Overall I found the manuscript to be import and interesting. I defiantly believe it should be published, and is within scope of Hydrology and Earth System Sciences. Unlike many of the pedotransfer functions that exist, this paper has attempted to determine Ksat and K10 by insitu approaches, an approach I completely support.

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.

I had a few issues with the manuscript, I found the figures hard to interpret, I thought more could be made of the existing literature in relation to past efforts to measure Ksat, and reliability of pedotransfer functions. I would have been happier to see the machine learning approaches included in this publication given the poor correlations that were found.

We respond to these points later on, but briefly we can say that we will try to improve the clarity of figure 3 and we will also discuss the relevant literature in a little more depth. We would like to thank the referee for bringing to our attention some interesting and highly relevant papers.

Abstract I think it reads a bit blocky, and expression could be improved.

OK, we will try to improve the abstract in the revised paper.

Introduction I think the introduction needs to start somewhat simpler. A description of what is infiltration, what is hydraulic conductivity, why are they important and how do they differ.

We think that defining the terms infiltration and hydraulic conductivity is unnecessary (it's too basic for readers of HESS). But we agree that it is a good idea to stress why they are important and we did so in the paper (see next two points).

Explain why knowing the rate that water moves through soil is so important for biological systems and modelling soil–water–climate–plant systems.

We did already, at lines 19-23 on p.10846 and lines 0-6 on p.10848

You could also talk about how K is considered one of the most sensitive parameters for soil water modelling, catchment scale processes etc.

Yes, we did already (see lines 0-6 on p.10848)

P10847, Ln 8. . ..approaches often perform poorly. . .. I think this section needs greater detail, and more structure. Reasons why pedotransfer functions have difficulty predicting Ksat include; (i) range of measurement procedures used to asses Ksat vary both between and within databases. Ksat has been shown to vary enormously between different measurement approaches (i.e. Hardie 2013). Hardie reported a four order magnitude variation in estimated ksat based on methodology and antecedent moisture content, (ii) Hydraulic conductivity has been shown to vary enormously within small distances within the same soil (ie Davis 1999) and (Cox and McFarlane 1995) who found that the hydraulic conductivity of the B Horizon in a duplex soil varied by as much as two orders of magnitude over a 10 meter distance. (iii) sample sizes in the laboratory are often too small to capture the representative elementary volume of macroporous soils. Davis et al. (1999) found that measurement of saturated hydraulic conductivity in commonly small cores (6.3 x 7.3 cm) and constant head well permeameters were one to three orders of magnitude lower than for large cores (22.2 x 30cm) (Davis et al. 1999; Hutchinson and Moore 2000). Also field based measurements of hydraulic conductivity are frequently one to two orders of magnitude lower than values determined by calibration or inverse modeling (Brooks et al. 2004; Silberstein et al. 1999). (Vertessy et al. 1993) found that stream flow could be most accurately simulated using a lateral saturated hydraulic conductivity value which was nearly 10 times larger than the mean vertical saturated hydraulic conductivity measured using constant head well permeameters. (iv) hydraulic conductivity in vertic and water repellent soils is also influenced by antecedent soil moisture content which is not accounted for in pedotransfer models (Hardie 2012, Dekker and Ritsema, 2000; Lamparter et al., 2006, Greve et al., 2010;) (V) effects of macroporosity are not specifically accounted for (as you refer to). In describing the importance of macropores you could refer to Poisuellie's law and the importance of a few large macropores rather than lots of small macropores ie (Watson and Luxmoore 1986) who determined that 96% of the water flux through a soil was transmitted through only 0.32% of the soil volume, namely the macropores.

The idea of this section in the Introduction was just to emphasize that one reason why existing PTF's for K_{sat} often perform badly is because they are largely based on laboratory measurements and therefore fail to properly reflect soil structure and soil macropores. This is why we wrote … '<u>One</u> important reason for this ..' (line 10, page 10847). There are other reasons of course and several were discussed elsewhere in the paper. For example, the effects of swell-shrink were already mentioned at Lines 27-28 on page 10847. In the discussion section, the influence of different experimental methods was mentioned at lines 26-28 on page 10854 and the large short-range variability in K_{sat} at lines 0-6 on page 10855.

We are not keen to discuss inverse modeling in our paper. This is because when you start talking about inverse modeling you must consider the issues of equifinality and model error, which are inextricably linked. In other words, if calibrated K_{sat} values differ from direct measurements this could result from an ill-posed calibration problem or from an inadequate model rather than inadequate measurements! This kind of discussion is beyond the scope of our paper.

In our experience, the effects of sample size are not always consistent. Sometimes K_{sat} can be larger on smaller cores than larger ones, because dead-end macropores are artificially truncated, providing short-circuited preferential flow pathways that would not otherwise have existed. Nevertheless, we will add some text and references to emphasize the importance of sample size and also the effects of water repellency on K_{sat} (see also next point). We will also cite additional studies (including the paper by Watson and Luxmoore), which demonstrate that near-saturated flow in undisturbed soils is dominated by the larger macropores.

P10847 Ln 25-30. I think more detail of the effects of climate, and soil moisture on soil structure are warranted. I suspect this should include reference to vertic soils, water repellent soils and soils which are subjected to seasonal freezing.
We did mention swell/shrink effects, but yes, we will re-phrase this text to also mention water repellency and freeze-thaw effects, and add some references.

P10848 Ln 7. . ..In theory pedotransfer functions based on field measurement of ksat should give more accurate prediction of hydraulic conductivity than laboratory based methods. . .. . ..
OK, we will re-phrase this

Methods. Need to include information of soil depth sampling in the methods
This is included already (see table 2)

What about splitting / viewing the data by international soil orders.

This is an interesting idea, but unfortunately that information has not been recorded in the database, simply because so few studies reported it

P10850, ln 5-10 I think it should say . . .. . .in order to determine ksat from data determined at a range of supply tensions we . . ..

No, we do not just estimate K_{sat} with this model. We use the fitting to estimate K at all tensions.

I struggle with the analysis in so far as you recommend the use of machine learning approaches and suggest they are being developed but you haven't presented that analysis here. I would have suspected that they should appear here, especially as so many of the correlations appear to be non-linear.

We did not really recommend the use of machine learning techniques. We only suggested that they are worth trying. We cannot include the results of any analyses with machine learning techniques in the present paper, because this work is only just underway. Besides, the results of the bootstrapped regression are extremely interesting and valuable in themselves, as an explanatory analysis.

Results. I really struggled with figure 3. Can you somehow make what is represented by each row and column more apparent, ie Clay %, OM, Arable land etc..

Yes, we agree that this figure was difficult to read. We will try to produce an improved version.

Table 7. I think all the nomenclature should be in the figure texts, rather than having to refer back to other figures.

OK, we will do this.

Also I would have liked to compare the regressions produced by simple regression techniques compared to the bootstrapping.

We have now performed the equivalent regression analysis without bootstrapping (i.e. best subset regression for all possible models, using the Akaike criterion to select the best model). As expected, the R^2 values are larger with this approach (0.32 for K_s and 0.45 for K_{10} compared to 0.19 and 0.32 respectively validation R^2 values for the bootstrapped models). The models also include additional significant (i.e. $p<0.05$) predictor variables: clay content (+), precipitation (-) and measurement depth (-) in the case of K_s, and precipitation (+) and organic carbon (-) content in the case of K_{10} (signs in parentheses indicate positive or negative correlation with the dependent variable). It can be noted that the predictor variables included in the bootstrapped models are also included in the best regression models without bootstrapping, with similar regression coefficients (same direction, similar magnitude). Thus, the principal difference is that bootstrapping is a more cautious approach which avoids over-fitting at the cost of smaller R_2 values.

We do not want to present linear regression models without bootstrapping in the paper, because we prefer the cautious approach, which gives us confidence that the effects we discuss are more likely to be real effects and not the result of spurious correlations etc.

The identification of arable land as a binary factor in the prediction of ksat suggests the data needs to be split such that separate pedotransfer functions are developed for arable and non-arable land.

Yes, in principle, we agree. In the revised version, we will present bootstrapped equations for the arable land subset, in addition to the equations for the whole dataset. However, for non-arable land, there is really too little data to develop PTF's.

P10854, ln 22: The low r2 values for the selected models needs to be discussed in more detail. This is really important part of the findings. Most pedotransfer functions predict Ksat with r2 values around 0.4-0.6. Your values are much lower, Why? I suspect its due to most lab based studies undersampling the presence of large macropores and clay swelling, microbial blockage of saturated soil cores during analysis, thus resulting in lower ksat values but more importantly lower error terms between values due to the reduced importance of macropore flow. A table of your prediction factors and r2 compared to other pedotransfer functions would be really interesting. A good place to start is the back of (Cichota 2013).

Actually, we are not convinced that our models perform worse than existing pedotransfer functions for K_{sat}. This might be true if we compare with some local-scale PTF's, but we doubt that this is the case for global databases like ours. To take one well-known example, the European-scale HYPRES K_s function has an r^2 value of 0.19, which is identical to our validation R^2 value (Table 7) and slightly less than our calibration r^2 value of 0.25 (see table 4). Actually, most studies report RMSE values rather than r^2 (we gave both r^2 and RMSE in our paper). As we wrote in the paper, our RMSE values compare very well with those reported for the ROSETTA functions in the comparative study by Vereecken et al. (2010) (we have RMSE of 0.54 vs. ca. 1.3 for ROSETTA see our Table 4 and their figs. 13 and 14).

P10855 Ln 0-5 I think this needs more discussion. I'm not sure why disturbed soils have all of a sudden been brought into the discussion.

We simply meant that soil texture fractions and organic carbon are measured on disturbed (bulk) soil. But I can understand the referee's confusion. We will re-phrase this.

I thought much of the short range spatial variation in Ksat at the catchment scale was due to macropore systems becoming more important during wetter conditions (Tsuboyama et al. 1994). Sidle et al. (2001) demonstrated that although individual macropore segments are generally less than 0.5 m in length, they have a tendency to self-organise into larger preferential flow systems which expand upslope as sites become wetter (Tsuboyama et al. 1994).

We don't fully understand this point, but self-organizing pipe systems are a phenomenon occurring under quite specific conditions (steep slopes, undisturbed subsoil with conductivity-contrasting layers) that are not very relevant to our study (our PTF's were developed on topsoil). Anyway, we think it is well accepted that large (apparently random) short-range variation in K_{sat} arises because it depends on a few large pores that are not so strongly correlated with variations in soil texture, organic carbon etc. The paper we cite (Ghafoor et al., 2013) shows this clearly.

P10855, ln 17: compaction. I think this needs a bit more thought. What I suspect is not captured with the analysis is that bulk density only represents the total pore space in the soil, not how well the pore space is able to facilitate water movement ie bulk density doesn't measure pore connectivity, which may be more important for water movement than the total pore volume.

Yes, although it is speculative, we agree that this could be a reasonable explanation for our result and we will re-phrase the text along these lines. But interestingly enough, another recent study, which we also now cite (Hu et al., 2012), also showed significant effects of bulk density even for non-tilled soil under natural vegetation.

Important to recognise that land use and land management are different and should where possible be described separately. There are an enormous range of management factors that may influence soil porosity and ksat within arable systems.

Yes, we agree with this, and we did include tillage system in the database as a potential predictor (see table 1), although we did not include it in the analyses. However, we will include tillage system as a potential predictor variable in the new analyses carried out for the arable land subset.

I feel the manuscript is lacking a conclusion in which the authors recommend options for future development of pedotransfer functions.

The last two paragraphs did contain several such suggestions.

Also I think the manuscript could do with a simple statement about the results in comparison to traditional pedotransfer functions namely that estimation of Ksat from field based determination of Ksat resulted in lower correlation with measured values than traditional laboratory based PTF. However the field-based are thought to better represent the true flow rate and spatial variation in saturated hydraulic conductivity of in situ soils.

As mentioned earlier, we are not aware of any clear evidence to support the first part of the referee's statement, although we definitely agree with the second part.

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