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Hydrology and Earth System Sciences Discussions

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

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We are very appreciative of the comments and the time of referee 2. Below we have quoted the comments of the reviewer followed by our response. In addition we noted in red in the manuscript text the additions made. For clarity we did not mark the original text that was removed.

Comment:

The paper presents an empirical model to assess daily sediment concentrations at the outlet of a small catchment (Anjeni, 113 ha) in Ethiopian highlands. The model assumes saturated overland flow generation and predicts erosion from a fraction of the catchment that is defined as degraded land + saturated floodplains. Although the model addresses the need of improving runoff predictions to predict erosion, and struggles to keep modeling approach simple and with limited parameters, I fail to see the practical utility of the model proposed. Particularly: 1) the model cannot be used to identify where the sediment is coming from. The fraction of degraded area and saturated area are calibrated parameters of the hydrological model and are not identified in the space. The model utility would improve if at least these 3 areas were identified from the catchment geography rather than calibrated.

Response:

We are slightly disappointed by the reviewer's assessment of our efforts. First this is the first model that predicts daily sediment concentrations in Ethiopia for a watershed in Ethiopia with good NS Efficiencies. Other models such as by Setegn et al (2010): [Modeling of Sediment Yield From Anjeni-Gauged Watershed, Ethiopia Using SWAT Model JOURNAL OF THE AMERICAN WATER RESOURCES ASSOCIATION 46: 514-526] have slightly better NSE values but these are for monthly values and for sediment yield and not concentrations. Monthly values have in all cases better NS efficiencies than for daily values. Our model can be run in a spreadsheet and therefore is simple compared to models that uses thousands lines of code such as SWAT and many more opportunities to fine tune the results.

Our theory is an improvement over previous efforts and is therefore publishable. We agree that improvements can be made but that can be done subsequently. By publishing it now it makes this approach available to others for testing.

The reviewer mentions that our model is empirical. This is only partly true for the following reasons: 1) We satisfy the conservation of mass 2) Routing can be ignored because we use a daily time step 3) Saturation excess is modeled physically correct because it is the total amount of rainfall (and not the intensity) that determines the

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amount of runoff 4) The interflow component is based on Darcy law with the assumption that it is the hydraulic gradient is equal to the slope of the land (See Steenhuis et al. 2008) 5) Many models use a linear reservoir to model baseflow. More complicated approaches are hindered by insufficient information about the subsoil.

The reviewer is slightly confused about the capabilities of the model and what features are validated. The model clearly identifies the types of areas that cause erosion. In the Anjeni watershed it is the degraded areas that cause most of the runoff. We also identified the types of soils where it is occurring. On these soils the farmers have installed the small ditches that are used to carry off the runoff water while in other parts of the watersheds these ditches are not found. What we did not do is validate that this is indeed the case. This is for a following paper and is beyond the scope of the current paper because such an effort requires a significant amount of funding and time commitment.

The reviewer wants us to define the areas from the catchment geography. In Ethiopia where even for the well studied Anjeni watershed there is very little information about the subsurface flow. It was Elias Sime Leggesse, a graduate student in the Master's program at Bahir Dar University that in 2008 for the first time installed piezometers in the watershed and found that the areas where the water table intersected the surface was small and our model that uses data from the hydrograph predicted that correctly. Our calibrated model predicted the degraded soils areas correctly as well.

We agree with the reviewer that it would be ideal to obtain all the input date a priori from a map but we disagree with the reviewer that is possible from geography alone to determine what fraction of the surface watershed contributes subsurface flow to the gage. There is simply not sufficient information in these maps. The hydrograph contains this information and we use it, therefore, to parameterize the model and then check if the parameters that have been measured are in reasonable agreement with that in the landscape.

Finally if the reviewer's criteria would be enforced for publishing an article, very few modeling paper would be published since almost all models are calibrated on the outflow hydrograph and seldom on the spatial distribution of the runoff and sediment load predictions.

Comment:

2) the term 'degraded land' is generic and is never defined in the paper. the authors mention gully erosion in part of the catchment, but do not state if their model is built to assess gully erosion. Instead, they refers to models that work on hillslope (sheet/rill) erosion, therefore I assume they worked on the hypothesis that dominant erosion process was hillslope erosion. This needs clarification.

Response:

We keep the term degraded land generic as those land that generate surface runoff. We added the following definition in the text:

"Degraded lands are defined here as those lands that are shallow and store only small amounts of the rainwater and therefore produce runoff and can support very little vegetation."

Please refer the response given for comment 2 of Reviewer 1 as additional answer regarding the gully in the watershed

Comment:

3) the model has not predicting capability. It cannot be used to assess a likely impact of catchment management on water quality.

Response:

The assessment of the reviewer is incorrect. We have used the hydrology model to assess the effect of land degradation in the Blue Nile basin (Tesemma et al. 2010). This demonstrates clearly that the model can access the catchment management on runoff and sediment concentration.

Comment:

The organization of the paper should be improved. Introduction: in presenting then different models that were trialled in Ethiopian highlands, focus discussion on runoff predictions only, given that the hypothesis of the study was to improve runoff prediction by using a saturation excess approach instead of infiltration excess.

Response:

We have presented the models that have been tried in Ethiopia. It is cited below

“Erosion models applied in the Ethiopian Highlands range from the empirical relationÂňships (Universal Soil Loss Equation – USLE), to physical based models. Hurni (1985) adapted the empirical USLE for Ethiopian conditions. Eweg et al. (1998) and Zegeye et al. (2011) showed that the modified USLE can be used to estimate average annual soil losses but question the reliability of predicting the spatial distribution of erosion and temporal distribution shorter than a year. From the physical models available that predict sediment load, only the Agricultural Non-Point Source Pollution (AGNPS) model (Haregeweyn and Yohannes, 2003; MoÂňhammed et al., 2004), the Soil and Water Assessment Tool (SWAT) (Setegn et al., 2008, 2010), the modified SWAT-WB Water Balance model (Easton et al., 2010) and Water Erosion Prediction Project (WEPP) (Zeleke, 2000) are tested for the Ethiopian HighÂňlands. Except for SWAT-WB, these models are applied with the assumption that infiltraÂňtion excess runoff mechanism governs the runoff process in all areas. The application of AGNPS in Kori watershed (Haregeweyn and Yohannes, 2003) was for limited storm events and predicted the runoff and sediment with some success even though peak runoffs were not predicted well. The application of the AGNPS model in Awgucho catchment (Mohammed et al., 2004)was relatively poor for runoff production and application of WEPP in Anjeni slightly over predicts the soil plot loss for storms with low intensities, but overall Nash Sutcife were satisfactory(Zeleke, 2000).

Other sediment models that have not been applied in the Ethiopian Highlands are Areal Nonpoint Source watershed Response Simulation (ANSWERS) (Beasley et al., 1980), European Soil Erosion Model (EUROSEM) (Morgen et al., 1998), Physical Water Erosion Model (Hairsine and Rose, 1992a, b) and GUEST (Yu et al., 1997). Besides shear stress (Yalin, 1963), these models use a stream power function for predicting sediment carrying capacity (Rose, 2001) where the sediment concentration at the transport limit is related to runoff depth as a power function (Ciesiolka et al., 1995; Yu et al., 1997). Limited testing of these models has been done for monsoonal cliÂňmates. The Hairsine and Rose model (1992a,b) that resulted in linear relationship between sediment concentration and velocity of runoff predicted sediment concentraÂňtions successfully in the monsoon climate of the Philippines, Thailand and Malaysia using observed stream flows (Rose, 2001). In the foot hills of Nepal WEPP predicted soil erosion from USLE type plots the best followed by the GUEST Technology and EUSROSIM (Kandel et al., 2001.

The two models applied in Ethiopia using the SCS curve number approach to predict surface runoff (AGNPS, non modified SWAT) simulated daily stream discharge less than satisfactory. Implicitly, the SCS curve number assumes that plant and soil related factors determine amount of runoff while hydrology is topographically driven in the Ethiopian Highlands (Lui et al., 2008; Bayabil et al., 2010; Engda et al., 2011). Therefore, to improve the erosion predictions requires a runoff model that includes the proper hydrology.

Recently Steenhuis et al. (2009), White et al. (2009) and Easton et al. (2010) have developed distributed models that take the terrain topographic features into account that are suitable for monsoonal climates and can predict the runoff in the watershed based on a daily basis. The model of Steenhuis et al. (2009) is relatively simple and diÂňvides the watershed up into three distinct areas consisting of the periodically saturated bottom lands, severely degraded areas with very shallow soils over an impermeable layer and hillsides. The saturated areas and the degraded areas produce surface runoff and sediment and the hillside sediment free interflow and base flow to the river. Ten-day averaged discharge and sediment concentrations were well predicted for the Blue Nile at the border with Sudan. White et al. (2009) modified the SWAT model (SWATÂň-WB) by redefining the HRU's based on topography and soil depth and surface runoff was predicted as any excess rain after the soil became saturated. SWAT-WB simulated available daily sediment yield data in the Blue Nile Basin at several scales well (Easton et al., 2010). Input data requirements, however, for SWAT and SWAT-WB is cumberÂňsome especially in areas with limited data sources such as in Ethiopia."

We are not sure why the reviewer suggests that we should limit ourselves to the hydrology only. Adding sediment concentration prediction with two parameters and using the hydrological flow parameters is a "bonus"

Comment:

Material and Methods: need reorganization. I suggest: 1) model description. in the methodology the discussion on baseflow is excessive. It is quite common in fact to cut base flow and consider only event flow (techniques differ on how to assess baseflow); for example AGNPs is used only on event flow, not all the flow. I think this part should be reduced. 2) Study area. 3) Calibration and validation dataset.

Response:

Material and Methods are modified according to the comment but we kept data section within the material and method. In addition, we kept the discussion on base flow. This is because of the relevance of the discussion in the sediment model formulation. Our point isn't about how to separate base flow from the total flow. Instead, we argued and showed that daily sediment concentration determination from event based measurement is affected by base flow and interflow after sometime in July. The start of contributing significant subsurface flows of the hillside portion of the watershed during this time dilutes the sediment in the stream and resulted in a lower sediment concentration after July. Considering this concept in our model resulted in a better simulation.

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Comment:

There is considerable confusion in the presentation of the dataset used for the calibration and validation, given that some years were discarded becuase of incomplete or missing data. probably a table will help presentation and readability of the paper. also, present here data relative to Fanya Juu implementation in the catchment, as this comes at different times in the paper.

Response:

Table is incorporated as per the comment. In the document, it is labeled as Table 1 and can be found in the mauscript

Comment:

Results.I was not convinced of the good model predictions: what are the implications of overpredictions of flow at flow > 20 mm/day and underpredictions of flow < 20 mm/d, (page 2219, 17-21)?

Response:

In the text we did indicated that

"Despite the good statistics, the model overpredicted low flows and under predicted flows of greater than 20 mm day-1 during the calibration peÂňriod (Fig. 4a and 5a). During validation (Fig. 4b and 5b), there is a reasonable agreement between observed and predicted for low flows, even though there is under prediction for flows than 20mmday-1. The under prediction of peak flows is likely caused by an expansion of runoff producing areas in which the model fixes the fraction of these areas"

Thus we agree with the reviewer that indeed we have a problem but despite this the overall model fitted really well. As stated above we would need more fitting papameters to include this. We decided against it.

Comment:

Results. page 2220, 9: that there are the two surface runoff areas in the catchment is an assumption not a result of the model.

Response

The reviewer is correct. The text is as follows:

“The model calibration suggests (Table 2) that 14% of the Anjeni watershed area conÂňsists of degraded area with shallow soil or exposed hardpan, which requires only a little rain to generate direct runoff (i.e., Smax = 10 mm) and approximately 2% of the saturated bottom lands in the watershed needed 70 mm of effective precipitation to generate runoff (i.e., Smax = 70 mm).”

Comment:

page 2221 line 10: the incorportation..[].. helps to capture the higher sediment concentration before July". this is not correct: in the model sediment concentrations from the two eroding areas are constant (a1 and a2), and do not change in the model. do you mean change in the sediment load perhaps?

Response:

In the sediment model, we first computed the daily sediment load from runoff source areas and then divided by the total stream flow during that day to compute the daily sediment concentration using Eq.(7). The watershed responds to interflow and to higher baseflow after July making the total flow (the denominator in Eq(7)) higher relative to the time before July. The lower sediment concentration after July is then assumed because of these higher interflows and base flows that dilute the sediment. This assumption is incorporated in the model. To make this clear in the document, the following sentence is included in section 3.

“In the sediment model, daily sediment load was first computed and then divided by the total daily stream flow using Eq.(7) to compute the daily sediment concentration. In the equation, there are two calibration parameters consisting of the constants for each

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two runoff source areas a1 and a2."

Comment:

Conclusions: page 2222, line 6-7: the statement "Using these models it was possible to define the runoff sources areas" is not correct: you identified a fraction of land that generates erosion, but did not locate it in the catchment.

Response:

The sentence is modified as follows:"Using these models, it was possible to identify the proportion of runoff sources areas which are also sources of sediment.

Comment:

Table 1: take out the first column (Anjeni); re-organize in three columns: column 1= input parameter name; column 2 = units of measure, column 3 = calibrated value

Response:

Table 1 is modified as per the comment. Refer Table 2 in the manuscript.

Comment:

Table 2: Specify units of measure for hydrology (better "daily flow"?) and sediment concentrations; consider reverting columns/rows.

Response:

Table 2 is modified as per the comment. Refer Table 3.

Comment:

Figure 1: enlarge study area instead of Ethiopia map, revise legend of DEM

Response:

Figure 1 is corrected as per the comment See response to reviewer 1.

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Comment:

figure 2: I only find reference to Fig 2 at page 2219 when talking about the deep water table ner the stream. Figure 2 shows a hillslope with terraces and I cannot see how it is related to the text? Revise the grammar and English. one example over all: Fanya Juu (no reference given in the paper, I suggest: Thomas DB, Biamah EK. 1991. Origin, application and design of fanya juu terrace. In Development of Conservation Farming on Hillslopes, Moldenhauer WC, Hudson NW, Sheng TC, Lee SW (eds). Ankeny IA, Soil and Water Conservation Society: Ankeny; 185–194.) is written in a number of different ways, e.g. fanny juu at page 2217, maybe not that funny?

Response:

The reference of Thomas et al., 1991 is included in the document and the spelling for the Fanya Juu is also corrected.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/8/C1786/2011/hessd-8-C1786-2011-supplement.pdf

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