Hydrol. Earth Syst. Sci. Discuss., 9, C4495–C4501, 2012
www.hydrol-earth-syst-sci-discuss.net/9/C4495/2012/

Hydrology and Earth System Sciences Discussions

Interactive comment on "An efficient semi-distributed hillslope erosion model for the sub humid Ethiopian Highlands" *by* S. A. Tilahun et al.

S. A. Tilahun et al.

tss1@cornell.edu

Received and published: 1 October 2012

General response

The differences in opinion between the first two reviewers reflect the disparity in opinions in the modeling community on the direction of model development and use in the future. Reviewer 1 comments that our approach is the result of model calibration and not based on actual physical basin structure and therefore will not lead to future improvements. In our rewritten introduction, we have clarified that our model goes beyond the models that have been used in Ethiopian highlands thus far. In addition,

the proposed model is directly in line with Beven's philosophy (Beven 2001, 2007) that models can be used for learning more about "a place" (the semi humid and humid Ethiopian highlands here). In his view, when models are applied to new places, we can learn about the shortcomings of the model and in some cases we are able to reject previously used model structures. As described in the manuscript in HESSD (page 2125, lines 15-20) and rewritten in a revised manuscript (cited below), the early modeling attempts in the Ethiopian highlands to simulate water fluxes and erosion made use of models developed in the United States for a temperate climate. Consequently, modeling results were poor (only monthly values could be simulated), nevertheless, accepted because no better predictions were available at the time. Recent experimental results have unearthed the reasons for these early poor predictions and showed that the runoff is dominated by saturation excess while the early models assumed infiltration excess. For saturation excess, topography and soil thickness are the two main factors affecting the location of runoff producing areas while the early models assumed that runoff location were mainly dependent on crops and soil type and independent of the topological location in the landscape. Consequently, Easton et al. (2008) and White et al. (2008) found by adapting SWAT to include topography in the hydrological response unit (HRU) selection, water fluxes and erosion in watersheds of various sizes in the Blue Nile basin (after calibration) were simulated reasonably well on a daily time basis compared to a monthly time step of previously used models.

Steenhuis et al. (2009) and Tesemma et al. (2010) showed that saturated excess runoff and sub-surface flow from both interflow and baseflow could simulate the discharge equally or better than the more complex SWAT model. In this manuscript we use the output of this simple water balance model and add two parameters to simulate sediment concentration in a small and in a large basin with essentially the same model structure. As indicated by Rose (Reviewer 2) this is an original approach. In this paper there are no claims made that this is the best model or that we reached the optimal parameters. We, however, show that saturation excess concept is valid in this part of the world and performs better than the traditional infiltration excess models. The publication, therefore, should be seen as part of a continuous learning process on how to simulate sediment concentration across scales under monsoonal conditions in terrains that range from undulating to mountainous. At the same time, it will put the bar higher for the acceptable performance of more complex models. By publishing this paper, it also provides an opportunity for others to try the new concepts in other watersheds and refine them or perhaps, in some cases, reject them (as might be very well the case for the semi-arid regions in the Ethiopian highlands where the major runoff mechanism could be infiltration excess). It is for this reason that we have specified in the title the region in which the model was applied (i.e., the semi humid or humid Ethiopian highlands with over 900 mm/year on the average). We are readily willing to admit with respect to the comments of Reviewer 1 that by introducing more parameters we can simulate likely more places with a higher accuracy and represent soil and water practices in more detail. However, this should not be a reason to reject a paper that tries to simulate with a minimum set of parameters the observed hydrology and erosion processes.

Although we were mostly interested in presenting a new model structure, we need to address the issue of equifinality as mentioned several times by Reviewer 1. Equifinality suggests that there might be different sets of parameters values that give an equally good fit (or as expressed by reviewer 1 “I expect that there are many calibration parameters giving the same results”. Indeed with nine parameters that could be a real problem in the hydrology model. We, therefore, carried out a sensitivity analysis, and indeed, we found that the “maximum available water content” for plants was not very sensitive and could be changed significantly with similar results. However, we also found that the size of the three fractional areas was extremely sensitive. As documented in the supplementary material, increasing the saturated area by 15% resulted in a decrease of the Nash Sutcliffe efficiency from 0.88 to 0.46 or decrease in the degraded area by 50% resulted in a lowering of the Nash Sutcliffe efficiency to 0.07. This is not to say that we can interchange the size of the runoff source areas with a small decrease in Nash Sutcliffe values as long as the sum of the two stays constant. We

were already aware of the sensitivity to the fractional area when we used this hydrology model in a paper by Tesemma et al. (2010) where we simulate the difference in Blue Nile hydrographs during two different time periods 20 years apart at the Sudanese Ethiopian border by increasing the degraded area in the landscape. In addition another indication about the importance of the magnitude of the fractional areas is that by testing the model for watersheds in different parts of the world, the relative contribution of water by the three fractional areas was in accordance with the observed watershed characteristics (publications are in preparation). Thus there is evidence that the size of the fractional areas obtained by calibration are unique for Ethiopian and other landscapes where topography plays an important role in determining the hydrograph.

Equifinality for the sediment problem is less of a problem because physically based considerations lead to the concept that the process of sediment transport is almost fully controlled by the mechanism of runoff generation and could therefore be greatly simplified. We introduced only four parameters, of which we argued that, based on field evidence, two were zero, leaving us only two parameters for calibration. Consequently, for the sediment model equifinality was less of a problem. We have added the sensitivity analysis for the “a” parameters in the supplementary material and in the case of Anjeni watershed there is no sensitivity to the a1 parameter due to the small (2%) saturated area in the watershed. However, the model fit is very sensitive to the size (14%) of the degraded area.

Reviewer 1 would like us to assign values to the parameters a priori. Because the model uses parameters related to the patterns of self-organization, it is not clear a priori what the parameters represent in the landscape. The subsurface flow parameters in any modeling approach are based on the analysis of the hydrograph. We did this too. The values of the maximum storage capacity are averages over an area and not very sensitive and any value can be made to work within reason. The exact definition the three fractional areas are not known. It is interesting though that once the fractional area values were optimized, we found that they were in agreement with what generally is observed in the field. We write on page 2135 line 19-21 in the discussion manuscript: "In the Anjeni watershed, the small proportion of saturated area is consistent with the piezometer readings of Leggesse (2009) that showed a deep water table throughout the uniformly steep watershed except in very close proximity to the stream." Thus the small fractional saturated area for the saturated zone is in direct agreement with the observations as shown in the photo that is included with this response to reviewer 2. This photo shows that grass is planted in the area that is unfit for crop production because of the high ground water table. In addition, we have looked back in the thesis of Legesse (2009) and found that that the very shallow Regosols and Leptosols are covering 12% of the hillsides. These two soils represent the degraded area where the soils saturate above the hardpan. In our model we use a percentage of 14% for the degraded area (Table 2) closely resembling this information. The thesis of Legesse is found at: http://soilandwater.bee.cornell.edu/papers.htm. Finally we wrote on page 2137 line 21-26; "In Anjeni, these (degraded shallow) areas are located on the fields in which the farmers have traditional small drainage (or cultural) ditches on shallow and slowly permeable soils (Leggesse, 2009) while in the Blue Nile Basin, the degraded areas are located at Mount Choke in East and West Gojam where Anjeni is located, Lake Tana sub basin, Jema sub basin in Wolo and Abay Gorge in East Wollega (Hydrosult Inc. et al., 2006)."

Reviewer 1 commented that on the hillslope or catchment scale, sediment loss is an integrative response of various processes such as detachment, transport and deposition. Therefore, he/she suggests that in complex erosion models these individual sub processes are parameterized using measurements gained in small scale experiments. And he/she does not see another possibility to parameterize these isolated processes. We agree with the reviewer that the small scale processes are important but it is impossible to find the input data for these processes. Therefore, we assume that there is a direct relationship between the velocity in the stream and the sediment concentration as was done in the Hairsine and Rose model among others. (We tried also other assumption but this was the one that fitted best). Because over larger areas the

runoff peaks smooth out and the average daily discharge decreases, the velocities become lower and hence the sediment concentration decrease and sediment will deposit. So we do consider deposition but it is implicit in our approach. Clearly more research needs to be done, but it is interesting that the model gives such good results. As noted, our results are much better than the detailed WEPP model that was tried for the same watershed by Zeleke and co-authors.

Reviewer 1 noted correctly that when the relation between sediment concentration and runoff depends on one calibration parameter, it will not allow for modeling the effect of different management practices (see page 2138, line 26-29). We agree with reviewer 1 that with one (or two) parameter(s) the effect of soil and water conservation practices can be simulated only to a limited degree. (This was not our objective either). However, the model would indicate that moving the SWCP from areas where water infiltrates to those that produce runoff concentration would decrease the concentration in the runoff. In the published discussion paper our intent was to find a better sediment models. It was not our intent to indicate that our model could evaluate the effectiveness of the different management practices. However our model can lead to better models when more parameters are introduced that are sensitive to soil and water conservation practices. We rewrote, therefore, most of the introduction in the revised paper as suggested by reviewer 1. In the supplementary material after the response to the specific comments, the revised introduction organized according the reviewer is duplicated. Shortening was difficult because much of the misunderstanding about our concepts originated from a lack of information in the introduction.

References

Beven K.: How far can we go in distributed hydrological modelling? Hydrology and Earth System Sciences, 5(1), 1-12, 2001.

Beven K: Towards integrated environmental models everywhere: Uncertainty, data and modeling as a learning process. Hydrol, Earth Syst. Sci. 11: 460-467, 2007

Easton, Z. M., Fuka, D. R., Walter, M. T., Cowan, D. M., Schneiderman, E. M., and Steenhuis, T. S.: Re-conceptualizing the soil and water assessment tool (SWAT) model to predict runoff from variable source areas, J. Hydrol., 348(3–4), 279–291, 2008.

Legesse E.S.: Modeling Rainfall-Runoff relationships for the Anjeni watershed in the Blue Nile Basin M.P.S Thesis. Cornell University, Ithaca, NY, USA, 2009.

Steenhuis, T. S., Collick, A. S., Easton, Z. M., Leggesse, E. S., Bayabil, H. K., White, E. D., Awulachew, S. B., Adgo, E., and Ahmed, A. A.: Predicting discharge and erosion for the Abay (Blue Nile) with a simple model, Hydrol. Process., 23, 3728–3737, 2009

Tesemma, Z. K., Mohamed, Y. A., and Steenhuis, T. S.: Trends in rainfall and runoff in the Blue Nile Basin: 1964–2003, Hydrol. Process., 24(25), 3747–3758, doi:10.1002/hyp.7893, 2010

White, E. D., Easton, Z. M., Fuka, D. R., Collick, A. S., Adgo, E., McCartney, M., Awulachew, S. B., Selassie, Y., and Steenhuis, T. S.: Development and application of a physically based landscape water balance in the SWAT model, Hydrol. Process., 23, 3728–3737,doi:10.1002/hyp.7876, 2010

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/9/C4495/2012/hessd-9-C4495-2012-supplement.pdf

---

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 2121, 2012.