Dear Reviewer 2,

We are grateful for your constructive comments and suggestions. The comments are considerably useful to improve the quality of our paper. We've adjusted the structure and carefully revised the manuscript accordingly. Please find below your comments with our point-to-point responses as well as the corresponding changes in the modified manuscript.

Sincerely,

Hong Zhao, Yijian Zeng, Shaoning Lv, Zhongbo Su

Review of ESSD-2107-122, Soil Properties on the Tibet Plateau
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.
Response: Thanks for the comments. We reorganized the structure of paper and rewrote the outline to make the central strong points stand out.

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.

Response: Thanks a lot for the critical comments. We've reshuffled the manuscript to follow the above outline.

The **"What data did we gather and how"** is described in the "Materials and methods" of Section 2.1 and 2.2.

The **"How does this data fill gaps geographically and scientifically"** is described in the end of Introduction and Conclusions as well as in Section 4, and explained also as follows:

The first aim of collecting this dataset is to investigate the suitable parameterization schemes to estimate SHP & STP for land surface modelling on the Tibetan Plateau (see Sect. 4.1). The second purpose is to quantify uncertainties of existing soil database and their derived SHP & STP over the Tibetan Plateau through comparisons with this dataset (see Sect. 4.2). It is imperative to quantify the uncertainty of existing soil dataset, which will inherently affect the accuracy of modelled land states (e.g. soil moisture and temperature, surface flux) by LSMs as well as retrievals from remote sensing.

"How does this data compare to prior studies or to other high elevation regions"

Heterogeneity has had an effect on the values of soil properties and parameters throughout the sampling procedures, as with any soil field experimentation. Therefore, we evaluated the quality of the measured soil property dataset through comparisons to those in the literature to make sure that they were within a reasonable range. This is described in Section 2.2. Meanwhile, our focus is the *in situ* measurements of basic soil properties and SHP & STP on the TP and their potential applications for land surface modelling over the TP. We did not compare the soil data with those from other high elevation regions. Our concentration is the soils on the TP that are uniquely specific as the TP is located in the Asian monsoon region, and the difference of soil characteristics to other high elevation regions may be induced by different climate regimes. As such, the comparison of soil properties on the TP with those from other high elevation regions is beyond the scope of our current work.

The **"How do we as the data collection experts recommend to process the data for incorporation into land surface models"** is presented in Section 4.1 and Section 4.2. In Section 4.1, based on the *in situ* measured basic soil properties data, the estimated SHP & STP from various schemes were compared to the measurements, and **"the encouragements and cautions on using these schemes for land surface modelling"** are described. In Section 4.2, based on the *in situ* measured basic soil properties data, uncertainties of basic soil properties information from existing dataset and their derived SHP & STP were quantified, and the encouragements and cautions on using existing soil datasets for land surface modelling are described.

The **"Summary - how does this data help us understand soil moisture on the Tibetan Plateau"** is described in the Conclusions.

I believe the authors could eliminate about 60% to 70% of the present content. Some of the remainders 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.
Response: We've re-organized the manuscript and presented it in a more structured way to allow easier capture of the main points.

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.
Response: Thanks a lot. After digesting these valuable and helpful comments, we have tried our best to provide a shorter, better and more easily accessible paper. We also provide the responses to the comments point by point.

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?
Response: Thanks a lot. Elevation is one of the key soil formation factors. We add elevation information to the TP geographic description in the field experiments of Section 2.1. Elevation information was also provided for each sampling site in the updated spreadsheet (https://data.4tu.nl/repository/uuid:c712717c-6ac0-47ff-9d58-97f88082ddc0). However, it is to note that our main concentration is not to investigate how soil properties vary with elevation changing.

For snow, as it is not a part of basic soil properties, we did not consider it in this study. On the other hand, snow is very important land surface processes over the TP, and its interaction with the underlying land surface certainly is influenced by the soil properties though. It is to note that the change of soil properties under frozen condition is another topic and is beyond the scope of the current manuscript.

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.
Response: Thanks a lot for this comment. We have revised this correspondingly.

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 parameterization 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.
Response: The suggested structure of the Introduction is very useful for presenting why we implemented this research and the importance of this in-situ collected dataset. We have rewritten the Introduction accordingly.

First paragraph introduces the importance of TP research and the role of accurate soil moisture simulation in these studies. Then the decisive role of SHP & STP for SM simulation by LSMs is introduced along with the definition of SHP & STP. Second paragraph starts with the need for basic observations of soil properties to improve LSMs modelling and thereby for understanding land-atmosphere interactions. The “how LSMs parameterize SHP & STP and how those depend on good observations” is described in the following paragraphs. Fourth paragraph presents the issues of existing soil datasets describing soils over the TP. Fifth paragraph shows the objective of the paper.

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?
Response: Thanks a lot. We added a sentence of “Accurate SM information is claimed as a necessity for improving precipitation and hydrology forecasts (Drusch, 2007; Dirmeyer, 2000; Robinson et al., 2008) especially on the TP where it undergoes evident climate change (Ma et al., 2017; Douville et al., 2001; Yang et al., 2014; Yang et al., 2011).” in the Introduction.

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)?
Response: We asked for an English editing again to make paper become more public-friendly. Additionally, for the previous version, we have asked helps from a native-speaking academic English editor.

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?

Response: We have revised the Figure 1 of "The location of Tibet-Obs and the spatial distribution of soil sampling across three climate zones". Figure 1a is the Tibet-Obs networks distributed under the three different climatic zones where the zones were classified based on the FAO Aridity Index Map. Figure b, c and d are samplings distribution in the kml image from Google Earth. We removed the distribution of previous sites to reduce the ambiguity. The relevant information on vegetation, land use and annual precipitation were described in the first paragraph of Section 2.2.

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.

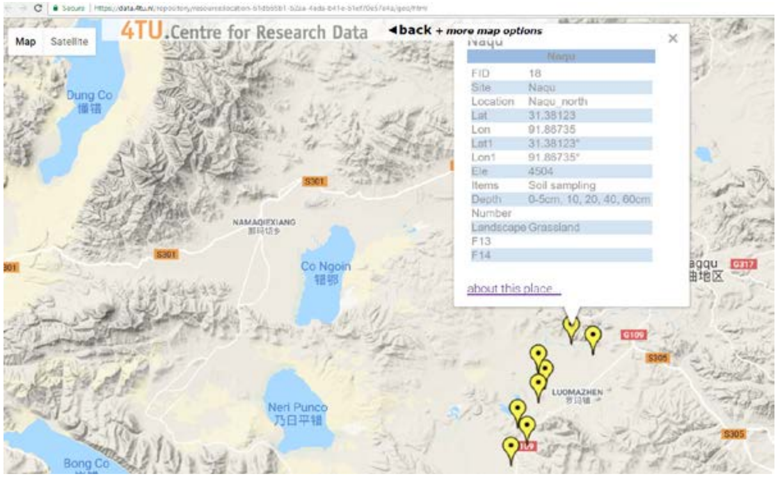
Response: Thanks a lot. Done.

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'?

Response: It is to note that the change of soil properties under frozen condition is another topic and is beyond the scope of the current manuscript. In this study, we only collected soil samples during the monsoon season. In spite of this, the current dataset is also the basic information needed for understanding soil hydrothermal processes during the freeze-thaw cycles, especially when combined with Clapeyron Equation to get the soil freezing characteristic curve.

We prefer not to move "155 total stations…" to the start. The field experiments of section 2.1 firstly introduce the different soil sampling schemes and soil data types that we tended to obtain. Then we describe distributions and amounts of soil samplings over the TP. The "155 total stations…" is summarized thereafter.

We are sorry that we did not provide the elevation information for each sampling station. Nevertheless, with terrain information provided in the base map, one can see the elevation information for each sampling sites (see picture in below, https://data.4tu.nl/repository/resource:location-61db65b1-b2aa-4ada-b41e-61ef70e57e4a/geo/html). Furthermore, we added elevation information for each sampling site in the updated spreadsheet (https://data.4tu.nl/repository/uuid:c712717c-6ac0-47ff-9d58-97f88082ddc0).



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?
Response: We agree with the comment. We deleted the same information about external datasets 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).
Response: Sorry for the confusion here. We actually want to express that with more SOC in the soils, both porosity and soil water retention increase. The inverse relationship refers to the relationship between porosity and dry bulk density. Here the focus is the porosity. To reduce the ambiguity, the part with soil water retention was removed.

Line 155 - “properties tend to decrease” Which properties. SOC? Again, porosity and bulk density should not both decrease.
Response: Sorry for the confusion. We revised it into “the porosity tends to 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?
Response: Thanks for this comment. The SWRC information in the Introduction describes how LSMs parameterize SHP & STP and highlight how those parameterizations depend on good observations of soil physical properties. Here we tend to repeat the information as such for readers to follow easily.

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 parameterizations 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!
Response: Thanks for the inspirational comments. In the revised version, we highlighted the uniqueness of this dataset and its applications for land surface modelling. We’ve reorganized the structure of the paper as two parts: the description of this *in situ* dataset and the application of this dataset.

We agree with the comment. We prefer to keep all figures about describing the mean soil properties at different depths in the text for showing a comprehensive analysis based on this dataset. As they are needed to understand the difference between the measured soil properties and the existing soil databases. Furthermore, they are also needed to understand how the soil properties differ with each other under different climate zones over the TP.

To support the discussion of the application of this dataset in LSMs, we keep figures describing the comparisons for SWRCs and K_s parameterizations, and figures describing the uncertainty of existing datasets compared to the measurements. We put other supported figures and tables in the Appendix.

This soil dataset has supported one of our team member to develop the frozen soil module for STEMMUS (Simultaneous Transfer of Energy, Momentum and Mass in Unsaturated Soils), which has reproduced the soil hydrothermal dynamics over TP (Yu et al. 2018, under review). The other usage of this dataset is to estimate the penetration depth of microwave radiometry (Lv et al., 2018). Furthermore, our *in situ* collected dataset has been incorporated into the Soil Water Infiltration Global Database (Rahmati et al. 2018, ESSDD).

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 Naqu 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 Nqari (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?
Response: Thanks a lot for reviewers' comments on this perspective. Indeed, in the Maqu network, eight samples were taken at each depth, and these soils can represent the soil conditions for most areas under the sub humid zone based on our field campaign (see photo of soil profiles taken in the field). We focus on analyzing the mean soil physical properties under three climate zones and their differences. These analyses are consistent with what we observed in the field, such as surface soils are silt-rich and with organic matter covered, and soils in deep layers tend to become fine sand dominated. We replotted Figure 3 with only presenting mean information and put max and min values in the data spreadsheet. The same was implemented for the other two networks. We've pointed out that "The mean and the standard deviation of the soil properties of the profiles in the three climate zones are listed in the Supplement (Table S2-S4)" in the end of analyzing basic soil physical properties. What is more, we've already included the raw data for all station data. The raw data for each site is available in

https://data.4tu.nl/repository/uuid:c712717c-6ac0-47ff-9d58-97f88082ddc0. Readers can have further information on the statistical reliability of the analyses provided in the paper.



(ELEBARA region)



(CST05-near)

We agree with the comment that the mean sand fraction at Ngari is not statistically different from that at Naqu. While the organic matter (SOC) and gravels (GGF) in soils were found significantly differing for these two networks. It is to note that currently there is no information available providing the comprehensive basic soil properties (as in-situ collected) and SHP & STP dataset across the three different climate zones over the TP. This kind of information might contribute to land surface modelling community for the third pole region, also to soils and hydrology communities. We added these discussions in the part of “how TP presents a challenge to observations” in the Introduction, which paves the way of highlighting the importance of Figure 3 (also Figure 4, 5, 6, 7 and 8). A short summary in each paragraph of “Soil texture” part in Section 3.2 describes a reliable vertical pattern for each network.

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 laying as Fig. 3c reveals.” because we have no confidence in the so-called SOC layering - except perhaps at 5 cm depth - in Figure 3c.

Response: We agree with the comment. We focus on analyzing the mean soil physical properties for each climate zone over the TP. We replotted Figure 4 as well as the updated Figure 3. We put the maximum and minimum values in the data spreadsheet.

Sorry for the inaccurate statement in line 233. We revised it as “The profile pattern of BD and porosity might be induced by SOC layering in the surface and variations of soil texture in the deep layers as Fig. 3c reveals”. This expression was demonstrated based on Fig. 3c and Fig. 4c.

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?
Response: We agree with your comment. SWRC and K_s are the important hydraulic properties. To describe a complete analysis of soil physical properties, we moved Figure 9 to Section 3.2 for providing measured SWRCs information for three climate zones. We also put analysis of SWRC and K_s together.

We replotted previous Figure 5 (Figure 6 in the modified manuscript (MM)) and just kept mean K_s and put the max and min values at different depths in the data spreadsheet. We kindly ask readers' attention on the magnitude of mean K_s in Ngari and Naqu. The magnitude of mean K_s is of the order of 10^{-5} (m/s) in Ngari network, while the mean K_s exhibits a variation of one order of magnitude with depth, namely 10^{-6} (m/s) at depths of 10, 20 and 50 cm and 10^{-5} (m/s) at a depth of 40 cm in Naqu network. K_s measurement and analysis of K_s at various depths are important for LSMs to get soil moisture simulation right, as evidenced in several studies from our group.

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?
Response: As explained in earlier responses, the emphasis of this manuscript is not to "discover" a "unique" soil over TP, but focusing on investigating the basic soil physical properties and corresponding soil hydro-thermal properties for land surface modelling. For the time being, there is no such comprehensive survey of soil properties across the three climate zones over the TP. Furthermore, we want to identify the uncertainty in the existing soil datasets when compared with the in-situ measurements. This will help researchers to select which soil dataset to use for their LSMs.

Gravels have different hydraulic and thermal properties than other fine mineral soils (particle size < 2mm), which have significant impacts on the hydraulic and thermal processes of soil. Gravel is common in soil profiles of the TP especially in its western part, and the gravel content data over the TP is precious. There is very little information in literature describing how the observed *in situ* porosity and K_s differ with gravel content over the TP. Previous Figure 6 (Figure 7 in MM) provides this kind of information.

We are sorry that we only obtained limited amount of samples containing gravels at depth of 5 cm in Naqu and Ngari networks due to the time and labor considerations but also the harsh climate over there. Nevertheless, the data analysis describes a qualitative finding for field reality, which is in line with reported findings based on laboratory experiments (Zhang et al., 2011; Koltermann, 1995; Sakaki and Smits, 2015). LSMs usually do not consider gravel impact on soil parameterizations. We tend to draw attention on the consideration of gravel impact in LSMs for soil moisture and temperature estimation over the TP. Furthermore, revisions in the Introduction paved the way of necessary analysis of GGF with porosity and K_s.

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?
Response: Again, as described in the Introduction, the *in situ* measurements of basic soil properties and SHP, particularly for STP over the TP are scarce. We plotted the mean heat capacity and thermal conductivity at different depths for three climate zones over the TP based on the measured dataset in previous Figure 7 (Figure 8 in MM). Previous Figure 7a, b and c show that the overall trends of the mean heat capacity increase with soil moisture increasing, while they also experience some fluctuations of going up and down, and this is because of difficulties in measuring thermal properties for soils with gravels by using KD2 device. We add these descriptions in the text for reader's caution.
Previous Figure 7d, e and f show how the relationship of λ-SM varies with depth. Analyzing mean soil physical properties at different depths can identify the pattern. We replotted previous Figure 3, 4, 5 and 7 and used only mean values in the profiles. The raw data for the measurement of heat capacity and thermal conductivity have been included in the updated spreadsheet for readers to check the statistical reliability.

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?
Response: We agree with the comment. We presented a conclusive statement in the first part of Section 4.1 and referred readers to Table A2 in the Appendix.

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?
Response: Figure 9 belongs to the analysis of determined SWRCs based on pressure-cell measurement for three climate zones over the TP. SWRC is the important hydraulic properties, and the amount of soil water retention at any soil matrix potential can be calculated based on this curve. We moved Figure 9 in Section 3.2 as the Figure 5 to do a complete analysis of soil physical properties in Section 3.2

As described in the Introduction, since direct measurements of SHP & STP are always too much time, labor and cost consuming, pedotransfer functions (PTFs) (Bouma, 1989) using basic soil property information are developed to estimate SHP & STP. Various PTFs (see Table A1) can be used in LSMs to estimate SWRCs and K_s and STP, while the uncertainty of using various PTFs for SHP & STP estimates over the TP is not available. Based on *in situ* basic soil physical properties data, we estimated SWRCs-CH and SWRCs-VG by using various PTFs and compared to the measurement determined SWRCs. The performance of each PTFs in estimating SWRCs and K_s over the three climate zones was evaluated and the appropriate parameterization schemes were recommended. Previous Figure 10 (Figure 9 in MM), Figure 11 (Table A3 in the appendix in MM) and Figure 12 (Figure 10 in MM) show the results.

Figure 9 in the modified manuscript was updated to illustrate the comparison between the estimated SWRCs from PTFs (color lines) and the measurement determined SWRCs (black line with marker) at 5 cm.

This figure together with Table A4 (i.e. biases based on the comparison) in the Appendix present a case about how well these PTFs predicted SWRCs over the TP. We also provided comparisons of the estimated SWRCs with the measurements at 10, 20 and 40 cm in the Supplement (Fig. S1).

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?
Response: As described in the "parameterization schemes" of Section 2.3, K_s is used in both CH and VG schemes for modelling the unsaturated hydraulic conductivity (see Eq. (A12, A14)). Actually, the PTFs used for SWRCs-CH and SWRCs-VG estimates also have their own corresponding K_s equations (see footnotes in Table A1, Appendix). Using *in situ* basic soil physical properties data with three schemes (i.e. the PTFs, the empirical PTFs-VGF and BM-KC), K_s was estimated and results were compared to K_s measured. Previous Figure 12 (updated Figure 10 in MM) illustrates the biases of predicted K_s when compared with the measurement. Such comparison can help suggest appropriate schemes for K_s estimation for land surface modeling over the TP.

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?
Response: Using *in situ* basic soil physical properties data with the specific schemes, heat capacities and thermal conductivities were estimated and results were compared to the measurements (previous Figure 13, 14). The uncertainty of using various parameterization schemes for STP estimates was analyzed, and the appropriate schemes were recommended for land surface modelling over the TP. We updated previous Figure 14 into Table 3 and kept its relevant discussion in the text, and updated previous Figure 13 into Table A5 in the Appendix.

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?
Response: We agree with the comment. This figure describes average biases in soil properties between the existing soil datasets and the measurements taken for the three climate zones. Biases present positive values for some existing datasets while being negative for others, and are larger for some soil properties but lower for others. All these led to the white space in the Figure. We've added the data source acronyms in the caption for previous Figure 15 (Figure 11 in MM).

As descried in the paragraph 3 of the Introduction, no *in situ* BD or porosity profiles were found available over the TP from the existing soil maps. Soil datasets over the TP were generated via interpolation techniques using profiles from other regions. This caused the difference between BD and porosity extracted from existing datasets with the *in situ* measured values. Previous Figure 16 (Figure 12 in MM) presents comparisons of measured porosities to those extracted from the existing soil datasets. It shows that the porosities extracted from the existing soil database are more or less at the same range across the three climate zones over TP, which is not the in-situ case as we surveyed. Based on previous Figure

15 and Figure 16 (Figure 11, 12 in MM), readers can have a clear sight on how well these existing soil datasets describe basic soil properties over the TP.

Indeed, the Tibet-Obs dataset can fill geographic gaps of current existing soil maps of this remote and harsh environment, the third pole region. As such, this work can contribute to updating global soil dataset over the TP and hence help on improving the performance of LSMs over the TP.

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.
Response: Previous Figure 17 shows how SWRCs estimated from different existing soil datasets were close to the measurements at different depths. To clarify the purpose (and values) of this Figure, we prefer to update Figure 13 in MM describing the comparison at 5 cm and put other figures illustrating the comparisons at other depths in the Supplement.

Previous Figure 18 presented the estimated field capacity and permanent wilting point values based on FAO-UNESCO, HWSD, SoilGrid1km and SoilGrid250m datasets. Their max, mean and min values at different depths are close to each other for the three climate zones especially for Ngari and Naqu networks. It shows that the mean differences from some datasets (e.g. between FAO- UNESCO and SoilGrid1km for Ngari network) are greater than 0.04 cm^3 cm^{-3}, which happens to be the needed accuracy of soil moisture retrieval from remote sensing. These analyses are important for understanding LSMs' uncertainties potentially caused by the bias of different soil datasets.

Global and regional efforts have been made for providing better soil databases. While no information shows how well these developed datasets are used for estimating SHP & STP over the TP compared with the *in situ* measurements (see previous Figure 18, 19, 22). We updated previous Figure 18 into the Table A5 (in the Appendix) and cited numbers in the text for highlighting which existing datasets could provide the reasonable field capacity and permanent wilting point, when compared to the measurements. We also updated previous Figure 19 into Table 4 and kept its discussion in the text to ensure the consistency of analyzed parameters.

We do can lump all derived data from different schemes and present the mean and the range. On the other hand, such a lumped way will then lead to no suggestions on which soil dataset is appropriate for the land surface modeling over TP.

No basic soil properties and SHP & STP dataset over the TP are found available before this extensive data gathering effort. The Tibet-Obs dataset refers to the *in situ* basic soil properties (i.e. soil texture fraction, SOC, gravel content) data over the TP. This definition was described in the Laboratory experiments in Section 2.2. We used basic soil properties from the independent datasets (i.e. Tibet-Obs, FAO-UNESCO, HWSD, SoilGrid1km and SoilGrid250m) with correspondingly recommended schemes (see Section 4.1) for SHP & STP estimates. Compared with the *in situ* measurements, the performance of these existing datasets in terms of estimating SHP & STP over the TP were evaluated.

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 'aright' 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?
Response: Thanks a lot. We've rewritten the "SHP & STP in LSMs" of section 4.2 and put previous Figure 20-22 (Figure A1, A2, A3) in the Appendix for readers' further information.

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.
Response: Thanks a lot. We have adjusted the structure of paper, kept the interesting figures and outcomes in the text and put others in the Appendix and Supplement, and provided the raw data for each station in the https://data.4tu.nl/repository/uuid:c712717c-6ac0-47ff-9d58-97f88082ddc0.

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.
Response: We added the raw data in the updated spreadsheet in https://data.4tu.nl/repository/uuid:c712717c-6ac0-47ff-9d58-97f88082ddc0, a newer version of this dataset in previous https://data.4tu.nl/repository/uuid:61db65b1-b2aa-4ada-b41e-61ef70e57e4a. Readers can access the dataset via both linkages.

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.

Response: As described in “how TP presents a challenge to observations” in the Introduction, the soil texture types from existing datasets were defined the same for Naqu and Maqu networks (Su et al., 2013), which is not consistent with what we measured in-situ. Based on the *in situ* and laboratory measurements over the TP, soil physical properties (i.e. soil texture, BD, porosity, SHP & STP) were demonstrated differing for each climate zone and varying at different depths.

To our knowledge, there is little information available on assessing the appropriate soil parameterizations for land surface modelling over the TP across the three climate zones. In this paper, we provide the *in situ* measurements of soil physical properties over the TP and conducted comprehensive analyses of soil physical properties for three climate zones. With these collected data, we expected the LSMs can provide the physically consistent land surface states temporally and spatially. In fact, the collected data has been applied in our group to improve the understanding of different land surface processes/dynamics over the different climate zones, which is, however, beyond the scope of current manuscript.

In order to highlight our contributions, we have revised the short summary in the Conclusion.

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