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Comment

Interactive comment on “Impact of gravels and organic matter on the thermal properties of grassland soils in southern France” by J.-C. Calvet et al.

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General comments:

This paper investigated how gravel and organic matter (SOM) impact thermal properties of grassland soils in southern France. Authors first obtained site-specific values of soil thermal conductivity at saturation (λ_{sat}) and quartz fraction (q) for 11 stations by reverse modeling from derived λ and soil moisture saturation (S_d) data. The soil compositional characteristics, including fraction of gravel and SOM and porosity (θ_{sat}), were then used to develop pedotransfer functions for q and λ_{sat} parameters. Impact

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of fraction of quartz, gravel and SOM on λ_{sat} was evaluated by sensitivity analysis of pedotransfer function for λ_{sat} . Results showed that gravels have a major impact on λ_{sat} and that omitting the SOM information tends to enhance this impact.

Accuracy of predicative λ models highly depends on accurate estimation of λ_{sat} and q , which has been oversimplified as sand fraction. It is interesting and important to predict q and λ_{sat} in λ models using data of soil texture and gravel and SOM and to further examine their impacts on λ models. The methodology in this work to address the research question is appropriate. Discussion of model applicability is covered. The new pedotransfer functions for λ_{sat} and q derived from their original data will add good contribution to the literature.

I however have major concerns about the presentation/organization of this paper that I feel in some sections focus is lacking and/or reorganization needed. Better justification of adopting some key empirical models and more relevant discussion are also desired. See my specific comments below.

I therefore would like to recommend to accept this paper (with major revision) pending comments below properly addressed.

Specific comments:

1. On obtaining site/station specific λ_{sat} and q values. Equations 7-11 are the core functions for authors to enable retrieval of the site/station-specific λ_{sat} (and q value accordingly) by parameter fitting via reverse modeling. I think these equations/models (specifically Lu et al 2007 and Yang et al 2005) should to some extent be justified why they were chosen as opposed to other alternative equations in the literatures.

2. On discussion. First, the pedotransfer function for q (and thus λ_{sat}) was evaluated with 11 stations/sites in this study but not tested. One alternative to be discussed is to divide the 11 stations that some are used for model development and others for testing its predictive/generalization power. Second, the impact of q on λ prediction actually has

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been studied in Tarnawski et al 2009, in which q was shown mostly linearly dependent on coarse fraction including sand and gravel. Authors recognized that work in this paper yet need to perform enough comparisons with that work and/or other related previous work in the literatures.

3. On presentation.

1) Focus. I believe the pedotransfer function and its evaluation constitute the main contribution of this work. The derivation of soil thermal properties from soil temperature profile, the soil temperature resolution (0.1 C) and its impact on the model applicability can be concise. To me Figure 3 seems dispensable. The Conclusion section also needs revision with a concise description concerning these.

2) Organization. Section 4.1 is about evaluating impact of gravel and SOM with sensitivity analysis. I suggest it be included/appended following the pedotransfer functions in the Results section. Indeed authors intended doing so (in Page 740 Line 6 “in Sect 3 a sensitivity analysis of λ_{sat} to SOM and gravel fractions”).

3) On Abstract. Authors should do better job in these sections. In Abstract the last three sentences are key results and conclusions of this work and need a great expansion with details; conversely the remaining should be more concise. Please rewrite it and include question, significance, methodology, results, conclusion and this work’s impact.

4. Page 738 Line 11. “there is no map of q ”? Reword to clarify.

5. Page 745 Line 9. How/why is 0.4 chosen/set as cutoff of saturation degree?

6. Page 745 Lines 15-17. I suggest an explicit specifying that the three “contrasting retrieved values of λ_{sat} ” are for high, medium and low levels of λ_{sat} values respectively.

7. Page 746 Eq 13. I suggest relating this θ_{satMOD} equation to Eq. 12 for quartz pedotransfer function and further to λ_{sat} .

8. Page 747 Lines 1-4 about Eq 14. I do not see how $dMOD$ is related to λ_{sat} here. I

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do not see dMOD is mentioned elsewhere. This dMOD is distracting/interruptive to the λ_{satMOD} and can be deleted.

9. Page 756 Table 2. The 6 stations with no eligible observations ($n = 0$), filtered by saturation degree of 0.4, can be simply omitted since they are not informative.

10. Page 762 Figure 4 legend. These three stations were chosen as examples to illustrate contrasting levels of λ_{sat} values. I suggest specifying this in legend.

11. Page 764 Figure 6. I may have missed, but I do not see the top and middle plots mentioned in the text.

Technical corrections:

1. Page 739 Line 15-16. “hydrom-eteorology” should be properly hyphenated as “hydro-meteorology”.

2. Page 751 Line 16. To be more accurate, change “. . . proposed for quartz” to “. . . proposed for volumetric fraction of quartz”.

3. Page 760 and page 761. Figure 2 and Figure 3 are misplaced and with wrong legend; the figures should be swapped if they are to be included.

Please also note the supplement to this comment:

<http://www.soil-discuss.net/2/C482/2015/soild-2-C482-2015-supplement.pdf>

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