The manuscript presents a deep and thoughtful analysis of a vast dataset of published data on soil hydraulic conductivity (K). The authors reveal and highlight the frequently neglected importance of many site-specific factors in the most widely used pedotransfer functions as previously highlighted by Vereecken et al. (2010) among others. They elucidate the factors that limit the application of the traditional approaches and present a first attempt to predict Ks with the multiple linear regression. The topic is highly relevant for both the HESS readers and the community of hydrologists as K is one of the most important factors in (eco) hydrological modeling. The manuscript is well written, concise, to the point and would only require minor to moderate revision before being published.
We would like to thank the reviewer for these kind words and also for the many thoughtful comments and suggestions, which will help us to produce an improved manuscript.

Given said that I still have a few major and minor comments for the authors that I think could help them to improve the manuscript.

Mention to the support of the soil volume sampled is limited to a sentence in the introduction referring to the soil sampling and subsequent analysis in the laboratory. Information about the confined infiltrometers size is limited or absent but may be interesting for the readers. Their size will determine the extent to which macropores and soil cracks are accounted for in the experiments.
We are not exactly sure what the referee means by confined infiltrometers, as the entries in the database are dominated by unconfined 3D infiltration tests (see table 1). However, after we submitted the paper to HESS, we did complement the database with information on the diameter of the infiltrometer, which should give at least some kind of indication of the soil volume sampled. We will add this information to table 2.

The authors acknowledge the poor performance of the multiple linear regression to predict K from site-specific information even though they include the parameters that influence most on K at low suctions such as macroporosity. Given the poor performance and the availability of data I think the authors should apply non-linear methods to predict K and go further with the analysis of data given the rather explanatory than predictive nature of the multiple linear regression under these circumstances.
We believe that an explanatory analysis is in itself very valuable, even if the predictive power of the relationships is somewhat limited, which is also the case for other global PTF's for K. Please also see our response to reviewer 1 on this point.

Minor comments:
Introduction. Nothing is said about inverse modeling to obtain soil hydraulic parameters and more specifically K. Recent studies, e.g. Hardie et al. (2013); Martinez et al. (2013); Schindler (2014), present alternative ways to estimate Ksat and, in my opinion are worthwhile to be mentioned.
Please see our response to the same comment made by reviewer 1.

P. 10850, L. 6. *The authors mention, "mean values were recorded in the database for a given plot". Given that Ksat distribution is usually assumed to be log-normal why not to use median rather than mean values? Also, just as auxiliary and illustrative purposes I would appreciate to know the range of variability in the Ks for plots where they have several observations*.
Actually, this statement in the paper was probably a little misleading. We did not calculate any mean values. We only took them from tables or scanned them from figures. Often, authors did not state whether they were arithmetic or geometric means. We will clarify this in the revised version.

P. 10850, L. 28. *I would include here that the method of interpolation in LocClim is the inverse distance method to provide with more information to the readers*.
Yes, OK, we will add a mention of this.

P. 10852. L. 7-16. *Is there any study in the database in which the direction of the sequence of infiltration is compared? I guess that if the variability inside the group of ascending or descending sequence is very large it will mask the fact that there could exist or not a significant difference between both groups. Therefore to assure that performing an ascending or descending experiment will significantly affect the K that you are measuring you will need replicates of both of them under the same conditions*.
Yes, good point. There are two such studies in the database, both from the same authors (Bagarello et al. 2007. Geoderma, 137, 394–400; Bagarello et al. 2000. Soil Sci. Soc. Am. J. 64:1203-1210). If the samples are large enough and not biased by correlations with other variables (e.g. texture), then analysis of variance should tell us whether hysteresis is important (it seems to be). But we agree that it is a good idea to support this conclusion by citing a local-scale study which compares methods on the same soil, so we will add a reference to Bagarello et al. (2007) in the revised version of the paper.

P. 10856, L. 4. *"significant". I would use a different term if I were not performing a statistical analysis.*
Yes, OK, we will change this.

P. 10856, L. 5-9. *The effect of climate on K is not sufficiently supported given that the only graph showing this is the scatter plot of logK vs estimated rainfall in figure 3 and temperature.*
The results should be quite robust in this respect. Please see our response to the same comment made by referee 2.

P. 10859, L. 27. *The reference is not included in the text.*
True, but it is cited in Figure 1.

Figures and tables must be self-explicative. Meaning of the abbreviations might be included in the captions.
Yes, we will do this.

Figure 3. *I miss more information about the graph. Are the graphs in the principal diagonal giving the pdf of each variable?*
Yes, they are. We will add this information to the caption.

I would include also the Pearson' correlation coefficient
They were already included in the panes in the top right of the figure. This information will be added to the caption to clarify this.

and increase the size of the text and figures in the text. They are hardly readable.
Yes, we will try.

At least a sentence or a reference to explain what is LOESS (locally weighted scatterplot smoothing?) might be needed.
Yes, it is locally-weighted least-squares regression. This is now explained better in the caption.

Otherwise, the red lines might be omitted to avoid the readers to get lost as they don't add to much value to the description of your results and their discussion.
We would prefer to keep them.

References:
Hardie, M.A., S. Lisson, R.B. Doyle, and W.E. Cotching. 2013. Evaluation of rapid approaches for determining the soil water retention function and saturated hydraulic conductivity in a hydrologically complex soil. Soil and Tillage Research 130(0): 99–108

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Schindler, U. 2014. A Novel Method for Quantifying Soil Hydraulic Properties. p. 145–158. In Mueller, L., Saparov, A., Lischeid, G. (eds.), Novel Measurement and Assessment Tools for Monitoring and Management of Land and Water Resources in Agricultural Landscapes of Central Asia SE - 7. Springer International Publishing.