

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

### **Anonymous Referee #3**

Received and published: 30 May 2012

#### Summary

This paper describes the relationship between the runoff coefficient and soil retention. The runoff coefficient is used in a standard method to estimate the flood peak discharge in ungauged rivers. There are existing approaches to estimating the runoff coefficient, but this paper develops another equation, whereby this parameter can be estimated from widely available soil, geology and land use data. It builds upon many existing equations but in principle offers great potential in both improving the estimation of the runoff coefficient and the ease of doing so. However, I have a few concerns over both the content of the paper and how it has been written.

#### Major Comments

1. The title does not reflect the contents of the paper and should be made more spe-

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cific e.g. Improvements to the estimation of the runoff coefficient through considering potential soil retention and climatic factors.

2. The abstract provides a good overview of the paper

3. Structure - The way the paper has been written makes it confusing to identify exactly what has been done, and what the novel parts are. I would suggest a more standard approach, where the background literature, methods and results are separated rather than being written as a narrative. All the methods should be written first, fully justified, before any results are given.

4. I would say there is a lack of results within the paper, which is dominated by a review of literature and how this leads to this slightly new approach. The method proposed is novel but builds greatly on existing equations. It would be useful to compare this approach to the existing methods and comment on how much the estimates are improved. Currently the paper doesn't even comment on how much the consideration of antecedent soil moisture improves the relationship over the initial formulation. Therefore, greater discussion of this is required. Can some analysis of the errors associated with this method be done, ideally with observed data.

5. I was a little confused how the geology and land use maps are relevant, as the relationship is with soil type and the SCS CN. This should either be made clear or removed if not relevant. It may also be useful to consider whether these factors improve the relationship with runoff further.

6. The use of certain data and methods should be more fully justified, along with why other approaches were rejected e.g. data sources line 22. Also common use does not justify its use here (data sources line 27)

7. I believe the conclusions need major revision, especially the comment on how the mapping is physically based. This should either be explained fully or my preferred option would be this to be removed.

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Minor Comments 1. Abstract line 2 – Which tables and graphs? This should be clarified.

2. Introduction line 16 – The index flood is a key principle within the paper and should be explained in more detail.

3. The data sources should be more clearly referenced (i.e. Pubblicazione n. 17)

4. More detail should be given on the pre-processing of the data or at least referenced (Data Sources line 10). What are the “data reliability tests”? line 12

5. A map of the area under consideration would be useful to put the study into context

6. Problem definition line 21 – “most precise estimates” but is this good enough?? Also should computed values be referred to as observed?

7. Section 5 pg 4928 line 11 – other factors, like what??

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 4919, 2012.