Hydrol. Earth Syst. Sci. Discuss., 12, C5172–C5179, 2015
www.hydrol-earth-syst-sci-discuss.net/12/C5172/2015/

Hydrology and Earth System Sciences Discussions
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Interactive comment on "HydroSCAPE: a multi-scale framework for streamflow routing in large-scale hydrological models" *by* S. Piccolroaz et al.

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Received and published: 26 November 2015

We thank the Reviewer for her/his comments, which were instrumental to better clarify the main message of the manuscript. Indeed, based on the Reviewer's comments, we realized that in some parts of the manuscript the objective of our work was not adequately described and focused. We plan to revise the entire manuscript, in particular the abstract and the introduction, to better elucidate the main scopes of the work. In the following pages all Reviewer's comments are addressed and discussed.

To facilitate the reading of the present document, comments from the Reviewer are in BLUE.

1. This paper proposes some improvements to the WFIUH approach.

   From this and a few other comments (see our replies to the following points) we realized that the main message of the work was not sufficiently clear in the original version of the manuscript. Indeed, we are not proposing any improvements to the WFIUH approach here, but rather an innovative routing scheme based on the classical WFIUH approach. The scheme aims at upscaling river network dispersion at the scale of the block in large-scale hydrological models.

   Although we believe that the main scope of the work was rather clear (from the title to the conclusions), we acknowledge that the first sentence of the abstract (in particular) was misleading. We identified in the abstract and the Introductions the main parts of the manuscript that need to be revised in order to better clarify the aims of our work.

2. The first argument of the paper is the emergence of socio-hydrology, which is not in scope of the paper. Then, justification of the paper is based on a literature review of Earth System Models and Large Scale Hydrological Models, which leads to the choice of the WFIUH approach for its parsimony, conceptualization, scalability...

   We agree with the Reviewer that the first sentence of the introduction was not within the scope of our contribution. Therefore, we plan to fix that and other points in a revised version of the manuscript.

3. But finally the literature background of the geomorphology-based approaches, including the WFIUH, is not comprehensive and well displayed forefront, so that several claims of the "innovative", "perfect scaling" etc. proposal are not demonstrated.

   We agree with the Reviewer that a more detailed literature review on geomorphology-based approaches would have been necessary if the manuscript was about the description of an improved WFIUH approach. However, the objective of the work is different (please, see our reply to the first point) and we think that the references we included are sufficient for the scope. The action we would like to undertake to address this point is revising the Introduction to clarify the reasons why the proposed routing scheme is innovative. Perfect scaling is not claimed, but it is rather embedded in our scheme, as clearly shown in Section 3.2.

Nonetheless, we feel that some references addressing the adoption of distributed versions of the GIUH should be included in our manuscript, since they share the common purpose of delineating a possible application of the classical geomorphological approach in the case of spatially variable rainfall/infiltration patterns. They are listed below:

- Naden PS, 1992. Spatial variability in flood estimation for large catchments: the exploitation of channel network structure, Hydrological Science Journal, DOI: 10.1080/02626669209492561
- Moussa, R., 1997, Geomorphological transfer function calculated from digital elevation models for distributed hydrological modelling, Hydrological Processes, DOI: 10.1002/(SICI)1099-1085(199704)11:5<429::AID-HYP471>3.0.CO;2-J
- Rinaldo, A. , Botter, G., Bertuzzo, E., Uccelli, A., Settin, T., and Marani, M., 2006, Transport at basin scales: 1. Theoretical framework, Hydrology and Earth System Science, DOI:10.5194/hess-10-19-2006
- Hallema, R., Moussa, R., Andrieux, P. and Voltz M., 2013,Parameterization and multi-criteria calibration of a distributed storm flow model applied to a Mediterranean agricultural catchment, Hydrological Processes, DOI: 10.1002/hyp.9268

4. The main underlying issue is the dealing with the emergence of dominant hydrological processes and the relevance / improvement of the WFIUH in this regards when applied to mesoscale basins (as exemplified with upper Tiber basin in Italy): between hillslope / channel / drainage network; between grids and basin sizes / scales; between dispersion, space variability and simplifying assumptions (average velocity or not, rainfall spatial variability assessment and accounting...) – which could be made more explicit.

   We agree with the Reviewer that the scope of the work could have been made clearer and more explicit. Please, see our replies to points 1 and 2.

5. Literature about hillslope/channels (individuals and networks) articulation is acknowledged here and there, but the one about accounting for spatial variability in geomorphology-based IUH is not acknowledged. Papers do address this issue with different rainfall data input (radar, interpolation. . .), convolution enrichments, notions of effective networks, sub-basins nestings. . . The approach presented here should be framed in the whole landscape of the corresponding literature.

   We partially addressed this comment in our previous replies. We stress again that the scope of the paper is not to propose an improvement of the WFIUH approach by accounting for spatial variability of rainfall. Rather, we are proposing a new routing scheme based on the classical WFIUH approach. The proposed scheme is designed to be easily coupled with weather forecasting and climate models providing the meteorological forcing. In this sense, we account for spatial variability of rainfall without modifying or improving the WFIUH method. No assumptions are made on spatial variability of meteorological forcing, which is totally inherited from the meteorological, or meteoclimatic, model.

   Nonetheless, at page 9066 of the original version of the manuscript a few references to the literature that the Reviewer suggested are already included *". . . with the latter embedding the spatial variability of rainfall patterns according to the macrocell resolution (a similar approach, but based on a partition of the catch-*

ment into sub-basins, can be found in Rinaldo et al., 2006; Rigon et al., 2015; Bellin et al., 2015)." Given that the objective of the manuscript is not assessing the type of spatial variability, and at which scale, that is best suited for a given application, we believe that the above citations are enough.

6. Further, even if the griding and nodes rationale presented here allows in theory to account for spatial variability of runoff, it is not clear how calculations are operationalized. Hillslope runoff relies on classical models such as the SCS one, but how is this run at the hillslope level before downstream aggregation? How are soils and land covers described and conceptualized at the elementary level of this rationale? Runoff is in fact closer to net rainfall than to gross rainfall. This "hillslope production function" is very contingent across hillslopes and along time non linearities and is a major epistemological obstacle in the geomorphology-based literature which this paper somehow overlooks.

   When we introduced the "hillslope production function" η (at line 20 of page 9062 of the original manuscript) we intentionally left it unspecified in order to describe the routing scheme in the most general form possible. We emphasize that the proposed routing model is independent from the choice of the hillslope production model. Indeed, the routing scheme has been designed with a flexible structure, which makes possible to implement any rainfall-runoff model, according to specific users' needs and preferences. A comment on these model's peculiarities was already included in the abstract, and we plan to emphasize it more both in the abstract and in the concluding remarks. In the revised version of the manuscript, we will also emphasize that in order to focus on routing we deliberately kept the rainfall-runoff model (which again is not the focus of our work) as simple as possible. Thus we opted to use the widely known, and applied, SCS-CN model. We are aware of the limitations of the SCS-CN model, but the hillslope model is not the focus of our contribution and our routing scheme can be coupled with any conceivable hillslope model. We will revise also section 3.3, where the applica-

tion example is described, making clearer how soil and land covers are described within the macrocell.

7. Spatial explicitation / Interpolation of rainfall (ideally net rainfall before the convolution with the transfer function) is also a major issue which is here solved by kriging with external drift from the network of available raingauges (changing from one event to the other). The influence of this interpolation approach on the rainfall-runoff modelling is not neglectable compared to the geomorphometric side. Is kriging relevant at the used modelling time step? Is'nt the geostatistical structure changing for changing rainfall fields under convective, advective and orographic influences? Further the griding scheme could be more linked/discussed in conjunction with the raingauge geometry and resolution.

   We understand and agree with the worries of the Reviewer concerning rainfall interpolation. However, as we already mentioned in the previous points, the application example has been intentionally kept as simple as possible in order to focus on the routing scheme, which is the objective of the work. Indeed, the example application should be seen as an ancillary part of the work, whose core is Section 2 (where the routing scheme is described) and section 3.2 (where we demonstrate that our routing scheme enjoys perfect upscaling, irrespective to the size of the overlying blocks depending on the model providing the meteorological forcing). For this reason, we decided to use a simple method, as SCS for runoff production and kriging for rainfall interpolation with a semivariogram structure tailored to the case at hand. However, we remark again that the precipitation pattern can be any, the kriging tool employed here being just an application example. See also our reply to the previous point.

8. A full WFIUH approach is developed for nodes corresponding to macro grid cells, and then "rigidly translated" to downstream nodes. The relevance and interest of this nesting approach with a jump in simplifying assumptions are not discussed whereas it is at the origin of the high calculation cost (and so parallelization chal-

lenge) and whereas the classical WFIUH is parsimonious in calculation as based on a simple convolution.

We have read this comment several times and we are unsure to have correctly understood what the Reviewer wanted to say. If he/she is wondering about the correctness of rigidly translating streamflow between nodes, we remark that the assumption is conceptually fully compatible with the WFIUH approach, which, in the case hydrodynamic dispersion is neglected (as e.g. in Botter and Rinaldo, 2003, Scale effect on geomorphologic and kinematic dispersion, Water Resour. Res., 39, doi:10.1029/2003WR002154), allows a rigid and time-invariant translation in time of water parcels injected in the system. Hence, there is no jump of assumptions. Concerning the hypothesis of a constant stream velocity, we cited the fundamental literature at page 9063 of the original version of the manuscript.

Finally, we note that there is not any parallelization challenge in the routing scheme we presented. On the contrary, what we claimed is that the scheme is well suited for parallelization, which can be easily implemented thanks to the linearity of routing and independency of the runoff generation module adopted at the cell scale (see abstract, introduction, model description, and conclusions). Certainly, the computational cost increases with increasing number of macrocells, but this also allows for a more detailed description of hydrological processes compared to the case when a single convolution is done for the whole basin. This is discussed in Section 3.3, where we compared results obtained considering macrocells of increasing size, from 5 km to 150 km.

9. The proposed approach is exemplified with two historical events of the upper Tiber basin. Results obtained do not allow to conclude 1) if the proposal performs better than “classical WFIUH”, including options which already account for spatially-variable rainfall; and 2) about relative errors, uncertainties and improvements of the rainfall space-time variability accounting, the hillslope production and transfer modelling, and the “innovative” network transfer modelling.

At the risk of being redundant, we wish to point out again that the scope of the manuscript is not to propose a new WFIUH method accounting for spatially variable rainfall. Rather, we want to present a simple and fast routing scheme based on the classical WFIUH approach to be easily coupled with weather forecasting or climate models that use a gridded computational domain. Therefore, the Reviewer is right in saying that the results presented in the manuscript do not allow to draw the conclusion written in her/his comment. Indeed, these considerations are beyond the scope of the paper and are not the message that we want to convey.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 9055, 2015.