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Interactive comment on "Autonomous Ice Sheet Surface Mass Balance Measurements from Cosmic Rays" *by* Ian M. Howat et al.

Anonymous Referee #1

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This manuscript presents the results of a nearly 2-year long test at Summit, Greenland of a commercial device (Snowfox) developed to measure snow accumulation in water equivalents (SWE). The Snowfox sensor measures neutrons produced in the earth's atmosphere by cosmic rays, these neutrons are attenuated by water accumulating above the sensor in the form of snow. While this technique has been applied fairly widely in alpine snowpacks, the authors assert that the test described in the manuscript is the first application in the firn on a polar ice sheet.

The results are very encouraging (perhaps not surprising), but oversold in my opinion. Specifically, I find the claim that this test confirms better than 0.5% accuracy for SWE < 20 cm and better than 0.7% accuracy for SWE up to 140 cm implausible. I note that previous studies with similar devices (cited in this manuscript) conclude that accuracy

in the 3 to 5% range could be achieved, and that data with this quality were useful. My skepticism is based on a combination of 4 factors: 1) the corrections to account for temporal variations in neutron flux at the snow suface and variations in the column of water vapor in the atmosphere above the buried sensor do not seem likely to be accurate to better than 1% (perhaps considerably less precise), 2) the noise in the estimated accumulation (Figures 4 and 5) appears to be at least +/- 2%, 3) comparison to 2 independent manual techniques to measure accumulation do not show consistent agreement at < 1% across the 10 month comparison period, and 4) over the full 22 month long trial the maximum SWE reached just 42 cm, suggesting strongly that the claimed accuracy at 140 cm is entirely theoretical, and hinting that the same might be true for SWE <20cm.

Regarding the corrections to raw data, more detail is needed to explain how the neutron monitor at Thule is used to provide No (neutron flux at the surface). It is likely that the Snowfox and the Thule monitors are not measuring neutrons with identical efficiency across the energy range, and I would be surprised if hourly changes in neutron flux were perfectly in sync, given $\sim$6 degrees in latitude and nearly 3 km in altitude separation between the 2 sensors. (Are the Snowfox devices inexpensive enough to use one mounted above the surface to provide No (near a single buried Snowfox, or in the middle of a regional array of buried sensors in a future study)). Atmospheric water vapor is not a simple linear function of atmospheric pressure, varying depending on synoptic conditions in addition to pressure. Assuming solely pressure dependence has to introduce more than 0.5 or 0.7% uncertainty in the derived SWE. Note that there are several data sets on water vapor above Summit that might allow more precise treatment of its impact on neutron flux, or at least provide an estimate of the magnitude of uncertainty introduced by neglecting changes in water vapor that are not just a function of pressure.

I also find that the manuscript is a little sloppy, particularly in describing the corrections applied to convert measured neutron counts to SWE above the sensor. For example, in the discussion of equation 1 used to calculate the relative count rate Nr is defined as the reference sensor count rate while Ns is the reference count rate??? If Nr=Ns then the relative count rate from this equation is always 1. Next page in discussion of equation 3 No is defined as the reference count rate (3rd ref ct rate) at the surface obtained before burial of the sensor (at time = 0). The term N/No in Equation 3 suggests No should be the count rate at SWE = 0 (i.e., flux reaching the snow surface), both N and No should be measured (estimated) at every time (it does not make sense to ratio N at each time to No measured just once, given time variations in both cosmic ray flux and water vapor/pressure).

Below are listed a variety of additional editorial comments (some are additional examples of sloppiness, a few more substantive), referenced by page/line #.

1/5 “background cosmic ray intensity” is probably not the correct term. What is really needed is variation in the neutron flux reaching the surface above the sensor at Summit, which could vary widely due to solar events (likely to dwarf changes in the flux of “cosmic ray background” impacting the solar system)

1/21 I would be very hesitant to claim that accumulation at Summit is “consistently low in June/July” based on less than 2 year record (not even considering prior results that find different results)

1/28 measuring the volume of accumulation (delete “of” before volume)

2/14 mˆ2 (superscript)

2/29 the statement here that “neutron counts increase with altitude and latitude” (more specifically geomagnetic latitude) demands that more be said later regarding how well a monitor at Thule can constrain neutron flux at Summit

3/4 “calibration data sets” suggest that there will be calibration data presented later. Turns out that all of the (critical) parameters in Table 1 appear to be taken from specs provided by Snowfox vendor.

3/12 and 13 Juxtaposing statement attributed to Alley, 1993 that Summit snow has “average surface density of 0.35 gm cmˆ-3” and citation of Dibb and Fahnestock, 2004 is sloppy. Latter paper presents density profiles from 22 “monthly” snowpits sampled at Summit over 2 years and shows that the mean density in the top 99 cm never exceeded 0.336 g cmˆ3 and averaged 0.305 g cmˆ3. This is also relevant to the Snowfox “validation” presented later. (Also note that the “-3” in manuscript should be super script.)

3/18 MSF is an acronym for the “Mobile Science Facility”. Until summer 2017 the main science facility at Summit was TAWO.

3-4/25-30 and 1-5 (equations 1-3) see comments above. Also, N in Eq 3 is never defined (think this is the actual measured neutron count, at a given time T, from the buried Snowfox)

4/12 Fig 2 does not show any time series, rather a curve based on assumed performance of the Snowfox sensor. Also confusing to introduce N*/No here, since Eq 3 defines N* to be a function of N/No.

4/16-17 such that the resolution is (delete “that” before resolution)

4/30 “42 observations” (snow cores)? ? Earlier in this paragraph it is stated that cores were sample every 10 days. 42 x 10 is 420. 13 Mar 17 to 17 Jan 18 is ∼310 days. Plot in Fig 7 seems to show 36 cores. These are not all consistent (sloppy)

5/1 Differences in the values of hw derived from any single core by weighing and by measuring the volume of melted snow are not due to “unconstrained errors in the sampling procedure.” These are 2 different measurements of the same sample, so the errors have to be in the measurements.

5/3 Given estimate of the mean density in the snowpack from surface down to depth of the Snowfox from 42 (or 36 or 31, whatever may be the actual number) cores over 10 months, what can you say about 1) whether constant value of 0.35 g/cmˆ3 is reasonable, 2) is any variation in the measured density seasonal, 3) does it look like what Dibb and Fahnestock saw, 4)why not use these measured values to convert the stake measurements rather than a constant, loosely defined “surface” value from the literature?

5/6 No good justification to use constant value for density, given that you have measured it at fairly frequent intervals, and that Dibb and Fahnestock showed that it is not constant (and was always lower than the assumed constant value used here).

5/19-20 There is an overall decline

5/26 0.4 g cmˆ3 is a pretty high value for the density of a wind slab at Summit, also note that it is sloppy to change the units to kg/mˆ3 here

6/1 should “0.013 cm + 0.007” just be 0.02, or 0.013 +/- 0.007?

6/1-9 this paragraph does not support the very high accuracy claims made for Snowfox in the astract.

6/19 “that much of the” (delete extra “the”)

6/21 seems that the overall rate should refer to 16 May ’16 to 18 Jan ’18 (not Jan ’17)

6/28 given that the Snowfox estimated SON accumulation differed by more than a factor of 2 between 2016 and 2017, how confident can one be that JJ are consistently low accumulation months based on the same almost 2 years of record.

6/30 “change in water equivalent” (add in)

7/1-8 Make it clear what is signified by the “mean difference” (i.e., is Snowfox biased high or low by mean of 0.77 cm vs cores and 0.22 cm vs stakes). Also consider redoing the stake comparison using a better estimate of density, with seasonal variation (from measured density of the cores in this study and/or values from Dibb and Fahnestock.) (Note that the agreement with Snowfox is likely to be worse using more realistic, lower, density for snow in the top 42 cm of SWE.

Also, why not show the comparison to stakes for the entire 20 months? It is unfortunate that the validation cores started almost a year late, but the stakes were measured ∼ monthly for a different project since 2003.

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