

**Review** for 'The Cryosphere', Jul 2016.

Paper tc-2016-57, "An assessment of two automated snow water equivalent instruments during the WMO Solid Precipitation Intercomparison Experiment", by Smith et al..

## Comments to the editor

### General comments

The goal of this paper, as stated by the authors, is, "to assess the use and accuracy of two instruments that were tested during the WMO-Solid Precipitation Intercomparison Experiment the Campbell Scientific CS725 and the Sommer Messtechnik SSG1000 snow scale" as well as, "to inform users of the best way to use these instruments and of any potential measurement issues that may influence their data interpretation."

Unfortunately, however, I don't feel I get the promised info by reading that paper. The deficiencies of the CS725 (first 5 lines on page 4) are simply confirmed and no convincing, in depth analysis of possible source of errors are addressed for any of the two instruments. Instead, spatial variability is invoked to explain the mismatch between the continuous measurements and the manual, punctual (in time) reference measurements, the error of which are not quantified either.

In view of the above and my comments below, I can hardly recommend to accept that paper for publication. Indeed, I really doubt that the authors have enough convincing arguments and data to bring the paper in line with their goals, even after major revisions.

### Specific comments

- p. 1, lines 25-26: "These manual measurements are considered to be the reference for the intercomparison." This is one of the crux of that paper. The devices are hardly looked at while a whole lot of blame goes on these manual measurements, the error of which are hardly addressed though.
- p. 1, line 30: "throughout the intercomparison periods" is absolutely misleading and false. One full ablation period is missing and the problems of the instruments were not looked at.
- p. 1, line 33: "seasonal melt" suggest replacing by "**ablation period**" [throughout the paper]. Furthermore, is pre-melt = accumulation period? I strongly suggest that you define these terms properly once and use them consistently throughout the paper. See for example on p. 6, line 31 for "point of maximum seasonal SWE"
- p. 3, line 13: "2 Instrumentation and Methods" Should try to not give interpretation in that paragraph but include it in the results section, for example as "previous intercomparison"
- p. 3-4; lines 26-5 : Is this the correct place for such comments? Should be moved to discussion part as an introduction to it.
- p. 4, line 15: "... impact of the move are considered to be negligible." Why? Later you speak of spatial variability influencing the results.
- p. 4, lines 29-30: "... to stabilize the overlying snowpack and prevent ice bridging." Why does the snowpack need to be stabilized? How is ice bridging prevented? What observations do corroborate this?
- p. 5, line 3: "..., and the only snow scale provided ..." is incorrect. There is another SPICE site (Weissfluhjoch) equipped with a snow scale ... and a snow pillow next to it from the same provider.
- p. 5, line 8: "... reliable manner ..." but not always. The simple regression does not reveal the true problems!
- p. 5, lines 13-15 : "The sensitivity ..." Such a sentence belongs to the summary and conclusion section.
- p. 5, lines 24-25: "... has a mean measurement error less than 0.5 %." 0.5 % of what? Does this refer to the repeatability of measurements? Overall, the number looks very optimistic and the reference Farnes 1983 is hardly available. From other publications by the same author (1980 and 1982, see Kinar & Pomeroy, 2015b), this figure can hardly be reproduced. I'd strongly suggest to be more precise here.
- p. 5, line 25: "were taken just inside the footprint of the CS725" How do these disturbances affect the measurements?

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- p. 7, lines 14-15 : “*the instrument trends are the same as for the manual measurements*” In my view your simple regression analysis fails here and does not look at problematic features. For example, how do you explain the apparent loss of mass around mid February 2014? Similar unexpected wiggles are also seen at other times on both seasons. These spurious measurements are also known to occur at Weissfluhjoch and are not to be expected from a well designed, continuously recording device.
- p. 7, lines 15-21: This comparison or ‘tracker’ does not appear to work very well. Indeed, in January 2014 there is a large increase in the ‘Difference in SWE’ while air temperature plummets! Similar behaviour can be found at other times. In summary, there is another reason behind these large increases, but which?
- p. 11, line 11: “*systematic sampling errors*” Can these be avoided?
- p. 11, lines 24-27: “*Although ...*” A somehow simplistic view. In the paper you never assess any of the errors you assign the outliers to. This is definitely the biggest weakness of that paper.
- p. 11, lines 30-36: Poor conclusions! What does this linear relationship show? Would you calibrate the CS725 with a SSG1000? Were the deficiencies of the CS725 not already known (see your introduction)?
- p. 12, line 4: “*... not all increases in the bias ...*” Interesting, you don’t even mention those in the discussion!
- p. 12, line 8: “*... errors in the manual SWE measurements. ...*” I agree that measurement errors can amount to a certain percentage of SWE. But you don’t even quantify these errors, even though you use them as reference. Blaming not quantified errors for the observed mismatch seems simplistic indeed.
- p. 12, line 32: “*... have a good agreement ...*” Here I have really hard times to follow the logic of your conclusions. First you blame the manual measurements for mismatch and then you claim a good agreement!

### Minor comments

- Please ALWAYS put a space between numbers and units (often wrong)
- p. 1; line 28: Replace “(w.e.)” by “(mm w.e.)”.
- p. 1; line 29: Replace “*Creek respectively*” by “Creek, respectively”.
- p. 2; line 7: Are these two units equivalent?
- p. 3; line 1: Replace “(SPICE; Nitu)” by “SPICE (Nitu”.
- p. 3; line 28: Replace “*snow cores*” by “snow courses”.
- p. 3; lines 28-29: What are “*snow pit densities*”? Please describe. Sampler?
- p. 4; line 30: “*0.3 % of full scale*” that is 3 mm w.e.! Under what conditions? Moreover, the high resolution seems useless.
- p. 4, line 35: What is drifting? Snow? The electronics?
- p. 5; line 23: “*snow tube*” Is this the correct term? I’d suggest using “snow sampler” - as found elsewhere in the literature – throughout your paper.
- p. 7; line 5: Replace “*almost*” by “by almost”.
- p. 7; line 29: Replace “*offset*” by “intercept”, throughout, as used in the tables.
- p. 7; line 32: What does “*differential*” mean?
- p. 8; line 8: Try to read “*2013/2014 only due to data unavailability for 2014/2015*” and replace that sentence.
- p. 8; line 30: “*melt and re-freeze occurred*” That really questions the term “*pre-melt*” used elsewhere in the paper.
- p. 9; lines 32-33 : “*gravimetric water fraction*” vs “*volumetric water content*” What is the relation?

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- p. 10; line 27: “*bias drop*” Do you mean the  $-39 \text{ kg m}^{-2}$  value? Hard to tell whether the short, hardly above freezing period or the following sub-freezing period is responsible for that, isn’t it?
- p. ; line : Replace “” by “”.
- p. ; line : Replace “” by “”.
- p. ; line : Replace “” by “”.

### **Tables**

Tables: Replace “*multi-season*” by “two seasons”

Tables: “*Combined*” What is combined?

### **Figures**

Figures : *To avoid confusion, use an ISO date format, for example, 2014-04-01 for 1 Apr 2014. Is “UTC” the unit of date? As no hour time stamps are shown, you may drop it altogether.*

Figure 5: The division by two of the “Difference in SWE” is somewhat annoying. You could also use the right-hand axis to avoid this.

2016-07-07 / Charles Fierz