Interactive comments on
"Representative surface snow density on the East Antarctic Plateau"
by Alexander H. Weinhart et al.

Comments by the referees will be displayed in italics, the response from the authors in normal font

2 Review by Eric Keenan, Nander Wever, Jan Lenaerts

The authors present a suite of highly accurate surface density measurements taken during a traverse in Dronning Maud Land, East Antarctica. These observations have the potential to offer the ice sheet scientific community a unique and very useful dataset to evaluate and improve snow and firn densification models. The authors present principally interesting spatial analysis of the measured snow density, which adds to the presented data. That said, we have significant concerns that should be addressed before publication, namely

1) a more detailed description of density uncertainty quantification,

2) the method used to quantify the impact of density on surface mass balance retrieval, and

3) a more detailed description of observed small scale density variability in the top 1 m presented in Figure 5, as well as its potential drivers and implications for interpretation of satellite altimetry observations.

Please find a more detailed description of these suggestions and others broken into major and minor comments below.

> We kindly thank Eric Keenan, Nander Wever and Jan Lenaerts for their detailed and productive feedback. The deliberate comments will definitely help to improve this manuscript, in particular the discussion and application of the presented dataset.
>
> Regarding the impact on satellite altimetry, we will consider adding another section in the manuscript.

2.1 Major comments

Section 2.1: This section would benefit from a general discussion of weather and climate conditions in the area, in relation to how they may impact surface density (in terms of variability of yearly accumulation rates, wind speed and temperature). This would help setting the stage for the discussion in section 4.4.

> We agree and will rearrange the section and add a climatic overview about the area (especially temperature, accumulation rates). This should also clarify unclear passages in the text like (see also minor comment on P3, L3-6).

P7, L12: "Breaks and lost snow in the snow profiles haven been corrected." This needs more explanation.

s. response below

P8, L9-11: "It is generally possible that at the liner top and bottom some snow is lost, but as the exact snow volume is determined with the µCT, we overcome this error source." It's not clear how the microCT can compensate for errors due to lost snow. It's the same liner that's measured by microCT and the scale, so if the snow is lost, both methods should be affected.

s. response below

P8, L15: "Therefore…" I don't see how this sentence follows logically from the previous section.

We realized that parts of section 2.3 were not coherent enough and will update it. Then the logical sequence should be clearer.
Regarding the missing snow: You have to distinguish between lost snow at the liner top or bottom and lost snow in the middle of the profile, we also will make it clear in the manuscript. The length of every individual snow profile is measured within the procedure of the µCT scan, we do not assume 1 m as standard length for every profile (e.g.: 1 cm lost snow at the bottom equals a 99 cm profile). This way we overcome an under-sampling error due to lost snow at top or bottom. In contrast: some mm lost snow at the edge of a depth hoar layer affects the under-sampling error of the volumetrically derived density. But this does not affect the µCT density, as only a central segment is used for the density profile reconstruction and – if a break is continuous over the whole horizontal plain – positions with breaks are spotted and values set to NaN.
Still, for 1 m segments, the volumetrically derived density has a higher precision as Fig. 7 shows; we will explain in more detail the text.

P8, L19: "spatially independent" Not really clear. Is the measurement setup in Fig.3 considered spatially independent? I.e., are liners X, A, B, C considered spatially independent? According to the text they are, but those four liners are not really independent.

Fisher et al. (1985) defined local noise as "random element caused by the surface irregularities", which is present in any taken snow profile or ice core. Stratigraphic noise and depositional noise are used as equivalent terms for local noise as they are more descriptive. This noise is mainly caused by spatially inhomogeneous deposition in combination with wind, leading to snow patches or dune structures that usually have a spatial extent of several meters.
To still be able to get a representative value or profile (of density or other parameters) at a given spot, several samples have to be taken at a distance, at which they have not recorded the same depositional (or stratigraphic) noise. For example, samples should not be taken from the same dune or snow patch. By stacking or averaging the samples, the noise is minimized. The (minimum) sampling distance between two samples was quantified by Laepple et al. (2016) with 5-10 m, as the correlation factor between single profiles decreases with increasing distance. Also the sampling distance in this study was chosen according to this finding. In an (unpublished) test using our density profiles we also

come to a similar result. Note: Laepple et al. (2016) sampled perpendicular to the dune direction like we did in the OIR trench. For sampling parallel to the wind, the sampling distance between two samples should be higher.

This condition (not recording the same depositional noise several times) was called ‘spatially independent’ in our manuscript. The problem of stratigraphic noise is generally higher in regions with low accumulation and has also been recorded for e.g. isotopes (Karlöf et al., 2006; Münch et al., 2016).

For a better understanding, we will add passages of the explanation above in the text.

Section 2.4: This section is difficult to comprehend, and is written very compact. Particularly, please expand on: “This way we use the maximum sample size without an artificially caused bias in the data.”

For the whole section, there might be many different approaches to determine a certain number of precision for our data. The method we use has also recently been applied in a study by Dallmayr et al. (in review). We are aware of the not-straightforward method and will try to explain in more detail. With ‘artificially caused bias’ we mean the instance of arbitrary picking sets of different numbers of ρ_L (e.g. a certain number of independent profiles out of the 30 trench profiles). Instead we suggest to take the maximum possible number from the beginning. For more clarity, we also put the formula

$$\sigma^{1m}_{H} \Big/ \sqrt{x}$$

in a more explicit position in the manuscript.

Figure 5: The large variability in observed density, particularly in the top meter, is very interesting and is a very nice inclusion in this paper. Can you please elaborate on what might cause this (surface topography? winds?) and what this variability means for interpretation of satellite altimetry. In particular, the observed variability is in apparent contrast with the title of this paper “Representative surface snow density…”. If surface density is highly variable, is a representative density truly the best approach or should the scientific community make an effort to model this variability? A related comment is that Section 3.5 is really short and only mentions results. It’s not clear which conclusions the authors draw from this and how it is important to understand surface density variability.

The surface topography is definitely one of the driving factors for the high horizontal variability of density we see in the top 20 cm in the OIR trench. It can be seen as a complex interaction between accumulation (a combination of calm diamond dust deposition as well as event-based accumulation), redistribution of soft snow by moderate wind and the existing topography, with the topography being the dominant factor (height & shape). Additionally, long residence time of snow at the surface due to low accumulation rates enhances the chance of metamorphism and sublimation (due to the vertical temperature gradients and low humidity), which also can have an impact on the surface snow density. We will expand the discussion of the driving factors in section 4.1.

As we mention in sect. 2.2.3 and the previous comment, the OIR trench was excavated perpendicular to the main wind direction, which causes a higher variability than parallel to the wind. We will include this in the discussion.

Indeed, the high (local) variability at the snow surface (especially in the upper 20 cm) is a major finding of this manuscript. But as a consequence this is also the reason, why we argue for the use of a 1 m mean density as a more robust parameter for surface snow density. This way the density variability at the uppermost surface is compensated by using enough depth (or "annual layers", in other terms) without having the influence of a densification effect. We will emphasize this statement in the conclusions.

Otherwise, the high variability at the surface can also be an argument for a representative density obtained with the method we presented. Especially as altimetry measurements have a certain footprint area, representative regional density values can be of particular interest.

Despite the argument for the 1 m snow density, from our point of view a representative vertical variability is a more important aspect than modelling the horizontal variability. This is why we included the density distribution of density (Figure 9) in the manuscript and calculated the distribution also according to the presented subareas. The vertical variability can be interpreted as a measure of snowpack layering (i.e. layers of high and low density). The layering definitely has a strong influence on the densification from snow to ice on one hand (and therefore also on the bubble close-off at the firn-ice transition and subsequent effects). On the other hand, also satellite altimetry can use this information of vertical layering due to penetration depth into the snow and reflection at layer boundaries. We will elaborate to what extent we can include this topic in this manuscript, as the topic of snowpack layering demands another dimension (esp. further parameters to describe the snowpack) that goes beyond the current manuscript frame.

Regarding section 3.5: We included this – indeed – very short section as a baseline for the argument of dune height and surface topography on the East Antarctic Plateau. We mention the relation of dune height to accumulation rate later in the manuscript. One might argue, that the dune height of 30-40 cm is not that high. But in contrast to the annual accumulation of ~10 cm, the dune height becomes enormous. We will mark in the text when we refer to the measured surface heights in the text.

P11, L1-3 and in the following sections: If the standard deviation for 1m sampling intervals is 5-10 kg/m3, how can the error quantification for the average be only +/- 2 kg/m3?

We assume, you refer to the horizontal standard deviation, as also displayed in Fig. 5.

The horizontal standard deviation (σ^{1m}_{H}) for the liner mean density (ρ_L) in the OIR trench is 9.63 kg m^{-3}. The standard error then is defined as the standard deviation divided by the square root of the samples size: (σ^{1m}_{H} / √30) = 1.76 kg m^{-3} ≈ 2 kg m^{-3}

Figure 8: Observations report a near uniform mean density in the four different subregions. For me, this is a surprising result. How might the different dates the observations were taken affect the measured density, i.e. do you expect a seasonal cycle in surface density. The way this dataset is currently presented, does not take into account this possibility. Additionally, if you are not already planning on doing so, can you please include exact observations date and time in the final dataset publication on Pangaea?

To be honest, we also expected to see a larger difference between the subregions (in both, mean density (Fig. 8) and density distribution (Fig. 9)) and a clearer trend along the traverse before the measurements.

To clarify your remark we add the sampling date at each location in table 2 (we planned to add the sampling date to the Pangaea dataset). From first (14.12.2016) to last (28.01.2017) sampling date we do not expect to see a seasonal cycle or bias due to sampling, especially as we did not notice significant accumulation during the traverse (main synoptic features: some diamond dust above 3500 mSL during the nights, during some days moving/drifting snow. The temperatures varied between -20 and -40°C during the night). We also add a sentence on that to section 4.4.

From extensive sampling programs at Kohnen station we can generally say, that it is possible to detect seasonal cycles in density profiles – but only with a sufficient amount of samples (if local noise can be eliminated) (Laepple et al., 2016). On the EAP, this may be more difficult as the accumulation rate is much lower. To derive a representative density profile from the OIR trench can be a part of a future study.

P18, L18: This line mentions natural variability due to antecedent weather conditions. Section 4.2 needs to put the analysis based on climatological trends in perspective to possible year-to-year variability due to antecedent weather conditions. Since accumulation depths in dunes could be up to 30cm, this may impact top 1m density significantly.

In this context, high mean density of single profiles can also be explained with a high percentage of dune snow in a profile. A major problem here is that we do not know anything about the persistence of the surface roughness or surface features. If the surface is reshaped once or twice a year by stronger wind (>10 ms^{-1}), it is hard to attribute this variability to a variability of climate. Also the distribution of snow accumulation during the year is poorly known (keeping in mind the differences at Kohnen station with higher synoptic influence than the remote plateau with diamond dust deposition).

We will elaborate on the year-to-year variability a bit further by comparing samples from Kohnen station from different seasons (15/16, 16/17, 18/19, maybe more) and present values in the next version of the manuscript. Here we want to show the high influence of noise in contrast to climatic factors on density.

For further discussion about the climatic influence on surface snow density, we want to refer to section 4.2., where we test, whether the discrepancy in density between two datasets can be ascribed to rising temperatures in DML. We come to the conclusion, that the warming of 1°C alone cannot explain the difference in 1 m surface snow density and ascribe this to the term ‘natural variability’ or ‘noise’.

Section 4.3: The authors aim to provide the impact of density on the uncertainty in SMB, but they fail to do this correctly. First of all, 3% uncertainty in the firn column does not directly translate to a 3% uncertainty in the overall firn+ice column (since there is much more ice than firn on East Antarctica). Secondly, this calculation pertains to mass, not mass balance (i.e. the change in mass per unit time). Instead, the authors should think about focusing on the application to altimetry, which needs surface density to convert volume to mass. As most of the elevation changes on East Antarctica measured by altimetry are SMB-driven, the observed elevation change will be associated to the layer of recently accumulated/ablated snow/firn, with an extremely spatially and temporally variable density. Since this (near-)surface density is much more variable than at 1 m, this volume-to-mass conversion is highly uncertain, especially when focusing on small scales such as in this study. The error here is directly proportional to density, i.e. there will be 100% error in mass change if the assumed density is 100% different than it is in reality.

We admit using wrong terms (especially in the section head) as well as unclear explanation in this section. Instead of a SMB, we rather want to show a simple underestimation of mass of East Antarctica here. Our calculation has been very simplistic, but regarding mass only we consider it to be principally correct.

We assumed an average thickness of 2000 m of firn and ice combined in East Antarctica. The firn column has a length of 93 m according to Herron and Langway (1980) with an initial density of 320 kg m^{-3}. This corresponds to 59 mWE. Calculating the water equivalent with our presented surface snow density, we get 61 mWE, meaning +3% mass in the firn column. This again corresponds to +0.11% in relation to the 2000 m firn and ice combined. This 0.11% of the water equivalent of East Antarctica (51.69 m SLE (Rignot et al., 2019)) leads to a ~5 cm SLE estimation.

We will probably add another section (and make section 4.3 more comprehensive) specifically discussing the applications of this dataset for satellite altimetry on ice sheets, which is more helpful than this mass budget calculation. For this purpose we will discuss our data more interdisciplinary with colleagues working with remote sensing data.

Figure 11: How is surface roughness and sub-grid topography, e.g. using REMA, related to observed density in this figure? It is recommended to analyze the liners from Kohnen station from different seasons (as mentioned P9,L15/16) to show to what extent there is year-to-year variability.

We also thought about this aspect in preparation for this manuscript and compared REMA surface and bedrock topography with our dataset. For drawing a significant conclusion from the comparison, we consider the local sampling distance (10 m) as too small and the regional distance (100 km) as too large. Still, this would make sense for adjusted sampling intervals (like 10 km or so) along transects with samples of representative density.

The resolution is simply a limiting factor here (in both dimensions, depth and space).

Your idea for samples on Kohnen station will be implemented (see your comment on P18, L18).

P21, L5-8: First of all, a primary source of error in modelled snow density by the Ligtenberg et al. (2011) model could as well result from the meteorological driving data for the FDM simulations. Second, the text now seems to imply that more snow *redistribution leads to lower densities. However, it has been demonstrated that snow redistribution tends to increase hardness/density (see Sommer at al. 2017, 2018).*

We will mention the meteorological forcing as a potential error source in our discussion.

Regarding the influence of snow redistribution on surface snow density, the wind speed and surface topography have to be taken into account. In wind speed maps (Parish, 1988; Sanz Rodrigo et al., 2012; van Lipzig et al., 2004), we see low wind vectors or mean wind speed from Kohnen station along the ice divide up the EAP and even lower values for the region around Plateau Station. Rather than with only the mean wind speed, we also want to argue with the distribution of wind speed instead of average wind speed (generally low wind speeds on the East Antarctic Plateau with occasional strong winds causing drifting snow; the latter can happen more often at Kohnen station). We will back our discussion with the cited wind maps.

We are aware of the mentioned article (Sommer et al., 2018) and included it into the discussion as an example for wind packing with (in contrast to the remote plateau) relatively high wind speed. Knowing that there are many complex possibilities for depositional processes, we want to contrast two scenarios:

1) At higher wind speeds, snow gets redistributed and sorted, after deposition that snow has a higher density mainly due to wind packing (sorted, high density, example by Sommer et al. (2018)).
2) Between dunes, soft snow (low density) gets deposited at low wind speeds, which causes a high variability (not sorted, low density) over depth in a given period of time.

Scenario 2) is the explanation we give for the lower densities in the major part of the interior plateau, as we sum up in section 4.4.

We thank the authors for taking the substantial time and effort to collect, describe, and distribute these density observations. That said, we believe these observations would best serve the community if they were also included in a unified and publicly available dataset such as SUMup (Montgomery et. al., 2018).

The data will be available on open-access repository Pangaea. We are happy to collaborate with our data on further datasets.

2.2 Minor comments

P1, L11: Underestimations or overestimations.

At the introductory part of the abstract, we kept the term ‘uncertainties’ deliberately as a general term. The under- or overestimations are dependent on the density measurements, of course. In our case, with higher density than assumed, we speak of an underestimation (s. same page, line 26)

P1, L25: Density errors can be due to errors in parameterizations or atmospheric forcing.

Will be added.

P2, L3: "Greenland Ice Sheet"

Will be corrected.

P2, L3-5: Please reformulate. Either "accurate quantification is important" or "The current state and rate are some of the most important quantities…"

Will be corrected as suggested.

P2, L11: "Especially in the interior of the ice sheets, the exact surface snow density is a limiting factor in precision." Please amend why that is, with appropriate references if available.

This sentence originated from a discussion with colleagues working with altimetry data. We found the following passage in Thomas et al. (2008) and will include it as reference: "Radar return-pulse waveforms from high-elevation parts of Antarctica are affected by various characteristics of the snowpack, such as snow density, distribution of ice, wind-crust and depth-hoar layers (…), and by wind-induced surface roughness. (…) Near the coast, radar penetration into the snow is of far less concern than the local surface topography, which becomes quite rough, particularly in the most active parts of outlet glaciers where thinning rates are highest."

P2, L23: "Small variability" → "A small part of variability" I assume.

Will be corrected as suggested.

P3, L1 and L9: "stratigraphic noise" please explain.

Please see our answer to your major comment on P8, L19.

P3, L3-6: "In this paper, we present surface snow density data with high precision from a traverse covering over 2000km on the East Antarctic Plateau (EAP). In order to avoid misunderstandings we follow Stenni et al. (2017) using the term EAP for the region higher than 2000m above sea level (asl). The coldest 10m firn temperature is recorded at Plateau Station (Picciotto et al., 1971), which makes the area the best modern analog of glacial firn." Don't understand this section. Please explain in more detail.

We do not have access to glacial-climate firn but firnification during glacial climate periods is modelled to calculate for example the Δage (the gas age-ice age difference), and to infer the phase relationship between temperature derived from the isotopes and the CO_2 concentration measured in ice cores. Modelling glacial-climate firn faces some problems, e.g. at the pore close off (firn to ice transition). Firnification models simulate a deeper pore close off than

δ^{15}N data predict. In this sense we understand modern firn from the coldest regions of the EAP as the best modern analogue of glacial-climate firn (for some regions, e.g. Kohnen station). The acronym CoFi stands for Coldest Firn. Within the project, five 200 m firn cores have been drilled on the EAP to investigate the firn densification.
In general, we will move the project description to section 2.1 and add climatic information about the area. The snow profiles presented here were taken in the framework of this project. But as this information is not necessary for the further manuscript, we probably decide to remove this sentence.

P3, L10 Average local snowpack density.

Will be corrected.

P4, Fig 1: Please add elevation contour labels.

Will be added.

P6, L20: The sentence "The trench surface was measured…" needs to be placed before P6, L19: "The total height difference between the lowest…"

Will be corrected.

P7, L9 weighted → weighed?

Taken as suggested.

P8, Fig 4: What explains the occasional large difference? Please add linear regression statistics.

We add linear regression and elaborate in more detail in the text (see also comment on P8, L9-11).

P8, L15: "Therefore, to quantify the 1m snowpack density we use L, to investigate smaller intervals we use the 1mμCT (Tab.1)" Since 1mμCT is CT density over 1m, and L the liner density over 1m, how should it be interpreted that 1mμCT can assist in investigating smaller intervals? Or should it read 0.1mμCT.

The latter, it was a typo. Will be corrected.

P8, L17: Snow density profiles?

Will be replaced.

P9, Section 2.6 Optical levelling needs to be placed before Section 2.2.3., since the optical levelling is already mentioned there.

We carefully thought about this suggestion, and discuss a suitable solution.

The profiles investigated with optical levelling are completely independent from the liner sampling procedure and should be seen as an additional information, with which we want to address the topic of surface roughness. As the snow liners are the main samples in this study, we want to keep them as the first method presented.

Maybe we find an option to include the levelling in section 2.2.3.

P9, L17: "Furthermore, a possible effect of the station itself should not be migrated into the other subsets." Please explain what effects are meant here.

Around Kohnen station an effect of increased accumulation has been observed, which might influence density as well. We probably will erase this sentence, as the samples at Kohnen (previous studies) have an adequate distance to the station itself.

P10, Figure description: What is meant by raster?

We started a common depth scale for the whole trench at the top of the highest (relative) snow profile. Then we calculated the mean value in 0.1 m intervals of each profile but according to the common depth scale (as visible in Fig. 5, bottom left). We used the term raster (or grid) as a more descriptive term, but will add a sentence like above for clarity.

P12, Table 2: Can you create maps of p_loc and sigma_1m?

We will include a map in the next manuscript version.

P18, L9: Please quantify dune height

Dune heights on EAP can be up to 30-40 cm, we will include concrete values from section 3.5 here.

P19, L4: What exactly leads you to make this claim. Could density errors be due to errors in atmospheric forcing? Temporal variability in snow density?

As mentioned in your comment on P21, l5-8, atmospheric forcing was neglected in our manuscript as an error source and will be added to the discussion. Regarding temporal variability we refer to the comments above (P18, L18).

Still, the large difference we observed between our dataset and Kaspers et al. (2004) & Ligtenberg et al. (2011) should at least be partly accounted to the parameterization of surface snow density.

Many figures have missing axes. Please correct.

Missing axes will be added.

References: please provide doi's for easy lookup of literature.

We have added the DOIs to the references, but also want to mention, that the original template for EndNote did not show DOIs by default. This should be updated by Copernicus.

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