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.

1 Review by Anonymous Referee #1

This paper describes density observations along two over-ice transects from Kohnen station to Dome F on the plateau of the East Antarctic ice sheet. The observational techniques are state-of-the-art, resulting in small errors and highly significant results. These results show that 0-1 m average density shows little variation along the traverse, with a mean value of about 355 kg m^{-3}. This is an important result, as it can be used to improve the snow/firn modules in (regional) climate models and the interpretation of satellite altimetry observations. However, the writing needs to be improved, as many formulations are unclear (for some examples, see below, but this listing is not exhaustive). The figure quality can also be improved in places.

We thank the anonymous referee for his feedback on our manuscript. We carefully will go through the manuscript again and try to rewrite unclear passages and elaborate further on the influence of the presented data on satellite altimetry.
Generally we will include axes in the figures where missing.

1.1 Major comments

p. 1, l. 25: "The difference in the total mass equivalent of measured and modelled density yields a 3% underestimation by models, which translates into 5 cm sea level equivalent." It is unclear how these numbers are obtained, see comment below on Section 4.3.

See comment on section 4.3.

p. 2, l. 3: "Accurate quantification of the current state and rate of change of SMB is therefore one of the most important quantities..." A quantification is not a quantity. Please critically re-assess your formulations to improve this throughout the paper.

Thank you very much for this advice. We will clarify unclear wording or passages in the text, including the mentioned one. We noticed also necessary improvements especially in section 2.4 and 4.3 (s. below).

p. 3, l. 5: "The coldest 10 m firn temperature is recorded at Plateau Station (...), which makes the area the best modern analog of glacial firn." This is another example of a sentence that is really hard to understand. Coldest on Earth? What do you mean by "an analogue of glacial firn"? Please clarify.

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 $\delta^{15}N$ 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.

Section 4.3: It is unclear to me how the density errors in previous studies lead to the SMB error results in a 5 cm sea level equivalent? Over what period? SMB is usually derived from regional climate models that quantify mass directly, i.e. irrespective of density. Mass changes by GRACE are also direct mass measurements, only satellite altimetry suffers from uncertainties in the density of the material at which the elevation change takes place, but this is valid for changes in elevation, not for steady densities as presented here.

This comment also refers to your general comment on a proper use of terms, here we should rather refer to simply mass instead of a mass balance.

We want show the underestimation in mass in the firn column running the model by Herron and Langway (1980) with two different initial densities. According to our calculation, this underestimation (3% of the firn column) in East Antarctica can lead to an underestimation in total mass of 5 cm sea level equivalent.

We will update the section with proper terms and describe our line of thoughts at length. We also add another section with focus on the impact of our findings on satellite altimetry of ice sheets. For this we will discuss our data in more detail interdisciplinary with colleagues working with remote sensing data.

Section 4.4: Are the Ligtenberg (2011) data also valid for the first 1 m? I think they use a simple parametrization to calculate surface density, hardly a 'model' as it is called here. It would also be valuable to provide the time span covered by the first 1 m of snow, this will vary with accumulation. In how far can climate variability be responsible for part of the differences with other studies? Based on its findings, does the current paper recommend to redefine 'surface density' as the average density of the first m? If so, this is an important recommendation that could be made more explicit.

The suggestion to redefine the term surface snow density is an important point that will be addressed. Also in preparation of this manuscript we stumbled upon several opinions about how 'surface snow density' is defined ('fresh snow density', 'near surface density' and 'density of the uppermost snow layer' are used sometimes arbitrary for different purposes). Generally, for density smaller intervals than 1 m a lot of additionally different parameters would have to be considered. For practical reasons, 1 m intervals are not only a commonly used interval for density, but also stable water isotopes in surface snow (e.g. Masson-Delmotte et al., 2008). According to the dataset description, the data from Ligtenberg et al. (2011) are modelled (IMAU Firn Density Model) for the near-surface (0-1 m depth) and gridded to 33 km resolution.

The problem of time span and the – in turn – advantage of 1 m density compared to smaller intervals, is tackled in the conclusion, but should be emphasised in the discussion as well. As mentioned before, we added a section about the climatic setting of the area. This includes information about accumulation rates as well as temperature. At Kohnen station the accumulation rate is 64 mm we a^{-1} (we = water equivalent) (Oerter et al., 1999) with increasing tendency over the last decades (Medley et al., 2018), at Dome Fuji 27.3 mm we a^{-1} has been measured (Hoshina et al., 2016). For the locations along the traverse a precise value is difficult to obtain. A 1 m snow profile therefore can cover a time period of four years at Kohnen station to 20 years on the remote Plateau.

Regarding your comment on climate variability: if we understood correctly, you ask whether changing temperature can be responsible for a difference between two density datasets. Here we want to refer to section 4.2, in which we elaborate on this thought. We show, that climatic driven changes in density are too low to ascribe the difference in density between the datasets only to temperature.

1.2 Minor comments

p. 1, l. 10: "Wrong estimates of snow and firn density can lead to significant underestimations of the surface mass balance." underestimations of → uncertainties in

p. 1, l. 17: "liner"? This has not been explained yet, so please don't use it here.

p. 1, l. 23: "provided by a regional climate model" These models usually don't 'provide' density, but either prescribe it or use a simplified expression based on temperature, wind etc. Suggest to replace by 'used'.

p. 1, l. 25 and further: Note that regional climate models DO explicitly calculate accumulated mass, so using a wrong surface density does not influence the surface mass balance directly, only indirectly (through blowing snow threshold friction velocity, vertical heat transport in snow affecting surface temperature and hence sublimation etc.).

p. 2, l. 2: on → of; Greenlandic → Greenland.

p. 2, l. 5: "Satellite altimetry is state of the art" → Satellite altimetry is a state of the art method/technique...

p. 2, l. 13: "snow density is parameterized" → snow density in models is often parameterized

p. 2, l. 30: This sentence is unclear, please reformulate.

p. 3, l. 4: Remove "In order to avoid misunderstandings"

p. 3, l. 5: coldest/warmest temperatures → lowest/highest temperatures (change this throughout the text, please)

p. 9, l. 8: good → well

p. 18, l. 23: This AWS has been installed and serviced by Utrecht University and AWI; please provide proper credit.

Figures: Please include solid axes where missing.

All minor comments are taken as suggested by the referee.

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