

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

Anonymous Referee #2

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General assessment

This manuscript develops a method to regionally predict the main catchment-specific parameter of a flood frequency estimation method, called runoff coefficient (ϕ). If my interpretation is correct, the m/s demonstrates that a fitted conceptual model using (1) a SCS-CN method S parameter (estimated from geology and land cover mapping) and (2) a climate humidity parameter (estimated from precipitation and temperature) explains 55% of the variation in ϕ estimated from streamflow data for 50 catchments. I am not intimately familiar with the literature on design flood estimation, but imagine that this level of performance may be useful and potentially worthy of record in the literature. However, before being considered for publication, I believe there are several important aspects that need attention. These include the manuscript structure, missing details, lack of discussion, and a few methodological issues. I also include some minor

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comments.

Manuscript structure

I was initially confused by the intent of this m/s. There were a few reasons for this. 1) The title is not descriptive; ‘runoff coefficient’ can relate to any expression of runoff to another variable. For example, before further reading I personally had interpreted to mean the ratio of long-term average streamflow over precipitation. Please revise the title to make it more descriptive. On my reading that could be ‘Spatially predicting flood frequency characteristics in southern peninsular Italy’ (‘Continental’ strikes me as a misnomer).

2) After reading the introduction I was under the impression that you would be analysing the uncertainty in the method presented in equations 1 to 3. You need to make it more clear that you already have made a priori decisions on the estimation methods (eqs. 1-3 but also eqs. 4-6; they should be presented together) and that that is not the focus of this m/s. You will also need to justify your choices however. The frequently mentioned popularity of your choices (e.g. p. 4920 line 15, p 4921 l 4, p 4922 l 27) is no justification per se, however if that coincides with a more widespread evaluation of the method, greater understanding of its performance, comparative performance against alternative methods, and/or official operational adoption in Italy, these may be stronger arguments (in any of these cases, references are required).

3) the m/s is poorly structured. There is no reason why it cannot be cast in a conventional scientific article structure. As it is the ‘Introduction’ contains one part of the theory and the ‘Data sources’ another part; the subsequent 4 sections all present part of the methodology with results mixed in.

Missing details

This is a short m/s by most standards. That is a good thing in principle, but unfortunately there are important details missing. In particular needed are

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* Supporting evidence, references or argumentation for p 4920 l 15 and p 4921 l 4 (see above), p 4921 l 21-23; p 4924 l 1; p 4925 l 11 (there are many different conceptual interpretations possible and indeed proposed for S, e.g. see Van Dijk, doi:10.5194/hess-14-447-2010)

* Explanation and/or discussion of 1) The way in which K-T is chosen and why that means that “the main technical problem” is not estimating K-T (p 4921 l 3-4) 2) How Xi-Q (Eq. 1) is to be estimated from mu-Q (eq. 6) in application 3) How and which rainfall-runoff models are used in the phi-zero data (page 4923 l 20) and what uncertainty this introduces

Methodological issues

I have a few questions about the methodology applied in this m/s:

1) How is the apparently considerable dependency of phi-zero on catchment area (eq. 6; it affects ARF, Tc and A) to be reconciled with the apparent lack of dependency of the empirical phi-e estimates (eq. 7/8; only an area-weighted average is calculated). Should phi-e not be subjective to the same area relationships in the empirical equation?

2) Why was geology mapping rather than soil mapping used to determine drainage class values for the SCS-CN method?

3) Why use the antiquated Lang’s factor and not climate humidity sensu Budyko, that is, the ratio of long-term mean precipitation and potential ET (P/PET)? Lang’s factor would seem to have meaningless units, suffer from the choice of temperature unit (degrees Celcius or Kelvin gives very different behaviour) and behave non-linearly and downright strange as one approaches melting point...

4) Your methodology implicitly assumes that geology and land use are more important than climate. Why not test how much less variation a prediction equation with only climate (LF or P/PET) and not S can explain?

Lack of discussion

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There is very little discussion in this paper. In particular, does the predictive power that was achieved lead to a notable performance improvement and greater practical utility, when compared to other approaches commonly used for design flood estimation? The innovative aspects of this research will need to be pointed out more clearly and convincingly for it to be acceptable for publication.

Minor comments

P 4919: authors affiliation is misspelled

P 4920: l2: pls define runoff coefficient here already; l4: what does “somewhat” mean; “Continental” confusing, consider “peninsular”; l9: “With this aim”; l 15-18: break up long sentence and rewrite.

P4921: l11: define Tc and restructure to combine this with eqs 4 -6; l23-25: rephrase more concisely

P4924: l 1: how to they depend? Through Tc or mu-Q?

P4925: l 18: “assumed “

P4929: l4: What exactly do you mean by “physically-based”? It strikes me as conceptual-empirical, not based on physical laws..

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

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