Geoscientific
Instrumentation
Methods and
Data Systems
Discussions

Interactive comment on "The Sodankylä in-situ soil moisture observation network: an example application to Earth Observation data product evaluation" *by* J. Ikonen et al.

Anonymous Referee #3

Received and published: 9 February 2016

The Sodankylä in-situ soil moisture observation network: an example application to Earth Observation data product evaluation

GID, Ikonen et al., 2016

This paper describes the Sodankyla in situ soil moisture network and compares the measurements from this network with time series of ESA CCI soil moisture at one pixel. The paper contains useful descriptive information, but the results bear very little scientific value. I will suggest a few other analyses to make the paper slightly more relevant. The paper could also benefit from a revision by a native English speaker.

1) Perhaps update the title to be more specific: "an example application to ESA CCI

soil moisture product evaluation"

2) abstract line 18 and throughout the text: how exactly is "top layer" defined? Here it says 5-10 cm, but later on the sensors appear to be installed at -5 and -10 cm. The measurement centralized at -5 cm is valid for a top layer of 0-10 cm, not 5-10 cm. Please be more precise throughout about the "top layer", because this also affects the comparison with the remote sensing data (which don't give information below 5 cm).

3) I do not see any use in looking at the "weekly running" average values. Those weekly data points are sometimes made of no more than a handful of measurements and not really representative for "weekly" data, but instead simply filtered to remove noise, which evidently improves the statistics (except bias, as expected). Yet, the latter is no surprise and not very useful for remote sensing validation. I would not include that in a paper.

Instead, I suggest to focus more on the spatial scaling and averaging, see below.

4) p.601, line 16: why are ECHAM and JSBACH mentioned? They do not strike me as 'well known' models.

5) p.601, line 27: SMAP has been launched more than a year now. That is not "just recently".

6) p.602, line 19 (and other places in the paper): rewrite as "spatially weighted average of top layer (XX0-10 cm? See aboveXX) soil moisture"

7) p.602, line 25: aren't they using this site in SMAP CalVal as well?

8) p.604, line 21: "These measuring points… some ten meters…": which points are you referring to? Are these the 2 additional "horizontal measuring points"? At what depth? Please clarify.

9) p.606, line 2: why is 2013 skipped in this discussion? (Table 2 shows that 2013 received an additional sensor)

10) p.606, line 15: "within the area". Which area? The area covered by the ESA CCI pixel, 25x25 km^2? Related to this, p.600 line 21 says that the CCI pixel 'encapsulates' the Sodankyla observation sites, but figure 1 shows that the pixel excludes 2 CalVal sites. Please clarify.

11) p.606, line 17: what is meant by "continuously redistributed"? continuously in time? I think this word can be removed here. In general, it does not seem a good idea to create a time series with varying number of sensors, because it will alter the climatology of the time series. It can be compared to the ESA CCI product: they applied CDF-matching to each individual period to get all data in the same climatology. Similarly, the in situ data should be made 'consistent' if additional sensors are included. In order to do so, we'd need to look at the time series with and without the additional sensors and then match the climatology to be consistent across all years.

12) p.607: the weighting happens based on soil types. What about terrain (e.g. slope) and vegetation?

13) p.609, line 2: one example of several English issues: "does not have an effect" instead of "affect".

14) p.609, line 15: ASCAT passes over at specific time steps. When the 'daily' values are compared to in situ data, did you match up the exact overpass time steps? Please clarify in the text.

15) p.610, line 3: "apparent" noise? That is not apparent, that is real noise. . .

16) Figure 7 and (8, 9) show the same information. It is sufficient to show the time series (Fig 7) only. Give units to all axes.

17) p.605, line 17 and Table 2: The last 2 sensors are not calibrated, so why include them in the analyses of this paper?

18) Table 3: no need to discuss the "weekly filtered" statistics, they do not add any value. Instead, I would suggest looking at the statistics for 2015 with its 8 sensors

(adequately spatially averaged), then with its 6 sensors (same as in 2013-2014) and then with its 5 sensors (same as in 2012). Then discuss the impact of the additional sensors. Same for 2013-2014: compare the results for 5 (as in 2012) and 6 sensors. So, in short, discuss the effect of the spatial averaging in more detail, rather than the weekly filtering.

Ultimately, if I want to validate a product over a longer time period, I will want to use a consistent time series of in situ obs, that is, one with just the 6 sensors going from 2012 through 2015 (unless some clever CDF-matching would have been performed to bring the various chunks of data to the same climatology).

19) Figure 1: perhaps enlarge the spatial map to focus on the CCI pixel and only the few sensors outside of it.

20) Instead or complimentary to Fig 2-4, can you provide a spatial map with the areas (weights) attributed to each sensor based on soil types in "the area" (assuming the CCI pixel), rather than over this big region?

21) Figure 7: remove the connector lines between the observations to clarify that the data points are only taken at times when satellite data are available and not interpolated.

Interactive comment on Geosci. Instrum. Method. Data Syst. Discuss., 5, 599, 2015.