 20-21: the authors of this data set mention that the Norwegian Meteorological Institute has a data set extending back to 1935 but argue that the higher temporal resolution of this new data set, which is substantially shorter in length, "will be of value for atmospheric process studies on shorter time scales." While I agree with this in principle, it would be very useful if the authors give an example of an analysis that can be done with this new 1-minute or 5-minute data set that can't be done with the longer-term one. They also don't mention the time-resolution of this longer time series. Also, where is the measurement location of this long-term data set with respect to this one from AWI? In complex terrain, short distances can produce meaningful differences.

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3) Comments related to the representativity of the data

a) pg. 1060, l. 23-25: " . . . Ny-Ålesund may not be a representative site location for the Arctic in general, but surely indicates Arctic variability and change when looking at synoptic time scales." I am confused by this statement, and have a major concern with the spatial representativity of this data. Are the authors saying that one should not use this data for looking at variability and change other than on the synoptic time scale (e.g., 3-5 days) and longer? If so, they have then contradicted their argument for the uniqueness of this data set compared to the much longer data set from the nearby Norwegian Meteorological Institute.

Furthermore, variability and change on even these longer time scales are strongly impacted by the local topography and are likely not representative of variability and change elsewhere. The local terrain is very substantial, and the interaction of the synoptic flow with the local terrain can produce events and phenomena at this site that may not be produced anywhere else or be representative of changes occurring even short distances away. For instance, if a certain climatological storm track produces synoptic winds that interact with the local topography to produce local downslope winds and relatively warm temperatures, a change in such a storm track due to global warming can locally produce any magnitude of warming or even cooling, depending on how this storm track change impacts its interaction with the local terrain. Ralph et al (2003; JHydrometeorology) illustrates how the change in airflow relative to the topography can locally produce the opposite effect of the larger-scale climatological trend for a mid-latitude site.

Also, Ny Ålesund is located on the north side of a 1500 m high ridge that extends ESE to WNW, and this ridge blocks much of the southerly Atlantic Ocean flow at low levels. Hence, the temperature, humidity, precipitation, and winds in Ny Ålesund undoubtedly don't represent the climate or weather in the lowest 1500 m just to the south of this ridge. The wind data presented in this paper shows that at low levels the winds are parallel to this ridge, with flow almost exclusively from the glaciers to the SSE in winter.

Hence, the low-level Ny Ålesund data are more representative of the climate in this particular fjord than of anywhere else.

In summary, the word “surely” is more of a hope by the authors and is not substantiated by anything in this paper. Variability and change at this site almost surely does NOT represent variability and change elsewhere, except perhaps within this fjord, even at longer time scales. To convince readers that your claim of representativity of variability and change is true, you must first characterize the microclimate of the site, and then discuss how this microclimate would change with a changing synoptic (or larger) environment. This may be beyond the scope of this paper. I think the best that you can claim for this data set is that it provides an opportunity for scientists to examine the detailed processes producing the microclimate at Ny Ålesund. These processes are undoubtedly present elsewhere in the Arctic with complex terrain in a coastal region, though the Ny Ålesund microclimate and the variability occurring there may only be occurring at a few (if any) other locations in the Arctic.

b) This paper clearly needs a topographic map of the area surrounding this site. This map needs to include the topographic features influencing the local observations, including the nearby ice fields, mountain heights, etc. The locations of other observing sites, such as that of the Norwegian Meteorological Institute, should also be shown.

4) Comments related to data interpretation

a) pg. 1064, l. 17-20: The authors give two possible explanations for the larger day-to-day surface pressure variations in winter (either more frequent passage of pressure systems or “steeper pressure gradients (larger amplitudes) of passing systems). Which of these two explanations is it? Also, the authors claim that “enhanced cyclonic activity” (does this mean they are claiming that there are more frequent cyclonic systems or systems with larger amplitude?) are causing a greater surface temperature variability in winter. Unless additional evidence is presented, I don’t accept this explanation for the temperature variability. Surface temperature variability in the Arctic is often explained by the longwave radiative forcing from clouds near the top of the Arctic temperature inversion and the strength of that inversion. Fig. 8a of Persson et al (2002; JGR) illustrates this variability, which is further discussed by Persson et al (1999; Preprints, 3rd Symp. on Integrated Observing Systems, Dallas, TX). Hence, the greater wintertime temperature variability may be caused by a stronger wintertime inversion and/or by more frequent inversion-top clouds.

b) pg. 1065, l. 6-30, pg. 1066, l. 1-8: The authors attempt some interpretation of the wind data, but Fig. 4 begs for additional analysis. There are clearly two upfjord wind regimes and one downfjord one, and the frequency of these vary monthly (seasonally). Since one of these 3 regimes appears to be present the vast majority of the time at this site, it would be very useful to characterize the air for these regimes (temperature, humidity, etc). This would provide some understanding of the causes for these regimes. It would also be very useful to evaluate which regime corresponds with the approach/departure of synoptic storms in the region. Are the southerly winds typically found to the east of a low-pressure system (e.g., one moving northward through the Fram Strait).

c) Figs. 5 and 6 and the discussion of the trends. The moistening and warming shown in these figures have no context. Since the microclimate is clearly topographically regulated, what is changing to cause these trends? Are there fewer cold katabatic wind events from the ice fields in winter? Are there more warm downslope wind events in winter? Are there fewer sea-breeze events in summer (I'm assuming sea-breeze events cool the site – but this is perhaps not true)? As mentioned previously, trends in complex terrain can vary strongly over short horizontal distances, so the meaning of these trends is unknown. It would be folly to claim that these trends represent a broad area around Ny Ålesund.

More minor comments:

1) bottom of pg. 1058: Curry et al (1995 JClim) is one of the first papers discussing the

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ice-albedo feedback, and should be referenced.

2) pg. 1060, l. 6: There is an update to the Stroeve et al 2007 paper, using more recent ice extent data.

3) What are the implications/explanations of the larger interannual variability for temperature in winter than summer? For the larger variability (Tmax-Tmin) for winter than summer?

4) pg. 1064, l. 15: rather than "more stable", do you mean that the atmospheric pressure systems have a "smaller amplitude?"

5) caption to Fig. 1: Are the minima/maxima based on 1-h or 5-min (1-min) data?

6) Fig. 2: The upper panel needs a curve for the monthly means of the years used.

7) Fig. 3: I assume that the pressure change shown in the lower panel is the absolute pressure change?

8) It is unfortunate that the data does not include downwelling longwave radiation, as this is a major contributor to the surface radiation, especially during winter. This omission degrades the usefulness and completeness of the data.

9) reading the text files is difficult because missing data is just left blank. If a character (e.g. -999) were included for missing data, this would be helpful. Or if software to read the data were included, this would also be helpful.

10) the time code is also not easily separated. It must be parsed before it can be read.

Interactive comment on Earth Syst. Sci. Data Discuss., 5, 1057, 2012.