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Interactive comment on "Gap-Free Global Annual Soil Moisture: 15km Grids for 1991–2016" *by* Mario Guevara et al.

Anonymous Referee #1

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This paper presents a 15 km annual-average soil moisture product that is generated by machine-learning the relation between 0.25 degree ESA CCI soil moisture estimates and topographic indices derived from a higher-resolution DEM.

I have several major concerns regarding the hypothesis/assumptions on which the methodology is based as well as the employed validation methodology, and consequently also the conclusions drawn from the presented analysis:

The methodology is based on the hypothesis that topography is a main driving factor for soil moisture patterns. However, the reference used to support this claim (Mason et al., 2016) presents only a very local analysis of differences between soil moisture values at low-slope and high-slope areas over grasslands only, and only in a small region over the UK. The observed relation is relatively low (R^2 = 0.21) and the authors

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conclude "[...] a topographic signal can be seen in high resolution remotely sensed surface soil moisture data [...]. Unfortunately this signal is relatively weak." Moreover, Mason et al. (2016) uses 1 km SAR data for which topographic corrections are applied in the pre-processing, which likely induces a sensitivity of the measurements to topography parameters. These topography corrections are usually not applied to coarse-resolution measurements such as the sensors used within the ESA CCI SM, because topographic effects average out at these scales. The presented paper itself also does not analyze the predictive power of the used topographic indices for soil moisture (e.g., the godness-of-fit for the obtained regression, variable importance, etc.).

Hence, there is no evidence supporting the reliability of topographic indices as predictor for soil moisture, especially on a global scale. Even more doubtable is the assumption that the developed regression function can be used to extrapolate soil moisture to regions not covered by the ESA CCI SM, which are mainly the arctic ice sheet and tropical forests. Tropical rainforests, for example, have a quite unique moisture regime that is expected to be largely rainfall dependent. It is very questionable to use a soil moisture - topography relation that is trained over non-tropical regions to predict soil moisture there. Moreover, no in situ measurements are available in these regions to verify the validity of these predictions.

Also, the presented validation does not support the conclusions. First, the statement (L234) "In all cases, the evaluation statistics are equal or better for the downscaled soil moisture predictions based on digital terrain analysis (Table 3) than the original ESA-CCI soil moisture product (Table 2)" is wrong. In fact, results are quite balanced, sometimes the downscaled product is "better", sometimes the original is "better", but most likely results are not actually distinguishable within reasonable confidence limits (which should be estimated). The authors do indeed acknowledge (L252): "The downscaled predictions based on digital terrain analysis are not significantly different compared with the ESA-CCI soil moisture product [...]", but the subsequent conclusions are not supported. Specifically, "[...] but they provide (1) gap free soil moisture-related information ": while they are provided, there is no evidence that they are of any reasonable accuracy (for the earlier discussed regions), and "(2) higher resolution (from 27 to 15 km grids)": This is a mix-up of resolution and sampling.

Improved resolution would imply that there is different / more information in the downscaled product, but the indistinguishability of performance metrics (see above) suggests that this is not the case. Also the comparison in Figure 6 (b and c) shows that the original and the downscaled products exhibit the exact same behaviour with a slightly lower overall variability in the original product. It is, however, not clear whether this different overall magnitude reflects an actual improvement, because soil moisture varaibility and trends are actually supposed to be different at a point scale and at a satellite scale (see e.g. Famiglietti et al. 2008). Also, the soil moisture mean is supposed to be different at different scales, hence the negative bias between point and satellite measurements cannot be reliably interpreted as error, and a reduction of this bias may as well be a going in the wrong direction with respect to the true areal-average mean.

In other words, even though the generated product is sampled on a higher-resolution grid, it can not be concluded that this product contains higher-resolution information. Given the low amount of evidence that topography (alone) is a good predictor for soil moisture, observed differences may well be a result of the smoothing-nature of the KKNN approach, and any spatial-window resampling approach may lead to a seemingly "higher-resolution" (which is truly only a higher-sampling) product with the same (or even better) performance, but this is not tested.

Therefore, I recommend to reject this publication. However, I do believe that topography may well be an important complementary predictor for soil moisture at higher-resolution when combined with other dominant factors. I therefore encourage the authors to pursue this approach addressing the concerns outlined above.

References: Famiglietti, J. S., Ryu, D., Berg, A. A., Rodell, M., & Jackson, T. J. (2008).

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