Response to interactive comment on *"The effect of input data complexity on the uncertainty in simulated streamflow in a humid, mountainous watershed"*

Suggested title based on the reviewer 2's comment: "Effect of input data resolution and complexity on the uncertainty of hydrological predictions in a humid, vegetated watershed"

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Point by point response (indicated by ➔) to reviewers' comments

Please note that the page and line numbers referred to here are from the revised manuscript.

RESPONSE TO REVIEWER 1

W. Vervoort (Referee)

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Comment

This manuscript describes how an earlier developed version of SWAT with a wetness index spatial layer as an additional input, is tested for different input layer uncertainties. In particular, the paper focuses on the scale of the DEM and scale of the land use and soil layers. In principle, this is an interesting concept, however, I believe that, at the moment, the authors don't do a good job identifying and describing what the important (of interest to a global audience of HESS) findings are. As a result, I struggled to understand why this paper should be published in its current form.

Response

➔ While earlier studies have looked at the effect of differences in scale of DEMs, land use, and soil layers on SWAT model predictions (streamflow, nutrients etc.), the focus of our study is on the uncertainty in parameter values and output uncertainty (streamflow and saturated areas in this study) due to differences in input layers. We found that increasing

input data complexity/resolution does not help to reduce parameter uncertainty and the uncertainty in model predictions. However, using multiple types of observed datasets such as spatial observations in addition to the conventional temporal observations (flow at the outlet) can eliminate a high number of unsuitable parameter sets and guide selection of the appropriate parameter sets that give good temporal and spatial predictions for streamflow and saturated areas. These results are important and have wider applicability when identifying critical runoff generating areas and locations within the watershed where management interventions for water quality improvement (e.g. Phosphorus loading) are most effective. Our results are applicable to regions with similar land use, topography, and climate that are dominated by saturation-excess runoff. To apply to other regions, similar/independent work is needed using the methodology described in this paper.

Although we think that our study provides a good scientific contribution, we agree with the reviewer that we did not give a proper explanation in the two major issues that the reviewer mentioned. We tried our best to respond to all comments you have made below, and revised our manuscript based on those responses.

Comment

There are to me two major issues that need to be expanded on in more detail and can actually be the bit that makes this paper acceptable:

1. What is the influence of adding the wetness index layer to the input mix, and how does this interact with the other layers? I checked Hoang et al. (2017), which is actually twice in your references, and I did not see any analysis of this either. It mainly covers the improved predictions, and this is an important addition. However, thinking about the input layers, there has to be some sort of interaction between the slope, soil and wetness index, and this is not really explored. Note that I am not saying the algorithm is not valuable, it clearly is, and however, what remains unexplored is how this addition interacts with the existing components. For example you would expect that at some of the DEM and soil map resolution, the soil slope interaction would be similar to your wetness index layer. Of course without the underlying lateral flow and surface aquifer algorithm, this would be useless information. What is valuable in your current research is that you seem to discover this in your results. All your results, I think, point to the fact that the wetness index layer dominates the actual flow behavior, but I am not sure if this is specific to your water shed. For example, the latb parameter is a sensitive parameter. Is this because the watershed is dominated by lateral flow, or is it because you have introduced the wetness index layer? As you only calibrate on streamflow with some comparison of the saturated areas, we don't actually know. The dominance of the wetness index layer also explains why

the uncertainty in the soil and land use layers is minimal, specifically since you a-priori decided on a 10m DEM for that test. In summary, I believe you need to investigate this further and figure out how this exactly works in relation to the algorithm that you have introduced.

Response

➔ We agree with the reviewer that we did not make clear the importance of the wetness class map and its interaction with other layers. We hope our response to your comment below will clarify it.

1. The role of wetness class map

The SWAT-HS model uses topographic index (TI) as the basis for hydrological modeling, like some other variable source area models: TOPMODEL (Quinn and Beven, 1993;Beven and Kirkby, 1979), SWAT-VSA (Easton et al., 2008), SWAT-WB (White et al., 2011). To keep the model semi-distributed, we divide the watershed into a limited number of wetness classes (maximum 10 classes in the current SWAT-HS). Each wetness class is assigned a soil water storage capacity (or called as saturation deficit in the TOPMODEL). This is different from the TOPMODEL in which saturation deficit is calculated from the value of topographic index; soil water storage capacities in wetness classes in SWAT-HS are assumed to follow Pareto distribution as:

$$edc_i = S_{max}\left[1 - \left(1 - A_i\right)^{1/b}\right] \tag{1}$$

where edc_i is the soil water storage capacity in wetness class i, $Smax$ is the maximum soil water storage capacity of the watershed, A_i is the fraction of the watershed for which the storage capacity is less than edc_i, and b is the shape parameter.

The lower edc values are assigned to the wetness classes having high TI values, located in downslope areas ("wetter" wetness classes) while higher edc values are assigned to wetness classes with low TI values in upslope areas ("drier" wetness classes). *Smax* and *b* are two parameters controlling the Pareto distribution that we use in calibration. Note that this Pareto distribution is already used in other models to simulate saturation-excess runoff like: the Xinanjiang model (Zhao et al., 1995;Zhao et al., 1980), the VIC model (Wood et al., 1992;Liang and Lettenmaier, 1994) and the PDM model (Moore, 2007). These three models simulate saturation-excess runoff and estimate the saturated fraction of the watershed, however, they are not able to identify the specific spatial locations of saturated areas.

In conclusion, the role of the wetness class map is to divide the watershed into areas with different saturation deficits that are lower in areas with high TI values and higher in areas with low TI values.

2. *Interaction of wetness class layer with other input layers*

To create HRUs in SWAT-HS, first, the soil map was overlaid with the wetness class map to create a new soil map in which the same soil types in different wetness classes have different soil names but retain the same soil characteristics (Hoang et al., 2017). The new soil name reflects both wetness class and soil type. Subsequently, this new soil map is overlaid with the land use map to create HRUs using the regular procedure in SWAT for HRU definition. As we mentioned in the paper, we assumed that slope is not a part of HRU discretization for simplification purposes, although slope is used in topographic index calculation and thus incorporated into wetness classes.

Once HRUs are created, each of them has details of its land use type, its soil type with soil name reflecting the wetness class number in which it is located. Each HRU has the initial soil water storage capacity (saturation deficit) depending on which its wetness class. This storage capacity will change over time depending on climate inputs and other processes occurring in the soil profile (percolation, uptake, evaporation, generation of different types of flow) which are affected by soil and land use information.

In conclusion, the wetness class map defines the initial saturation deficits in HRUs while the land use and soil type give information to calculate hydrological processes that change the saturation deficits and subsequently update the saturation deficit values for HRUs.

We agree with the reviewer that all our results point to the fact that the wetness index layer dominates the actual flow behavior. It is reasonable because SWAT-HS uses topographic index as the basis for hydrological modeling. We think this is applicable to all watersheds where the application of SWAT-HS is suitable, i.e. watersheds dominated by saturation-excess runoff. We also agree with you that the dominance of the wetness class layer also explains why the uncertainty in the soil and land use layers is minimal. When the appropriate DEM resolution is used, soil and land use information become less influential to hydrological predictions.

3. *The latb parameter is a sensitive parameter. Is this because the watershed is dominated by lateral flow, or is it because you have introduced the wetness index layer?*

Yes, *latb* parameter is a sensitive parameter because our watershed is dominated by lateral flow. The figure below, which is in our previous paper (Hoang et al., 2017) with DEM

10m and the most complex soil and land use maps, shows the comparison of different types of flow in the Town Brook watershed. The figure shows that lateral flow is the dominant type of flow. Our result is compatible with the finding of Harpold et al. (2010) in an intensive field survey in a 2.5 km² headwater watershed of Town Brook watershed where hillside lateral preferential flow paths rapidly transported water to near‐stream saturated areas during runoff events under relatively dry antecedent conditions. Harpold et al. (2010) also suggested that the lateral redistribution of water from hillside areas reduces the influence of surface topography and channel topology on the sources of stream runoff.

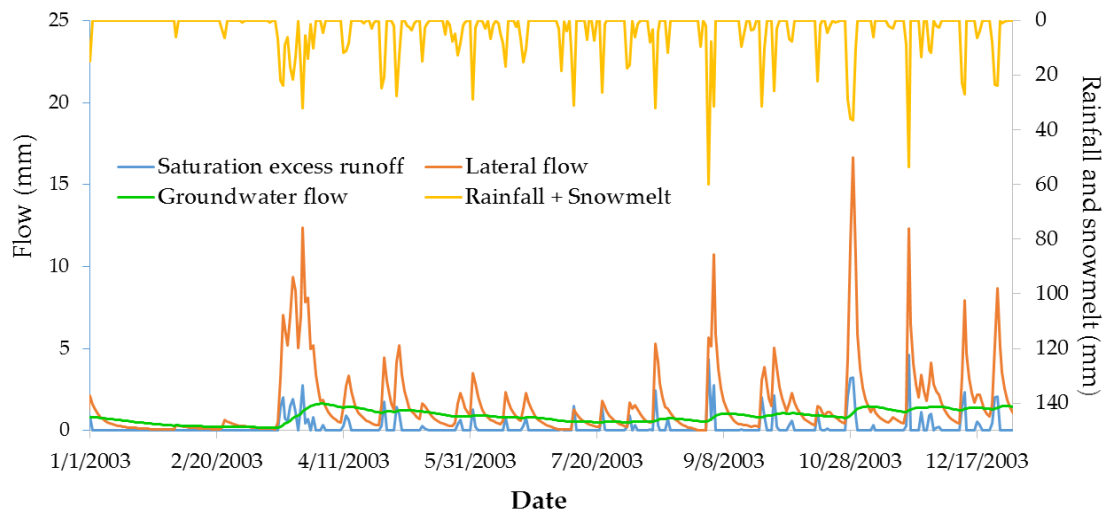



Figure 1: Time series of flow components simulated by SWAT-HS in the Town Brook watershed (Hoang et al., 2017)

Comment

The second major issue is that it is unclear from your research whether the results are more generally applicable. What is the global significance of your research? I am asking this as there is now real way of telling whether your results are water shed specific. You even seem to write to a local audience in the paper, often referring to your results as being specific and decisions being specific for this watershed. In a way, this is fine, but for HESS, the real value is in research that is of interest to a global public. This means that I believe that you need to define this better or test this better.

Response

➔ Thank you very much for your constructive comment. We are confident that our results are not only important in the New York City watershed region, but also have wider applicability.

For the New York City watershed region, our study will provide a guidance for choosing input data (DEM resolution and the degree of complexity for soil and land use map) to apply SWAT-HS for a larger scale watershed which requires division into multiple subbasins and a certain degree of complexity for soil and land use information. Our results are important when we use SWAT-HS to identify critical runoff generating areas and locations within the watershed where management interventions for water quality improvements (e.g. Phosphorus loading) are most effective. In our follow-up work which uses the results of this study to scale up the application of SWAT-HS to a larger scale watershed to simulate streamflow and phosphorus (paper under review), we got good results for phosphorus calibration due to the correct simulation of hydrology (surface runoff in particular). Identifying the correct/optimum input layers is thus an important step. Our experience also suggests that model run time can also be reduced when using the optimum input layers, which is particularly important during the calibration process.

Our results are applicable to watersheds with similar land use, topography, and climate but similar/independent work is needed in other regions using the methodology described in this paper. In our case DEM 10m gives the best results due to better physical representation of the landscape and is a compromise between the high resolution 1m and 3m that provide too much spatial details that interfere with calculation of upslope contributing areas and topographic index, and the coarse resolution 30m DEM that averages out the necessary fine details. DEM10m was also chosen as optimal in several studies in other regions by Kuo et al. (1999) and Zhang and Montgomery (1994), but was deemed to be worse than other finer resolution DEMs like in Buchanan et al. (2014). Therefore, we think that the sensitivity of DEM resolution depends on the scale and characteristics of the watershed. Accordingly, to choose the appropriate DEM resolution for other region, we recommend carrying out similar work with the methodology used in our study.

What we learned from this study is that, hydrological prediction is very sensitive to the choice of DEM (with greater effects on prediction of saturated areas than streamflow), when using a hydrologic model that uses topographic index as the basis for hydrological modeling, in a watershed that is dominated by saturation-excess runoff. Besides SWAT-HS, some other watershed models using topographic index are: TOPMODEL (Quinn and Beven, 1993;Beven and Kirkby, 1979), SWAT-VSA (Easton et al., 2008), SWAT-WB (White

et al., 2011). With SWAT-HS and models that are based on topographic index in general, DEM resolution is more sensitive than the complexity of soil/land use information. When the appropriate DEM resolution is used, soil and land use information become less influential to hydrological predictions.

We regret that the manuscript in its previous form did not transfer sufficient knowledge and information to the general audience. In response to the reviewer's comment, we revised significantly the **Summary and Conclusions** to clarify the general application of our study. Since the edits in the revised manuscript is almost same with the response, we did not show the text here. Please see the changes in **Summary and Conclusions (P23 L23 – P24 L25).**

Comment

An example is your DEM result, you point out that the 10m DEM seems best comparing the NSE and looking at saturated, but you don't seem to be able to explain why (which is the real wider interest). Is this because of the specific physiography of your watershed, or is this due to your specific model algorithms? Citing other literature that found similar things does not really help unless this helps you explain your result. So in summary, your results need to be explained better and it should be clearer what the value of your results to the wider research community. I have added many more comments on the attached pdf that are probably useful to address these issues.

Response

➔ We totally agree with the reviewer for lack of proper explanation on why DEM10m is the optimal resolution in our study. We tried our best to find the answer as below.

The prediction of saturated areas is based on wetness classes which are classified based on values of the topographic index. Therefore, the sensitivity of DEMs on saturated areas predictions can be explained by the effect of DEM resolution on topographic index (TI).

Note that the basic equation for topographic index is TI = ln (contributing area/slope angle)

The below figure shows the relationships of TI with slope angle, upslope contributing area and elevation using two representative DEM resolutions: 1m and 10m. It is clearly observed that DEM 1m can capture a significantly wider range of slope than DEM 10m because of its finer resolution. Also, the percentage of grids that has low values of TI is significantly higher in DEM 1m than in DEM 10m (in figure below use red lines for reference), which also can be seen in figure 3d in the main manuscript. Low TI values are usually found in grids with steep slope or with low upslope contributing areas. Because

DEM 1m captures steep slope at local scale and has a high number of grids with low upslope contributing area (figure 3c in the main manuscript), the percentage of low TI values in DEM 1m is much higher. If we look at the relationship between TI and elevation, we can see that the distribution of TI values in DEM 1m spread out wider than in DEM10m at all elevations. This explains why the distribution of wetness classes in DEM1m has a more complex pattern with every wetness class spread-out while DEM10m has a more coherent pattern with high TI wetness class well compatible with the stream network (Figure 4 in the main manuscript).

Our findings are in agreement with Lane et al. (2004) who used high resolution LiDAR 2m DEM in the TOPMODEL which simulates hydrology based on topographic index. The TOPMODEL predicted the widespread existence of disconnected saturated zones that expand within an individual storm event but which do not necessarily connect with the drainage network. They found that using the LiDAR 2m DEM, the topographic index has a complex pattern, associated with small areas of both low and high values of the topographic index, leading to the appearance of disconnected saturated areas. After remapping the topographic data are remapped at progressively coarser resolutions by spatial averaging of elevations within each cell, they found that as the topographic resolution is coarsened, the number and extent of unconnected saturated areas are reduced: the catchments display more coherent patterns, with saturated areas more effectively connected to the channel network. Moreover, in another study, Quinn et al. (1995) showed how progressively fining model resolution from 50 m to 5 m reduces the kurtosis in the distribution of topographic index values and increases quite substantially the number of very low index values. Wolock and Price (1994) showed that hydrological predictions are affected by DEM resolutions in TOPMODEL, a well-known topography-based hydrological model.

In the revised manuscript, we added the text in this response to the **Discussion section 4.1. What is the most suitable DEM resolution to use in SWAT-HS?** Because the edits are almost same with the text in the response, we did not show the text here. Please see **section 4.1 (P18 L1 -P19 L7).**

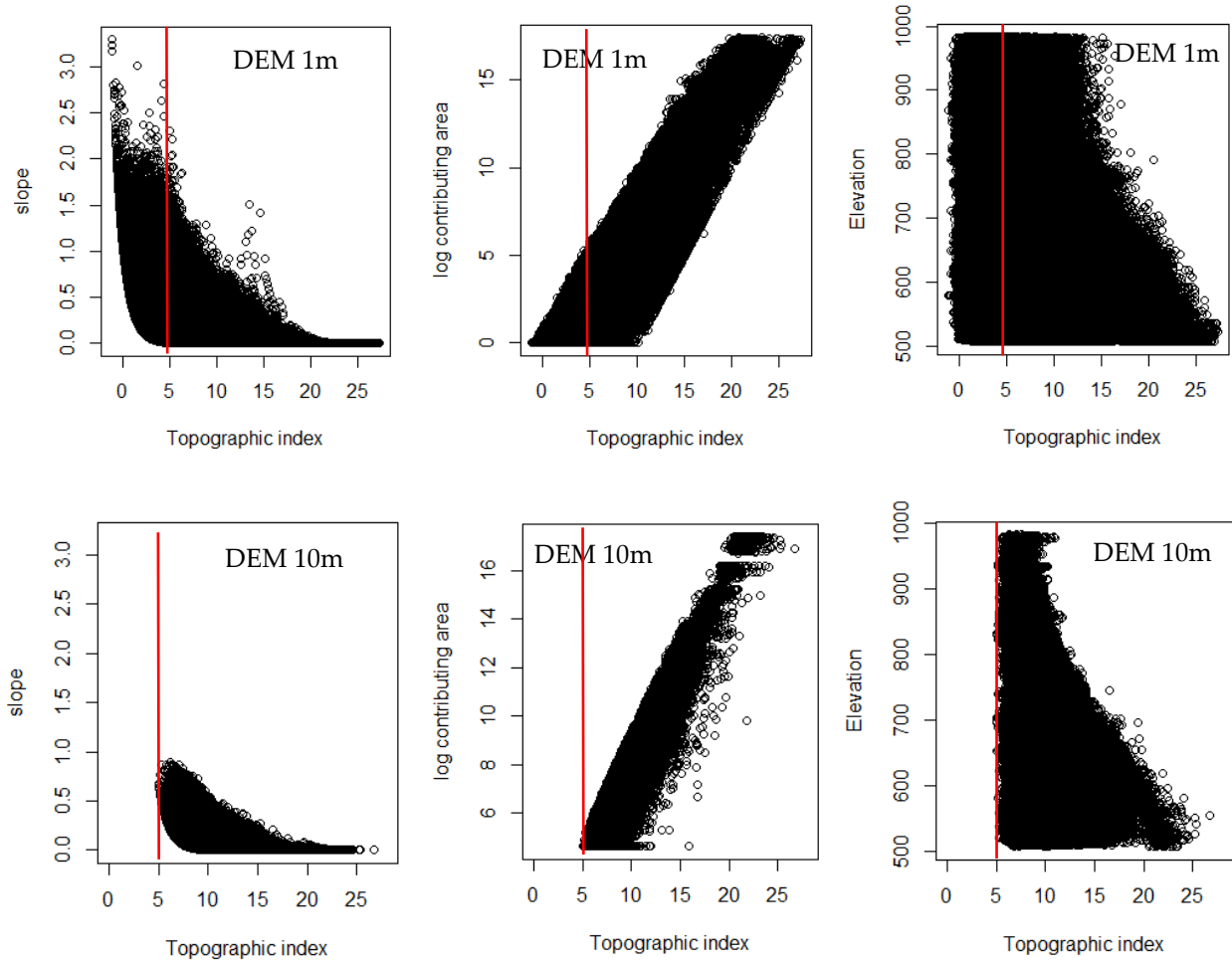



Figure 2: Relationship of topographic index with slope, upslope contributing area and elevation with two resolutions of DEM: 1m and 10m

Comment

I have added many more comments on the attached pdf that are probably useful to address these issues.

Response

➔ We copied all the comments in the attached pdf excluding the comments on grammar and English as below and responded to them one by one.

Other comments in the supplement

Comment

P3 L1: *"This so called equifinality is very common in hydrological models and 1 is the cause for uncertainties in model predictions"* The main cause? The only cause? I think you just argued that there are three sources of uncertainty, but here you reduce this back to parameter uncertainty?

Response

➔ We are sorry for creating confusion here. We edited the text in the revised version as:

"This so called equifinality is very common in hydrological models and is one of the main causes for uncertainties in model predictions" **(P2 L26 – P3 L2)**

Comment

P5, L20: The research questions "*what is the suitable DEM resolution in order to get good model performance*" and ", and "*what is the appropriate complexity of the distributed input data*".

How dependent is this on the location where you are? How can you test that your results are generally applicable. I am worried that given the number of studies in this area that your results are in fact watershed dependent. This is also true for your second objective.

Response

➔ We agree with your comment. As stated in our response to your similar question earlier, our results are applicable to watersheds with similar land use, topography, and climate, and dominated by saturation-excess runoff. However, similar/independent work is needed in other regions using the methodology described in this paper.

The novelty of our study is that the focus is on the uncertainty in parameter values and output uncertainty (streamflow and saturated areas in this study) due to differences in input layers, while earlier SWAT studies have looked at the effect of differences in scale of DEMs, land use, and soil layers only on model predictions (streamflow, nutrients etc.).

We clarified the general applicability of our study by significantly revising the **Summary and Conclusions (P23 L16 – P25 L5).**

Comment

P8, L3-5: "*Subsequently, we divided the remaining areas into 8 wetness classes (wetness class 2 – 9) with approximately equal areas (~ 6% each) based on TI values.*"

So is there any chance that this fairly arbitrary, but practical, division is suited only to your water shed? Or is this a general rule that should be applied if including the wetness index in SWAT. What is the best way to decide how the wetness classes should be scaled?

Response

➜ SWAT-HS gives flexibility for users to divide the watershed into wetness classes with the maximum number of wetness classes being 10. The division of wetness classes is arbitrary and requires general knowledge of the studied watershed.

In this study, we divided the Town Brook watershed into 10 wetness classes based on our expert knowledge of saturation, observations (Harpold et al., 2010) and predictions by other watershed models (SMR (Agnew et al., 2006), SWAT-VSA (Easton et al., 2008) and SWAT-WB (White et al., 2011). Agnew et al. (2006) developed a relationship between topographic index (TI) and probability of saturation P_{sat} for the Town Brook watershed using DEM 10m and suggested that the areas with TI >17.7 is always saturated. Based on this, we grouped the areas with $TI > 17.7$ as the "wettest" wetness class (wetness class 1) for DEM10m setup. For other DEMs resolution, we based on the distribution of wetness 1 in DEM10m to decide the TI threshold to use to create wetness class 1 for each DEM setup. Based on all mentioned information that we gathered, we know that saturated areas never exceeded 50% of watershed, we grouped 50% of the watershed with lowest TI values as the "driest" wetness class (wetness 10). Assigning the driest wetness class to half of the watershed allowed us to classify the remaining areas, which are more prone to saturation, into a greater number of wetness classes. We divided the remaining areas into eight wetness classes (wetness class 2–9) with equal areas based on TI values.

To apply SWAT-HS in a new watershed, we recommend users to initially adapt the procedure of wetness class classification used in this study. Wetness class 1 can be classified by choosing grid cells with upslope contributing areas higher than a reasonable threshold that makes wetness class 1 comparable to the stream network. Wetness 10 can be classified based on an expert knowledge on maximum estimate of saturation percentage, we believe that this information should not be difficult to find. Subsequently, wetness 2-9 can be created by dividing equally the remaining area.

Comment

P8, L13-14: "…*the model was calibrated and validated for the periods 2001-2007 and 2008-2012, respectively.*" What made you decide this order and this division? Why not the reverse and is there any difference in terms of climate between the periods?

Response

➔ We did not have a particular reason to decide this order. We just wanted to have a reasonable number of years for the calibration and validation periods.

Comment

P8, L14-16: "*We excluded the year 2011 from the validation period because there were two extreme events (Hurricane Irene and Tropical Storm Lee) in August 2011 that the model could not capture well.*" Isn't this a concern and actually worth investigating? Why does it not capture this well and how does it not capture this well? Is this related to the input uncertainty?

Response

➔ The figure below shows the modeled streamflow versus observation in the year 2011 that we excluded from the validation period. Hurricane Irene and Storm Lee are the two extreme events indicated in the green box. Hurricane Irene and Storm Lee brought high rainfall amount for several continuous days with very high intensity for several hours of the day to the Catskill system of the New York City watershed while SWAT-HS predicts streamflow at a daily time step. Therefore, at a daily time step, SWAT-HS underestimated the magnitude of high streamflow caused by these two extreme events. However, the model captured the flow variation very well (see the below figure). We excluded the year 2011 from the validation period because we do not want the streamflow underestimation in these two events leads to an unfair evaluation of the model performance.

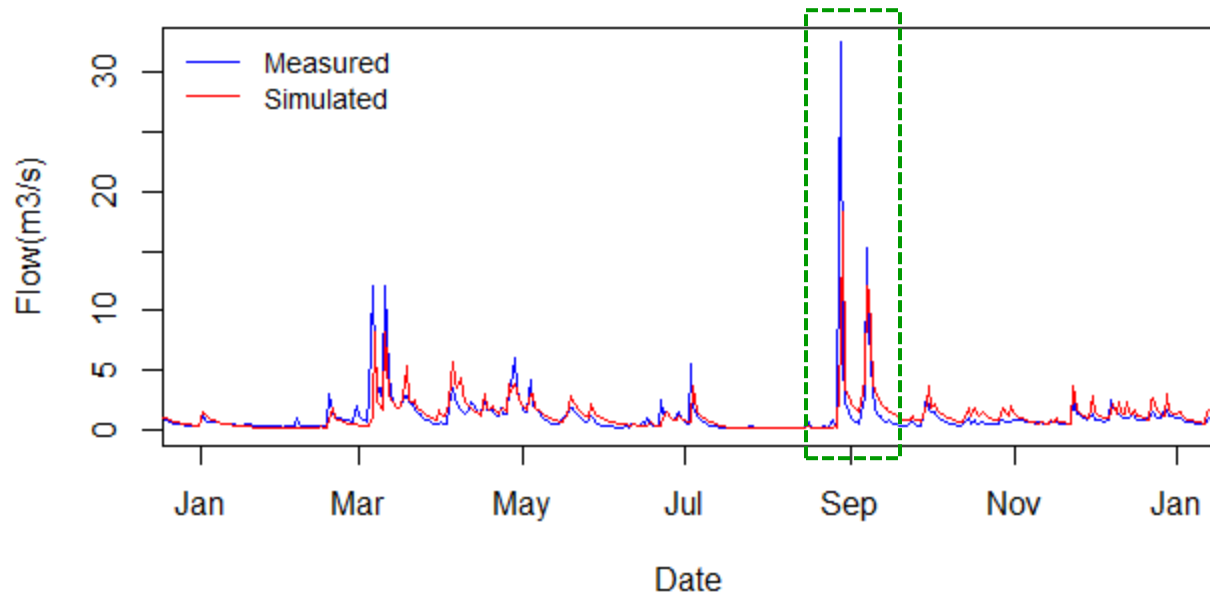



Figure 3: Comparison of simulated streamflow by SWAT-HS and observations in 2011 (peak flows caused by Hurricane Irene and Storm Lee are indicated in the green box)

Comment

P8, L20-22: "*The optimal parameter set from the snowmelt calibration was used in the final streamflow calibration.*" How did you evaluate whether a model was "calibrated"? What did you use as objective function to assess your Monte Carlo output?

Response

➔ In the snowmelt calibration stage, we randomly generated 10,000 parameter sets for 5 snowmelt related parameters, ran those sets with SWAT-HS and compared streamflow predictions with observations The best parameter set giving the best fit to streamflow observations (highest NSE value) was chosen for the flow calibration stage. We had to use streamflow observations in the snowmelt calibration stage because observed snow data is not available.

We clarified our calibration procedure by editing the text in the revised manuscript as:

"The calibration was carried out in 2 stages, i.e. snowmelt calibration and flow calibration, and by applying Monte Carlo sampling method. For snowmelt calibration, we calibrated 5 snowmelt related parameters in group (i) (Table 1) by generating randomly 10,000 parameter sets, running these sets using SWAT-HS, comparing the streamflow predictions with observations and choosing the best parameter set giving best fit to streamflow observations (highest value of daily Nash Sutcliffe Efficiency (NSE)) to use for the flow calibration stage. For flow calibration, 10,000

parameter sets of 9 flow parameters in group (ii) (Table 1) were generated which were then run with SWAT-HS. The simulations in the flow calibration stage were used for uncertainty analysis." **(P8 L27 – P9 L7)**

Comment

P9, L4-6: "*For each model setup, "good" simulations were identified as those with a Nash-Sutcliffe Efficiency (NSE) greater than 0.65 for use in uncertainty estimation of streamflow.*" How and why did you choose 0.65 as a threshold? How does this affect your results?

Response

➔ Figure 5a and figure 10a show the maximum daily NSE values in all our SWAT-HS setup which is 0.68-0.69. From our results, daily streamflow predictions with daily NSE higher than 0.65 results in monthly streamflow prediction with monthly NSE higher than 0.8. Based on guidelines for model performance evaluation by (Moriasi et al., 2007) that suggested "*good*" model performance for streamflow as corresponding to monthly NSE higher than 0.65, we are confident that our choice of NSE higher than 0.65 as good model performance at daily time-step is a reasonable choice.

Comment

P10, L4-5: "*In order to simplify the setup, we assumed that slope does not have an impact on HRU discretization.*"

Is there an interaction between slope and wetness index? I think this should be discussed. For that same matter, if a more detailed soil or land use map is available there could be an interaction between slope, wetness index, soil and land use. So in what way is the extra wetness index just a summary of other landscape elements? And how would this influence results. In a way you are creating a further split of the HRUs, making them more "specific", but a similar result could be achieved with a more detailed soil or land use map? So I think you need to be careful here to argue why the wetness index split is a better summary (thus can be coarser) then just using land use and soil and slope detail? As you have chosen to just use one DEM here, you cannot really investigate that detail.

Response

➔ The below figure shows the relationship between topographic index (TI) & the classified wetness classes and slope & upslope contributing area. This classification of wetness

classes based on TI is used in our previous study (see Table 2 in Hoang et al. (2017)) using DEM10m. Wetness class 1 with highest TI values include grids with gentle slope and high upslope contributing areas. Wetness class 10 which actually covers 50% of the watershed include grids with low contributing areas and wide range of slope angles.

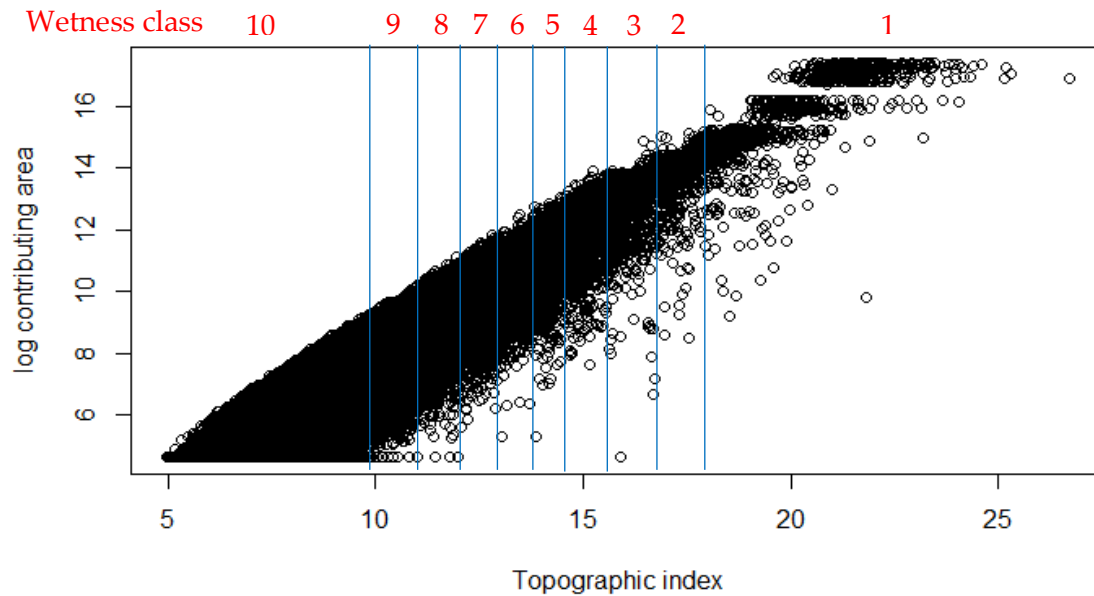
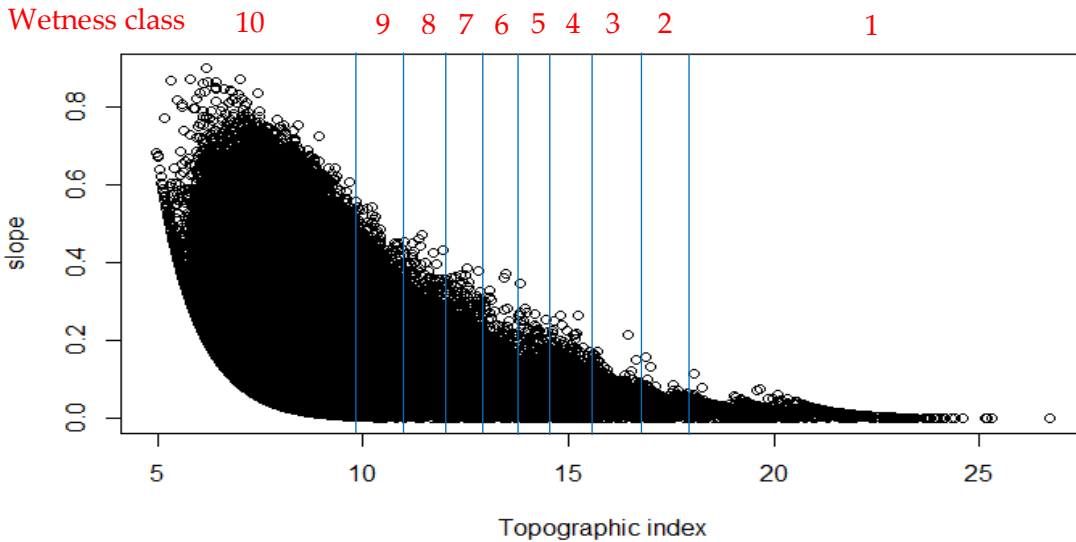
As explained above, the role of wetness class map is to divide the watershed into areas with different saturation deficits. The most important processes of SWAT-HS: the simulation of saturation-excess runoff and the generation of lateral flow in saturated areas are based on the values of saturation deficits. Therefore, wetness class map is very important in HRU definition in SWAT-HS. We cannot replace it with more details of soil, land use or slope.

(a) Relationship between upslope contributing area and topographic index, in relation to wetness classes

(b) Relationship between slope and topographic index, in relation to wetness classes

Figure 4: Relationship between slope/upslope contributing area and topographic index, in relation to wetness classes using DEM10m

Comment

P12, L10-12: "*Generally, there are only slight differences in SWAT-HS performance on streamflow using different DEMs implying the insignificant effect of DEM resolution on streamflow simulation and the uncertainty of streamflow outputs.*"

I am not sure I would agree with you, you have just said that the finer DEM give fewer "good" performances. I would think that is quite an effect and requires discussion

Response

➔ We agree with the reviewer that we underestimate the effect of DEM resolution in this sentence. In the revised version, we deleted this sentence to prevent confusion because the following paragraph of this sentence already discussed about the effect of DEM resolution.

Comment

P12, L17-18: "*This implies better streamflow prediction by these two setups even without calibration*" Why?

Response

➔ In our response to your previous comment that required explanation on why DEM 10m is the best resolution to use, we already explained why the distribution of TI and wetness classes in fine resolution DEM (represented by DEM1m) has a more complex pattern low and high TI value grids spread out while DEM10m has a more coherent pattern with high TI grids well compatible with the stream network. Here we show the distribution of TI values using different DEMs in the below figure. It clearly shows that the coarser DEMs (10m and 30m) have grids with TI values compatible with the stream network, while in the finest DEM1m, it is hard to recognize where stream network is based on TI values. Realistically, highest TI value grids should locate in downslope, near-stream, low elevation areas while lowest TI value grid should be in upslope, high elevation areas. However, fine DEMs do not capture this physical representation of the landscape well while coarser DEMs do it better. We think that this is the reason why coarser DEMs give better predictions in both streamflow and saturated areas than the finer DEMs.

We included this response in the discussion section *4.1. What is the most suitable DEM resolution to use in SWAT-HS?* in the revised manuscript as:

"…Realistically, highest TI value grids should locate in downslope, near-stream, low elevation areas while lowest TI value grid should be in upslope, high elevation areas. Therefore, in this case study, the coarser DEMs (DEM10m and 30m) give a better and more realistic representation of the landscape than the finer DEMs (DEM1m and 3m). This is possibly the reason why the coarser DEMs setups have higher probability to get a good performance (higher number of 'good' parameter sets) and have better performance in all aspects than the finer DEMs setups." **(P18, L13-19)**

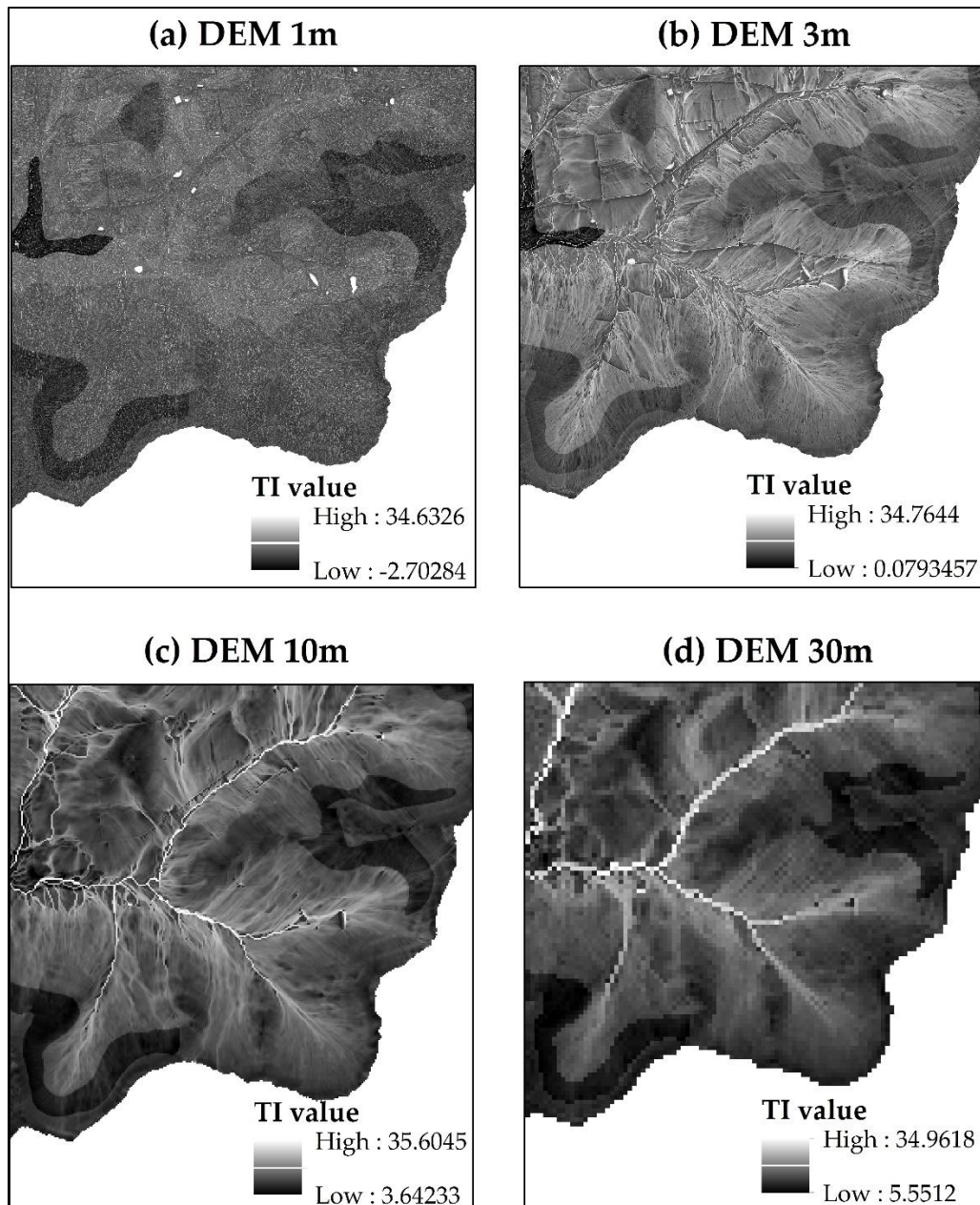



Figure 5: Distribution of topographic index values using different DEMs

Comment

P12, L24-25: *"The probability of saturation, which indicates the number of days in the calibration period when the wetness class is saturated, showed no significant difference among the four setups indicating that DEM resolution does not have an impact on the probability of saturation."*

Again, the real question here is why? If you look at your mapping of saturated areas against observed, it seems the coarse DEM does better (fig 8). Why is this? Is this related to your

choice of dividing up the classes of the wetness index? I think this is a somewhat unexpected result

Response

➔ We tried to keep the areal percentage of each wetness class approximately similar in the four setups using different DEMs. The '*good*' parameter sets in 4 setups should give comparable predictions of streamflow, percentage of watershed area that is saturated, and the time that each wetness class is saturated. That is the reason why Figure 6 shows that the probabilities of saturation for each wetness class are similar in 4 setups.

In the revised manuscript, we added the following text to clarify this issue:

"…Applying the same procedure of wetness class division using four DEM resolutions, four SWAT-HS setups have approximately similar areal percentage of each wetness class." **(P8 L13-15)**

"…It is important to note that we tried to keep the areal percentage of each wetness class approximately the same in the four setups using different DEMs. The *'good'* parameter sets in four setups should give comparable predictions of streamflow, percentage of watershed area that is saturated, and the time that each wetness class was saturated, which results in similar probability of saturation." **(P13 L11-15)**

Comment

Section 3.2.2: I would just delete this section. You have just argued that the only difference in saturated areas is due to the DEM so using the same DEM would never give you different saturated areas? I guess this just points to the fact that your wetness index layer is overriding other effects (i.e. is much stronger than any land use and soil effects). But you can deal with that in one sentence, not a whole paragraph.

Response

➔ We agree with the reviewer. We replaced this section with one sentence in the revised manuscript as:

"All nine setups use the same DEM with 10m resolution and have the same distribution of wetness classes; therefore, the distributions of their predicted saturated areas, are similar and thus are not shown here." **(P16 L8-10)**

Comment

P16, L2-4: "*Similar to the comparison of four setups using different DEMs, the nine setups with different degrees of complexity produced different numbers of good parameters for streamflow and saturated areas, but were similar in the shape of their distributions and value ranges.*"

Are these distributions very different from the DEM distributions? Not really, so why not? Why would two quite different input variations give you similar parameter distributions? You say *latb* is sensitive, but this again seems to point to an overriding effect of your wetness index distribution on the overall results. This is basically masking any other behavior. I think that is a worry. Or maybe a good thing and actually a reflection of your physiography and climate??

Response

➔ Yes, the parameter distributions in nine setups using different soil/land use complexity and four setups using different DEMs are similar. We think this is because we run sufficiently high number of Monte Carlo parameter sets that has good coverage of parameter space. Although different inputs result in varied number of good parameter sets, the numbers of 'good' parameter set in all setups are also sufficient to represent the distribution of 'good' parameter which reflects their sensitivities to hydrological prediction.

As discussed previously, *Latb* is a sensitive parameter because our watershed is dominated by lateral flow. This was supported not only by our results of the contribution of different flow components but also supported by field results (Harpold et al., 2010) in the watershed.

In the revised manuscript, we added the following text to clarify this issue:

"The number of randomly generated Monte Carlo parameter sets is sufficiently high to give a good coverage of parameter space. Although different inputs result in varied numbers of "*good*" parameter sets, those numbers in all setups are adequate to represent the distribution of '*good*' parameter which reflects their sensitivities to hydrological prediction." **(P22 L19-23)**

Comment

P16, L13-14: "*However, after calibration, the effect of DEM resolution on the uncertainty of streamflow prediction is very minor.*"

I agree, but....You found that in the Monte Carlo, the finer DEM had fewer "good" results. I still would like to know why! This is unexpected isn't it? Or does this have to do with how the wetness index is implemented?

Response:

➔ Our response to this comment is same with the response to your previous comment and was addressed in the revised manuscript. Finer DEMs had fewer good results because they do not capture the physical representation of the landscape as well as the coarser DEMs.

Comment:

P16, L18-19: *"These studies found that discharge was simulated equally well irrespective of DEM resolution as long as parameters are calibrated properly."*

See earlier, I think this is not actually a good thing. This just means the model is flexible enough to "recover" from a bad input layer. But does this actually mean we get the "right answer for the right reasons"

Response:

➔ We agree with the reviewer. This sentence is based on the review of the previous studies, and we had the same finding in the study. However, we do not think it is a concern. It means that we need to use more than one type of observations in evaluating the model performance. That is the reason why in our study, we compared the four setups based on not only calibrated streamflow but also the streamflow results without calibration (considering all random parameter sets) and predictions of saturated areas.

Comment

P17, L3-8: "*In our analysis of effect of DEM resolution on topographic characteristics, we observed that the statistical distribution of TI is very sensitive to DEM resolution (Fig. 3d), which results in considerable differences in spatial distribution of wetness classes (Fig. 4). This explains why the distribution of simulated saturated areas by SWAT-HS is also very sensitive to DEM resolution.*"

So this is a concern isn't it? In the end the spatial distribution would affect water quality estimates and other things that you might want to simulate. Basically anything other than simply streamflow.

Response

→ We do not think this is a concern, but tells us why it is important to identify the right DEM resolution for both streamflow and saturated area prediction.

SWAT-HS predicts saturation-excess runoff and saturated areas based on the classification of wetness classes. The classification of wetness classes is based on values of topographic index which is calculated from DEM. Therefore, it is logical that the distribution of simulated saturated areas by SWAT-HS is also very sensitive to DEM resolution. We are aware that the distribution of simulated saturated areas will control the prediction of water quality. This is actually our motivation to conduct this study to ensure that we use the appropriate DEM before applying SWAT-HS to predict water quality.

Comment

P17, L13-15: *"Therefore, DEM10m is the preferred choice to scale-up the application of SWAT-HS to larger watersheds in the New York City water supply system for future applications."*

OK, that is very specific. So what is the general knowledge that we can gain from your test, or are you just doing a sensitivity analysis of your specific model?

Response

→ As stated earlier, our results are applicable to watersheds with similar land use, topography, and climate. However, similar/independent work is needed in other regions using the methodology described in this paper.

Comment

P18, L7-10*: "The difference in scale of case studies (field scale vs. watershed scale) and characteristics of case studies (agricultural fields vs. a mixture of forest and agriculture) between Buchanan et al. (2014) and our study may have resulted in different conclusions on choice of the appropriate DEM resolution."*

Ok, so this is the interesting stuff, and I think you need to spend more discussion on this point. It is not that interesting to argue why this is the "right" DEM, more interesting is how your "best" DEM relates to the local topography and therefore the "right" DEM can be chosen a-priori? You suggest some relationship between topography and variation of land use? How can we generalize this? What type of research would be needed to identify this? You just

quote other case studies, are you saying there is no literature that looked at DEM resolution versus scale or spatial variation?

Response

➔ Based on our responses to your previous comments, we think that the sensitivity of DEM resolution depends on the scale and characteristics of the watershed. The dominant hydrological process in the watershed has a big impact on the sensitivity of DEM on hydrological prediction. For example, in our watershed, lateral flow is a dominant flow component and saturation-excess runoff is a dominant type of surface runoff, thus, topography is the most important factor. Therefore, the DEM that represents a realistic distribution of topographic index (TI) with high TI area compatible with the main stream network gave a better model performance. In a field scale watershed, finer DEM is probably better because it can capture a more detailed and realistic representation of TI distribution. In an agricultural area dominated by tile drainage, DEM resolution may not be sensitive.

Some of these questions about relationship between topography and variation of land use) are interesting but beyond the scope of this study. Topography is certainly related to land use type. For example, in this watershed, agricultural areas are located in the downslope, near stream areas which has high topographic index while forest is mostly distributed in the upland. We will consider researching on this subject in our future study.

Comment

P18 L24-27: "*However, with proper calibration, all nine models are able to provide good performances and their "good" parameter sets continue to perform equally well in the validation period. In addition to streamflow, all nine setups are able to capture saturated areas correctly on specific*"

Again this does not answer the question whether in your specific watershed the wetness index dominates over the other layers? I think this really needs to be discussed and I think that is what happens (based on your results). But that kind of makes your tests slightly irrelevant...

Response

➔ As our response to your previous comment, SWAT-HS is a topography based hydrological model. Therefore, the wetness class map dominates over the other layers. However, we do not think that this makes our test of comparing setups with different soil and land use complexity irrelevant. This test helps us to learn that DEM is the most

important input for SWAT-HS and it is very important to choose the appropriate DEM resolution. The importance of soil and land use information is not as significant as DEM in hydrological modeling, but it will have a considerable impact when we use the model to simulate water quality.

Comment

P19, L1-3: *"We conclude that increasing spatial input details does not necessarily give better results for streamflow simulation as long as the model is properly calibrated."*

That is a bit of a cop out. That is saying: oh well I have enough parameters to fiddle with, so I can just feed the model rubbish. I don't think that is really the point. You have only compared to streamflow and saturated areas, but what about other variables (Actual ET, water quality?). So if you just are predicting water quantity, then yes maybe your statement is true given the large number of parameters that you can manipulate in SWAT, but it is not saying that the model is then a good representation of the catchment.

Response

➔ As mentioned above, SWAT-HS uses topographic index as the basis of hydrological modeling. Therefore, from our results, DEM resolution is more important input than soil and land use. This is the reason why we did not see significant differences from nine setups with different degrees of complexity when the appropriate DEM (DEM10m in this case study) was already used.

In the revised manuscript, we added a paragraph discussing about the importance of soil and land use information on hydrological predictions compared to the importance of DEM resolution in the Discussion section **4.2 What is the appropriate complexity of the distributed soil and land use inputs?** The edit is as below:

"In comparison with the effect of DEM resolution, the importance of soil and land use information is not as significant in the prediction of both streamflow and saturated areas. As our studied watershed is a rural area and dominated by saturation-excess runoff, topography and the wetness conditions of areas in the watershed are more important than land use in water quantity modeling. Moreover, SWAT-HS uses topographic index as the basis for hydrological modeling, thus, the effect of DEM resolution on hydrological predictions is dominant. Therefore, when the appropriate DEM resolution is used, soil and land use information become less sensitive to hydrological predictions. We think that this finding is applicable to watersheds that SWAT-HS is suitable to be used, i.e. watersheds dominated by saturation-excess runoff. This finding may be also valid in applications of other topographic index based watershed models including:

TOPMODEL (Quinn and Beven, 1993;Beven and Kirkby, 1979), SWAT-VSA (Easton et al., 2008), SWAT-WB (White et al., 2011). These results will not be applicable in water quality modeling. Since land use information controls the inputs of nutrients and information of other human activities that affect water quality, the water quality prediction is expected to be very sensitive to the details of land use." **(P21 L25 – P22 L12)**

Comment

P19, L7-8: *"It should be noted here that in this paper, hydrological response is the main focus of this study, and streamflow may not be very sensitive to the details of land use."*

Indeed, water quality would be much more sensitive.

Response

➔ Yes, we totally agree with the reviewer. Land use information controls the inputs of nutrients and information of other human activities that affect water quality, therefore, the water quality is expected to be very sensitive to the details of land use.

Comment

P19, L13-16: "*Petrucci and Bonhomme (2014) show that the inclusion of some basic geographical information, particularly on land use, improves the model performance, but further refinements are less effective."*

This really depends on what other info is available as well. As I have indicated in your case I think wetness dominates and therefore land use is less important for water quantity.

Response

➔ We agree with the reviewer. Petrucci and Bonhomme (2014) conducted their study in a small residential area where the correct estimate or identification of impervious cover and path for surface water are very important for modeling. Therefore, including these information in the model setup helped to improve model performance.

We think that the importance of input data depends on the characteristics of the case study and the aspect that we want to model. Our studied watershed is rural and dominated by saturation-excess runoff. Therefore, topography and the wetness conditions of areas in the watershed are more important than land use in water quantity modeling. However, in water quality modeling, the importance of land use information which controls the input of nutrients is expected to be significant.

Comment

P19, L17-19: *"Finger et al. (2015) compared different setups with increasing detail in input information using the HBV model and three observational data sets. They found that enhanced model input complexity does not lead to significant increase in overall performance,…"*

In water quantity?

Response

➔ Yes, the study of Finger et al. (2015) focuses on water quantity evaluation. We clarified this by editing the text as:

"Finger et al. (2015) compared different setups with increasing detail in input information using the HBV model and three observational data sets. They found that enhanced model input complexity does not lead to significant increase in overall performance in water quantity,…" **(P21 L17-20)**

Comment

P19, L21-23: *"Muleta et al. (2007) also showed that streamflow simulated by SWAT is relatively insensitive to spatial scale when comparing multiple watershed delineations from different soil and land use input data details."*

Again, just water quantity??

Response

➔ Yes, we quoted this reference for its result on the sensitivity of streamflow to spatial scale of input data. In our text, we wrote "*streamflow simulated by SWAT*", so we mean water quantity here.

Muleta et al. (2007) also studied the sensitivity of sediment yield to soil and land use input data details and found that sediment generated and sediment that leaves the watershed decreases as spatial scale gets coarser. However, in our study, we focus on only hydrology, therefore, we did not refer to the sediment result.

Comment

P20, L3-9: *"Therefore, we conclude that for this case study and the particular model SWAT-HS, using higher resolution DEM or adding complex information on soil or land use did not reduce parameter*

uncertainty or solve the equifinality problem. This statement may not be valid for other areas that are characterized by numerous land use and complex variations in topography and soil types. This is also not valid for physically based models which require detailed soil and land use information and a minimum number of parameters for calibration."

So what did we learn that is not "just for your case study"? I think this is important to highlight for HESS

Response

➔ What we learned from this study is that, hydrological prediction is very sensitive to choice of DEM (with higher effect on prediction of saturated areas than streamflow), when using a hydrologic model that uses topographic index as the basis for hydrological modeling, to simulate hydrology in a watershed that is dominated by saturation-excess runoff. Besides SWAT-HS, other examples of watershed models using topographic index are: TOPMODEL, SWAT-VSA, and SWAT-WB. With SWAT-HS and models that are based on topographic index in general, DEM resolution is more sensitive than the complexity of soil/land use information. When the appropriate DEM resolution is used, soil and land use information become less sensitive to hydrological predictions. Also our experience shows that with appropriate model input (using DEM10m and intermediate soil and land use complexity), model run-time can be reduced which makes calibration procedure faster.

This response is alike to our previous response to one of your similar question, and was included in the **Summary and conclusions** in the revised manuscript. **(P24 L18-25)**

Comment

P20, L16-17: "Our study is not aimed at solving the equifinality problem, but rather reduces the number of solutions considered when using SWAT-HS to predict streamflow and water quality for decision-making."

I did not see any water quality results, so I think this over reaching.

Response

➔ We agree with the reviewer, we removed the "water quality" part in this sentence. We will revise the text in the revised manuscript as:

"Our study is not aimed at solving the equifinality problem, but rather reduces the number of solutions considered when using SWAT-HS to predict streamflow." **(P23 L8-9)**

Comment

Reference Hoang et al. (2017) repeated twice

Response

➔ We removed the extra reference in the revised manuscript

RESPONSES TO REVIEWER 2

Comment

In this manuscript, Hoang et al. evaluated the effect of input resolution (digital elevation model) and input complexity (number of soil and land use classes) on model output uncertainty of the SWAT-HS model. Model output uncertainty is evaluated in terms of streamflow, saturated areas and parameter uncertainty. They conclude that uncertainty does not necessarily decrease when increasing input resolution or complexity. However, selecting parameter sets based on the combined information on streamflow and the spatial extend of saturated areas positively affected uncertainty.

This is an interesting study and I like the clear and well described concept. The results are illustrated and described in detail for different model outputs and clearly support the conclusions. The main results are well discussed. To further improve the manuscript, I have some suggestions listed below. Major comments address the potential calculation of additional streamflow criteria, the calculation of a complementary measure for the saturated areas, or a figure showing simulated hydrographs. I hope that the comments below will be helpful for the authors to improve their manuscript.

Response

➔ Many thanks for the positive comment about the manuscript. Your comments are very helpful for us to improve the manuscript. We tried our best to respond to every comment and revised the manuscript based on our responses to your comments.

1. MAJOR COMMENTS:

Comment

P L4: The model you used in this study is called SWAT-Hillslope. For me the word hillslope implies that one is working at the hillslope scale of an undisturbed catchment. However, you use the model at a much larger scale and for a catchment that is probably highly influenced by human use (urban areas and lots of agricultural area). I think it would be helpful if you shortly reflect on that and give the reader a good reason for using SWAT-HS.

Response

➔ "Hillslope" in SWAT-Hillslope does not mean hillslope scale, but means hillslope hydrology that describes paths of water through hillslope into streams. The standard SWAT does not have the ability to represent hillslope hydrology because there is no interaction between modelling units (called Hydrological Response Units, HRUs) in a

subbasin. In SWAT-HS, we enabled the interaction in flow and substance transport between upland areas and the valley bottom by creating a surface aquifer. More details can be found in (Hoang et al., 2017). We will also provide a general description of SWAT-HS in supplementary materials that will be attached with the revised manuscript.

In the revised manuscript, we edited the text to clarify the meaning of hillslope in SWAT-HS as:

"SWAT-Hillslope (SWAT-HS) (Hoang et al., 2017) is a modified version of the Soil and Water Assessment Tool (SWAT) that improves the simulation of saturation-excess runoff and creates interaction in flow and substance transport between the upland areas and the valley bottom." **(P3 L3-6)**

Comment

P5 L25-P6L8: It would be interesting to have some more information or numbers about human disturbances within the catchment: are there any major water withdrawals for agricultural use? Is there a reservoir that is used to guarantee the drinking water supply for NY in dry spells? How are these human influences affecting your model assumptions, such as a closed water balance?

Response

➔ We added some information about the Cannonsville Reservoir that the Town Brook watershed is part of the drainage area. There is no major water withdrawal for agricultural use. There is also no effort to change the hydrology in this watershed. There are a lot of activities on watershed protection programs on farms area implemented by New York City Department of Environmental Protection (NYCDEP)that helps to improve water quality, particularly phosphorus, such as cattle fencing in pastures, manure storage installation, upgrades of WWTPs. Because this manuscript only focuses on hydrology, we think it is not necessary to add this information. We will provide this information in our upcoming paper.

We revised the text in our manuscript as:

"The 37 km^2 Town Brook watershed is located in the Catskill Mountains, Delaware County, New York State (Fig. 1) and is the headwater of the Cannonsville Reservoir watershed which is the one of four reservoir watersheds in the New York City's Delaware system." **(P6 L3 – P6 L5)**

Comment

P6 chapter 2.2: I am not very familiar with the SWAT model and when reading the second paragraph of chapter 2.2 it was not clear to me how the structure or the combination of subbasins, wetness classes and HRUs look like. Would it be an option to include a schematic of the model structure to visually support what you are writing?

Response

➔ Thank you for the comment that reminds us about the need to provide sufficient information to non-SWAT modelers. We provided a more detailed description of SWAT-HS setup which include maps of input data in the **Supplementary Materials** attached with the revised manuscript (see the **Supplemental Materials**).

Comment

P9 L2: I like the idea of using the principle of GLUE the select behavioral parameter sets. However, I am not sure if I would agree in using a Nash-Sutcliffe efficiency of 0.65 as a threshold for good simulations. How do you know that 0.65 is a good model result for your catchment? Nash-Sutcliffe is known to be high in catchments with a high discharge variability and model efficiencies also tend to be better for humid catchments than for dry catchments. Shouldn't good efficiencies for a catchment like yours be around 0.8 (I know this is a bit provocative)? Why did you decide to take a fix efficiency threshold and not just the best 10?

Response

➔ Figure 5a and figure 10a show the maximum daily NSE values in all our SWAT-HS setup which range from 0.68 to 0.69. Therefore, it is impossible to set the threshold at 0.8. From our results, daily streamflow predictions with daily NSE higher than 0.65 results in monthly streamflow prediction with monthly NSE higher than 0.8. Based on guidelines for model performance evaluation by Moriasi et al. (2007) which suggested that good model performance for streamflow corresponding to monthly NSE higher than 0.75. Therefore, we are confident that our choice of daily NSE higher than 0.65 as good model performance is a reasonable choice.

We decided to take a fixed efficiency threshold and not just the best 10 parameter sets in each setup because we would like to know how many parameter sets in each setup can give good performance above the threshold. The ratio of the number of good parameter sets of a setup to the total number of Monte Carlo parameter set (10,000 in this case) can

tell us the probability of the setup to get a good model performance, which we use as one
of the criteria to compare the setups.

Comment

*P9 L2: Linked to using GLUE: it would be interesting to also see the simulated and observed
hydrographs with the confidence intervals.*

Response

➔ As the reviewer suggested, we added the two figures below, which show the comparison
between observations and simulated streamflows from SWAT-HS setups with 90%
confidence intervals, in the *Supplementary materials* that will be attached with the revised
manuscript. We chose to show the comparison by plotting flow duration curves because
it is very difficult to see in daily streamflow plots.

We mentioned and discussed about these two figures in the revise manuscript as:

"The comparison between observed flow and 90% prediction uncertainty measured between 5th
and 95th percentiles of predicted flows from "*good*" parameter sets is shown in Figure S3 in the
Supplementary Materials." **(P12 L11-13)**

"The uncertainty ranges of predicted flows, particularly intermediate flows are wider in the finer
resolution setups (Fig. S3) although uncertainty bounds capture very well observations in four
setups." **(P12 L123-25)**

"This is also reflected in Figure S4 (Supplementary Materials) showing that all setups capture
measured streamflow well within their uncertainty ranges with TB3 being the poorest setup with
the widest uncertainty range." **(P15 L23-25)**

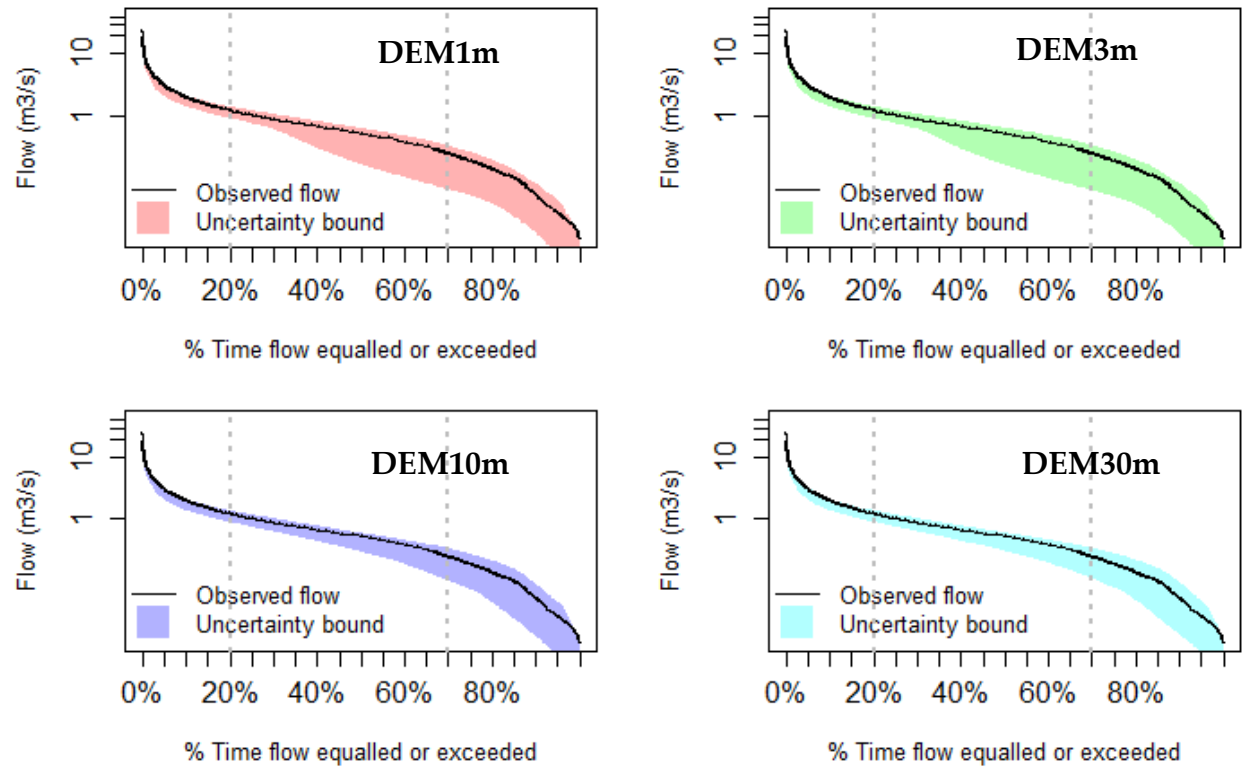



Figure 1: Uncertainty in streamflow predictions by SWAT-HS using different DEM resolutions

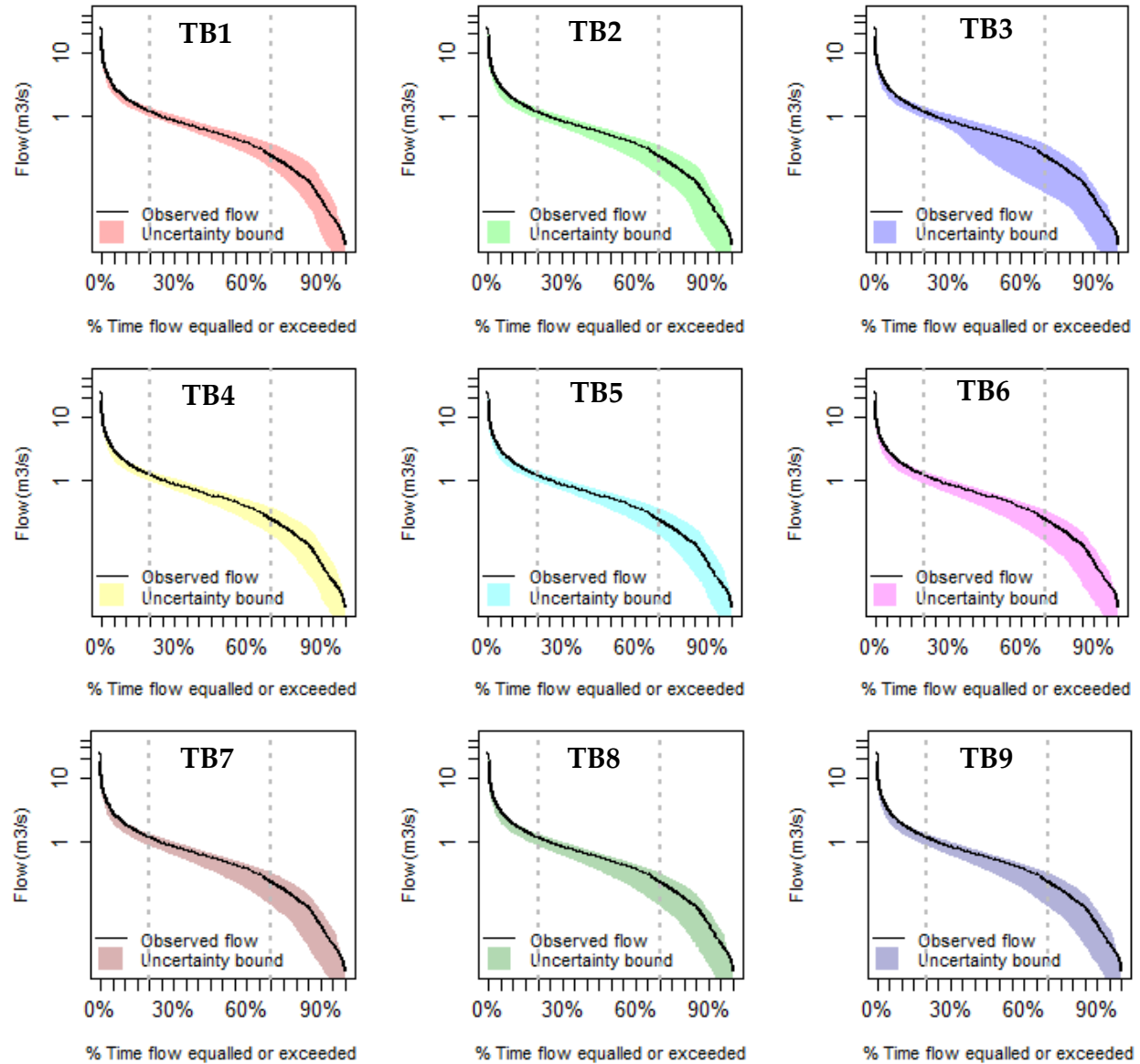



Figure 2: Uncertainty of streamflow predictions by SWAT-HS using different degrees of complexity

Comment

P9 L5: Your final goal is to make some recommendation about the appropriate model input complexity and resolution based on streamflow, saturated areas and parameter uncertainty. Evaluating the streamflow simulations on a single efficiency measure has therefore a strong impact on your final recommendations. After all the discussion on the use of Nash-Sutcliffe (e.g. Do Nash values have value from Schaefli and Gupta, 2007; Decomposition of the mean squared error and NSE performance criteria: Implications for improving hydrological modelling from Gupta et al., 2009) I would suggest

that you calculate one or two additional efficiency criteria for streamflow simulations. This could for example be a criteria representing low flow or discharge volume.

Response

➔ Thank you very much for the reviewer's comment. We agree with your suggestion. Therefore, we calculated two additional efficiency criteria: (i) NSElog: logarithm of Nash Sutcliffe Efficiency which is a good indicator for low flows, and (ii) KGE: Kling Gupta Efficiency. We added the calculated values in table 3 and 4. We showed our edits in table 3 as an example here.

Table 3: Statistical criteria to compare the effect of DEM resolution on model uncertainty

		DEM1m	DEM3m	DEM10m	DEM30m
Calibration period: based on 10,000 Monte Carlo parameter sets					
Number of *"good"* parameter sets (%) for streamflow		1362	1890	2180	2293
Number of *"good"* parameter sets (%) for both streamflow and saturated areas		27	49	66	67
NSE	Max	0.69	0.69	0.69	0.69
	Mean	0.09	0.05	0.33	0.34
NSElog	Max	0.82	0.82	0.82	0.83
	Mean	0.43	0.41	0.56	0.59
KGE	Max	0.81	0.81	0.81	0.81
	Mean	0.53	0.53	0.59	0.59
Validation period: based on "good" parameter sets from calibration					
NSE	Max	0.66	0.66	0.66	0.66
	Mean	0.60	0.62	0.62	0.62
NSElog	Max	0.82	0.82	0.82	0.82
	Mean	0.70	0.70	0.69	0.71
KGE	Max	0.79	0.78	0.79	0.79
	Mean	0.70	0.70	0.70	0.71

Comment

P14 L1: If I understand correctly, you evaluate the model simulations by the percentage of simulated areas that intersect with the observed areas (purple color in Fig. 8). This corresponds to my

interpretation to the percentage of correct classifications. To me it seems logical that the DEM30m performs best, because it cannot be too wrong due to its coarse resolution. Therefore, I think that evaluating the percentage area of misclassification (percentage of simulated area that does not intersect with the observation green color in Fig. 8) would give additional and important information for the evaluation of the various DEM resolutions.

Response

➔ We confirm that the reviewer understood correctly that we evaluated the model simulation by comparing the percentage of simulated areas that intersects with the observed areas. We also think that it is logical that DEM30m has the highest percentage due to its coarse resolution. However, the most important reason is that the coarse resolution DEMs (DEM 10m and DEM 30m) gave a realistic distribution of topographic index (TI) values with the high TI grids well compatible to the stream network. Thus, with the classification of wetness classes based on TI values, coarse resolution DEMs also provide a better distribution of wetness classes with the highest TI wetness classes (which supposed to be 'wet') locating in the downslope, near-stream areas while the lowest TI wetness classes ('dry' wetness classes) being in the upslope areas.

In our opinion, we would like to focus on the percentage of simulated areas that intersects with the observed areas (the correct classification) rather than the percentage that does not intersect with observation (the misclassification) to evaluate the model simulation. We think that the correct classification reflects the model performance which we are evaluating. The misclassification is easy to calculate by deducting the correct classification from 100%.

Comment

P18 L2: In the discussion you provide some good reasons why a relatively coarse DEM resolution (DEM 10) can lead to good/ acceptable results. However, I don't understand why a better resolution does not result in even better results. Could you maybe write a few sentences that elaborate on that?

Response

➔ Same as our response to your previous comment, a relatively coarse DEM resolution (DEM10m) can lead to a better performance. It gives a more realistic distribution of topographic index (TI) values which results in a better distribution of wetness classes. The reason is explained as below.

Note that the basic equation for topographic index is TI = ln (contributing area/slope angle)

The below figure shows the relationships of TI with slope angle, upslope contributing area and elevation using 2 representative DEM resolutions: 1m and 10m. It is clearly observed that DEM 1m can capture a significantly wider range of slope than DEM 10m because of its finer resolution. Also, the percentage of grids that has low values of TI is significantly higher in DEM1m than in DEM10m (in figure below use red lines for reference), which also can be seen in figure 3d in the main manuscript. Low TI values are usually found in grids with steep slope or with low upslope contributing areas. Because DEM 1m captures steep slope at local scale and has a high number of grids with low upslope contributing area (figure 3c in the main manuscript), the percentage of low TI values in DEM 1m is much higher. If we look at the relationship between TI and elevation, we can see that the distribution of TI values in DEM 1m spread out wider than in DEM10m at all elevations. This explains why the distribution of wetness classes in DEM1m has a more complex pattern with every wetness class spread-out while DEM10m has a more coherent pattern with high TI wetness class well compatible with the stream network (Figure 4 in the main manuscript).

Our findings are in agreement with Lane et al. (2004) who used high resolution LiDAR 2m DEM in TOPMODEL which simulates hydrology based on topographic index. The TOPMODEL predicted the widespread existence of disconnected saturated zones that expand within an individual storm event but which do not necessarily connect with the drainage network. They found that using the LiDAR 2m DEM, the topographic index has a complex pattern, associated with small areas of both low and high values of the topographic index, leading to the appearance of disconnected saturated areas. After remapping the topographic data are remapped at progressively coarser resolutions by spatial averaging of elevations within each cell, they found that as the topographic resolution is coarsened, the number and extent of unconnected saturated areas are reduced: the catchments display more coherent patterns, with saturated areas more effectively connected to the channel network. Moreover, in another study, Quinn et al. (1995) showed how progressively fining model resolution from 50 m to 5 m reduces the kurtosis in the distribution of topographic index values and increases quite substantially the number of very low index values.

Figure 3: Relationship of topographic index with slope, upslope contributing area and elevation with two resolutions of DEM: 1m and 10m *(the red lines are used as reference lines to compare the two DEM resolutions)*

In the revised manuscript, we added the text in this response to the **Discussion section 4.1. What is the most suitable DEM resolution to use in SWAT-HS?** Because the edits are almost same with the text in the response, we did not show the text here. Please see **section 4.1 (P18 L1 -P19 L7).**

Comment

Table 3 and 4: I agree that the number of parameter sets and the median efficiency values are interesting. However, I would recommend to add the values directly to the corresponding figures to have the information where it is relevant. I think that max and min efficiency values can also be seen/ guessed from the figures and are not that important that they need to be in a table.

Response

➔ Based on your previous suggestion to add more efficiency criteria, we added the values of maximum and mean NSElog and KGE in these two tables. We removed median NSE because the readers can read those values from figure 5 and 10. **(see Table 3 and 4)**

2. MINOR COMMENTS:

Comment

P1 L1: After reading the manuscript I would suggest to adapt the title, because is only addresses part of the actual analysis. I am also not sure if the study catchment can be considered as mountainous. So maybe the title could be adapted to something similar as: Effect of input data resolution and complexity on simulation uncertainty for a simple runoff model.

Response

➔ Thank you for the comment. We will revise our title as: "**Effect of input data resolution and complexity on the uncertainty of hydrological predictions in a humid, vegetated watershed**

Comment

P1 L11: I would be careful with using the term "water quality" in the very first sentence of the abstract as it suggests that the study is about water quality, which is not the case.

Response

➔ We agree with the reviewer. In the revised manuscript, we removed the term "water quality" as:

"Uncertainty in hydrological modeling is of significant concern due to its effects on prediction and subsequent application in watershed management." **(P1, L12-13)**

Comment

P2 L4: The nine model setups not only had a similar effect on parameter uncertainty, but also on streamflow simulation.

Response

➜ In this sentence, we mentioned about the effect of input data complexity first while the following sentence is about the effect on streamflow simulation. Therefore, we did not make any edit regarding this comment in the revised manuscript.

Comment

P2 L7: The term spatial input details was a bit confusing to me. I would rather use the same terms as before: input resolution and complexity.

Response

➜ We changed the term "increasing spatial input details" to "improving input resolution and complexity" **(P2 L8)**

Comment

P2 L16-17: Is data used to calibrate the model (e.g. discharge data) included in your list? For me it would be a fourth point called output data uncertainty.

Response

➜ In this study, we do not consider measurement error as uncertainty. Model uncertainty from the three components (model structure, input data and model parameters) that we mentioned are caused by assumptions or simplifications made for model structure, averaging impact or lack of data in input data and the effect of equifinality on model parameters. We assumed that all available measurements for input data and data used for calibration are true. Therefore, measured data uncertainty is not in the list. Moreover, this categorization of model uncertainty is not our own but based on Lindenschmidt et al. (2007).

Comment

P5 L12-22: I recommend to reorganize this paragraph. Having two listings in a row makes it more difficult to understand what the focus of your study is.

Response

➜ We reorganized the paragraph and hope the focus of the study is clear now.

The text was edited in the revised version as:

"The two main objectives of this paper are to evaluate: (i) the effect of DEMs of various spatial resolution (1, 3, 10, and 30 m) on the uncertainty of streamflow and saturated area predictions, and (ii) the impact of combinations of soil and land use data with various degrees of complexity on the uncertainty in model simulation. In both analyses, we not only investigate the effect on model prediction/output uncertainty but also discuss their effect on the uncertainty in parameter estimation. Through this study we seek to answer specific questions including identifying the suitable DEM resolution in order to get good model performance, and the appropriate complexity of the distributed input data. Answers to these research questions will be the basis for reducing decision uncertainty on model input selection in our future applications of SWAT-HS in the NYC water supply system." **(P5 L14-23)**

Comment

P7 L4: I recommend to mention the concept of "hydrological connectivity", which is the argument for you lateral surface aquifer.

Response

➔ We agree with the reviewer. We provided a more detailed description of SWAT-HS which includes the explanation of the 'hydrological connectivity' concept in more detail as supplementary material attached to the revised manuscript. **(see Supplemental Materials)**

Comment

P7 L20: Please add the reference for the LiDAR data.

Response

➔ We added the reference for the LiDAR data in the revised manuscript as:

"The 1m DEM (DEM1m) was derived from 2009 aerial LiDAR data acquired by New York City Department of Environmental Protection (RACNE, 2011)." **(P7 L26-P8 L2)**

Comment

P7 L25. I would refer to Figure 4. (": : :divided into 10 wetness classes (Fig. 4))

Response

➔ We edited the text as:

"Based on TI values, the watershed was divided into 10 wetness classes (Fig. 4)." **(P7 L27 – P8 L1)**

Comment

P8 L 9: Please add the reference for the solar radiation data.

Response

➔ We added the reference for the solar radiation data and revised the text as:

"Solar radiation data was derived as the average of airport stations at Albany and Binghamton as supplied from the Northeast Regional Climate Center." **(P8 L19-20)**

Comment

P8 L21: Using a model with a snow routine I would mention the percentage of precipitation falling as snow in the section where you describe the catchment.

Response

➔ We added the information of percentage of precipitation falling as snow in the case study description as:

"Approximately 1/3 of the total precipitation in the region fall as snow (Pradhanang et al., 2011). **(P6 L7-8)**

Comment

P8 L19: Why do make 10'000 MC runs and not 100'000? Do you think your parameter distributions would look differently with more random parameter sets? Please give some reasons for your choice.

Response

➔ The calibration procedure in this manuscript is based on our previous study (Hoang et al., 2017). Our calibration procedure includes 2 stages: snowmelt calibration (5 parameters) and flow calibration (9 parameters among which 1 parameter, ALPHA_BF, is not very sensitive based on our previous study). In each stage, we generated 10,000 parameter sets which were run with SWAT-HS and the results were compared with observations.

We assume that we generate 1 random sample in at least high, middle, and low range for each parameter. In the stage of snow melt calibration, with 5 parameters involved, $3^5 = 243$ is the minimum number of MC parameter sets required to cover the parameter space.

In the flow calibration stage, with 8 sensitive parameters involved, 6561 combinations are the minimum number required. Therefore, we think that using 10,000 MC parameter sets for each stage of calibration is sufficient.

Comment

P10 chapter 3.1.1: You could think about moving this chapter to the methods part.

Response

➔ We would like to keep this section in the *Results* part because this is a part of our analysis which is the basis to explain the effect of DEMs on prediction of saturated areas. In the *Methodology* part in the revised manuscript, we mentioned about this work as:

"We evaluated the effect of DEM resolution on representing topographical characteristics of the watershed by comparing the statistical distributions of elevation, slope angle, upslope contributing area, and TI using DEMs with various spatial resolutions (1m, 3m, 10m and 30m)." **(P9 L8-9)**

Comment

P13 L7: Could you briefly explain what the percentage of saturated areas is? It would then also become clearer what / how many data points the corresponding boxplots (Fig. 7) contain.

Response

➔ The percentage of saturated areas is defined as the percentage of the watershed area that is saturated in the simulated day. The number of data points for each DEM in figure 7 equals to the number of days in the calibration period (2556 days) multiplying the number of good parameters for both streamflow and saturated areas in each DEM setup.

Comment

Fig. 1: Could you increase the resolution of this figure? Because it is not sharp when printing it out on A4.

Response

We provided a high-resolution figure for figure 1 in the revised manuscript **(see Figure 1).**

Comment

Fig. 5 and 10: I recommend to adapt the y axis scales to better use the available space. And I also suggest to use the same style/ content of figure caption for Fig. 5 and Fig. 10.

Response

➔ Thanks to the reviewer for the detailed comment.

We edited the caption for Figure 5a and 5b and the general caption of figure 5 to be consistent with figure 10 (see figure 5). Moreover, we also adjusted the y axis scales of plots in figure 5 and 10 as the reviewer suggested **(see Figure 5 and Figure 10)**.

Comment

Fig. 6: Again, I recommend to adapt the y axis scales to better use the available space. Additionally, I would add the information that only the good parameter sets for both streamflow and saturated areas are used in this plot to the figure caption.

Response

➔ We would like to keep this figure as it is. The reason is that this figure does not only show variations of probability of saturation in each wetness class using different DEMs, it also aims at comparing the difference of these variations between wetness classes. Therefore, we purposely kept similar value ranges for y axis in all plots.

As the reviewer suggested, we changed the caption of figure 6 to: *Probability of saturation of wetness classes in SWAT-HS set ups with different DEM resolutions using good parameters for both streamflow and saturated areas* **(see Figure 6)**

Comment

Fig. 9 and 11: It is almost impossible to see the distributions of the good parameter sets for both streamflow and saturated areas. Why don't you scale the y axis differently?

Response

➔ We agree with the reviewer. We changed the y axis of figure 9 and 11 to logarithmic scale. The new figure 9 is shown below as an example of our edits.

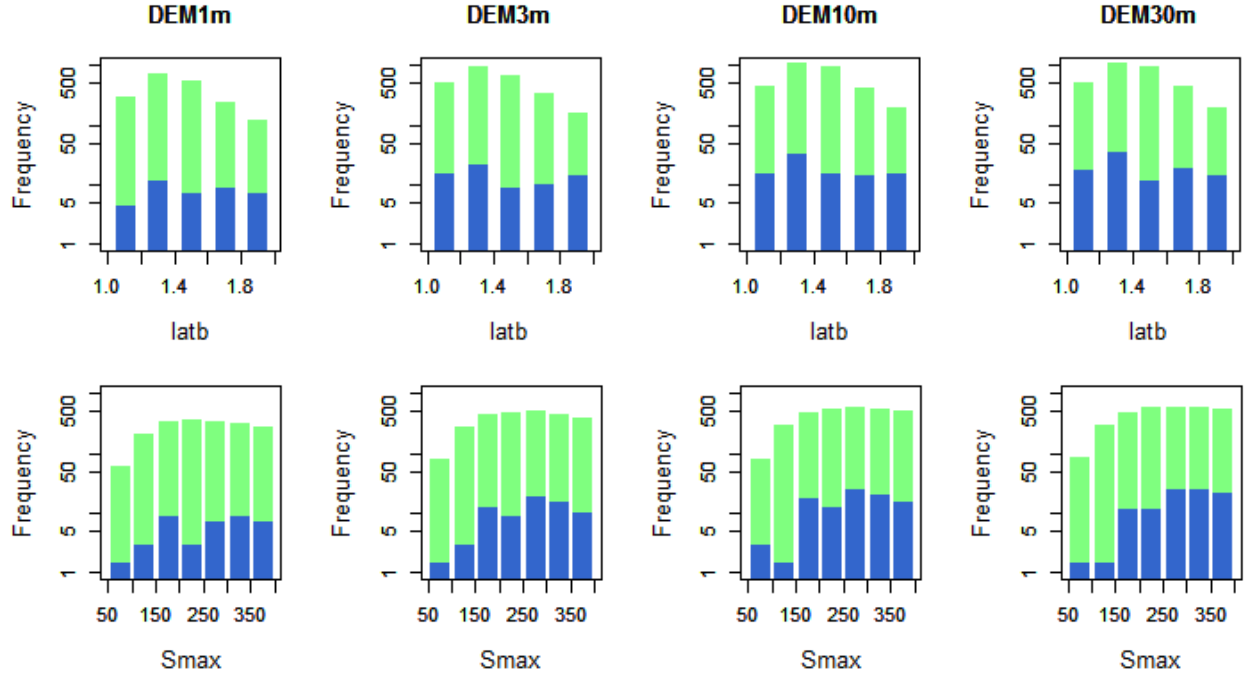



Figure 9: Distribution of *"good"* parameters for streamflow (in green) and for both streamflow and saturated areas (in blue) with log y axis in four SWAT-HS setups using different DEM resolutions

Comment

Table 1: What is the difference between latA and latB? It is not clear since the definitions are identical.

Response

➔ The lateral flow in SWAT-HS is generated as the following equation using the linear (*latA*) and exponential (*latB)* coefficients:

$$\overline{latQ} = latA * \overline{S_1} \wedge latB$$

Where ($\overline{latQ}$) is lateral flow for the sub-basin, $\overline{S_1}$ is the amount of water stored in the surface aquifer, *latA* and *latB* are constant coefficients.

The equation can be found in Hoang et al. (2017). We also added a description of SWAT-HS in the **Supplementary Materials** attached to the revised manuscript. **Please see Supplementary Materials.**

Comment

Generally: I suggest to use the HESS guidelines for making references and also for formatting units.

Response

➔ We edited our manuscript to follow the HESS requirement of units and reference style.

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