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Hydrology and Earth System Sciences Discussions

Interactive comment on "A multi basin SWAT model analysis of runoff and sedimentation in the Blue Nile, Ethiopia" *by* Z. M. Easton et al.

Z. M. Easton et al.

zme2@cornell.edu

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Reviewer 1 General Comments This paper attempts to provide an informative and interesting narrative on the potentials of topography- and water balance-based hydrologic modeling on the Blue Nile Basin. Conceptually, the effort is plausible as the conjunctive use of water balance based hydrology and topographic characteristics may potentially provide a sound tool for the betterment of hydrology representations for the BNB settings. As such, the effort in this manuscript appears a useful contribution to the literature, but major work is needed in many areas of the paper, such as, in properly framing the scope of the study; in clearly presenting specific objectives; identifying the focus of the research and providing details on the most important aspects of research; careful

proof reading to avoid non coherent statements and redundant sentences; improving the logical flow between paragraphs; technical errors related to missing references, misspelling of references, wrong table/Fig. reference, potential data./unit errors, and others. Therefore, prior to considering for publication, significant revisions are needed.

Response: We thank the reviewer for the valuable comments most of which we incorporated into a revised manuscript. Below the review is given in its entirety followed by our responses and or clarifications. We have provided additional evidence to support our work.

Specific Comments Comment: This manuscript needs to mention clearly the specific objectives (goals) of the study and presentations need to be farmed with focused scope of the study. The introduction falls short of presenting the specific objectives and scope of the study. Also, throughout the article it is hard to follow what the main focuses are without framed objectives and scope of the study.

Response: We have more clearly presented the focus and scope of the study throughout the introduction.

Comment: The purpose of the paper is not clear if it is a model development or an application, or both.

Response: It is to some extent both, since the model has not been tested in numerous basins, there was model testing involved as well to ensure it is capable of correct prediction, as well as application.

Comment: As it now reads, the authors have previously developed and applied this water balance- and topography-based SWAT (yet to be published), which makes the effort in this paper to be merely an application of SWAT-WB on BNB.

Response: We argue that it is not “merely an application of SWAT-WB on BNB” but rather a robust testing and refinement of the applicability and scalability of the model across a complex basin. Sediment predictions we never tested against observed data,

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thus, this is the first test of how the model predicts erosion and sediment dynamics following modification. Furthermore the methodology tested here vastly reduces the calibration effort of the model, which is clearly important in regions such as the BNB, where there is often little to no data to test model predictions.

Comment: The authors need to mention the focus and the specific contributions of this paper and its additions to the previous papers, in other words, there needs to be a clear statement with regard to what this paper adds on top of these previous papers.

Response: We have added more to focus the paper, specifically highlighting the reduced data requirements for calibrations, and the implications for watershed management from the model results.

Comment: If the authors's focus is on SWAT-WB application of on BNB, the article also needs to be refocused to do so. Presenting a summary of the methodology of model development and calibration process from the previous papers was helpful in obtaining general insights; however, careful considerations need to be made in having more focus on model applications specific to BNB (examples: information on management model inputs- tillage, -crop planting. important default inputs used; calibrated parameters if changed from default; validation processes, lessons learned that may be useful to other watershed with similar settings).

Response: We have added discussion of these throughout.

Comment: This modeling approach, developed to identify runoff contributing areas, is evaluated by traditional-approach where by modeling results are only evaluated by directly comparing observed and predicted flows at the outlets of the test watershed. Though, of predictions with regard to distribution of runoff contributing areas is unknown. Hence this means that the findings and results achieved using this approach is only not definitive, unless they are tested with field-based data. In addition, the application of the model at such a larger scale, such as BNB, makes the modeling results to be more likely dedicated by calibration process rather than the actual processes

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involved. Therefore the authors need to acknowledge this and restrain from making definitive remarks/comments/conclusions with regard to predicted spatial runoff distributions presented that are not tested against field data.

Response: We have added references to data sources that support “definitive remarks/ comments/ conclusions” And as we stated several times in the manuscript, we have dramatically reduced the calibration effort and data needs, and feel that the model results are driven much less by calibration techniques/parameter adjustments than many previous model applications. That being said, there is certainly a great deal of uncertainty in the model predictions (as with most models) and the BNB does not have a large, distributed, well defined data set against which we can easily and reliably test model predictions. Perhaps the best data exist in the SCRP watersheds, which we have used extensively to first determine relevant process and, second, to test the model against.

Comment: This study also fails to perform model validation, a necessary and critical step in model applications. Validation of streamflow predictions could have been done on selected subbasins, by excluding them from the calibration process (Or it could be done by dividing the observed data for (a) selected subbasin(s) into calibration and validation periods). Without validation, it is hard to establish the credibility of the modeling results and their ability to replicate predictions. Moreover, at the scale of application and in the context of BNB, there is no mention on how these-HRU-based runoff results will be communicated to the ground (at the first place) for them be useful in guiding any management decision making.

Response: To the calibration section we have added “Flow calibration was validated against an independent time series that consisted of at least one half of the observed flow time series” and “Since limited calibration data precludes the use of more traditional calibration and validation data sets sediment parameters were calibrated using a leave-one-out cross validation time series (McCuen, 2005) to ensure the model stability. For the leave-one-out cross validation one observation point is successively omit-

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ted in a series of steps. The model is calibrated with one data point withheld and the resultant calibration, based on n-1 data points, is used to predict the withheld point. This process is repeated until each data point has successively been withheld from the calibration and predicted using the corresponding n-1 calibrated model. The predicted data points are then combined into a leave-one-out cross validation time series that can be compared to the data to derive goodness of fit statistics, referred to as a leave-one-out cross validation statistics. Since each point in the leave-one-out cross validation time series is predicted by a model that was calibrated with the corresponding data point excluded, it represents a model prediction that is independent of the model calibration."

Comment: As mentioned in the manuscript, the main hydrology model parameters controlling the amount of infiltration and runoff were determined mainly by the baseflow data, which in turn was determined from baseflow separation (Arnold et al., 19950. More elaborations on this technique may be desirable as this is the key part of your calibration process. The suitability of this baseflow separation technique at the BNB context and for the various subbasins, varying in size, location, and may be other watershed characteristics.

Response: We have added what the technique is (digital signal filter). The method and other similar methods are widely used across the globe, and are generally accepted as valid methods to determine the respective contributions of baseflow and runoff.

Comment: The study sends a conflicting message in the abstract with regard to modeled sediment results.

Response: We disagree. In the abstract we state "Analysis of model results indicate that upland landscape erosion dominated sediment delivery to the main stem of the Blue Nile in the early part of the growing season before the soil was wetted up and plant cover was established. Once plant cover was established in mid August landscape erosion was negligible and sediment export was dominated by channel processes and

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re-suspension of landscape sediment deposited early in the growing season" And discuss this more thoroughly later in the manuscript, particularly with respect to how the model predicts landscape or channel based erosion.

Comment: Overall this abstract needs careful revision to reflect precisely the findings of the study, limitations with the sediment predictions, and its implications or the next steps suggested. Also, besides the runoff transport factor, timing and amount of sediment predictions by the model may be governed (among others) by land use and management, such as tillage timing and equipment, and planting dates assumed/used as an input to the model. Information related to these model inputs and how they might dictate the model outputs need to be presented.

Response: We have added that tillage and land management influences sediment losses as well.

More specific comments /technical errors Abstract

Comment: Page 3840 line 10-11: The abstract doesn't reflect the study findings, particularly model limitations with respect to modeling sediment losses as presented in the discussion and conclusion part of the manuscript.

Response: If the reviewer is referring to pg 3838 ln 10-11 we have added reference to data showing this to be a consistent statement. "Based on SCRP data from the Anjeni watershed that shows sediment losses to be inversely related to slope position (e.g., steeper slopes produce less erosion) it appears that the hydrologic underpinning of the SWAT-WB model (e.g. less runoff is generated on steeper slopes) provides a physically correct platform to asses erosion."

Comment: Page 3838 line 12: check subbasin area, comments on the watershed description; Table 2

Response: we have corrected these

Introduction

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Comment: Page 3841 line 20-25: Revisit the remark by Liu et al., 2008. "that infiltration-excess runoff is rare (Liu et al., 2008). I don't believe Lie et al., 2008 findings are a definite comment based on field-based assessment, rather a suggestive note.

Response: We do not present it as definite, and noted on pg 3840 ln23 that the results of Lui et al., 2008 "suggest" saturation excess process to be an important contributor to runoff generation.

Comment: Page 3842 line 4-9: revise the whole paragraph, and also make sure specific goal/objective (s) is/are stated precisely and clearly.

Response: We have modified the paragraph, which now read "We apply the SWAT-WB model to the Ethiopian portion of the Blue Nile basin that drains via the main stem of the river at El Diem on the Sudanese border (the Rahad and Dinder subbasins that drain the Northeast region of Ethiopia were not considered). We show that incorporating a redefinition how HRUs are delineated combined with a water balance to predict runoff can improve our analysis of water resources. The model is initialized for eight subbasins ranging in size from 1.3 km2 to 174,000 km2. We calibrate the model for flow using a priori topographic information and validated with an independent time series of flows. For sediment, since there is little data to split into calibration and validation data sets, we employ leave-one-out cross-validation (McCuen, 2005) (e.g., model is calibrated with one n-1 data points to predict the withheld point and repeated until each data point has been withheld from the calibration and predicted using the corresponding n-1 calibrated model). Finally, we discuss the implications of watershed management with respect to the model results."

Comment: Page 3840 line 20: Add reference for Steenhuis et al.(2010)

Response: Should be Steenhuis et al (2009), we have made the change

Comment: Page 3841 line 1: Check the spelling for the "Asharge "

Response: We have corrected the spelling

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Comment: Page 3841 line 20-25:add reference for White et al., 2009

Response: should be White et al 2010, we have made the change

Comment: Page 3842 line 5-9: HRUs define the first time it is used.

Response: We have defined HRUs

2 Materials and methods

Comment: Page 3842 line 20-25: specify that ARCSWAT is SWAT version with ARC GIS interface.

Response: We have added “The interface combines SWAT with the ARCGIS platform to assimilate the soil input map, digital elevation model and land use coverage.”

Comment: Page 3843 line 5-10: add reference for Steenhuis et al.(2010)

Response: Should be Steenhuis et al (2009), we have made the change

Comment: Page 3844 line 12: reference Easton et al., 2008a, or proof read.

Response: We have made the change

Comment: Page 3844line 15: provide the range of intensity for the “high intensity” storms (low), and the proportion of rain corresponding to this and/or “low” intensities.

Response: We have added “(e.g., storms with rainfall intensities greater than the infiltration capacity of the soil)” This infiltration capacity and rainfall intensities vary across the basin, and thus there is no one number to provide. We have also added “. Indeed, data from Soil Conservation Reserve Program (SCRP) watersheds (Engda, 2009) show the probability of rainfall intensity exceeding the measured soil infiltration rate to be very low, only 7.8% of storm intensities exceeded the lowest measured infiltration rate.”

Comment: Page 3845 line 2-4: indicate this is annual rainfall. Response: We have added annual rainfall

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Comment: Page 3845 line 19: check the area of Anjeni watershed in the test and Table 2.

Response: We have corrected table 2

Comment: Page 3846 line 5: BNB define when used first time.

Response: We have added this

Comment: Page 3847line 5: ENTRO define when used first time.

Response: We have added “the Eastern Nile Technical Regional Office (ENTRO),”

3 Model calibration Comment: Page 3848 line 21: add reference for Arnold et al., 1995

Response: We have added this.

Comment: Page 3850line 14: SCRP-define when used first time.

Response: We have done this

Comment: Page 3850 line 20: MUSLE-define when used first time, it is only defined later in Line 23

Response: We have done this

Comment: Page 3850 line 27: add “in” before the Watershed

Response: We have done this

4 Results

Comment: Page 3851 line 4: the use of “sediment yield” here conflicts with your definition in line 7-10.

Response: We have clarified this section “We determined the respective amounts of landscape and channel sediment by comparing the sediment yield from each HRU summed within a subbasin to the channel sediment yield, which, when summed equal

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the subbasin sediment export. The HRU sediment yield is an estimate of sediment delivery from an HRU into the main channel during the time step, while the channel sediment yield is any sediment eroded or re-entrained from the channel. Thus, sediment export from a subbasin includes both the sediment yield from the HRUs and any sediment eroded or entrained from the channel."

Comment: Page 3851 line 16, 18: It seems that you have referred to a wrong table

Response: We have corrected this

Comment: Page 3851 line 16, 21, be consistent with the use of E/Nash Sutcliffe Efficiencies/NSE

Response: We have changed this

Comment: Page 3851 line 26: indicate whether the normalized discharge is measured or predicted. Response: It is measured, we have added this

Comment: Page 3853 line 3-4: Data (if taken from Table 2) only matches for Anjeni, not for Jemma. "4mmy$-$1 in the Jemma subbasin to as high as 44mmy$-$1 for Anjeni".

Response: We have correct the text

Comment: Page 3853 line 7-9 and Page 3853 line 11-14: Conflicting comparison sentences, particularly with the Gumera sub-basin, Gumera has relatively higher, value.

Response: We have fixed this, the correct basin should be Angar not Gumera

Comment: Page 3853- line 24 - 3853 line 1-3: Results that are HRUs-based may be difficult to translate into actual fields for management purposes?

Response: We are not sure we understand this comment. Is the reviewer asking if the results are interpretable at a field scale? If so, then the answer is a qualified yes. Data from SCRP watersheds indicated that there was a large gradient in runoff production based on topographical position, and thus our results are consistent with

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these findings. We have added “These results are consistent with data collected in the Anjeni SCRP watershed (SCRP, 2000; Ashagre, 2009), which show that runoff losses roughly correlate with topography.”

Comment: Page 3854 line 23-25 : “This sediment was subsequently mobilized during the higher flows that are typically peak after the sediment peak is observed (e.g., the sediment peak occurs approximately two weeks before the flow peak) (compare Figs. 4 and 7).” It is hard to observe this from the figure presented and the scale it is presented.

Response: We agree we have added dates to make this comparison more clear

Comment: Page 3855 line 9-15: revisit the sentences. In the writing, make clear distinction between (1) the occurrence of landscape erosion and gully erosion (during and following peak flow) and potential gully erosion following accumulation of interflow, and 2) how SWAT represents these processes. Obviously, SWAT doesn’t have a routine to predict gully erosion. Hence, it seems that predictions from landscape erosion (over-predicted to compensate erosion coming from gully erosion), from gullies (during and following peak flow), and again from gullies (during interflow accumulation), and channel are not represented accurately. With such problematic findings, I am not sure if the predictions are any good to be of use for directing management decisions. Or, it may be intricate to extract practical implications from these modeling results (particular regarding sources of sediments, as indicated in the abstract.

Response: We disagree, but recognize that we did not adequately preset compelling evidence to support this speculation. We have added reference to these findings in the text. Tebebu, 2009; Zegeye, 2009 measured the differences in gully and rill/ inter-rill erosion and concluded that active gullying occurred after the soil was saturated by interflow, thus we believe that this statement is at least plausible.

Comment: Page 3855 line 28-29: the data in Table 3 has problem (unit, magnitude??). Descriptions made from this table may also have a problem. For example, for Ribb and Border, modeled sediment yield = modeled sed. Export/area (off course the units are

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wrong), but this also doesn't seem to work for Anjeni watershed.

Response: The numbers and units in the table are indeed correct, however we have added a qualifier that these measurements are made during the rainy season.

Comment: You noted in the text, Page 3851 line 7-10, "The sediment yield is an estimate of sediment delivery from an HRU into the main channel during the time step, while the sediment export from a subbasin includes both the sediment yield from the HRUs and any sediment 10 eroded or entrained from the channel". So, are the modeled sediment yield = sum of sediment losses from all HRUs in the subbasin?

Response: Yes, sediment yield are the sum across all hrus in a suibbasin. We think that the clarifications made above have fixed this problem

Comment: Page 3856 line 1-4: Avoid making conclusive statements without providing actual data supporting it. In addition to the hydrology, there may be other factors, such as landuse and its managements, playing important role here. I don't completely agree that gully erosion also occurs only as a result of only wetting up of soils from the interflow. Typically, gully erosion may also occur from running concentrated water forming narrow channels during or immediately after heavy rains. Make sure your statements are also supported with references.

Response: We agree, and in fact have qualified this statement in the text on the line directly below "The high sediment yield areas are generally predicted to occur at the bottom of steep agricultural slopes, where sub- surface flow accumulates, and the stability of the slope is reduced from tillage and or excessive livestock traffic."

Comment: Page 3856 line 5-8: There was no any mention of what the "surfical geology" of the region look like, their formation, and/or how they were utilized in the modeling process. It seems inappropriate at this stage to make such conclusive statement, without describing what they are and how they helped confirm accuracy of sediment sources predictions.

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Response: We have added the formations that tend to promote erosion (Leptosols and vertisols) to the test. Furthermore we do not think that this is a conclusive statement. We say that is seems reasonable to assume that the results are accurate if not in exact location, than in magnitude, the results do seem to support this. We were able to predict the magnitude and timing of subbasin sediment export with good accuracy

Comment: Page 3856 line 17: describe the specific surfical geology of the Jemma that contributed partly to the high sediment loss.

Response: We have added that "The Jemma subbasin also shows high predicted sediment losses, mainly a result of the surfical geology (e.g., leptosols overlaying a basalt formation), high agricultural activity, and steep slopes."

Comment: Page 3856 line 27-29-: Wrong Fig. is referred; and even after correcting it, the whole sentence will be redundant as you have already made the same statement in line 9-14.

Response: We have corrected this.

5 Discussion

Comment: Page 3857 line 9-10-: revise this sentence, Avoid such vague statements; be specific as to what aspect of water resource you are referring to, availability, quantity, quality.

Response: We have changed this to "Flows in the Blue Nile Basin. . .."

Comment: Page 3857 line 15: again, check the spelling "Asharge"

Response: We have corrected this

Comment: Page 3857 line 20 and Page 3858 line 15: The values presented in Table 4 seem to have problems. May be the unit should be t/km2, even then, the annual sediment losses from each land uses presented are minimal in a practical sense. Note that if you the units are in t/km2, all land cover will have sediment yield <0.2 t/ha.

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Response: The units should be t ha yr-1

Comment: Page 3857 line 23-24: Revise the sentence, to parallel it with previous sentence by avoiding the use of “however”.

Response: We have made the correction

Comment: Page 3858 line 1: modify the first sentence. . ..suggestion an application of, or Use of the modified SWAT “SWAT-WB”..

Response: we have made correction

Comment: Page 3858 line 9: check spelling “gulley erosion”

Response: We have made correction

Comment: Page 3858 line 10-14, revise the sentence. The earlier version SWAT considers, slope soil erosivity and management into consideration also, in SWAT not all land covers produce the same erosion, unless they happen to have these factors (among others) matching.

Response: We have revised accordingly “SWAT predicts erosion to occur more or less equally across the various land covers (e.g., crop land produces approximately equal erosive losses, pasture produces approximately equal erosive losses) provided they have similar soil and land management practices throughout the basin.”

Tables and Figures Comment: Table 2: For the table to be able stand alone, define r2, NSE (also note that be consistent with the abbreviations used in the text throughout the manuscript. Indicate for each subbasins if the analysis are daily or monthly. Be consistent with the naming of the outlet, El Diem, border (figures, text, and tables) Area for the Anjeni watershed presented in the table is not consistent with the text. Under the Normalized, indicate it is, “discharge” Are all predicted flows? If yes, explain why normalized discharge is not equal to “direct runoff” + “ground water”. If there normalized ‘discharge’ presented in the 6th column is observed/measured, then you

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need to indicate that, and revise the Table caption to capture any changes.

Response: We have revised accordingly

Comment: Table 3: Again, define r2, NSE; also read comments made for Page 3855 line 28-29.

Response: We have revised accordingly

Comment: Table 4: Read comments made for Page 3857 line 20 and Page 3858 line 15.

Response: We have revised accordingly

Comment: Figure 1: Include countries boundary, and/or include name the outlet (El Diem) a “border” to be consistent with the naming system used in the Tables, Text, & Figures.

Response: We have made the change

Comment: Figure 2: What is the significance of ENTRO in the braket?

Response: ENTRO is the data source

Comment: Also, use capital A and Bin the Figure Caption.

Response: We have added this

Comment: Figure 5: Proof read the Fig caption, delete “s” after subbasin

Response: We have revised

Comment: Figure 6: Delete the second sentence in the figure caption.

Response: We have made change

Comment: Figure 8: Define BNB; the legend for the discharge for the Gumera is too small to read.

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Response: We have made change

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