Hydrol. Earth Syst. Sci. Discuss., 10, C4788–C4814, 2013
www.hydrol-earth-syst-sci-discuss.net/10/C4788/2013/

Hydrology and Earth System Sciences
Discussions

Interactive comment on "An interdisciplinary swat ecohydrological model to define catchment-scale hydrologic partitioning" *by* C. L. Shope et al.

C. L. Shope et al.

clshope@gmail.com

Received and published: 11 September 2013

Prof. Dr. Volk, Thank you for considering our manuscript. The authors gratefully appreciate your review and insightful comments. They have greatly improved the quality of our manuscript. If you feel that we did not adequately or clearly answer any of your comment, please let us know. We address each of the comments that you provided below.

1) Reviewer Comment: The authors present an interesting study about the potential of using the integrated river basin model SWAT in a mountainous, monsoonal environment in South Korea. Although the general objectives of the study are very good, the authors need to shorten the paper and focus much more on the actual and specfic

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

problems in their study areas and the methodological challenges. I recommend "major revision" for the study – after a sound revision of the paper this could make a valuable contribution to the journal HESS. I have listed my comments and suggestions in the following - I hope that it helps the authors to improve the quality of the paper.

Author Response: We agree that there are areas where the manuscript can be shortened and have eliminated nearly 30% of the original text for the resubmission. We believe that with the shortened text we provide a more concise focus of the catchment problems and the methodological challenges present. We very much appreciate the detailed and insightful comments that have greatly improved the manuscript quality.

2) Reviewer Comment: Title: The current title is confusing. As an integrated river basin model SWAT is interdisciplinary and you do not need to mention that in the title. I would also be careful with using the term "eco-hydrology", since there are several discussions from different sides about its meaning. As options, you could use something like

i) "Using SWAT to define hydrologic partitioning to support river basin management in a South Korean region" or ii) "Improving process description in a South Koeran catchment by using SWAT to define hydrologic partitioning"

These are just a suggestions, you can modify it like you want. In general, the title should express the main message of the study / paper. Think in general about why you used SWAT. Usually, it is used for simulating the impact of land use / land management on hydrology, nutrient- and sediment transport, etc. This is currently not in the focus of your study (might be the base for your next steps). Might be important to explicitly mention your reasons to choose the model in the paper. Have also a look at Ann van Griensvens (et al.) critical reflection of using SWAT in the Nile basin (Hydrol. Earth Syst. Sci., 16, 3371–3381, 2012). It is a different region, but with similar problems.

Author Response: Thank you for your comment. We believe that our use of multiple metrics (statistical, hydrologic, and plant growth) are a novel means to calibrate the model and the wide variety of data collected are interdisciplinary and warrant inclusion

HESSD

10, C4788–C4814, 2013

in this paper. However, for the purposes of this manuscript, we concede that another title might be more descriptive. We have adjusted the title to “Using SWAT to improve process descriptions and define hydrologic partitioning in South Korea”.

As you mention, this study describes the characterization of catchment hydrologic partitioning with little emphasis of land use impacts on nutrient and sediment transport. Land use, nutrients, and sediments modeling information and simulations are being produced and interpreted, which will be used in a future for a manuscript. We also added a few sentences in section 3.1 Model Description that explicitly describe why we chose to use the SWAT model. Thank you for the reference to Dr. van Griensven’s 2012 paper; it reminded us of several items to consider.

3) Reviewer Comment: page 7237 Abstract General comments: The use of the term "ecosystem services" (ESS) is at present very popular. You start your abstract with it, but "ecosystem services" as a (methodological) concept play actually no role in your study. You mention it, but you do not mention explicitly on which services you work nor do you work on trade-offs amongst land use and ESS or between ESS itself (important part of the ESS concept). In addition, what you define as ESS in the first sentence of the abstract are to a large part not ESS: Water provision is a service (includes provision of clean water), water quality and quantity themselves are not. Biodiversity is not regarded as an ecosystem service itself, but rather as a pre-requisite underpinning each of them. You further write then that "to account for future effects on ESS, the integration of physical, biological, economic, and societal data must be implemented..." . Could be that this all will be of growing importance in the TERRECO project that you mention later in the introduction, but it plays yet no major role in the paper. I would suggest to skip the ESS part and start with problems in (East Asian) river basins like the presented study, caused by human impact, (extreme) natural conditions, data scarcity, and the resulting monitoring and modelling challenges to describe the processes. Then you can continue with the objectives. Please check if the mentioned objectives are really your main one - also in that order. - line 19: "The results of this study provide.."

HESSD

10, C4788–C4814, 2013

Please focus on the results (what you "really" investigated in your study and what you present in the paper). Sediment load and nutrients are not (yet) in the focus. - line 24: I would remove "ESS" (see my comments above) and write "...understanding of of their hydroecological impact."

Author Response: Thank you for your comment. The ESS described in this study is access to high quality and yield of water from the stream network for the provision of potable water supply. While we do not explicitly describe the provision of potable water, the quality and quantity aid in the estimation of water provision. This section provides the basis of the research studies completed with the TERRECO Interdisciplinary research and teaching group and we further define how this study uses this data to advance our understanding of the ESS.

However, because this research is not one of the main objectives of the SWAT modeling construction and simulations described in this manuscript, we have decided to remove the references to ESS. We have replaced these sentences at the beginning of the introduction with a description of how spatially distributed meteorology in high precipitation locations with complex terrain is difficult to quantify. We also discuss issues of groundwater and surface water abstraction, engineered water management systems, and forest encroachment leading to increased sediment transport. We then describe the natural and anthropogenic conditions, data scarcity, and how modeling can help.

We believe that the stated objectives are accurate for the presented study and have clarified the statements into a more appropriate order. We 1) assess spatiotemporal algorithm to improve precipitation discretization, 2) characterize spatiotemporal surface water quality throughout the catchment with a unique multi-optimization model, 3) determine capability of SWAT to capture daily rainfall-runoff, and 4) quantify significance of engineered structures on flow partitioning.

For the comment regarding line 19, you are correct that we do not discuss landscape controls on sediment transport and nutrient loading. Therefore, we removed the entire

HESSD

10, C4788–C4814, 2013

sentence as suggested.

We removed the reference to ESS in the last sentence of the abstract on line 24 as suggested.

4) Reviewer Comment: page 7238-7242 Introduction General comments: As already mentioned before, please focus to the actual and specific problems in their study area and the methodological challenges. The introduction is currently very long and includes redundant or partly "trivial" information (you want to publish in the journal "Hydrology and Earth System Sciences"). Either remove the ESS part or make it a stronger part in your study – you need then explicity mention the ESS you work on and also look for the trade-offs and discuss it also at the end of your paper. I would recommend to skip it and focus on the natural conditions / environmental and monitoring - modelling challenges in your area. The strong part of your paper is the attempt to improve the process description by using monitoring and a model - environmental management is not yet really addressed.

Author Response: We believe that we have successfully focused on the actual and specific problems of the Haean catchment and the methodological challenges in simulating hydrologic partitioning. For example, we describe topographical complexities, water abstractions, and modeling challenges and how we overcame these issues with a novel multi-location, multi-metric calibration technique and other approaches.

We have significantly reduced the introduction in particular and the entire manuscript in general. We believe that we have eliminated the redundancy and information that is secondary to the focus of the paper.

As stated in a previous author response, we have removed the ESS references in our manuscript. Per your suggestion, we have focused on the 1) natural and anthropogenic conditions of land use on surface water quality and yield, 2) the specific monitoring framework and implications in the Haean watershed, 3) the importance and challenges of using a simulation model, and 4) our procedure to investigate this.

HESSD

10, C4788–C4814, 2013

5) Reviewer Comment: line 6: "..(Goldstein et al., 2012)". If you are interested in trade-off studies (partly using SWAT) have a look at:

Lautenbach, S., M. Volk, M. Strauch, G. Whittaker & R. Seppelt (2013): Optimization based trade-off analysis of biodiesel crop production for managing an agricultural catchment. Environmental Modelling & Software 48: 98–112

Seppelt, R., S. Lautenbach & M. Volk (2013): Identifying trade-offs between ecosystem services, land use, and biodiversity: a plea for combining scenario analysis and optimization on different spatial scales. Current Opinions in Environmental Sustainability (article in press): http://dx.doi.org/10.1016/j.cosust.2013.05.002

Whittaker, G.W., R. Confesor Jr., S.M. Griffith, R. Färe, S. Grosskopf, J.J. Steiner, G. Mueller-Warrant & G.M. Banowetz (2009). A hybrid genetic algorithm for multiobjective problems with activity analysis-based local search. European Journal of Operational Research 193, 195-203.

Author Response: Thank you for the great references, which will be useful in our future manuscript. However, we have removed all references to ESS and therefore trade-off studies.

6) Reviewer Comment: - line 12: "..(Forman and Alexander, 1998)." If you are interested in a newer study working on best management practices with regard to sediment reduction in a tropical river basin:

Strauch, M., J.E.F.W. Lima, M. Volk, C. Lorz & F. Makeschin (2013): The impact of Best Management Practices on simulated streamflow and sediment load in a Central Brazilian catchment. Journal of Environmental Management (article in press): http://dx.doi.org/10.1016/j.jenvman.2013.01.014

Author Response: Again, we appreciate the informative and useful reference on sediment reduction using best management practices. We have exchanged the Forman and Alexander (1998) reference with the Strauch et al. (2013) reference for a more

HESSD

10, C4788–C4814, 2013

recent example as you suggested.

7) Reviewer Comment: - line 19-22: As mentioned before, skip this and focus on your topic in this paper.

Author Response: Again, we removed the sentences to focus on the manuscript objectives.

8) Reviewer Comment: page 7238 (line 26) - page 7239 (line 14) Please shorten this section. You will find several papers with reviews on river basin models or scenario applications (e.g. Volk et al., 2009, Land Use Policy, many others more). Just tell the reader why you decided to use SWAT (see my comments before).

Author Response: Thank you for your suggestion. We have significantly reduced the section length and focused on why we used the SWAT model.

9) Reviewer Comment: page 7239 - line 15-20: Shorten this, belongs to the calibration / uncertainty analysis section later in the paper. - line 20: I would cite the paper by Gassman et al. (2007) that give a great overview on world-wide SWAT use :

Gassman, P.W., M.R. Reyes, C.H. Green and J.G. Arnold (2007): The soil and water assessment tool: historical development, applications and future research directions. Transactions Of The Asabe 50, 1211-1250.

Author Response: Thank you for the suggestion. We agree that lines 15 – 20 are better positioned at the beginning of section 4.2.1 Sensitivity and model parameterization. We have moved the statement to that location. We have replaced the reference of Borah and Bera (2004) with Gassman et al. (2007) as you suggested.

10) Reviewer Comment: - line 22: "..simulation code.." What do you mean with that?

Author Response: SWAT is a Fortran based code that simulates watershed quality and yield and this is why we chose to state it as a “simulation code”. However, for the sake of argument, we have simply changed the text from “. . .long-term simulation code. . .”

HESSD

10, C4788–C4814, 2013

to ". . .model. . .".

11) Reviewer Comment: page 7239 (line 28) to page 7241 - Think about moving parts of the text concerning your catchment more to the beginning of the paper. Just an idea (possible structure "Big Problem", own area, models, own procedure). Just think about it, no must.

Author Response: This is a great idea and we have incorporated your suggestion as much as possible. We have shifted the paragraphs within the introduction to 1) a description of natural and anthropogenic conditions on surface water quality and yield, 2) local description of Haean study, 3) importance and methodological challenges of a model, and 4) our procedure to meet the goals and objectives of the study.

12) Reviewer Comment: page 7241 - line 14-23: I very much like your monitoring and field measurement program. Perhaps it needs a bit more related to the modelling strategy. As mentioned a few times before, right now there is not much focus on (simulated) management, sediment or nutrient transport. You could mention that in the outlook.

Author Response: Thank you for the positive feedback on our intensive and interdisciplinary sampling and monitoring program. In regard to describing the modeling strategy in the introduction, we feel that the description is better placed in the goals and objectives. As previously described in this response to reviewer comments, we are not focusing on simulated management, sediment transport, or nutrient loading. Therefore, a brief discussion of how the model will be used to reach the stated objectives should be sufficient. Therefore, we believe that the revised paragraph at the end of the introduction adequately describes the modeling strategy in terms of estimating the catchment-wide hydrologic partitioning.

13) Reviewer Comment: page 7242 - line 7: Write Shope et al. (2013)

Author Response: Modified as suggested.

14) Reviewer Comment: page 7242 - 7243 Catchment characteristics I would not di-

HESSD

10, C4788–C4814, 2013

vide that in sub-chapters. Give a short but precise description of your catchment (location, terrain, elevation, soils (including characteristics such as permeability / infiltration capacity), river network, hydrologic regime (is needed!!), climate, land cover, land management... It is important for the reader to get a first, precise overview on the study area (specifics, problems, etc.).

Author Response: Thank you for the suggestion. We have added the additional descriptive comments as suggested and removed the subheadings in the section. We have also reorganized the section per your suggestion.

15) Reviewer Comment: page 7243 - line 11: Is the minimum temperature (January) really -26.9 _C??

Author Response: That is a great observation which forced us to verify the instrumental data. Yes, the minimum temperature was recorded on January 17, 2001 at 05:00 and is consistent with recorded low temperatures at each of our 15 meteorological stations distributed throughout the catchment. Annually, the minimum temperature is consistently near -20ÌŁC in Haean. These records are also consistent with record lows in Seoul, although the Haean catchment is at a significantly higher elevation.

16) Reviewer Comment: page 7244 General comment: Please shorten chapter 3. Not all information that you provide here is relevant. Suggestion for slight changes of section titles (at the beginning): 3. Methods, 3.1 Model description, 3.2 Model inputs, 3.2.1...(as it is)

Author Response: Thank you for your suggestion to shorten the chapter. While we did make significant reductions in the length of the text, subsequent reviewer suggestions have added additional information not included in the first draft. However, we believe that section 3 Methods and model construction now provides a concise but detailed description of our data sources and model construction. We have also incorporated your suggested subheading titles.

HESSD

10, C4788–C4814, 2013

17) Reviewer Comment: page 7246 - Did you develop this algorithm by yourself or do you have to refer to a reference here? - line 19 to 25: Perhaps you should mention already here that you examined the model sensitivity to alternative precipitation interpolation methods (as described at page 7253) - this is good and important.

Author Response: We did develop this algorithm ourselves. We were interested in gap-filling our raw data and interpolating the raw data over the entire catchment. Therefore, we developed this repeatable gap filling algorithm that incorporates a distribution of meteorological time series in order to interpolate the results as a function of elevation, aspect, and proximity to adjacent stations. Because we developed it specifically for our purposes and it has not been previously published, we do not have a reference for it.

It is a great suggestion to add our precipitation interpolation sensitivity analysis at the end of section 3.2.1 Climate Data. We describe the difficulty in interpolating precipitation in these topographically complex settings. We then briefly describe the 4 methods used to interpolate the observed meteorology and grid it throughout the catchment including IDW, spline, nearest neighbor, and Kriging. We then continue with the original results describing that the IDW method was optimal and used for our study. Thank you.

18) Reviewer Comment: page 7249 Land use and land cover - How did SWAT perform with the forests? There are several studies where the simulation quality for the forest regions have been more or less weak.

Author Response: This is a great question. We realize that this issue was raised in the SWAT literature, which concerned us regarding our approach. However, with our comprehensive integration of meteorological data, soil data, discharge data, evapotranspiration data, and plant growth dynamic models in the TERRECO project, we feel that this robust analysis alleviated any issues of concern with adequate observational analysis. We found that the SWAT simulated results were acceptable in the few observational forested monitoring locations (S1 and SW). For example, the R2 and NSE at S1 were up to 0.84 and 0.92, respectively. We also found that forest growth dynamics

HESSD

10, C4788–C4814, 2013

measured through transpiration and LAI were consistent with simulated results. Positive results were also seen by Kushwaha and Jain, 2013, Water Resources Management regarding simulation of forests with SWAT. In an earlier paper, Kiniry et al, 2008, Hydrological Sciences, found that accurate simulation of plant growth can improve the accuracy of hydrologic and biogeochemical cycles. Therefore, we feel that our robust observational dataset played a key role.

19) Reviewer Comment: Management parameter estimation - Very good. However, I think that you did not use all this information for your study. Focus on what you used. If you like to extend your SWAT study (simulation of sediments, nutrients, pesticides) explain it in the outlook. In the cited reference of Shope et al. (2013), the word(s) (land) management appears two times with a - assumed ! - influence on the hydrologic dynamics; the term heat units does (of course) not appear. Any other problems with plant growth simulations? Dormany / Non-dormancy of non-agricultural land or any other issues? If yes, mention it.

Author Response: Thank you for the complement in our estimation of management parameters. We have eliminated some of the information in this section 3.4.1 and provided a more concise description. We describe how and where we collect all of the information on the Haean agricultural management practices. These multiple sources were then used to estimate fertilization, irrigation, planting, and harvesting quantities and timing. We also incorporated the tillage methodologies. This information is important in the SWAT management tables for accurate estimates and optimization of plant and crop growth dynamics, which was a metric used to calibrate our model. Per your suggestion, we did remove information referring to demographic and sociological data.

We are planning on extending this study to the terrestrial and aquatic simulation of sediments and nutrients but we are not prepared to explicitly describe the outlook in this text. In fact, we feel that this would be secondary material not relevant to the current text, which would simply increase the length. Therefore, we feel that the modified section adequately describes the parameterization of our management files.

HESSD

10, C4788–C4814, 2013

You are correct in pointing out that the Shope et al. (2013) reference does not describe land management activities in detail, nor does it describe daily heat unit summations for crop growth. The paper which, predominately describes multiple methods of discharge estimation for better hydrologic partitioning, was significantly different in scope during submission to HESS than it is now published. We have removed the reference from the section.

20) Reviewer Comment: page 7251 Rice paddies, potholes, and water abstraction - line 9: There is a big difference if you abstract the water from the river reach or from the deep aquifer. As far as I know, if you take the water in SWAT from the deep aquifer it has no impact on the landscape water cycle anymore - in contrast, if you take it from the river reach, it has. How and why did you decide which amount you took from which "source? - line 12: Rice paddies. Beside the study of

M.S. Kang, S.W. Park, J.J. Lee, K.H. Yoo, Applying SWAT for TMDL programs to a small watershed containing rice paddy fields, Agricultural Water Management, Volume 79, Issue 1, 10 January 2006, Pages 72-92

there is a lot of work going on using SWAT for rice paddies. I do not know if you are familiar with it, just a hint. You should contact Phil Gassman (he was recently at a SWAT conference in Indonesia (June 2013), have a look at the SWAT homepage) or R. Srinivasan - perhaps you know that all better than I do ;o)

Author Response: That is a great point and thank you for catching it. You are correct that water abstracted from the deep aquifer has no impact on the landscape water cycle and contributes no flow return flow to streams within the watershed. We inadvertently put deep aquifer withdraws rather than shallow aquifer withdraws. We have modified the statement as "...extracted from an adjacent river reach or from shallow groundwater."

Based on three years of intensive field observations, we found that groundwater was extracted predominately for orchards in higher to mid elevation locations that were

HESSD

10, C4788–C4814, 2013

away from the river. The total orchard land use both adjacent to and distanced from the river is < 2 % of the total watershed area. Low elevation rice paddies were also supplemented with groundwater irrigation; however, the magnitude and timing of this abstraction was questionable and likely much less than the surface water abstractions for the rice paddies. The remainder of the crops throughout Haean did not require or utilize any groundwater abstractions.

As stated in the text, the withdraws were defined through auto irrigation at the HRU level. Irrigation from the established source is automatic when the crop experiences water stress limiting the need to prescribe exact amounts and timing for irrigation. The soil water deficit was the trigger that initiated the automatic irrigation.

Thank you for the great reference of Kang et al, 2006 and for your suggestions for accurate rice paddy modeling. We are aware of a number of studies, particularly in South Korea, that have investigated rice paddy dynamics. We have thoroughly discussed our modeling conceptualization, procedures, and implementation with Kyong Jae Lim and Jaehak Jeong, among others. The lead author spent time at the USDA ARS Grassland Soil and Water Research Laboratory discussing these issues with many of the SWAT agricultural and soil engineers. Dr's Lim and Jeong are particularly knowledgeable of rice paddy modeling in South Korea. However, thank you very much for the suggestions.

21) Reviewer Comment: page 7252 Results and discussion - line 3: "...modelling applications." Have a look at

Strauch, M., C. Bernhofer, S. Koide, M. Volk, Carsten Lorz & F. Makeschin (2012): Using precipitation data ensemble for uncertainty analysis in SWAT streamflow simulation. Journal of Hydrology 414-415: 413-424

Author Response: Thank you for the suggested reference. We have added the reference to the end of the statement.

HESSD

10, C4788–C4814, 2013

22) Reviewer Comment: page 7253 - line 3-12: This is good and important. As mentioned, you should mention that already earlier in the methods section (see my comments for page 7246).

Author Response: Thank you for the complement. As both reviewers have pointed out multiple times, the manuscript is long and was the reason to eliminate the sensitivity analysis and focus simply on the method used in the methods section. We feel that these are sensitivity results and not appropriate for inclusion in methods. Therefore, we only described what method we actually used in the methods section for repeatability. However, as you have suggested, we briefly incorporated the multiple methods examined in the methods section 3.2.1 Climate Data.

23) Reviewer Comment: page 7253 ff Sensitivity and model parametrization - Please shorten and focus. You will find a lot of your "starting text" (lines 15, p 7253 to line 2, p 7254) in this section in a huge number of other papers and you do not have to repeat that all.

Author Response: Okay, we have shortened the text and removed extraneous information. However, we did want to keep the study specific details in the text for the reader. For example, we want to detail how we compared the sensitivity of lumped, semi-distributed, and fully distributed model constructions. We also wanted to explicitly describe our use of multi-location, multi-criteria optimization.

24) Reviewer Comment: page 7254 ff General comment: - Please explain the SWAT parameters in a table. You use a lot of the SWAT abbreviations for these parameters and there might be a lot of people that might have no idea what that means (CH_K2, REVAPMN, GWREVAP, GWQMN, GWDELAY, ALPHABF, ESCO, CN2, etc.). In the interpretation / explanantion of the results in the text, I would group their "impact" into groundwater, baseflow, river channel, plant- or soil-processes-relevant, etc. It makes the reader much easier to understand their impact. Some of these parameters are "measureable" or derivable others are not (problematic!), which makes also a differ-

HESSD

10, C4788–C4814, 2013

ence. You should mention that.

Author Response: This is a great point and unfortunately these descriptions were removed in an earlier draft for brevity. We have included the attached table as suggested (Table 5) that includes the parameter name, the parameter description, the hydrologic impact and the estimation method. We have attempted to group the parameters as a function of hydrologic process within the text. However, we found that with the comparisons that this was difficult. We believe that the suggested table is a very concise means to identify the parameters and their influence in the catchment hydrology. Finally, we feel that we do adequately discuss our SWAT parameters in terms of hydrologic response throughout the text. Therefore, we feel that the hydrologic impact is fully included.

25) Reviewer Comment: page 7255 ff Metrics of model performance for calibration procedures General comment: - Please think about using also other metrics to evaluate the model performance (instead of using only the "classical ones"). Have a look at the paper:

Bennett, N.D. et al. (2013): Characterising performance of environmental models. Environmental Modelling & Software 40: 1-20; http://dx.doi.org/10.1016/j.envsoft.2012.09.011. (http://www.sciencedirect.com/science/article/pii/S1364815212002435)

Author Response: Again, thank you for a great reference and the suggestion to use alternative model performance metrics. As discussed throughout the text, we used several concurrent metrics in a hierarchical approach. The metrics we examined included the water balance, R2, NSE, PBIAS, baseflow contribution, and plant growth. We did evaluate the RMSE but did not include this in the manuscript. We feel that each of the criteria used provides a different metric of model optimization and when used concurrently in this hierarchal approach, provides a weighted optimization of the hydrologic impacts. We did consider your suggestion of adding alternative metrics;

HESSD

10, C4788–C4814, 2013

however, we feel that a novel feature of this manuscript is the use of 1) overall water balance, 2) classical calibration metrics, 3) relative baseflow for hydrologic partitioning, and 4) plant dynamics. To our knowledge, this type of optimization has not been completed to date. Therefore, adding additional metrics would reduce the impact of our study.

26) Reviewer Comment: page 7256 Manual and automated calibration - I know that in order to cover the heterogeneity of your relatively small study area you might need to "discretize" it in many subbasins and HRUs. You write correctly that due to some studies the number of subbasins and HRUs might have only small influence on the results, but others (also my experiences) showed in contrast that it can have a big influence - it depends on the natural conditions, data quality and scale. Sometimes it is a only a pretence of a accuracy that is actually not there or not covered by the real accuracy / solution of the data. But how did you come to 13 subbasins (instead of 142 "topographically based" at the beginning)? Any investigations on that before (sensitivity?). Please excuse me, perhaps I did not get the point before.

Author Response: For clarity and unbiased assessment, we have added references that do show that subbasin or HRU discretization can have a significant effect on the simulated results. We have also stated explicitly that this depends on the physical catchment conditions, the data quality, and the investigative scale, as you suggest.

We also realize from your statement that we were not clear in how we discretized the subbasins. We had 129 topographically based subbasins at a certain threshold when we started to construct the model. However, we found that some of the subbasins were long and narrow with a wide range in elevation from the mountain highlands to the valley rice paddies. Because the parameterization of such a large and topographically variable subbasin with a single parameter is unrealistic, we chose to subdivide some of these subbasins “manually”. This subdivision allowed us to better simulate or quantify precipitation and other meteorological changes on the relative humidity, evapotranspiration, and plant growth dynamics. Therefore, we did not limit the 142 subbasins to

HESSD
10, C4788–C4814, 2013

only 13, instead we added the original 129 subbasins to 13 manual subbasins for a total of 142 simulated subbasins. Sorry about the confusion. We have removed the statement of the 13 subbasins to preclude any misunderstanding.

27) Reviewer Comment: page 7257 - line 3: Write "The SWAT model was ..." - line 3 ff: The simulation periods are relatively short - why? For the calibration period, you usually use years that cover a relatively wet, dry and normal year (or an extreme) - did you consider that?

Author Response: Page 7257, line 3 was modified as suggested.

Unlike many previous studies, this area has been relatively ungaged with limited meteorological data recorded and therefore long-term records are unavailable. It was not long ago that this area did not have a direct military presence that prevented research activities. The total available weather data for this study area is reliable over the past 15 years, which compared to many published studies is quite limited. However, we have a strong distributed network of gages throughout the catchment that are consistently quality assured and controlled. As discussed in an earlier comment response, we have re-examined the raw weather data collected throughout the catchment and compared it to other independent regional sources. We are confident that our meteorological data, which is distributed throughout the catchment, is accurate and reliable.

As it so happened, 2009 was a relatively dry year, followed by the high precipitation year of 2010, followed by an average 2011. The fact is that South Korea has extreme weather variability on an annual basis. What we are trying to simulate is how well the model can predict rapid, event-based changes in the hydrograph throughout the catchment. The variability in total rainfall between years is not as important for this type of analysis as the variability between events. While a longer simulation period with high variability in boundary conditions and a large number of observation points is preferred, typically field investigations lack this information.

28) Reviewer Comment: page 7258 - line 8: "..was sufficient". How did you "measure"

HESSD

10, C4788–C4814, 2013

that your manual calibration was "sufficient" - there are many ways to come to good results with a manual calibration. What was the criteria for "sufficient"? Maybe one step back - why did you decide to perform also a manual calibration? To get an idea about the initial parameter ranges for the autocalibration? Should be based on as much as possible on measurements and "expert knowledge".

Author Response: This is a great point and the source of difficulty in this study. We performed manual calibration to better understand the catchment system dynamics and to minimize the acceptable parameter ranges. We used expert knowledge and a robust, interdisciplinary database from a large number of high-level field studies to define crop parameters and the parameters that contribute to discharge. The manual calibration constrained this observational variability. However, as described in the text, the correlation between individual parameters is not evident, resulting in non-unique simulations.

After we manually calibrated each of the observed subbasins, starting with the outlet and then moving from high elevation to low elevation, we began automated calibration.

In response to the original question about ". . .was sufficient. . .", we removed this vague term and replaced the statement with "After manual calibration was optimized through the weighted, multi-criteria metrics previously discussed, automated model calibration, validation, and uncertainty analysis was completed using the Sequential Uncertainty Fitting Algorithm (SUFI-2) (Abbaspour et al., 2004;Abbaspour et al., 2007)."

29) Reviewer Comment: page 7259 (line 8ff) - page 7260 (line 6) - Sorry, but this section reads as you just modified the parameters to get higher model performance metrics. These parameter all have a more or less physical meaning (see my comments for page 7254 ff). Group them more, let the reader know which one are "measureable" or derivable and which ones are problematic (such as the channel parameters (CH_K, CH_N, etc.), to name a few examples of numerous!!). You should try to relate your parameter adjustments more to "reality". I am sure you did that, but you need to explain

HESSD

10, C4788–C4814, 2013

that also explicitly in the paper - tell the reader also all the problems of that - which is not new, but necessary to mention, even in a more or less data scarce region like yours.

Author Response: Thank you for your comment. As you suggest, we did modify the parameters, within observational and manually calibrated ranges, to optimize the model performance metrics. Care was taken to avoid extreme parameterization without physical meaning through our robust and interdisciplinary field-based and model-simulated dataset. We also spent a great deal of time manually calibrating the Haean catchment to understand the influence of specific parameters or groups of parameters on hydrologic responses in this physical setting. We also used a novel approach with multiple types of calibration metrics to better constrain model parameterization.

We very much appreciate your earlier suggestion to incorporate a table of SWAT parameters discussed. We have done this and added the hydrologic impact of each of the parameters and the method used to quantify these numbers. We have grouped them in the table to let the reader know the hydrologic influence of each parameter through the parameter description. We think that this is a better alternative than to take up costly space in the text.

We also believe that we have related the parameter adjustments to reality in a clear and concise manner. For example, we describe how we adjust ESCO and GWREVAP parameters to minimize the PBIAS and improve annual river discharge and water balance differences. While we can elaborate extensively on what each parameter means, the hydrological influence of the parameter, and whether it is simply a coefficient or a measured value, we feel that the table provides this with brevity. In this paragraph, we wanted to state specifically what we changed and why. We also think that the parameterization difficulties are voluminous as any SWAT modeler can attest to. While a manuscript that provides all of the steps in model calibration and a detailed list of the problems encountered would be useful, this is not the focus of this paper. We do describe for example how we did not have adequate data for calibration and validation and so we used an alternative location with similar physical characteristics.

HESSD

10, C4788–C4814, 2013

30) Reviewer Comment: page 7260 Spatiotemporal flow partitioning with respect to river discharge - line 22 ff: Which methods did you use? The baseflow filter by Arnold and Allen (1999)? I cannot find the "digital filter hydrograph separation technique" you mention in the cited refernce of Shope et al. (2013). Please explain. These filters are great, but sometimes not suitable for some river types / hydrological regimes. If you used the baseflow filter by Arnold and Allen (1999) (which is downloadable from the SWAT homepage), did you use also the ALPHA_BF value from it? You could also have a look at non-linear filters, which do sometimes better (Wittenberg and Sivapalan, Journal of Hydrology 219 (1999)).

Author Response: We did use the hydrograph separation digital filter of Arnold and Allen (1999) with mixed results. We were able to compute the estimated baseflow and the ALPHA-BF at several monitoring locations, but not all of them. We found that the ALPHA-BF values that were produced varied little and so we used these results in the subsequent baseflow estimation techniques. Several attempts were made between the lead author and Jeff Arnold to reconcile why the Arnold and Allen (1999) baseflow separation method would not produce the desired results. In later discussions, Jeff suggested that the proposed Eckhardt (2005) digital filter would be sufficient as long as we have some estimate of baseflow contribution prior to simulations with SWAT. Therefore, we used the Eckhardt (2005) method for spatiotemporal baseflow comparisons between each of the monitoring locations. We also estimated baseflow contributions using differential discharge and recession analysis as described by Shope et al. (2013) and heat transport modeling as described in Bartsch et al. (submitted).

The published Shope et al. (2013) reference did not end up having a description of the digital filter technique used in the study. During the six months between submission of the currently discussed manuscript and these review comments, the Shope et al. (2013) manuscript has been significantly modified into its current contribution. Therefore, the reference to Shope et al. (2013) has been removed.

Again, thank you for the Wittenberg and Sivapalan (1999) reference. While the authors

HESSD

10, C4788–C4814, 2013

discuss several algorithms to estimate baseflow contributions, we feel that the methods we used are adequate and comparable. We have used several different digital filter methods (not discussed) and compared them with results from baseflow recession analysis, and differential discharge techniques, which have produced consistent results. We believe that the differences in methods described in the Wittenberg and Sivapalan (1999) text are outside the scope of this text since we simply wanted a comparative and consistent method between locations.

31) Reviewer Comment: page 7261 - line 4: Write "PBIAS statistics" or "value". - line 22: "..significantly decreased observation data..". Please change expression (e.g. less observation data, only scarce observation data, ..). - line 22: Write "NSE statistics" or "value".

Author Response: Changed to "...PBIAS statistics ..." as suggested.

Changed "...significantly decreased..." to "...scarce..." as suggested.

Changed to "...NSE statistics..." as suggested.

32) Reviewer Comment: page 7262 - line 19: Write "As shown in Fig. 5 and..."

Author Response: Modified as suggested.

33) Reviewer Comment: page 7263 - page 7264 Agricultural management and production - Think about to skip the crop part (line 10 to 25, p7264) and focus on the LAI studies (good!) related to your hydrological topic. You might have the chance to publish another paper that is related more to the land management part of your study.

Author Response: Thank you for your suggestion. We agree and have removed the last paragraph discussing the crop analysis since it is not the focus of the manuscript and for overall brevity.

34) Reviewer Comment: page 7264 - page 7266 Influence of engineered landscape structure - In general, this is a great idea. However, I do not understand how you route

HESSD

10, C4788–C4814, 2013

the water through the roads and culverts (if I understand it correctly). Could you please explain that? There is a difference between the rivers, roads and culverts ;o) The influence of the roads and culverts on the hydrology and transport processes depends on how you have routed the water here.

Author Response: Thank you for your positive feedback on our analysis of the engineered landscapes. Yes, we did simulate water being routed and exchanged with the river. We also added the culverts and then the roads and culverts as impervious channels. Essentially, the culverts were connected tributaries to the river as a stream network that were "burned" into the DEM. As previously discussed, we had to superimpose the highly managed and anthropogenically influenced Haean stream network onto the DEM and the addition of the roads and culverts was of minimal effort. This method improves hydrographic segmentation and subbasin boundary delineation. Each of the routing reaches in the catchment that were culverts or roads was assigned a transmission loss of zero.

35) Reviewer Comment: page 7266 ff Summary and conclusions - line 16: I would title it just Conclusions. Think about adding a separate discussion section. It is currently somehow mixed. - line 17: Make in the paper more clear what is really "novel".

Author Response: While we feel that this section summarizes the key findings and provides conclusions to our work, we have modified the section title as suggested to "Conclusions".

The way that this manuscript has been organized is that the results are presented and the discussion of the hydrologic importance follows. We feel that this design is logical to the reader and that as the results are presented, they are put in overall hydrologic perspective. This linear trend from methods to model construction to results and discussion somehow seems more appropriate and intuitive.

We believe that the text successfully describes what is novel in this manuscript. We describe 1) our high-frequency, quality-control, gap-filling algorithm for detailed mete-

HESSD

10, C4788–C4814, 2013

orological interpolation, 2) implementation of a multi-location calibration method distributed throughout the catchment along an elevation gradient for multiple scales, 3) our multi-objective calibration routine using water balance, classical statistical, baseflow hydrologic, and plant growth metrics concurrently, and 4) our determination of the importance of culverts and roads on hydrologic partitioning in the landscape scale. We have added specific points throughout the document in the methods, the results, and the conclusions.

36) Reviewer Comment: page 7267 - line 22: You could have used the Green-Ampt infiltration method for the subdaily level.

Author Response: That is correct and initially we employed the Green-Ampt infiltration method. However, we began to focus on the daily and multi-day time periods for a number of reasons not discussed in this manuscript. Since we do not discuss subdaily behavior of the model in this manuscript we have removed this portion of the statement.

37) Reviewer Comment: page 7268 General comment: It would be helpful for the reader if your would structure this section by answering explicitly in how far you accomplished the four objectives that you list in the intro. - line 12-13: "However, the addition od roads..". The impact of roads / culverts depends on how you realized the routing in the model (see my comments for page 7264).

Author Response: We feel that the last paragraphs of the Conclusions section adequately describes how far we accomplished the objectives outlined in the introduction section. However, we have modified them and reorganized the section for clarity.

1) Our objective was to characterize the spatiotemporal river discharge patterns at multiple locations through multi-objective optimization. We state how our results indicate that using the metrics we did, we were able to quantify scale-dependent watershed processes and hydrologic partitioning. We discuss that results of typically integrated catchment response at the outlet do not provide the hydrologic partitioning that our results do by spatially distributing throughout the catchment.

HESSD

10, C4788–C4814, 2013

2) Our objective to assess the potential of a spatiotemporal algorithm to improve precipitation discretization was accomplished. We state that this method provided a better estimate of dynamic variability of storm events and were then input into each of the subbasins.

3) Our objective to determine the capability of the model capture short duration events is explained throughout the Conclusions section. In particular, we state that our results show that major shifts between surface driven runoff and baseflow hydrologic partitioning occur within the catchment.

4) Our objective of quantifying the significance of roads and culverts on flow partitioning is extensively covered in the Conclusions. For example, we state that the spatially extensive irrigation culverts adjacent to most fields play an important role in flow routing and by including additional metrics, we were able to better quantify the role that these features played.

As stated in a previous comment response, we routed the roads and culverts as impervious tributaries to the river. Field observations corroborated the influence of these features on overland runoff (lack of soil infiltration), routing, and lack of transmission losses. Therefore, we feel that we adequately incorporated the roads and culverts into the model.

38) Reviewer Comment: Tables - page 7277, Table 1, line "Discharge and loads" - Which loads? - page 7278,

Author Response: We removed loads from the label as it is not part of this manuscript.

39) Reviewer Comment: Table 2. 1. Comment: I think it is not necessary to list the number with two decimal places (see also "drainage area" in Table 6). 2. Comment: Give the reader an information about the hydrological characteristics of the soils (in the text). - page 7279, Table 3. Do you have comparable values from literature? - page 7281, Table 5. You need to explain the parameters (see my comments before).

HESSD

10, C4788–C4814, 2013

The "normal" reader that is not familiar with SWAT mght have no idea if the parameter ranges are reasonable. – page 7283, Table 7. Think about skipping this table. Not in the focus of your paper.

Author Response: Okay, we have changed Tables 2 and 6, drainage area to a single decimal point.

We have supplied the reader with hydrological characteristics of the soils in section 3.3.2 Soils. We have added a sentence describing the hydrologic group and general texture for each of the soil types.

For Table 3, we did extensively compare our management information with referenced literature. However, there was substantial variability in some of the results due to geographic location, climate, crop type, species, and catchment characteristics. Rather, we used extensive field plots, farmer surveys, and regional product sales data to identify typical conditions and methods in this traditionally agricultural environment Therefore, we chose not to present referenced results as they would differ in a number of issues and are not comparable, requiring further explanation.

As described in a previous comment response, we have prepared another table (shown above) that provides the parameter name, a description of the parameter, the hydrologic influence, and the method to determine the parameter. Unfortunately, a reader that is interested in the exact number of for example, GW_REVAP will not be able to determine if the parameter range is reasonable. We believe that this kind of information is lengthy, requires expert knowledge to evaluate, and is not a major focus of this manuscript. The information provided in this table is specifically for SWAT users for model comparison and these readers would understand or know where to investigate further. However, we do believe that the additional table provides useful information in a concise format for the reader.

Per your suggestion, we have removed Table 7 and all information associated with it.

HESSD

10, C4788–C4814, 2013

40) Reviewer Comment: Figures - Partly to small (legends, "labelling", etc.)

Author Response: Okay, we adjusted the presentation of Fig. 1 and the text size. We also increased the text size in Figs. 2, 3, 4, 5, 6, and 7.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 7235, 2013.

Parameter Name	Parameter Description	Hydrologic Impact	Estimation Method
CN2	Initial SCS runoff curve number for moisture condition II	To calculate the runoff coefficient as a function of soil permeability, land use, crop development, and antecedent soil moisture	Calculated
CH_N(2)	Manning's "n" roughness coefficient for the main channel	Parameter used to estimate the resistence to flood discharge in stream channels and floodplains	Calculated
ALPHA_BF	Baseflow alpha factor or recession constant (days)	Direct index of groundwater flow response to recharge as measured from streamflow	Calculated
ALPHA_BNK	Baseflow alpha factor or recession constant for bank storage (days)	Direct index of bank storage flow contributions to the main channel reach	Simulated
CH_K(1)	Effective hydraulic conductivity in tributary channel alluvium (mm/hr)	Controls transmission losses from surface runoff to subbasin channel	Measured
CH_K(2)	Effective hydraulic conductivity in the main channel alluvium (mm/hr)	Used to control magnitude and direction of stream and groundwater exchange	Measured
GW_REVAP	Groundwater "revap" coefficient	Water movement from shallow aquifer into the overlying unsaturated zone through evaporation or plants	Optimized
REVAPMN	Threshold depth of water in the shallow aquifer for "revap" or percolation to the deep aquifer to occur (mm H2O)	Defines vertical movement of water to unsaturated zone or deep aquifer	Optimized
GWQMN	Threshold depth of water in the shallow aquifer required for return flow to occur (mm H2O)	Groundwater exchange to the reach is determined by this parameter.	Optimized
GW_DELAY	Groundwater delay time (days)	Measure of lag time between water exiting the soil profile and entering the shallow aquifer.	Simulated
ESCO	Soil evaporation compensation factor	Coefficient to modify depth distribution of soil evaporative demand	Optimized
SOL_AWC	Available water capacity of the soil layer (mm H2O/mm soil)	Calculated as the difference between water present at field capacity and the permanent wilting point.	Calculated
SOL_BD	Soil moist bulk density (g/cm3)	Ratio of the mass of solid particles to the total volume of soil. Used to assess proximity to fully saturated flow conditions.	Measured
SOL_K	Saturated hydraulic conductivity (mm/hr)	Relates the soil water flow rate or flux density to the hydraulic gradient to provide a measure of water movement resistence in the soil.	Measured

Fig. 1. Table of SWAT parameters used.

HESSD
10, C4788–C4814, 2013