Review of ESSD-2017-13, Montzka et al., Global Data Set of Soil Hydraulic Properties ….

Overall, a data set of very high quality and strong usefulness. Very good fit to this journal. Good descriptions and very good explanations. Data set downloads easily from Pangaea.

Some additional explanation of the netCDF data files would help. The 'Schaap' in the filename refers to application of the ROSETTA PTF (reference Schaap) but implies that one might in the future encounter a parallel data set produced using some other PTF strategy? And the 'sl' refers to soil level but it took me a while to confirm that. Some clearer naming or readme file could better help link these individual files to this description?

A few other comments and suggestions:

Many instances e.g page 2 line 24 where sequence of references lacks needed spaces between references (e.g Dimitrov et al., 2014;Jadoon et al., 2012;Montzka et al., 2011). This comes from the bibliometric software used by the authors. They should fix the problem rather than relying on proofreaders to catch all these small formatting errors?

On page 4, line 8, the use of the phrase 'character size' in this sentence opens some confusion? Earlier in the sentence the authors wrote about hydraulic characteristics but here 'character' size refers to the magnitude of a soil property? Or to grain size characteristics or pore size characteristics. The reader doesn't understand what the authors intend here.

On page 6 lines 20 to 26 the MvG factors α and η are introduced as scaling factors. But in fact both terms derive from measurable soil properties? Fundamental physical parameters then applied as descriptive (e.g. scaling) factors? One gets the impression here that soil scientists use them for mathematical convenience, not as measured properties.

Figure 1 provides a very useful graphical map of the data processing. Likewise the manuscript itself provides very useful, well-documented descriptions. But, as the authors know, and as careful reading exposes, the processes used here to assemble the global reference data set and then to support - statistically - up-scaling to GCM grid scales both rely on and introduce some additional uncertainty? Having - and plotting in geographic space - the variance terms will as the authors say prove very valuable, but calculating variances does not remove or explain underlying uncertainties? This comment does not criticise the work, only suggest that, perhaps in a short paragraph in the conclusions, the authors assemble some of the uncertainty terms they have mentioned throughout the manuscript, as useful reminder to (perhaps) less-familiar users about the need for some caution. This cautionary paragraph could include the scaling uncertainty (page 4), the fact that the authors have (probably correctly) used mean values rather than full confidence interval data from the SoilGrids source (page 6), that they derive HCC from WRC rather than calculating independently (another necessary choice but a choice none-the-less, e.g page 8), their hypothesis / expectation that variance scales with spatial resolution (page 10), that they report here only data from top-most soil layers (page 11), that they recognise deficiencies in application of global PFT to specific boreal soils (again page 11), etc. Again, the authors have presented and defended good choices necessary to make a very good product! But, as part of their conclusions, they should remind readers / users of the uncertainties avoided or introduced by those choices. A short addition to the conclusion section?

Page 10, lines 3, 4 - some numbering problems in this section? '(ii)' used twice?

Page 10, line 9 - 'VGM' a new term (if so need definition) or in fact a typo of MvG?

Page 14, lines 14, 15 - this sentence "On the other hand, the relative decrease of variance with coarser resolution for this region is compared to the other ones (Figure 11 right panel)." The authors mean to write 'comparable' rather than "compared"?

Figures 2, 4 - Very useful plots of this 0.25 degree product on top of the original 1km SoilGrids product. But the authors have chosen sand fraction and a very compressed colour scale in both figures. Does sand fraction represent the obvious comparison parameter for both German and global sites? Figure 3 indicates that most variance in soil properties for the German sites occurs along the sand-silt axis but from the compressed colour scales we can't tell whether the final product can distinguish 20%, 50%, 80%, etc.

Figures 5, 6 - This may represent a too-simple visual-only analysis, but I think the authors with their descriptive text for these two figures have allowed a question to arise. Given the distinctly higher (relative to other large regions of the planet) variance (visual, not calculated!) in soil hydraulic properties in the region across north Africa (no other large region shows such consistent extreme values in 5 top through bottom), and the authors's plausible explanation of a strong wet-dry seasonal signal in the Sahel (page 14) and predominance of sandy soils in the Sahara (page 12), wouldn't we expect distinctly high values of scaling variance at least across the zonal boundaries of that north African region (e.g. Figure 11 left)? It might be very useful to have the regional boxes from Figure 4 reproduced in Figure 6? Perhaps north Africa represents a region of hydrologic extremes but of relative spatial homogeneity in soil properties? Or a region of relative data sparsity? Perhaps these figures will provoke similar questions from other readers?

Figure 7, 8 and 9. This reader finds these plots very useful! But, if averaging the MvG parameters represents the preferred approach, at least from this work, shouldn't the conclusions (top paragraph of page 15) and these figures make a strong case? In the narrative and in the graphics? A clearer graphical and textual message could better promote the strong outcomes of this work?

Figure 11 - here again the distinct question about Africa emerges. Figure 11 left shows very high values for Africa but, to this reader's eye at least, Figure 6 does not support this?

Figure 11 right presents some very interesting challenges. In the figure, and in the supporting text (page 14, lines 19 to 21), scaling variance remains quite large (the authors use the word 'conserved') out to 100 km? A default resolution for earth system models used in CMIP6 might be 100 km. Some modelling centres will run higher resolution at perhaps 25 km globally. Ensemble work at global scales - which could and should take advantage of the variances recorded in this data set - will almost certainly occur with 100 km (or larger) resolution. In those 25 km to 100 km spatial ranges, for these soil properties, the upscaled products preserve more than 70% of spatial variability information? Steeper drops (reduced variance information fidelity) occur above 100 km? This seems like a very useful conclusion which might deserve more attention in the manuscript?