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Hydrology and Earth System Sciences Discussions

Interactive comment on "Inferring the flood frequency distribution for an ungauged basin using a spatially distributed rainfall-runoff model" *by* G. Moretti and A. Montanari

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We would like to sincerely thank Prof. Murugesu Sivapalan for his constructive comment on our discussion paper. We were very delighted to see that our contribution stimulated a discussion in our community soon after its publication on HESS-D. It was also very much appreciated that the comment came from a renowned scientist who is not a referee of our paper. We think that the possibility to open a discussion forum focusing on a paper which is under review is a very valuable opportunity offered by HESS through its publication model. We would enjoy very much receiving other similar comments which help to bring the attention of the community to a scientific contribution. Therefore we invite interested colleague to submit their ideas and comments, which would help us to substantially improve our analysis.

The key point raised by Prof. Sivapalan is related to the reliability of a distributed rainfall-runoff model, calibrated at the basin outlet, to effectively simulate the river flow in internal river cross sections, especially when the downscaling is significant. Indeed, in our case the downscaling is relevant. In fact, the model was calibrated by referring to a contributing area of 1214 km^2 and was verified in a cross river section whose contributing area is 337 km^2, while the application refers to a contributing area of 17 km^2. As Prof. Sivapalan rightly pointed out, proving the reliability of the rainfall-runoff model implies two main issues: the first is to convincingly demonstrate the model capability to effectively simulate the river flow at the basin outlet. The second is to prove the capability of the model to effectively downscale to the small sub-basin of interest.

About the first issue, we believe that a Nash efficiency of 0.81 and an explained variance of 0.83 for the simulation of the 1972 hourly flows (see page 11 of the paper, lines 19-20) are first indications of the acceptability of the model performances. We agree that it would be advisable to show also observed and simulated hydrographs for an extreme event. We will include this picture in the manuscript if we are allowed to revise our paper. Actually, on September 8, 1972, a flood occurred on the watershed with a return period of about 100 years and therefore we can provide a good proof of the model performances in flood simulation. Finally, we believe we provided another significant clue of the model performances at the basin outlet by showing the capability of the whole simulation procedure to simulate the flood frequency distribution. In our opinion this is an indication, referred to the basin outlet, about how the proposed method is effective in reproducing what we exactly want to estimate, that is, the flood frequency distribution.

Prof. Sivalapalan suggested also further analysis in order to check the model performances. Beyond the visual comparison of observed and simulated hydrographs,

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that the reader will be allowed to personally make once we included the related pictures in the manuscript, we believe it is interesting to discuss the other suggestions Prof. Sivapalan provided. We are certainly willing to include runoff coefficients for a number of simulated flood events and we can certainly draw some considerations about them. It would be certainly interesting to see how they change with peak flow, magnitude, season and different initial condition, but we doubt this could be useful for judging model effectiveness. In fact, in this way we can derive indications about the runoff coefficients simulated by the model, but we know nothing about actual event scale runoff coefficients for the study basin and therefore we could not make a comparison. We believe it is also extremely difficult to make extrapolation from gauged basins given that runoff coefficients are sizeably varying in space and time, even in the same basin. In this respect, it might be useful to say that we have a reliable estimate, provided by the Italian National Hydrographic Service, of the long term runoff coefficient. This information was not used in the context of our analysis and we agree that we could make an interesting comparison between observed and simulated long term runoff coefficients. We are willing to include this comparison in the revised version of our paper. We understand that this is an indication that is focused on the whole hydrograph and not only on the peaks, but we believe it could be indeed helpful, because its uncertainty would be not excessive.

About the second issue (capability of the model to effectively downscale to the basin size of interest), we agree with Prof. Sivapalan that this is a relevant problem which is not easy to address. We fully recognize that a number of uncertainty sources could make the model unable to properly downscale the simulation of the hydrologic processes involved in the rainfall-runoff transformation. Being aware of this problem, we tried to prove the capability of the model to downscale the flood frequency distribution, in validation mode, to the internal river cross section of Ponte Cavola, where the basin area is 337 km^2. The results were quite satisfactory, but one may note that passing from the basin outlet to Ponte Cavola there is a reduction of basin area of about

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72%, while passing from Ponte Cavola to the application site there is a further reduction of contributing area of about 96%. Therefore the indication we obtained for Ponte Cavola might be not representative with respect to the application site and therefore we recognize that we need to further substantiate the reliability of the proposed procedure.

Within this respect, we dismissed in our paper the alternative method based on hydrological similarity. We rejected it because we believe that the peak flow observed in the river cross section of the Secchia River that can be used as "twin" site for applying hydrological similarity are not reliable (see page 6 of the paper, lines 20-21). Therefore it was only a data problem and we did not reject regionalisation a priori. In fact, we used the term "hydrological similarity", and not "regionalisation", because we wanted to keep the two approaches well distinguished. In our opinion, the hydrological similarity that was applied in our paper is not a regional approach, as only one site was picked up, and extrapolated from, in order to derive a peak flow estimate for the site of interest. We considered one site only because we wanted to focus on the Secchia River basin. In our opinion, the term "regionalisation" implies the consideration of a wide geographical area from which flood data from many sites are pooled together and analysed.

Given that regionalisation is not dismissed by us, the suggestion of Prof. Sivapalan stimulated a possible idea for proving the reliability of the peak flows estimates we derived. We may try a regional approach, by pooling together and analysing peak flow data from the whole region around the Secchia River basin. Therefore we may focus not only on the watershed of interest, but instead on the whole region. This idea would be feasible because flood data for the considered region are available in good amount, and in fact a regional study was already performed with good results by Franchini and Galeati (1996) and Castellarin (2005).

We think such comparison with a regional approach would be more significant

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with respect to the comparison proposed by Prof. Sivapalan. This latter consists of deriving a design rainfall of a given return period T from intensity-duration-frequency curves for rainfall, computing the net rainfall by using a model produced runoff coefficient and then routing the net rainfall through a model produced unit hydrograph in order to derive a T year return period flood. In fact, we believe there is a relevant uncertainty in the estimation of the runoff coefficient and unit hydrograph, which would be model based and therefore would not eliminate the concerns originated by potential model unreliability. Moreover, we would implicitly assume that the return period of rainfall and river flow coincide, which is not true for non linear systems. To remove such assumption, a continuous simulation of river flows by using synthetic rainfall series would be needed and therefore one would get back to the procedure we used in our paper.

Finally, we would like to make a final consideration which we believe it is very important. Probably we need to better stress this issue in the case we are allowed to revise our paper. Given that we were fully aware of the uncertainty involved in our analysis, we dedicated much time to try to understand the Secchia and Riarbero watersheds. Actually, we went along the Riarbero river bed (the upper part is accessible by feet only). We went there with good weather to see the river, to survey some cross river sections and to assess the conditions of river banks and hillslopes. We may provide many pictures to interested readers. We went there during raining days also, to see how the river behaves when the water level increases. Basing on our assessments, we realised that the peak flow derived through the hydrological similarity is not reliable, because it is not consistent with the vegetation displaced along the river (see page 7, line 1; page 18, line 2 and line 15 of the paper). This was the reason why we dismissed the first peak flow estimate we obtained (based on hydrological similarity). Instead, we were satisfied by the output of the distributed model. We believe such assessment based on expert knowledge, although qualitative and subjective to some extent, is very important and is a very valuable

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indication of model reliability. What we did is to look for the hydrological signature on the river bed of the extreme floods. It was operationally very simple and intuitive. Moreover, it gives us the opportunity to express a feeling that we believe is important in this case, which we probably did not emphasise enough in the discussion paper.

We believe that, when dealing with ungauged basins, we have to look for hydrological signatures of the processes we are trying to simulate instead of excessively insist on pure modelling exercises and model testing. In the case of the Riarbero Torrent the hydrological signature was very simple to find and intuitive. In other cases it might be not directly visible and new analyses may be needed to identify it. For instance, we already know that the morphology of the river network is a signature of the river flow regime and we believe it would be extremely interesting to investigate how it could be used for checking the consistency of a model output. To investigate the hydrological signatures was usual practice for modellers in the past century and we are concerned that the increasing capability of computing power may induce the modeller to spend on his desk the time that previous generations spent looking at the watershed.

Looking for hydrological signatures may open new avenues of research, which may be even not difficult to follow. Accordingly to the thoughts expressed by Sivapalan et al. (2003), we believe we need practical tools in order to derive indications about the dynamics of the investigated processes from accessible information on the contributing watershed. We are willing to include these considerations in the revised paper and we are willing to better stress the importance of the indication given by the hydrological signature we identified. Another good opportunity, which was not considered here, is the use of orthogonal information (Winsemius et al., 2006). In this case also, new avenues of research might be opened by those who have original ideas and intuitions.

To summarise our reply to Prof. Sivapalan, if we are allowed to revise our paper we propose to:

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1) include a comparison between observed and simulated long term runoff coefficients to further substantiate model reliability at the basin outlet.
2) Include a picture of observed versus simulated hydrographs for one extreme event.
3) Include a comparison of the proposed simulation technique with a regional approach.
4) Better stress the importance of the indication provided by the hydrological signature we identified.

We will also address all the other minor remarks raised by Prof. Sivapalan. Therefore, we may include a picture of the watershed, we may discuss the suitability of the scaling exponent of $\frac{2}{3}$ we used in equation (1) and we may rewrite the abstract, in order to better synthetize the essence and limitations of our study.

Once again, we are very grateful to Prof. Sivapalan and would be very happy to know his opinion about our reply above. We are grateful to the journal too, for the very good opportunity we have to stimulate a discussion. We are particularly interested to know what the community thinks about the need for identifying effective hydrological signatures.

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