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Hydrology and Earth System Sciences Discussions

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 #3

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The authors present a three component method to describe infiltration-surface runoff partitioning of rainfall and subsurface-vaporization partitioning of infiltration. It is inspired by Ljvovic's six component relationship, which the authors have reduced to a three component relationship in the paper.

The paper crucially depends on the definition of permeable and impermeable terrains stipulated by the authors. In their definition, an impermeable terrain is the one where vaporization increases with increasing infiltration. A permeable terrain is where groundwater runoff increases with increasing infiltration (page 61 line24-26). However in their analysis of Ljvovic's definitions (on partitioning of rainfall into surface runoff and infil-

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tration, of partitioning of infiltrated water into vaporization and subsurface flows), they extended their definition to assume that the increasing relationships within the definitions of impermeable and permeable terrains is in proportion to available water for partitioning. That is, not only that their definition of impermeable terrains (for example) warrants an increasing relationship between infiltration and evaporation but also imposes an additional restriction that ratio of infiltration to rainfall (which is available for partitioning between infiltration and surface flow) equal to the ratio of vaporization to infiltration. I am unable to see how the definition of impermeable terrains that increasing infiltration leads to increasing evaporation lead them to assume the equality between Kw and Ke (the latter need not be necessary for the former to hold). Similarly I am confused about the extention of their definition for permeable layers (Kw = Ku => Kw+Ke =1 since Ku = 1-Ke).

It is also not clear on page 65 how the authors further add another restriction on a terrain being permeable as the ones with Kw >0.5. (root of quadratic equation on page 64) Their argument on that page on why when one of the roots of quadratic equation (in Kw) defined on page 64 is <0.5, "a problem appears because it is necessary to define is the terrain permeable or impermeable". But aren't these two roots from Ljvovic definitions and the authors definition of what is a permeable terrain. It seems here the authors were forced to argue and ignore the Kw value (as a root of the quadratic equation) that is less than 0.5 so that they can later use a restriction of 0.25 on E/P to classify terrains into permeable (if a basin's E/P ratio <= 0.25) and impermeable (E/P >0.25), see page 68 line 1. I also believe it will be hard for the authors to argue for this golden threshold of 0.25 without evidence from observations for the geophysical arguments made around line 15 on page 65 (which I didn't understand at all). This appears to be yet additional condition on top of definition of impermeable and permeable terrains.

If I understand it correctly, the comparison of hydrograph separation based W (infiltration) and W based on their 3 component relationships (simplified from Ljvovic's with

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additional definitions) in tables 2 to 4 (also S (surface runoff) and U (subsurface runoff) in table 2) is based on the following two steps. First classifying area (though classification of continent sized areas such Africa as impermeable is hard to physically appreciate) as permeable (E/P<0.25) or impermeable (E/P >= 0.25) and then applying the appropriate three component method of the authors. In a way, close comparison of figures from their method and from hydrograph separation seems to vindicate their method, in particular the golden rule of E/P!

But the evidence provided in favor of it is still weak. A good reference to how to further support their method can be the following:

Sivapalan, M., M. A. Yaeger, C. J. Harman, X. Xu, and P. A. Troch (2011), Functional model of water balance variability at the catchment scale: 1. Evidence of hydrologic similarity and space-time symmetry, Water Resour. Res., 47, W02522, doi:10.1029/2010WR009568.

Given the weak evidence in terms of multiple basins data from different climates, landscapes, and vegetation types, it is hard to believe that their definition of permeability and impermeability exhaustively separates all the basins in the world. Nonetheless, whatever little evidence that the authors have provided appears promising.

Further physics based arguments for their definitions of impermeability and permeability is needed. Their definition of an impermeable terrain is not unrealistic. Consider low conductive isotropic soil that allows infiltration through preferential flowpaths. Vertical direction of flow path inhibits lateral subsurface flow, creating effective anisotropic conditions. Increasing relationship between infiltration and evaporation may hold under dry climatic conditions. But to counter this case, one may argue that other climate-soil-vegetation combination may yield nonincreasing infiltration-evaporation relationship for low permeable soils. To this the authors can only argue by stating that it is not impermeable according to their definition (a physically unrealistic scenario for a hydrologist) or argue that no other climate-soil-vegetation type can exist (or rarely exists, to accommo-

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date some errors) that disobeys the golden rule and the reduced Ljvoviv definitions that they have proposed in this paper. The latter argument may be admissible on grounds that climate-soil-vegetation coevolves over time to converge around certain stationary points. But again this argument would need support from literature and evidence from basin datasets that these days are readily available online free of charge.

To add few other concerns, the English in the paper is hard to follow. This reviewer had to reread sections many times and has guessed what the authors mean at many places. Thus, this reviewer may as well have misinterpreted what the authors intended to communicate. I suspect if the general readership would not be that patient. The authors should get their manuscript reviewed by a native English speaker before they resubmit this work either to this journal or elsewhere.

I would also suggest the authors to get their manuscript reviewed by a hydrologist that they personally know. Certain claims such as Ljvovic's definitions are applicable at intraannual scale, that continental Africa is impermeable, as well as presentation and discussion of comparative results presented in tables are unacceptable. Many times, four dimensional figures presented by the authors were either redundant or indecipherable. I personally believe that the authors can do away with most of their figures as the mathematics presented is sufficiently simple (which is also well presented). Other figures such as 7 and 8 need more explanation).

To summarize, I donot believe that the manuscript is publishable in its current form. The authors should support their definition of impermeable/ permeable terrains as well as the E/P golden rule of 0.25 with physics based arguments and additional data. Without it, the arguments presented in this manuscript are too weak. The authors should also consult a native English speaker and a hydrologist as well inorder to improve the language used in the manuscript. However, I believe the manuscript may have an important contribution to make (depending on what comes out of authors' extended analysis) and therefore urge the Editor to reconsider this manuscript for resubmission and re-review after major changes.

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