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Interactive Comment

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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We thank the Reviewer for her/his comments, which were very helpful in clarifying and focusing a few aspects of our work.

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

Dear Dr. Moussa, This is an evaluation of the paper entitled "HydroSCAPE: A multi-scale framework for streamflow routing in large-scale hydrological models" (HESS-2015-371) submitted to Hydrology and Earth System Sciences by Dr. Piccolroaz et al. on August 24 of this year. In it, the authors present a hydrologic model that they named HydroSCAPE, and which uses the Width Function Instantaneous Unit Hydrograph (WFIUH) for flow routing in medium-sized watersheds and larger.

The study is both original and scientifically relevant as far as I can judge based on my knowledge of the topic, and provides a relatively new perspective that we have already seen applied in other fields of science, but now applied to watershed modeling. Given the importance of this contribution and the quality of the presentation, I recommend the paper for publication provided the authors apply a series of minor corrections to clarify some matters. Hoping that this review may prove helpful in your decision, I remain yours faithfully.

We thank the Reviewer for her/his appreciation of our work.

Suggestions for improvement:

1. p. 9058 l. 2: Please rephrase. Rather than being two separate equations, the kinematic wave equation is itself an approximation (simplification) of the Saint-Venant equation assuming uniform flow and a friction slope equal to the slope of the channel bed.

   The Reviewer is right. We will modify the text removing the explicit reference to the kinematic wave equation.

2. p. 9058 l. 10: "0.1 to 0.5 degrees" Please indicate the corresponding ground-projected area in km x km. Accepted. We will add the distance in km corresponding to 0.1° and 0.5° of latitude.

3. p. 9059 l. 23-26: I mentioned above that this paper provides a relatively new perspective. That being said, this paper appears to be very much an extension of last year's paper by Hallema and Moussa (2014) with regard to the application of the WFIUH and spatial subdivision of the watershed. Instead of describing

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the flow nodes, that paper refers to the flow vectors connecting these nodes, which they call land surface components and channel components, but represent essentially the same substance. The main differences I think are the size of the watershed used in the case study and the use of macrocells. Please cite and elaborate.

We carefully considered the analogies and differences of our method with the distributed GIUH proposed by Hallema and Moussa, 2014. Both have in common the derivation from the Geomorphologic Theory of the hydrologic response, the underlying idea of further exploring the capabilities of the GIUH approach, and the idea of watershed subdivision; we will mention the paper in the revised manuscript as it has some points in common. However, we also recognize that the two models show relevant differences for purposes, scales of possible applications, but also for their inherent structure. The main idea of Hallema and Moussa, 2014 is the representation of anthropogenic hillslopes, and their “representative unit” is a planar, rectangular element which embeds both hillslope “slow” response and channel flow routing within the REH; the scale of application proposed is 1 hectar as order of magnitude. This stems from the objective of representing in detail the specific configuration of anthropogenic terrain features, such as terrace cultivations typically found in agricultural regions. Small scale also explains why in the real-world application rainfall injection and infiltration are uniform in space. As we explain in the Introduction and Model development sections, we aim instead at (i) providing and effective and accurate routing scheme for large scale flood modelling, (ii) explicitly embedding the variability of rainfall and infiltration, though studying their impact on modeling is beyond the scope of this work (and literature is available on this specific issue), (iii) coupling different scales of representation for geomorphology-based components and meteorological forcing (this motivated the adoption of the “macrocells”), (iv) and clearly separating the different contributions from hillslope component (which could be treated with different case-dependent sub-models) and drainage network.

4. p. 9062 l. 4 “where streamflow is desired” Suggest: “where we want/need to calculate streamflow”

   We propose to modify the sentence as follows “where streamflow is simulated”. Similarly, we propose to modify accordingly a similar sentence in the abstract.

5. p. 9062 l. 25 “depends on the partitioning of hydrological fluxes” Explain which processes this refers to, i.e. the Hortonian mechanism, subsurface flow, etc.

   In principle, the routing scheme presented here does not impose any restrictions on the hillslope processes to be simulated. Therefore, here we are not referring to any particular process, and the sentence should be considered with its most general meaning. To make this more explicit, we propose to modify the sentence as follows “The resulting water flow is triggered by rainfall or snowmelt and depends on the partitioning of hydrological fluxes at the hillslope scale, according to the selected hillslope model and the hydrological processes that are simulated”.

6. p. 9063 l. 18 “In agreement with the WFIUH theory, stream hydrodynamic dispersion is neglected” Not sure if that requirement was explicitly defined for the WFIUH theory, suffice it to state that WFIUH simply does not account for hydrodynamic dispersion.

   Indeed, neglecting stream hydrodynamic dispersion is not required in the application of the WFIUH, while many works adopted this simplification after the role of Geomorphological Dispersion was recognized (Rinaldo et al., 1991). We agree with this Reviewer’s comment and we propose to modify the sentence accordingly.

7. p. 9067 l. 4 “Relevant flood events” Suggest: “Substantial flood events”

   OK, we propose to modify the sentence as suggested by the Reviewer.

8. p. 9067 l. 16 “multi-site model calibration” I gather from section 3.3 that the model is calibrated with regard to the Ponte Nuovo station alone, which would

make this a monosite calibration. Multi-site calibration implies that the model parameters have been calibrated to optimize performance at multiple sites at once, for example by optimizing average NS for all stations. This does not seem to be the case here.

The Reviewer is right. We propose to correct the sentence by replacing "calibration" with "validation". Indeed, in Section 3.3 we presented both multi-site and multi-site/multi-event validation of the model, which has been previously calibrated at the control section of Ponte Nuovo.

9. p. 9075 l. 20-25 and Table 2. As stated in this paragraph, the watershed model inherits parameters (and values of corresponding state variables) from the 'sub' models so to speak, but would the authors consider that consequentially, errors inherited from these underlying models can accumulate rapidly? The authors show this already given the near optimal Nash-Sutcliffe coefficient at Ponte Nuovo and lower performance for other flow stations. I think that Table 2 can list more criteria than the Nash-Sutcliffe coefficient alone, such as the (relative) peak flow and volume errors. This will help identify the strengths and points of improvement for this approach.

   We thank the Reviewer for this comment. Certainly, computing and including in Table 2 additional metrics would help to better quantify modeling errors and uncertainty. However, our focus here is not on identifying the best possible model, but rather to show how our approach is able to take into account in a proper manner the effect of routing along the river network irrespective of the scale at which meteorological and hillslope processes are represented. Of course the results depend on how the spatial variability of soil properties and meteorological forcing are reproduced (both are constant within the cell), but this is not, by any means, a limitation of our approach. We believe the NS coefficient suffices to illustrate this point. The deterioration of NS (averaged over all nodes) as the cell's size increases is due to the progressive lack of resolution in the spatial variability

Table 1. Nash Sutcliffe Efficiency index, Volume error, Peak flow error, and Peak time error for Ponte Nuovo and all nodes for different spatial scale resolutions. Errors are shown in absolute value. When referred to all nodes, errors are evaluated as the average of errors at each node. Note that for the case of a unique macrocell of 150 km, the significantly lower values of NS in validation (both multi-site and multi-site/multi-event) are primarily due to larger errors in simulating the timing of the peak flow rather than the volume or the maximum streamflow, compared to the other spatial scales.

	5 km		10 km	10 km		10 km	50 km	
10 km	150 km		10 km					
	Feb 1999	Dec 1992	Feb 1999	Dec 1992	Feb 1999	Dec 1992	Feb 1999	Dec 1992
NS (PN) [-]	0.99	0.57	0.99	0.60	0.97	0.61	0.94	0.28
NS (all nodes) [-]	0.64	0.27	0.69	0.31	0.56	0.38	-0.69	-0.65
Volume error (PN)	0.80%	27.60%	0.46%	26.26%	1.48%	20.95%	0.21%	22.93%
Volume error (all nodes)	7.83%	35.27%	8.94%	33.37%	10.01%	27.53%	11.63%	32.74%
Peak flow error (PN)	1.83%	3.90%	0.60%	1.36%	0.06%	2.49%	1.49%	8.41%
Peak flow error (all nodes)	16.76%	17.31%	14.02%	16.70%	13.88%	10.94%	16.51%	29.58%
Peak time error (PN) [h]	0.00	0.50	0.50	0.50	0.50	1.00	1.00	2.00
Peak time error (all nodes) [h]	1.80	0.88	1.90	0.75	1.30	1.25	3.10	2.13

of precipitation. Notice that calibration is partially able to compensate for this at PN (i.e., high NS values, larger than 0.9), where the model is calibrated, but not in the other gauging stations, which are used to validate the model. However, in validation NS averaged over all nodes remains closer to that obtained at the smaller grid's sizes, up to the largest scale ensuring a good reproduction of the spatial variability of precipitations (in our case about 10 km, which is smaller than the integral scale of the precipitation field, which is estimated in about 36 km). As required by the Reviewer we computed additional metrics (see the Table below). However, given that these metrics confirm what already observed with NS, we propose not to include them into the revised manuscript.

10. p. 9075 l. 25-27 “Parsimony is important for a meaningful and reliable parameter estimation procedure and uncertainty analysis, especially when dealing with largescale and complex basins.” If anything, basin models are often more accurate than models of smaller headwater catchments because of more and better quality data. Generally speaking however, it is more correct to assume that parsimony is equally important at all scales, whether for a hillslope runoff model or a soil infiltration model.

    We agree with the Reviewer. We only meant that uncertainty estimation can be very difficult as the size of the basin gets larger, due to the natural variability of the processes involved. We propose to remove the second part of the sentence from the revised manuscript.

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