

Review of ESSD-2107-122, Soil Properties on the Tibet Plateau

Potentially a very useful and important data set. Substantial effort to collect these samples, accompanied by or followed by skilful well-documented sample processing and analysis. Excellent data access tools on the 4TU landing page including nice kml file, easy-to-use Excel spreadsheet and links to many accessory files. (Other ESSD data providers could learn some best practices from this effort and the 4TU site.) Potentially, eventually a very good candidate for ESSD.

But, weak presentation, crowded with marginally-useful information. So much confusion that we miss the central strong points of the manuscript: new availability of valuable data from a difficult but important region, with large (?) potential impact.

So much material to evaluate, very long and difficult task for a reviewer. If reviewers find the information tedious or unconvincing, readers will get discouraged much more quickly and tend to ignore the paper and data for lack of accessibility.

The authors will need to sort through a long list of comments below, most of which I can summarize as: 'What does this <data, graph, analysis> tell us about the Tibetan Plateau?; How could I use it in my own research / modeling?, and - otherwise - why do I need to see this? I believe the authors could produce a much shorter, much clearer and much more useful paper following a clear concise outline:

- What data did we gather and how;
- How does this data fill gaps, geographically and scientifically;
- How does this data compare to prior studies or to other high elevation regions;
- How do we as the data collection experts recommend to process the data for incorporation into land surface models; and
- What encouragements and cautions do we offer to users?
- Summary - how does this data help us understand soil moisture on the Tibetan Plateau.

I believe the authors could eliminate about 60% to 70% of the present content. Some of the remainder could go into an Appendix. This 'thinning' process would allow the authors to focus on what they have provided new and on the potential impact(s) of that new data.

Many comments follow. I recommend the authors read them for specific issues, but not attempt to respond to and revise text in all cases. Instead, give us a shorter clearer product that better represents your efforts.

Apparently not one mention of snow in the entire manuscript. Likewise for 'elevation' - not mentioned. Can one really describe soil measurements for high elevation soils without mentioning snow or elevation?

Line 31: "huge" impact on global climate - probably not. Southern Ocean has a much larger impact on heat and carbon fluxes. At the moment, loss of Arctic sea ice and of northern hemisphere snow cover (including over the Tibetan Plateau!) probably have a larger impact on northern hemisphere circulation patterns than TP by itself. TP certainly has a substantial local and regional impact, including on the Asian monsoon systems.

Lines 31 to 91, Introduction: The introduction as written contains too many threads. After a brief geographic description, the authors take readers on a wandering tour through: how and why Land Surface Models work or fail; how other research groups have developed and used PTFs; how LSMs use PTFs; how one needs soil hydraulic and thermal properties to understand and predict soil moisture; how a long list of global observational data sets for soil properties include or do not include observations or correct observations from the TP region.

Some of this material we do not need. Some of it belongs near the end, in a section on impact and future use of the TP data presented here. The sentence at lines 53 to 55, about the need for basic observations of soil properties to understand land-atmosphere interactions and thereby to also improve LSMs, seems like the good place to start. Then how LSM need good parameterisation of hydraulic and thermal properties and how those depend on good

observations, how TP presents a challenge to observations, and therefore the importance of these new data products.

I missed seeing mention of some of the 'third pole' aspects: high elevations at low latitudes cause the unusual presence of ice and snow; precipitation and hydrology difficult to predict; importance both for climate (as mentioned) and for water resources (and biodiversity?); with accurate observations of soil moisture at the centre of all this?

We understand that the authors have extensive technical knowledge and good technical English. Although they thank a colleague for assistance with English translation, the manuscript could benefit substantially from improved science communication. Perhaps the journal can recommend someone who could help from technical English into public-friendly English (assistance that many native English speakers also need)?

Line 97: Sample locations. Figure 1 not useful as presented. Upper left, we do not know what the colours represent? Satellite image of what? We get no sense of terrain / elevation, seasonal snow cover, vegetation if any, etc. Plot the FAO Aridity Index? The browse map on 4TU looks useful, use that? Or use the kml image from Google Earth? In panels a, b and c for the three regions, we do not see elevation contours, vegetation, land use, annual precipitation? Can we even trust the Aridity Index in this region? The explanation of the blue dots vs. the purple triangles needs to appear more prominently, to make clear that it applies to all three panels. In this figure legend you could list the previous sampling networks / sites, then you would not need that information in the text?

Line 110: as written the sentence implies that Yang et al. in 2013 collected the Naqu samples. Because you have already associated the CTP-SMTMN network to Yang et al. 2013 (e.g. in lines 51, 52) you do not need to cite that paper again here.

Lines 112 to 116, "155 total stations ...". This quantitative summary and explanation of sampling constraints should go at the start of the field experiment section? You do not say anything about seasonal freezing of these soils. All samples collected at elevations clearly in the freeze-thaw zone? From the kml file we see Naqu at least 4500m, Ngari at 4800m, even Maqu at 3500m? At those elevations, seasonal snow cover and frozen soil? Elevation terms seem completely missing from presentation and discussion. In the very nice pop-up information boxes for each sampling station in the kml file, all elevations show '0'?

Lines 144 to 149 - Because a reader gets the list of external datasets used for intercomparison here, perhaps we do not need the same information in the introduction?

Line 154 - SOC may have stronger influence on water retention than on porosity, but porosity and water retention can not both increase, e.g. they have an inverse relationship (as you already said in line 152 and as you show in Figure 4).

Line 155 - "properties tend to decrease" Which properties. SOC? Again, porosity and bulk density should not both decrease.

Lines 159 to 161 - Because we get a description of the SWRC schemes - and their associated authors - here, we do not need the same information in the introduction?

Results and Figures - way too much! The authors have shown us everything. Instead they should show us what matters: unique features of these data, where these data confirm or fill gaps in the global soil datasets, the best or preferred parameterisations but not every possible combination? Instead of every possible parameter, what makes these data useful? Figure 2 and the Appendix can guide us through your processes. Show us your best or surprising outcomes! What strengths do these data offer to other users? What weaknesses? What further research - by your group or by others - now becomes possible? Give us the most interesting or useful examples but not every example. Eliminate many Figures? Some tables and figures could go in the Appendix, with the relevant equations. Focus on what new and what valuable these data provide to the research community!

Line 200 and Figure 3, soil texture: Here we need additional information to understand statistical reliability of these soil descriptions. If the bars in Figures 3a,b and c represent the minimum and maximum values for all sample locations (usually 8 or fewer?) in that region at those depths, then the authors really can not say that sand fraction at 5 or 10 cm exceeds sand fraction at 20 or 40 cm because the range of values appears very wide at all depths. Give us the sample numbers for each location (and perhaps for each depth if those vary)? In the very clean and helpful data spreadsheet, I find only the same summary data: mean, min and max. No indication of sample number or statistical distribution (e.g. std error). To this reviewer, sand fraction at Ngari not statistically different from sand fraction at Nadu and no reliable vertical pattern at either location? Figure legend says minimum and maximum values in the "profile". The authors must mean in all profiles (note the plural) from that region because they have given us no hints of replicate samples at any depth of any profile. Only GGF varies between arid and semi-arid? Otherwise the soil texture at these two sites appear the same. Maqu statistically different to the other two sites but with no significant vertical variation except perhaps for SOC? Likewise for the fine and gravel diameters? We do not get information needed to agree or disagree with statements like this one: "the gravel content increased the deeper the layer." at Ngari (line 208). This reader does not find these plots useful or surprising. Do the authors have something special to show us about TP soil texture? Or do these profiles look as expected? Either way, we don't need to see this? And we don't need more than a sentence or two of description? Could the authors make the raw data available? E.g. instead of listing only mean, min and max in the spreadsheet, could we get all the station data for each region so that we can plot our own statistical distributions and vertical profiles?

Line 223 and Figure 4, bulk density and porosity. Same statistical problem: seeing maximum and minimum tells us nothing about distribution. At least we can see different patterns for different regions but again, do the authors want to call our attention to anything special about TP soils? If not, why do we need to see this figure or read this discussion? We have no statistical basis to accept this statement (line 233): "The stratification of BD and porosity in the profile might be induced by SOC layering as Fig. 3c reveals." because we have no confidence in the so-called SOC layering - except perhaps at 5 cm depth - in Figure 3c.

Line 238 and Figure 5, hydraulic conductivity. Same statistical problem. With a log scale for the abscissa, this reader does not see any reliable vertical pattern or station difference between Ngari and Nadu. Magu might differ from the other two and might show one vertical outlying value. Anything special about TP here? The mean and st dev (from the supplement, mentioned in line 248) might prove more relevant here and then put this figure in an Appendix or supplement?

Line 250 and Figure 6, GGF with porosity and hydraulic conductivity. Nothing useful here at all? Anything that would distinguish these as TP soils? At least we can visually get some idea of number of samples? For 5 cm at these two locations, 5 or 6 samples in one case and perhaps only 3 for the other case? This reader finds no validity nor any information useful to TP in either text or figure. No significant slopes or significant R-square values for any depth at either location? Suggest that they really do not have enough reliable data collected across a wide-enough range of station locations and soil types to make this analysis useful? The sentence in lines 264, 265 represents everything we need to know here?

Line 267 and Figure 7, heat capacity and thermal conductivity. If "no distinct stratification" then we really don't need to see 7a, b and c? For 7d, e and f, these statements assume that we have accepted vertical stratification of soil texture above. If we have not, if the authors have not helped us by identifying statistically reliable patterns, then these figures and the associated paragraph have no utility. Again, anything special about TP or do these look like all other soils?

Line 280 and Figure 8, porosity measured and predicted. No need for all these correlation figures, you could do it all with simple correlation coefficient values. Or a simple statement: BD provided the best fit across a range of depths and porosities, as in lines 305, 306, then refer reader to Appendix. We do not need to see all these mis-fit figures. How do these data compare to any other high-elevation soils? Anything special or need special attention about the TP soils?

Line 308, SWRC comparisons. Figure 9: nothing useful. CH and VG not distinguishable at any station. Maqu different to Ngari and Naqu, but we already knew that. What, if anything, proves

specifically relevant to TP? I doubt the utility of this figure but if you keep it, it should go in Appendix or supplement. Figure 10: (why back to colour?) Again, no useful information by station, depth or parameterisation. If nothing unusual or relevant to TP, omit this figure. Figure 11: possibly some useful information here but one could do it in a small table rather than this mostly-empty (visually and intellectually) table. 90% of bias values identical regardless of station, approach or author. Overall we really don't need more than the concluding sentence (lines 346 to 348) for this entire section. How does this conclusion compare to other soils or other high-elevation soils? Anything special about TP here?

Line 349 and Figure 12, hydraulic conductivity: Colour, but nothing to help a reader extract useful information from this figure. Small biases for many parameterisations, but relevance? If you give us the summary (lines 362 to 364) we don't need to see this figure? Should we learn anything special about TP soils from this discussion?

Line 366 and Figures 13, 14, heat capacity and thermal conductivities. By now this reviewer feels like a broken record, repeating endlessly the same messages. I find nothing in this text or figures that shows me anything useful about TP soils or about the relevance of various parameterisation schemes to TP soils. We do not need to see any of this?

Line 395 and Figures 15 and 16, comparison of basic soil properties: This information should lie at the heart of this paper, to help users understand the impacts of this new data. Figure 15 potentially useful but it has too much white (empty) space. Because many potential users will skip text but look at this figure, the authors should define the data source acronyms in the Figure legend. In Figure 16, these observations for soil porosity in all three regions appear as clear outliers, at many if not all depths, to all the existing data sources. Why? What impact? What do the authors want us to know about the strengths or weaknesses of their data compared to the existing products? Do the authors have something special to say about TP soils? Missing answers to this question: how do these data help fill scientific or geographic gaps?

Line 414 and Figures 17, 18, 19, soil water retention curves: Good initial point that if the existing soil descriptions have uniform (but, inaccurate?) soil porosity profiles they will also necessarily have wrong water retention curves. Figure 17 emphasises this point, e.g. not possible to extract useful information from that figure? Suggest the authors omit Fig 17. Figure 18 potentially useful - first time we might actually see an impact of the TP observations - but visually not helpful. Most schemes, especially VG in the lower panel, look identical regardless of observation sources. If all the prior observations tend to converge on wrong values, then one would expect this uniformity? Perhaps lump all the prior obs with an uncertainty term and then show the impact of the current data? This statement (line 432) "With the Tibet-Obs dataset as input to the applicable PTFs good FC and PWP were generated." seems to defeat this entire effort, because measurements presented here do not converge with so-called Tibet-Obs and Tibet-Obs existed prior to this extensive data gathering effort! How do the present observations impact PTFs? We never get a clear statement? Figure 19, same concerns as Fig 18. If all the prior obs converge to a uniform but wrong data value, then all the hydraulic conductivity outcomes will necessarily look basically the same? Average and lump them all, then show the new data for comparison? Chance for a strong impact here but lost because of the vague presentation.

Lines 442 to 477, figures 20 to 22. Possibly these figures actually contain useful information showing impact of these observations? Typical author approach - show us all curves in every possible combination - defeats what might have appeared as useful information. Too many plots look indistinguishable: panel a and c in Figure 20, all panels in Figure 21, all panels except 'a-right' in Figure 22. (Figure 22 looks almost useless?). But the authors could help our eyes find useful information: where the SHP and STP factors differ substantially (higher or lower) from the observations? Reading the text, I found only a narrative version of the indistinct graphics: sometimes overestimated, sometimes underestimated, everything treated the same, no clear outcomes. If no clear outcomes, leave this out entirely? If distinct and useful outcomes, highlight those?

Personally I found this disappointing. At the end of all this work, at the conclusion of careful analysis, we basically get more curves and scatter plots that tell us almost nothing? If true, then the authors should simply present the data with its strengths and uncertainties and forego all of

this analysis, leave all analysis to others or to a subsequent research paper? If, instead, the authors have tangible results and unique features to point out, then they need to take a much clearer shorter route to explicit and compelling outcomes. Their approach of plotting all possible variables and schemes in all possible combinations simply doesn't work.

Line 478, Data availability: Technically very good as already mentioned. Spreadsheet contains the averaged values rather than the raw data, which prevents users from doing their own statistical analysis.

Line 483, Conclusions: Paragraph one, that the soil properties vary across climate zones and within profile seems not surprising and perhaps - because of the absence of necessary statistics - not valid. In any case, why would one expect anything different for any soils anywhere on the planet. What, if anything, do these data show us about TP specifically? Paragraph two, that some schemes work better than others seems also not surprising. We could have learned this from any series of soil samples taken anywhere in the world? Did we learn anything peculiar about high elevation soils generally or about TP soils in particular? Paragraph 3 about how these data impact (or improve?) estimations SHP and STP and about relevance to remote sensing of SM seems potentially useful, but we miss any explanation or emphasis about how these data have filled scientific or geographical gaps. We get only tentative recommendations applicable to each of the climate zones for the TP, but no assessment of if and how these data impact our basic understanding of those proposed climate zones? For what part of the year can satellites actually attempt to measure soil moisture (as opposed to snow cover) at these TP sites? Paragraph four seems like a short useful summary but it implies that the authors only evaluated the applicable schemes. It omits the essential fact that - apparently for the first time - we actually had observational data on which to base the evaluations of each scheme.