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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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We would like to thank Chirico (2005) for his comments and the opportunity to clarify some of the misunderstandings concerning our paper. In his comments Chirico noted that the model has been described before. This is correct. The aim of this manuscript was then also to check if the model produced correctly spatial trends in soil moisture distribution. Due to a lack of distributed data, this check was not done before and as noted by Western (2005) this is *"essential if a model's spatial predictions are to be utilized"*. Compared to the previous versions (globally called SMR at the time), we made a few changes in the model's implementation as well consisting of (in the slightly changed words of the referee) *"the soil profile is simplified to a single soil layer"* and *"deep ground water storage reservoir is omitted"*. Other refinements included improvements in the evaporation routines using the basal evapotranspiration coefficient K_c on a daily basis instead of a monthly basis; using the Priestley-Taylor model to compute reference evapotranspiration instead of the Hammond method), and employing an improved incorporation of preferential flow component by the depth-dependent lateral flow multipliers κ and the drainage limit θ_m.

In his comment Chirico claims correctly that *"the spatial pattern of lateral flow direction and distribution (in SMDR) is stationary and do not depend on the actual hydraulic gradients (but only on the slope of the impermeable layer)... This modeling approach has been employed in many other distributed models..."*. We agree and recognize with the refer that the simplification we made us of setting the hydraulic gradient equal to the slope is quite standard. Brutsaert (2005) mentioned that Boussinesq used this simplification in his book "Essai sur la théorie des eaux courantes" that was published in 1877. It is also the basic simplification in the kinematic wave approach (Woolhiser and Liggett, 1967; Beven, 1981) and TOPMODEL (Beven et al., 1984). Its ubiquitous use in terrains with slopes of more than 2-3% is a good indication of the satisfactory results that can be obtained.

As noted by the Chirico, only the base flow parameter was calibrated. All other input data were obtained directly from the SSURGO data base (directly derived from the soil survey) and weather records. The base flow parameter is related to the percolation and this is the only parameter for which we could not obtain the data from existing sources. We used the base flow during the summer of 1997 to find values for this parameter. The referee states then that our model validation *"cannot be considered successful, since during the entire validation period the model performance is significantly worse than in the calibration period"*. We have to partially disagree with the referee. First, it is hard to judge whether performance is "significantly worse" by only comparing some arbitrary efficiency criteria. Second, only one parameter is calibrated and even with this minimally calibration, the fit was correct, especially in light of other similar efforts, such as for example the early work by Grayson et al (1992). At last, we have to stress again that the efficiency of a distributed model cannot be assessed by merely comparing its integrated outputs with observations.

The referee suggests that *"this (poor fit) is due to a wrong model conceptualization"*. We do not think that the cause of a potential conceptualization error can be determined only from the integrated output. In fact, as we already stated in the paper, models with quite different conceptualizations can yield similar integrate results. A first example is the comparison between the minimally calibrated SMDR model (SMR at that time) with the calibration-intensive EPA's HSPF model (Johnson et al., 2003). The two models, which differ greatly in the conceptualization of flow processes, predicted the stream discharge approximately with the same accuracy. Based on these results, we could not discriminate between the flow processes deployed in SMR and HSPF. Another telling example is that models ranging from simple to complex and from saturated flow to unsaturated flow fitted all the discharge data of the Coweeta plot (Hewlett, and Hibbert,1963) equally well (Steenhuis et al., 1999).

Chiri suspects that *"given the network of ditches, drains and streams, ... there is a (permanent or seasonal) water table that is drained by this network and contribute to this base flow"*. We do not have any evidence of the presence of a permanent ground water table in the upper reaches of the watersheds consisting of a glacial till or bedrock with conductivities in the order of 1-10 mm/day. If an aquifer was present, the water from the glacial till area would infiltrate in the more permeable soil and fill up the ground water reservoir during most summers. The stream would then dry up during these periods, which is not the case. However, we agree that a perched seasonal water table in the lowest ares of the watershed can be present, and SMDR models it indeed. We also investigated more complex representations of a groundwater reservoir. These investigations proved to be inconclusive, and the inflation in the number of parameters to be estimated casted doubts on the validity of such an approach. It was therefore decided to abandon the reservoir concept, and to consider only the simple hypothesis presented in the paper.

In the same paragraph of the specific comments mentioned above, Chirico notes that *"it* *is striking to see that a fraction of deep percolation is routed directly to the outlet without any delay. In this way, especially during periods, the water reaches the outlet more rapidly by deep percolation than by lateral subsurface flow"*. This is not completely exact. In SMDR, lateral flows are computed before percolation. At each time step, most of the water that can be drained from one cell is routed to its downslope neighbors, and only a small fraction of the remaining soil water percolates through the impervious layer. During dry periods, percolation occurs only on cells that have enough water, *i.e.*, at the bottom of the slopes.

We admit that we do not understand the fate of the water that percolates through the subsurface precisely. So what can be the fate of the fraction of deep percolation ? One of the clues might be that there are springs throughout the watershed such as on the south west watershed boundary (Figure 7). The SMDR model was unable to simulate this wet area because it did not have any surface contributing area. The water in the spring might very well originate from soil water percolating through the fractured bedrock (that had a significant higher percolation rate than the glacial till) flowing along a highly conductive fissure network. Where else could this water be coming from ? Thus the notion that the deep percolation might reach the stream network before the lateral interflow could be true. Having said this, we wish that there were better routines and input data to simulate the distributed base flow and springs. We currently do not have enough data.

Finally, should we model the ditches drains and streams as suggested in the same paragraph ? May be we should have done so. It is actually pretty easy to do and was included in the implementation of SMR of Frankenberger et al (1999). However the real question is what the right complexity of a model (Grayson et al, 1994).

There are a number of other technical issues raised by Chirico which we address below. We appreciate the mentioning of our misprints. In all case they are corrected. .

Comparing peak timings and intensities of hydrographs on a daily basis is actually not as superfluous as the reviewer suspects, as the large peaks are caused by storms or snow melts with amounts of 10 cm or more over several days. First, such a comparison gives clues to assess the quality of input data. For example, the model was occasionally not able to reproduce some summer peak flows. A close inspection showed a discrepancy in the rainfall input data, that did not take into account some intense localized thunderstorms. In addition, this comparison permits to identify potential problems in the model. Thus, the imperfect simulation of winter peakflows hints that the temperature-index method the snowmelt algorithm is currently based on gave only crude estimates.

Many concerns concerning Figures 7 and 8 are addressed in the response to the comments on Western (2005) and will not be repeated here. In addition we note that frequently saturated areas ('FSAs') were estimated from farmer interviews and visual observations on the field. This data was not obtained in quantifiable form, .i.e., no measurement was actually taken to determine the actual extent of the FSAs, or their frequency of saturation. Stream location was not specifically taken into account in the model. Still, the actual streams (N-S permanent stream from the pond, W-E seasonal stream in the southern part of the watershed, N-S and W-E seasonal streams to the ponds), and the FSAs around those streams are well reproduced by the model, as areas generating a significant amount of runoff. However, the locations of FSAs presented in Fig. 7 is only approximate, which prevents any pixel to pixel comparison with the simulated maps. It is not to say that this type of subjective information should be discarded. It is what the farmer perceives his hydrology to be. To further study experimentally FSA's or VSA's, we have installed 30 piezometers in a different part of the Catskills to study their behavior. The results are given in Lyon et al (2005) and are being analyzed further.

The reviewer states that *"the model assumes that overland flow is produced by saturation excess only, not just "most" of it"* . In SMDR, rainfall and snowmelt occurring on impervious areas are directly routed to the outlet at the end of the same time-step. This mechanism corresponds in practice to infiltration-excess overland-flow. About 2%

of the study watershed consisted in impervious areas. As shown in our paper, the contribution of these areas to streamflow is not negligible during summer. Therefore, only "most" of the overland flow is produced by saturation excess.

As pointed by the reviewer, a more correct statement would have been *"saturation excess (SE) overland flows occurs when water cannot infiltrate because the soil is saturated"*. In its most widely accepted sense, the expression *"SE overland flow"* designates not only the case when precipitation falls on an already saturated soil (saturation-excess from above), but also the case when lateral flows converge on a poorly drained soil (saturation-excess from below). By opposition, the expression *'infiltration-excess'* is usually restricted to the case where the soil maximum rate of infiltration is exceeded. However, it should be noted that there is yet no "official" definition of the terns *'infiltration-excess'* and *'saturation-excess'*. The reviewer suggestions of using boundary conditions for the definition of these two expressions are valid and pregnant, and they have the advantage of relying on an objective criterion. We hope that these definitions will be more widely used in the near future.

The referee remarks that there are TOPMODEL versions that consider the possibility that the soil column has a 64257;nite depth. Indeed, Walter et al. (2002) presented recently STOPMODEL, an application of TOPMODEL to shallow soils. Both models rely on some particular moisture distribution function, and on the assumption that the watershed is at quasi steady-state. This latter does not only imply "an infinite celerity of subsurface lateral flows", as mentioned by the reviewer, but also the continuity of the saturated zone over the hillslope for TOPMODEL. As noted by the reviewer, SMDR does not rely on these two hypotheses. Instead, lateral flows are distributed on a cell-by-cell basis.

The refer views as inappropriate to consider the potential evapotranspiration as part of the weather data set. We define weather data as the set of inputs that are based on climate information, so that it includes potential evaporation.

We reckon that the expression “lateral out64258;ow are calculated with a simpli64257;ed Darcy’s law” is not precise enough. In fact, the volume of water transferred by lateral flows by a soil column of depth Z and width w during a time step Δt is computed as:

$$Q = \int_{t}^{t+\Delta t} \int_{0}^{W} \int_{0}^{Z} q(z, w, t) dz dw dt \tag{1}$$

where z and w are units of depth and width respectively. The water flux q is defined according to Darcy’s law as:

$$q = -K(\theta)\nabla h$$

where $K(\theta)$ is the hydraulic conductivity and h the hydrodynamic potential. Introducing this equation in (1), assuming that the hydrodynamic gradient is independent of depth z, width w and time t, and that the conductivity $K(\theta)$ does not vary significantly with width w nor with time t during one time step, it comes:

$$Q = \left(\int_{0}^{Z} K(\theta) dz \right) \nabla h W \Delta t \tag{2}$$

Noting $\overline{K(\theta)}$ the average hydraulic conductivity over depth Z, eq. (3) becomes:

$$Q = \overline{K(\theta)} \nabla h \; W \; \Delta t \tag{3}$$

Equation (4) is strictly equivalent to eq.(4) of the paper. The paper has been slightly rephrased accordingly.

In summary one might ask what is gained from this long discussion on the SMDR model applicability to the wet humid glaciated landscapes. One thing is clear, conceptualization of models bring out strong feelings. The comment by Chirico is a good examples of that. Can it be that these argument can be brought back to the simple fact that major landscapes in the world are unique. What is true and certain for one hydrologist in one landscape is clearly different than the truth of another hydrologist in

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another landscape. One promising modeling approach to combat these unproductive exchanges is the downward modeling approach such as advocated by Chirico himself (Chirico et al 2003).

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