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Interactive comment on "Natural laws of precipitation, great cycle, infiltration overland and groundwater runoff with a new formulas" *by* A. Dj. Valjarevic and D. J. Valjarevic

Anonymous Referee #1

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The paper aims at deriving the different components of the hydrological mass balance of a catchment based on limited information, i.e. total precipitation and discharge. Unfortunately, some conceptual errors are made which makes the proposed method incorrect. Furthermore, the paper lacks some basic properties of a paper making it unsuitable for publication in a highly-ranked hydrology journal, such as HESS.

First of all, in the paper, there is not one references to any other scientific contribution (paper, chapter in books, ...), even though a very limited list of references is given at the end of the paper (furthermore, these references don't seem to be very adequate).

The paper is completely 'based' on the six component method of M.I. Ljvovic. To be honoust, I have never heard of the method of Ljvovic, and a google search only resulted in websites referring to the HESSD paper which is under review. The paper refers to *the second sketch of M. Ljvovic* (cfr. page 69, line 6), but as the paper makes no reference to that work, it is impossible to assess the statements which are made.

Basically, the paper is playing with the mass balance at the catchment scale which (using the same symbols as in the paper) can be expressed as:

$$\frac{\Delta B}{\Delta t} = P - S - U - E \tag{1}$$

Where B corresponds to the soil moisture storage. I presume (as I cannot check it) that the method of Ljvovic basically assumes that there is no change in water storage in the soil, or:

$$0 = P - S - U - E \tag{2}$$

Which corresponds to the very first equation in the paper (unfortunately the equations are not numbered in the paper ...).

This assumption is acceptable if the fluxes are summed over a long period (e.g. 1 year), and when one can consider that the water storage in the soil is more or less similar at the beginning and the end of the period. Stating that this formula can be applied for whatever time period (as done on page 71, line 13) is not correct, as for smaller time periods, the change in storage can be relatively large (i.e. not negligible) compared to the fluxes of P, S, U or E.

The paper discerns between impermeable and permeable terrains. Unfortunately, the definition that is given to impermeable is different from the one that is widely accepted (i.e. impermeable = no water can infiltrate). In the paper impermeable refers to a terrain which has the following property: if infiltration increases, then the evaporation increases.

A permeable soil (i.e. a soil in which water can infiltrate) is defined as a soil which has the following property: if infiltration increases, then the recharge to the groundwater table (and thus the resulting baseflow) increases. The definitions used are never used in hydrology, and therefore it is very difficult to project the type of soil meant in the paper to the actual type of terrain/soil.

Without going into detail on individual parts of the paper, I would like to immediately point out some major conceptual errors in the derivations of equations for as well the ´´impermeable terrains´´ as the ´´permeable terrains´´

so-called impermeable terrains

On line 10 of page 63, it is given that

$$\frac{Kw}{Ke} = \frac{W^2}{PE} \tag{3}$$

In the following section (lines 11 and 12), it is then stated that

$$\frac{W^2}{PE} = 1 \tag{4}$$

from which is derived that $W = \sqrt{PE}$. However, this makes no sense. Basically, equating (3) to one, means that $Kw = Ke$, or: *the fraction of the total rainfall which infiltrates in the soil is equal to the fraction of the infiltrated water that is evaporated*. Thus, suppose, we have a vegetated soil with a low infiltration capacity for which on average only 10% is infiltrating (the remaining 90% is runoff), you assume that the plants will only use 10% of this infiltrated water (or only 1% of the total rainfall) for evaporation? I don't believe so. In such water limited environment, one can assume that plants will consume nearly all infiltrated water.

Thus, the assumption that $\frac{W^2}{PE} = 1$ is physically meaningless. Unfortunately, section 3.1 and a part of section 4 are completely depending on this assumption, and are basically wrong.

so-called permeable terrains

In equation on line 22, it is stated that

$$Ke = 1 - Kw \tag{5}$$

In fact, $Ke = 1 - Ku$, so what is assumed (and this is also stated in line 8 of page 66), is that $Kw = Ku$. Again, this assumption has no hydrological meaning. It states that *fraction of the total amount of precipitation which infiltrates in the soil equals the fraction of water that percolates to the groundwater table to form baseflow*. So, assume that 90% of the precipitation is infiltrating, then 90% of the infiltrated water has to percolate to the groundwater table. Well, suppose that we are in a water limited ecosystem, i.e. the total precip is low, and plant evaporate a lot, than the total amount of infiltrating water is small (90% of the already small amount of rainfall), then the plants can only use 10% for evaporation as 90% has to percolate? No, in this environment, one can imagine that nearly all infiltrating water is consumed by plants.

Unfortunately, the equations in line 22 of page 64 and the Kw values as given in the equation on line 5 of page 65 are based on a physically meaningless assumption. Section 3.2 and a part of section 4 fully depend on this assumption, therefore, both sections cannot be trusted.

Based on the above conceptual errors, the majority of the equations and figures given in the paper are hydrologically meaningless.

Because of these important errors, I need to advise the editor to reject the paper.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 59, 2011.