Hydrol. Earth Syst. Sci. Discuss., 10, C5491–C5494, 2013
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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Influence of soil, land use and climatic factors on the hydraulic conductivity of soil" *by* N. Jarvis et al.

Anonymous Referee #2

Received and published: 9 October 2013

This is a concisely written, well organized and matured manuscript reporting on a study that identifies and quantifies the controls of saturated and near saturated hydraulic conductivity of the soil – and their difference, here discussed as macropore conductivity – from available soil physical and structural properties, land-use information and certain climatic information. The study is based on a decently large data collection of tension infiltrometer-based measurements, collected from the literature.

The study is well justified, as roughly all similar estimation studies (pedotransfer studies) are solely concerned of the saturated hydraulic conductivity – lacking the distinction of matrix flow and macropore flow, and based on measurements obtained using techniques that do not distinguish between those two elements. The data set collected is a rather unique collection, and will expectedly remain a valuable information source

beyond this single study.

The study is carefully worked out, using ample planning and design, and data resampling for instance, and the phrasing of the conclusions is moderate and mostly well supported, and I have no doubts that this is a publication-worthy study. Nevertheless, I have three significant comments/suggestions that I believe should be elaborated on – followed by some relatively minor remarks.

Major comments:

1. Compared to the cited PTF works, this study is unique due to the collection of in-situ near-saturated conductivity data. Contrasting results are presented – which was expected. There is very brief reference in the very last paragraph to the differing data set; however, I think that the issue of laboratory measurements vs. field measurements and their differences that partially explain the differences in findings should be more emphasized than it currently is.

2. Table 4 presents method-wise averages, and methodology ended up being a selection criterion for the data subsets used in the analysis. I do not recall reading about any potential interaction between method used, and certain influential soil properties, e.g. texture. Is there a chance that some methods – whether for a practical reason or by accident – have been used on certain soil textures more than others? Is there a chance that this influenced the findings? A texture triangle with method-wise colors used for the soils may be helpful to support any related discussion. I can also imagine a similar interaction with the wetting sequence (wet to dry vs. dry to wet).

3. While I have no doubts that climate may affect K10, and the offered potential explanation of what is often seen in nordic countries (short season -> tillage may be performed in sub-optimal conditions to prevent compaction) may potentially stand, I think the statement was made a little enthusiastically, without examining (or reporting on) any correlations between the climatic factor and other factors/variables that may influence the reported finding between K10 and mean annual temperature. For instance,

is there any potential pattern in preference towards a given measurement technique in the North or the South? Is there any potential relationship between the climatic-location and soil type/texture/etc. that may be influential? Is there any potential correlation between the mean temperature (i.e. the climate) and the timing of measurements and the relation between the timing of measurements and the timing of tillage operations? Is there a balanced number of samples for the K10 analysis from a wide range of climatic zones, or are there perhaps a few influential samples that may affect the general picture a lot? Climate information may be strongly correlated with other factors (natural or not), and may mimic other effects; or a few dominant soils may drive the relations found. I think that elaboration on this aspect of the study is absolutely necessary.

Minor comments:

P10850, L6-8 and L21: It would probably be valuable towards the justification of this fitting approach if the distribution of the fitting error was also presented and briefly commented, not only the distribution of R2.

P10850, L20: this estimation at ÂńanyÂż tension is understood within the available data range, is that correct?

P10851, L21-22: the Âń250 bootstrap samplesÂż expression is misleading to me. I assume it refers to the test data sets separated by the 250 different bootstrap subset selections, which yield a variable number of test samples in most cases. The text could be adjusted.

P10851, L23-24: A reason and justification should be given why these exclusions took place.

P10852, L14 and on: I understand the placing of the report on hysteresis results still in the materials and methods section. Yet, I wonder if it is not better placed as the first paragraph of the results section.

P10853, L22: I wonder to what extent shrinkage cracks play a role between Ksat and

K10. Does significant shrinkage happen between saturation and -1kPa? In my opinion, at this moisture range, tillage voids and biopores are significantly more responsible for any differences, than the moisture-status dependent shrinkage is.

P10855, L17: perhaps add to the effect of tillage that it is primarily due to the removal of connected biopores.

P10855, L22: regarding temporal variation: Is there information in the data set about the timing of tillage operations vs. The timing of measurements? Undoubtedly, the effect of such temporal variation is logical, but the BD vs. Ks relationship is expectable in a data set without a temporal dimension as well.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 10845, 2013.