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Hydrology and Earth System Sciences Discussions

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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We appreciate the comments of Western and thank him for taking the time to give his insights on distributed modeling and our paper. We agree fully with his view in general but especially that model testing against spatial observations is essential to the credibility of a model. It is particularly essential when these models are used for planning measures to decrease non point source pollutant loads in streams, because water quality is directly related to where runoff is generated. Having said that we regret that we were only able to use observations of the surface soil to validate the predicted distributed moisture contents and ultimately the runoff locations. Practical difficulties such as the high stone content made it impossible to take systematically deeper samples. We are somewhat jealous on the measurements that Western and his coworkers were able to make in the Tarrawarra watersheds to much greater depth than we are able

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to do (e.g., Grayson and Western, 2001). These difficulties and the time consuming nature of taking spatially distributed data are likely the reason that so few models are spatially validated.

As suggested by Western (2005) we have extended our section at the end of the manuscript on the comparison of the predicted spatial distribution of runoff with both the expert knowledge of the farmer/landowner and actual landscape features that indicate wetness. The mention of 'wetlands' as natural hydrological features in the initial paper has been suppressed for clarity: they corresponded actually to the fern-covered area along the NE-streams, which can be observed on the orthophotographs. This information is in fact part of the "additional information" mentioned in the paper. On a pixel by pixel basis our predictions are reasonable but clearly not within the 10% accuracy proposed by Western as acceptable for management purposes. However, management on farms (for example, manure spreading spreading) takes place at the field scale and throughout the year. Our model might actually meet the 10% accuracy when for predicting average degree of saturation throughout the year.

Western notes that various modelers have found that using soil information in hydrological model challenging due to uncertainties involved. This is the case in our simulations as well with the detailed soil county maps that are available to us in the USA. We found that our distributed predictions for the undulating glaciated landscape are extremely sensitive to the accuracy of the Digital Elevation Map (DEM), to the estimates of the permeability of the underlaying subsoil and to the depth of the soil above the subsoil. The sensitivity to the DEM was earlier noted by Kuo et al. (1999) and is easily understood because both flow lines and hydraulic gradient depend on this map. A small error in elevation in the flat part of the field changes the location of the wet spots. The accuracy of the standard DEM for the Catskills is likely one of the best available but it still has problems because it is made by photo interpretation originally with a 10-m horizontal resolution and then later refined to 5-m by NYCDEP. The soil parameters (topsoil depth and subsoils permeability) can only be obtained from the soil survey in very limited form. The accuracy of the survey is limited by the original interpretation of the soil surveyor who made the map in a time without the advantage of modern techniques. Moreover, these maps were intended for agricultural purposes and the use as input for (semi) distributed modeling is only an afterthought. As a consequence, the exact amount of leakage through the subsoil can only be obtained by calibration with base flow data. The observations of the land owner on the hydrological behavior of the field might be currently our best way of fine tuning model parameters so that observations and model predictions agree fully. However, only in a few rare cases a farming family like the one we collaborated with is found that has the patience to deal with academics on their farm.

Specific comments

We concur with the comment that fully distributed models are not needed for providing distributed output and lumped models that conserve spatial information can successfully be used. TOPMODEL and its many different implementations indeed gives spatially distributed outputs for landscapes with a ground water table. We are working on a similar approach for these glaciated landscapes with perched groundwater table for part of the year using the modified SCS curve number method for variable source runoff together with the topographic index for shallow soils (Steenhuis et al., 1995, Lyon et al. 2004). These results approximate to what SMDR predicts and can be obtained in a fraction of the time. However, topographic index based methods rely usually on the hypotheses of steady-state and continuity of the saturated zone over the hillslope, two restrictions SMDR is not submitted to.

The soil samples were taken at a relatively shallow depth (2-6 cm), and compared to average water contents over the whole upper soil zone. As noted by Western, this set up may introduce to some systematic errors. However, it should be that the high stone and rock contents of the soils in the watershed makes it difficult to take samples at a deeper depth. Moreover, the samples were taken in periods for which we did not expect a significant gradient of moisture content with depth. At last, we were more interested in the relative differences of water contents along the transect than in the actual content themselves. Nevertheless, the moisture content for the top soil could systematically underpredict the average moisture content over the whole soil column. These comments have been introduced in the paper.

The errors in observed saturation degrees reported in the paper were estimated by propagation of errors, taking into account potential errors on the water loss, the mass of dry materials, the core volume, the mass of stones and gravels, the average density of stones and gravels. Combining rules-of-thumbs estimates for these different errors leaded to estimates of relative errors (ρ) between 20% and 30%. These estimates correspond in fact to the highest relative errors that can be expected, the actual relative errors being of course unknown. The error margins drawn in grey in Figures 6 around each estimate $\hat{\Theta}$ of the saturation degree and each 3-point moving average estimate Θ_{MA} were defined as the interval $[\hat{\Theta} + \sigma_{low}; \hat{\Theta} + \sigma_{high}]$ where σ_{low} and σ_{high} were defined as:

$$\sigma_{low} = min(\hat{\Theta}, \Theta_{MA})(1 - \rho)$$

$$\sigma_{high} = max(\hat{\Theta}, \Theta_{MA})(1 + \rho)$$

Here again, these error margins were defined to be fairly large, so it is not surprising that they cover a wide range of data. Because of the difficulty in sampling, we were unfortunately unable to take replicates of the samples. Therefore, we were not able to apply more conventional methods for error estimations.

We did not include canopy interception, because we were simply not sure if it would improve the accuracy of our predictions. First, the amounts for canopy interception vary widely (Dunne and Leopold, 1978)and the fate of the water in the canopy is the same as the water that falls on the soil. It both gets lost by evaporation. For large storms that produce runoff the amount intercepted by the canopy is likely a small portions and it could be insignificant compared with the inaccuracies of measuring the rainfall or snowfall.

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At last, following Western's suggestion, the hydrographs presented Figure 5 were trimmed to years 1997, 1998 and 2000, 2001. The figure legends were also improved to come closer to self-containment.

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