# Comments on the revised version of manuscript hess-2019-0409

**-1- Comments on runoff generation and leakage**

From the text, it is not clear (at least to me) the type of mechanism for runoff generation simulated by the SVEN model. From my knowledge of the water balance bucketing approach as well as what written in this paper, it seems that SVEN can simulate only the Dunne runoff generation (i.e. the saturation-excess mechanism), but not the Hortonian mechanism. Please, clarify that point in the text to the benefit of a wider readership.

As for the computation of the leakage losses (or drainage rate; see my Comment #2), it is not clear from the text how these losses become active. Commonly, in a bucket hydrological model, the leakage losses occur only when the actual soil moisture content is greater than soil moisture at the condition of "field capacity". Please, clarify this point.

Table 2 reports the "soil wilting point" (although it is placed in a wrong row, I guess), but it is not clear where this variable comes into play in the model. Did the authors make a correspondence between the "residual soil moisture" and "soil wilting point"? However, these two soil moisture contents have completely different meaning and clarification is required.

**-2- Comment on the usage of the term "percolation"**

I understand that the term "percolation" is sometimes employed in the literature when describing a downward flux in a soil water balance model. However, when addressing the downward flux at the lowest boundary of the soil domain in a bucket-type model such that used in this paper, a more common term is "drainage" or "leakage".

I would suggest the authors should think about changing the term "percolation". Some reasons for suggesting this change are below:

- In most cases, percolation is used when a groundwater table is present in the flow domain;
- More recently, the term "percolation" is associated with the so-called "percolation theory" that is employed to model the permeability of saturated rocks, or to determine percolation thresholds. In a lesser extent, "percolation" is referred to the downward water flow in natural soils toward the groundwater (Ghanbarian and Hunt, 2017, Geoderma 303:9-18);
- Some authors prefer using the term "percolation" to account for the fact that depth variations in downward flows are also caused by lateral flow, besides the temporal variations in precipitation and evapotranspiration, but it does not seem to me that the SVEN model accounts for later fluxes..

At P.12, L.17, I disagree with this statement. I suggest writing as follows: percolation (or drainage, or leakage) "… is computed by assuming the condition of a unit gradient of the total hydraulic potential [i.e. $\nabla(z+\psi)=-1$] at the lowest boundary and using …".

**-3- Comments on Eqs. (30)**

I do not think that the reference to the Mualem (1976) paper is relevant to Eq.(30). Mualem developed the well-known integral form of the unsaturated hydraulic conductivity and only exploit the Brooks-Corey soil-water retention function to link the two relationships of the soil hydraulic properties.

Instead, Eq.(30) was developed in the renowned van Genuchten (1980) paper. This reference is reported below:

van Genuchten, M.T., 1980. A closed-form equation for predicting the hydraulic conductivity of unsaturated soils. Soil Science Society of America Journal 44(5):892–898.

At P.12, L.22, I suggest changing the sentence as follows: "… *n* is a fitting parameter depending on the pore-size distribution. …"

**-4- Comments on Eq.(32) and variable SWS**

Variable SWS should be clearly defined in the text.
If I understood well, SWS is the product of soil moisture ($\theta$) and the soil depth ($Z$). Is that correct?
If yes, then Eq.(32) can be read as follows: $SWS/SWS_{max} = (\theta \times Z)/(\theta_s \times Z)$
Therefore, I do not think that this equation is really required in the paper.

Moreover, at P.16, L.25-26, the authors reported a calibrated value for $SWS_{max}$ equal to 554.52 mm (i.e. about 0.55 m). From Table 2, $\theta_s$ ranges from 0.38 to 0.43 $m^3m^{-3}$. Does it mean that the soil depth $Z$ of your soil profile ranges from about 1.46 m to about 1.29 m (i.e. $Z = SWS_{max}/\theta_s$)?
If the above is correct, it seems a quite deep soil profile, doesn't it? Since $SWS_{max}$ is a calibrated value, I think that "soil depth" starts losing its physical meaning. Please, clarify that point to the benefit of a wider readership.

**-5- Comments on Table 2**

5.1) I suggest the author should always refer to $K_s$ as the saturated hydraulic conductivity (not sometimes as the infiltration rate at soil saturation). Please, chose only one definition for a variable and use it throughout the manuscript. Moreover, from Table 3 of Dettmann et al. (2014), the range of $K_s$ is from 0.05 mm/h to $50.0\times10^3$ mm/h. Please, clarify the range you used in your model.
5.2) $n$ is the parameter of the van Genuchen (1980) (vG) hydraulic conductivity relation $K(\theta_e)$. Actually, it is the shape parameter of the vG soil-water retention relationship.
5.3) I have a suspicion that the last two variables of this table have been shifted. Moreover, there is a mistake because the same symbol $\theta_s$ was used to address both the "soil wilting point" and "saturated soil moisture". Moreover, it does not seem to me that the paper by Carsel and Parrish (1988) reports values for soil moisture at the permanent wilting point. Please check.

**-6- Comments on units of variables**

Please be consistent with S.I. units of measurement. For example, SWS is in units of "m" (see also the Supplement document and its Table S3), therefore I suggest that in the main manuscript one writes $SWS_{max}$=0.55452 m (instead of 554.52 mm). In all cases, non-S.I. units can be put in parenthesis, if required.
Moreover, it is also good practice in a scientific paper to use the same number of digits. Therefore, one may write, for example, $SWS_{max}$=0.554 m (=$5.54\times10^{-1}$ m).