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Hydrology and Earth System Sciences Discussions

Interactive comment on "Factors affecting the runoff coefficient" *by* G. Del Giudice et al.

Anonymous Referee #1

Received and published: 28 May 2012

Dear Authors,

thanks for the long reply. I will wait the revised version to evaluate the improvements included in the paper. In the meanwhile, I would to better focus my point of view.

In my review I emphasized some problems:

1) The opportunity of investigating on the runoff coefficient. 2) The parameter uncertainty existing in the various empirical formulas used in the paper (Tc among others) that makes the conclusion difficult to be supported. 3) The relatively poor data set.

Probably I am wrong but I do not see in the Author's reply the answers to my questions.

Concerning the point 1.

the Author's reply says: "The Authors would also like to address one of the Referee

#1's comments about other advanced geomorphologic methods (GIUH, WFIUH) and the SCS-CN method itself; hopefully, it will be clearer now that the manuscript purpose is not the reconstruction of the flood wave, but an estimate of the mean of the probability distribution of annual maximum flood peak discharges. Hadn't it been so, the Authors would have certainly adopted more complex and complete methods"

In my opinion, the runoff coefficient is dated for flood protection work design. It could be still useful for the professional community in some specific cases but for major applications it is better to develop an event-based approach and select the peak flow information. Nowadays, there are enough available free data (rainfall, DEM, Soil Use) to perform the event-based approach where the rainfall excess approach allows a better representation of the net rainfall than the runoff coefficient. In addition, I do not feel that the GIUH model is advanced also considering that the WFIUH is adopted by several Italian and European Institutions.

Concerning the point 2.

This is the most important point and reading well the reply it is not mentioned. In my opinion, the "observed runoff coefficient", described in the paper, is a number without control. The formula (6) includes several types of error. I could lightly vary parameter values in this formula and obtain different phi values. The same concern is about the formula 10. The S parameter is affected, again, by errors difficult to quantify (it was calibrated in US on small plots). So, my conclusion is that the difference among R2 0.387 and 0.553 can not support any hypotheses because this difference could be due to several reasons.

Concerning the point 3

Authors recognize my point but I do not think that their valid justification can solve the problem: "The total number of gauged river sections (=50) may be considered small for statistical applications but they represent all available regional information."