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Interactive comment on "Development and Analysis of Soil Water Infiltration Global Database" *by* Mehdi Rahmati et al.

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Review ESSD-2018-11 : Development and Analysis of Soil Water Infiltration Global Database (Rahmati et al.)

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The paper describes the construction of the Soil Water Infiltration Global Database (SWIG). Based on a detailed literature research, as well the provision of data of the many co-authors of the manuscript, an infiltration experiment database is constructed that tends towards a global coverage. In total 5023 infiltration curves are compiled. In addition to the basic information related to the infiltration experiments, additional ancillary information is provided allowing to explain the infiltration. This allows constructing

explanatory and predictive infiltration models. Part of the infiltration experiments is analyzed using physical based infiltration models (1D and 3 D analytical infiltration equations). Finally, the authors discuss the strengths and the weaknesses of the database and the possible applications of the data in hydrological and environmental studies.

The presented work is novel, as no similar database of experimental infiltration curves exists with the ambition of a global coverage. This database may, therefore, play a key role in improving the parametrization of the infiltration process in global Earth System Models (ESM). This improvement is of major importance, as infiltration is a basic process controlling hydrological fluxes in earth systems but yet poorly represented. The database has, therefore, the potential to improve the parameterization of soil hydrological fluxes and to reduce the uncertainties associated with current soil hydraulic pedotransfer functions. The paper has, therefore, the potential to become a valuable contribution to ESSD. However, some concerns can be formulated that should be considered in a revision before the paper can be accepted as a full publication in ESSD. The major concerns can be formulated as follows:

1. The paper deals with infiltration, but a clear definition of infiltration is lacking. The authors generally refer in their manuscript to the infiltration that will be observed in controlled field experiments, without being explicit on this. Yet, infiltration is a more general hydrological process that occurs also in transient natural and uncontrolled conditions. Hence many statements referring to the controlled infiltration experiment will not hold for the uncontrolled natural infiltration process. This adds confusion in many statements in the paper (e.g. infiltration generally decreases in time…), that should be corrected by clearly defining the type of infiltration that is considered in the analysis.

2. The authors should more correctly define the extension scale of the database. The SWIG has the ambition to be global, but yet data were compiled from "only" 54 countries all over the world. It is not guaranteed that global soil variability is represented when collecting data from "only" 54 countries. The fact that all textural classes are nearly represented in the database does not warrant representativeness. It would be

better to evaluate other soil properties (e.g. major soil type according to FAO or WRB soil reference system), to demonstrate and claim global representativeness. The map in Figure 1 clearly shows that major regions of the world are not represented, which may considerably limit the global scope of the database or the application of data from the database in global Earth System Models. It may be suggested that the authors perform a representativity analysis, in which not only “countries” or “available texture class” are considered as a criterion for representativeness, but other criteria such as “WRB or FAO soil type”, “earth climate region”, “earth ecozone region”.

3. A set of ancillary variables are introduced in the data set. The intention of this is to apply data mining techniques to explain the infiltration process parameters and hence to allow developing new explanatory or predictive models. However, the quality of these models will depend on the quality of the ancillary variables that have been introduced in the database. Unfortunately, some ancillary variables are proposed in Table 4 that are not well defined or not well normalized or standardized. The added value of adding these ancillary variables to the database should be reconsidered. This is particularly the case for FC (many definitions of the field water capacity can be retrieved in the literature, see e.g. Nachabe, 1998), soil pH (measured in water or in KCl), and wet-aggregate stability.

4. A very limited and preliminary data exploration analysis by means of PCA is presented in section 3.6. The preliminary and limited scope of this analysis questions the overall results of this analysis. For instance, the sorptivity, S, has been integrated into the PCA analysis. Yet S is not an intrinsic time-invariant soil property, but a soil variable that strongly is affected by the initial and saturated water content. Mixing such time dynamic state variable with static properties (such as soil texture, Ks, . . .) in a PCA has little sense, as the results will strongly depend on the initial water content before the infiltration will start.

In addition to this major concerns, some minor concerns are listed below

Line 170. “In addition to its global coverage”. Cf. above. Global coverage should be demonstrated by a representativity analysis.

Line 176. We should expect that land use can be assessed for all the cases. If the spatial coordinates of the infiltration data are known, land use can be retrieved from historical land use data archives (see Google Earth Engine).

Line 199. “In general, the soil infiltration rate decreases nonlinearly over time”. Cf above. This is specifically the case when the infiltration process is studied under controlled conditions (typically as the cases where controlled infiltration experiments are performed). In general, infiltration is very time dynamic, conditioned to time variable climatic conditions, and in-situ infiltration rates will not ’in general’ decrease with time. It is therefore suggested to give a clear definition of "infiltration" in this paper and make a clear distinction between in-situ and controlled experimental infiltration processes.

Line 207. “However, as infiltration proceeds, the gradient. . ..” This is only the case when a pressure head boundary condition is used to define the infiltration process. This may not be the case when flux boundary conditions are used (e.g. constant precipitation). For instance, in case infiltration in a dry soil is analyzed subjected to constant flux boundary conditions, with an imposed flux that is smaller than the saturated hydraulic conductivity of the soil, than no ponding will occur, all water will infiltrate, and no decrease of pressure gradients will be observed.

Line 209. “. . .approximates saturated hydraulic conductivity”. This definition is often debated in the literature. For instance, Kutilek and Nielsen (1994), suggest Ks = 2/3 * the asymptotic value.

Line 218. The Richards equation written in water content form is often referred to as the Fokker-Planck water diffusion equation.

Line 245. “.. from all over the globe”. Be more rigorous. Many parts of the globe have not been considered for data collection.

Line 278. To avoid confusion, define Mi exactly.

Line 302. Please reformulate this phrase (what is the principle phrase?).

Line 327. This statement is clearly not supported by the data in Figure 4. Please avoid general statements that are not supported by the data, or introduce cautionary notes to put such statements in a correct perspective.

Line 363. Correct: “Matlab™”.

Line 372. The “material and methods” section does not explain in detail the difference between those two approaches. What is exactly meant by ’measured Ksat’?

Line 888. If Table 3 is the continuation of Table 2, then it should not be a new table. (So no increase in table number).

Line 911. For improving the comparability, please harmonized the data in the same units (eg. log values in cm/day).

Line 918. Figure 3 adds very little information to the manuscript and is not very useful for the reader. Please consider to eliminate.

References: Kutílek, Miroslav, and Donald R. Nielsen. Soil hydrology: textbook for students of soil science, agriculture, forestry, geoecology, hydrology, geomorphology and other related disciplines. Catena Verlag, 1994.

Nachabe, M. H. "Refining the definition of field capacity in the literature." Journal of irrigation and drainage engineering 124.4 (1998): 230-232.

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