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Interactive comment on "High-resolution hydraulic parameter maps for surface soils in tropical South America" *by* T. R. Marthews et al.

T. R. Marthews et al.

toby.marthews@ouce.ox.ac.uk

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Please find below our responses to the two reviewers who gave use such useful feedback in Feb 2014. There was substantial overlap between the reviewers' comments so we have grouped them together by theme below rather than dealing with each reviewer in turn:

Rev1: This is a very interesting paper that presents a new high resolution regional map of soil hydraulic parameters, which have a high potential for use by modellers of tropical regions. The authors used two best available sets of soil pedotransfer functions for tropical soils to generate and intercompare the output. The data used is field-based soil profiles of soil texture and chemical properties, mapped onto soil units from SOTERLAC. A quick assessment of the new dataset is done via a comparison against

several published values. Overall, this manuscript is acceptable for publication following some revisions.

Rev2: This paper describes the development of parameter values for common soil hydraulic models over a tropical South American domain. Experimental soil profile measurements from three sources were combined with soil polygons primarily from SOTERLAC. Lack of data restricted the analysis to the topmost 30cm of soil. The final products were high resolution maps of parameters used by land surface and other models. This paper is well written and acceptable for publication subject to (fairly minor) revisions.

- We thank the reviewers for these supportive comments.

Rev2 Point8. Fig.4 I would prefer to see a range of colours (e.g. rainbow) rather than what are essentially several variations of one colour. Also a clearer colour bar, with intervals labelled. At present these are just "pretty pictures" and much of the information content is difficult to see.

- The colour palette has been changed to a blue-green-yellow-brown palette which we hope makes it easier to see at a glance areas with below-average and above-average values for each parameter. It is of course difficult to display the soil variability of a continent in one easily-digestible figure, so we do not expect the reader to gain anything more here than an overall impression of continental trends (notably the great local heterogeneity in certain areas): because we are including all these GIS layers in the supplementary information, they can be downloaded and displayed in a GIS package (e.g. freeware QGIS) which allows zooming and the display of all numerical values.

Rev2 Point7. Fig.2 is too small when printed and I needed to magnify it via my PDF viewer. Even then the lighter grey text is rather faint.

- Fig.2 is now larger and a more contrasting shade of grey has been used for the within-panel labels.

Rev1 Point7. Appendix A appears to be a technical reference for the data use within a specific model and may not be a necessary for the paper. Rev2 Point9. Appendix A was obviously very specific to one model (JULES) and I don't feel that it added much value. That material is probably better removed and put in some sort of JULES model document (e.g.a a user guide).

- Appx. A has been completely removed.

Rev2 Point6. Several captions were too long, particularly those for Tables 1 and 2, probably also Figure 2. Some of that material should be moved, possibly to the main text or an appendix. A separate table of variables used/notation could perhaps be used to streamline the presentation. The PTFs in Table 2 should be presented more clearly, especially given their importance to the study - perhaps move to an Appendix (not a table)? The hydraulic models of Table 1 might also be clearer in an appendix rather than squeezed into a table.

- The captions of Tables 1 and 2 have been shortened as requested.

Rev1 Point3. The authors briefly introduced pedotransfer functions in the methods section. Considering that these functions are central to the development of the dataset, a more thorough discussion on these functions and their uncertainties would be beneficial in the introduction. Rev1 Point2. The authors presented 4 soil water retention models in the Introduction. The results are presented in the context of these models, but the text could use some quick editing to provide more clarity.

- Both hydraulic models (Table 1) and pedotransfer functions (Table 2) are now introduced briefly in the Introduction and we hope that the references given in the captions of Tables 1 and 2 serve as an adequate basic review of each topic.

Rev1 Point4. The evaluation of the new soil parameters is very limited, although understandably published values may not exist in abundance. I suggest the authors dis-

cuss the differences in the newly derived parameters from values in the literature/from Cosby (1984) PTFs in terms of compatibility with known behaviours of tropical soils. Rev2 Point1. Given that the paper is primarily about the data, the results section is rather brief in comparison to the discussion. But I found the discussion interesting and a good source of references and ideas for potential follow-up work. The PTFs (and possibly the hydraulic models) might also be given greater prominence - see my later comments on the tables.

- A few sentences introducing pedotransfer functions and hydraulic models has been added to the Introduction. We accept that the results section is fairly brief, but this is because there is a well-known general lack of good field data against which to compare our results: we have exhaustively searched but have not found even one study for tropical South America where these parameters were measured at more than a handful of sites (except for the work of Martin Hodnett and Javier Tomasella which we used for our fits and therefore could not use for validation) and no reviews of the spatial variation of these parameters across continental scales. These parameters cannot be remotely sensed so it seems that they have become the 'poor cousin' of the model parameter family despite their clear importance in the control of soil moisture flow. The main reason that the soil properties discussed in this paper are so seldom measured is because they are structural properties rather than compositional (as is soil texture) which means their values must be ascertained either in the field directly or from intact, undisturbed soil cores before they have dried out (Marthews et al. 2008). With the well-known challenges of tropical fieldwork and the difficulty of sending intact soil cores to labs routinely 100s of km away, it is perhaps unsurprising that there should be so few field-based measurements. Despite the growth of databases such as UNSODA, there is definitely plenty of scope for follow-up projects in this area (which we are, incidentally, planning in a new proposal).

Rev2 Point3. The discussion mentions the need to include deeper layers (>30cm depth) but does not suggest any approaches. Presumably a first step could be to analyse the limited data available, to get some idea of the effects. Could maps of bedrock/parent material help? I am not expert in this area and it would be good to hear the authors' suggestions, not least because modellers will do something to estimate deeper parameters in any case!

- We have added to the Discussion section on "Inclusion of deeper soil layers" the sentences: "Some soil parameters are generally assumed to remain constant with depth (e.g. λ) and others assumed to decrease exponentially (e.g. ksat, Clark and Gedney, 2008; although n.b. in many soils ksat actually increases with depth, e.g. saprolite layers or carbonate outcrops that have undergone karstification), but a full hydropedological survey is required to accurately describe the hydraulic properties of a soil profile at all depths.". We hope this both informs the reader of what is the most standard modelling approach at present (i.e. assume no change with depth for all quantities except ksat for which assume an exponential decrease) while at the same time sounding a note of warning that these assumptions are considered quite inadequate by the soil science community. At the moment, this is a fair summary of the state of the field (and there is a fair amount of friction between soil scientists and modellers on these points, but the absence of good data means there cannot yet be a resolution). The basic difficulty is the possibility of markedly different soil layers below 30 cm, which happens very often in many soil profiles. Maps of bedrock depths are very useful but parent material is surprisingly little related to soil type at continental scales which means there is no general way to estimate deep soil properties from parent material. Through the RAINFOR network better data is in the process of being collected by one of us (B. Quesada), but this will take a number of years yet to complete. In order to make the importance of deep soil layers more clear, we have briefly mentioned the famous "Australian Evergreen Paradox" in this section now: "A recent example of this is the "Australian Evergreen Paradox" where the marked dry season of the northern Australian monsoon tropics would lead a land surface model to predict dominance by deciduous tree species, while the actual dominance by evergreen species is a consequence of deep water aquifers (Bowman and Prior, 2005)"

Rev2 Point2. As a modeller I would have been interested in reading something about the impact of these new data on model results. The underlying premise is that better input data will result in better (more realistic) model simulations and although I agree that we should seek the best possible input data, in practice this does not guarantee better performance. Even when performance is improved, the improvement can be modest. I appreciate that quantifying any model improvement is not trivial and is perhaps reasonably left for subsequent studies, but I leave it as a suggestion that the authors could at least quantify the impacts of alternative parameter values on some model outputs (e.g. runoff, evaporation) even if they do not broach the more thorny issue of whether these constitute improvements. (Perhaps they could run at "representative points" to avoid running at all point across the domain.) I realise that any such results will strictly only apply to the one land surface and hydraulic models used (and slightly at odds with my later suggestion to remove the JULES-specific appendix!) but the authors might at least consider this work, and accept/reject it as they see fit.

- We believe that these points are well-made and we are indeed considering something very close to this in a follow-up study. The current situation is that most land surface modellers use the Cosby pedotransfer functions despite the fact that these functions were based on and derived for only US soils and may be inappropriate for tropical soils such as oxisols that do not occur in the US. We believe that to move from Cosby to tropically-based pedotransfer functions is a necessary first step for tropical land surface modelling and that is what we are concerned with in this paper. One possible next step would be to compare 'Brooks & Corey' soil hydraulics and van Genuchten as well as the various new models for soil hydraulics that have recently come out in the soil science literature (reviewed for example in Vereecken et al. 2010). However, such a large model intercomparison is substantially beyond the scope of this paper.

Rev2 Point4. There is little mention of issues related to the spatial scale of model grid boxes (e.g. a typical global climate model might represent the land surface in terms of cells of size O(100km).). I don't expect to see a detailed discussion of this complicated area which is beyond the scope of the paper, but I think some mention is warranted - even perfect "point-scale" parameter values are not enough if the important processes are different at the scale of model application. Rev1 Point1. The authors noted on page 6747, line 24 that the dataset should be used for profile and subprofile modelling and I agree that this is an important point, which perhaps should be highlighted further upfront in the abstract/introduction. The authors may want to include in their discussion some implications of applications at unintended scales (the issue of parameter scale).

- In the Introduction we have discussed issues of scale in hydrological processes in paragraphs 2-3 and we have returned to this in the Discussion where resolution is discussed. At the end of Discussion section 4.1 we have also now inserted “Note that because many model simulations using these layers may be run at coarser resolution than 15 arc-sec (e.g. climate models, usually run at resolutions of at least 0.5°), an aggregation step may be required to produce layers in a model-ready format: Aggregation may introduce uncertainties in addition to the base uncertainty of our layers (given in Table 2), dependent on the aggregation algorithm used.”. We do fully accept that these issues are important, but we believe that they are more relevant to climate models than land surface models (LSMs are usually run at much finer resolution than climate models) and it is not straight-forward to assess the uncertainty introduced by aggregation algorithms because they are usually written in an ad hoc fashion for each application. Both Reviewers have suggested they do not want to see a detailed discussion here and we have given all information that an LSM modeller would require so we believe this level of detail is appropriate.

Rev1 Point6. Page 6751 line 26: The authors should justify the given uncertainty value i.e. 10% of regional ranges. Rev2 Point5. Again, I’m not expert, but I was slightly surprised at the relatively small error (10%) estimated by the authors and wonder if this could be elaborated further (although they already state that this is difficult)? Perhaps quote uncertainties as estimated in other studies? At the end of the day this is probably the authors’ "expert guess" and might need to be better clarified as such.

- We have improved the discussion of uncertainty by removing our previous estimates and extracting uncertainty estimates directly from the source papers for the pedotransfer functions concerned. This is more robust than our previous estimate, although the magnitude of uncertainty is similar. Because we have now given actual example calculations, we have removed this text on p.6751 and replaced it with uncertainty estimates within Table 2 at the appropriate points.

Rev1 Point5. Since Tomasella (2000) assumed a Van Genuchten model, is a comparison between both the Tomasella & Hodnett (1998) (Brooks & Corey) and Hodnett & Tomasella (2002) (Van Genuchten) PTFs to their values unbiased?

- We believe that this is indeed a legitimate comparison: the standard parameter conversion between van Genuchten and Brooks & Corey (given in the footnotes to Table 1) means that any set of parameter estimates given for Brooks & Corey may be converted directly to equivalent parameters for van Genuchten and vice versa. Essentially, this comparison is equivalent to comparing several model fits to each other when they have been based on the same measurement data.

Rev1: Minor comments: 1. "Land surface models" is generally written without capitalization. - Corrected 2. The overuse of parentheses can be confusing table 1’s caption. Unless this is a convention of which I am unaware, I suggest removing the parentheses around long variable names. - Table 1’s caption has been shortened and the excessive parentheses removed. 3. Fig 2 needs to be larger. Some text labels are faint and difficult to read. - Fig. 2 is now larger and a more contrasting shade of grey has been used for the within-panel labels. 4. Table (1 and 2) captions are long. The authors should consider simplifying/moving some content to the results/discussion section. - Table 1’s caption has been reduced in length.

Rev2: Minor corrections and typos p6751 line 17 "and others of which left" - Corrected p6756 line 2 - I don’t think "publicly-available" should be hyphenated. - Corrected p6758 line 16 (App.A) - no full stop after "smcl()". - Only applied to Appx. A which has been removed.

Interactive comment on Geosci. Model Dev. Discuss., 6, 6741, 2013.