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Interactive comment on "Distributed hydrological modelling of total dissolved phosphorus transport in an agricultural landscape, part I: distributed runoff generation" *by* P. Gérard-Marchant et al.

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General comments

This paper discusses the application of a distributed model to a 164ha humid catchment in north-east USA. The model is tested against streamflow, soil moisture transect data and qualitative observations relating to runoff source areas. As noted in the paper, in addition to testing against stream flow, testing against spatial observations is important for establishing the credibility of the spatial model predictions and this part of the paper is the most novel and interesting. In my view, the paper could be strengthened by focussing more on this aspect. Testing using spatial patterns is both valuable as it aids in testing internal model predictions (see for example Western and Grayson (2000) for results with a similar model and detailed soil moisture patterns) and essential if a model's spatial predictions are to be utilized. Such model testing against spatial and other data is a valuable contribution. Another strength of the paper is the attempt to utilize available data rather than a calibration paradigm to estimate model parameters.

Having said that, I had some significant concerns about the conclusions drawn from the comparison of Figures 7 and 8. Figure 7 shows a variety of features associated with runoff source areas that are nominally localised within the lower slope / valley floor unit of the catchment. Figure 8 shows annual runoff prediction across the catchment. The results in Figure 8 seem to me to delineate roughly three zones in the catchment. These are the riparian zones with winter runoff rates of greater than about 0.45mm/d (? units aren't clear from figure and caption), a fairly extensive area of lower slopes with winter runoff rates of about 0.2mm/d and an area of middle and upper slopes with low runoff rates. The riparian and lower slope zones correspond reasonably closely to the agricultural area within the catchment and the great majority of runoff features (drains, source areas, etc) are smaller scale features located within the lowland and riparian areas. On the evidence presented, the model does not seem to predict these smaller scale features.

One possible interpretation is that the field mapping is not representative of reality. However, blaming the data is often incorrect and presumably the authors are confident in the overall picture implied by the data. The other interpretation is that the model is lacking either structurally or in terms of the quality of the inputs.

This is important from both scientific and application perspectives. From a science perspective, it means that there is something important not being captured by the model as it stands. My intuitive feel is that there is probably small scale soil and geological variation that is causing this result. Certainly the comparisons against the soil moisture transects suggest the soils data is not always accurate, although there is an interpretation issue with comparing surface samples against an upper profile integrated soil moisture prediction (see below). Various modellers (eg Houser et al., 1998; Richter et al., 2004; Western and Grayson, 2000) have certainly found using soils information in hydrologic models challenging due to the uncertainties involved, although the continental scale mapping in Australia used by Richter et al. (2004) lags that in the US for obvious resource reasons. Our ability to make use of such data needs further development and this paper makes a valuable contribution in trying to do so and identifying this issue further - the discussion of this issue in the paper deserves more than the brief mention made in the paper as it is a critical roadblock to advancing this style of modelling in my view.

From an application perspective it is important that spatial predictions are sufficiently accurate, as is argued in the introduction to this paper from a contaminant generation perspective. Such applications are a strong motivator for spatial modelling and they set a performance standard that we need to strive to meet. That performance standard could be something like "predicting high spatial runoff generation zones such that false negatives and false positive both constitute less than 10% of the area of the high runoff zone in the catchment." There can be debate over whether it should be 10% or some other number but to me this level of accuracy would mean that we could start recommending spatially targeted source management on the basis of model results. Of course we are a long way off this as a discipline and the fact that the author's model is not meeting such a stringent target is not meant as a criticism of their model over anyone else's but more as a philosophical point about needing to aim for some reasonably specific and high targets in the future. The way we test our models now needs to recognise this. If a standard deriving from this sort of view point were applied in this paper, I think the authors would conclude that the spatial prediction of runoff areas needs to be substantially improved, not that it is generally adequate.

Specific comments

Page 1539 (3) lines 20-25. An argument is made that explicitly distributed models are required to predict saturated source area runoff. I disagree with this statement as it stands. What is required is that the temporal evolution of the saturated fraction be adequately simulated. A variety of lumped models do this by a variety of parameterisations, probably the best know of which is Topmodel, which the authors list as an explicitly distributed model. In fact the water balance calculations in Topmodel are spatially lumped. It is true that spatial estimates can be made by mapping back to a spatial context using the wetness index distribution (Quinn et al., 1995). Of course if you wish to know the location of runoff areas explicitly you need a distributed model, as is also true for Hortonian runoff.

Page 1554 (18) line 12. The soil samples are taken from 2-6cm but are compared with model estimates over the upper soil zone, as I interpret the paper. There is an important assumption here in scaling up from the sample to the layer. This should be explicitly stated and the potential impact on uncertainty and bias discussed.

Page 1554 (18) lines 19-26. Several error estimates are mentioned here but it is not clear what they are (eg standard deviation, 95% conficence, some other measure). From comparing the error regions and the data points in Figure 6, it seems to me that these error estimates are very high and often cover the full range of the measurements i.e. the measurements look better than the error margins given but I'm unclear on how to interpret the error estimates.

Technical issues

Page 1542 (6), equation 1. This equation implies there is no canopy interception in SMDR, is this correct? What impact does this have in forested areas in particular?

Page 1543 (7), line 21. Typo - this should read " respectively, and (alpha) is a universal constant Ě"

Page 1548 (12) line 18. Typo "Allen et al." - missing space.

Page 1551 (15), line 9. It would be useful to mention what the flow transformation/modification to Nash-Sutcliffe is.

Page 1555 (19) line 10-13. This sentence could be reworded.

Page 1557 (21) line 8. Do “wetlands” correspond to VSAs in Figure 8? Please clarify.

Figures in general. Please also make figure captions/legends more self contained i.e. explain the legend in the caption where it is not obvious.

Figure 5. I thought that this figure could be trimmed back to present representative years rather than the full record without loss to the paper.

References

Houser, P.R., Shuttleworth, W.J., Famiglietti, J.S., Gupta, H.V., Syed, K.H. and Goodrich, D.C., 1998. Integration of soil moisture remote sensing and hydrologic modeling using data assimilation. Water Resources Research, 34(12): 3405-3420.

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