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Discussions

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

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Dr. Bieger, The authors are grateful for your review and your insightful comments. They have greatly improved the quality of our manuscript. We appreciate your consideration 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.

General comments:

1) Reviewer Comment: The authors present a SWAT application to a small watershed in South Korea, which is characterized by large differences in elevation and very steep slopes. Generally, I think that this is a very interesting topic as SWAT was not explicitly

developed for steeply sloping watersheds and still has some major shortcomings in representing hydrological processes in mountainous areas. Often, studies in such areas are hampered by a lack of field data to validate model results and identify the most important problems. In this study, a relatively good database is available with regard to climatic and discharge data as well as soil and land use/management information. The authors developed a new algorithm to improve the representation of spatial rainfall variability in the model using their extensive measurement data. I am absolutely sure that the SWAT modeling was done very thoroughly and scientifically sound. However, in my opinion, this manuscript needs major revision. Firstly, it is very long and contains a lot of information (which would probably be enough for two or three papers), so I recommend to shorten it and stay focussed on the main objectives. Secondly, in the Results & Discussion you give a lot of information that actually belongs in the Introduction or in the Materials & Methods chapter. Please structure the paper more clearly and make sure that in the Materials & Methods you give all relevant information that is necessary for the reader to be able to follow your analyses and explanations in the Results & Discussion.

Author Response: Thank you for the positive feedback on the scope and objectives of our work. We agree that the lack of field data is a major impediment to model validations. Also we very much appreciate your opinion of our thorough and exhaustively discussed modeling methods and techniques.

We have shortened the paper significantly and focused more on the specific catchment issues and methodological challenges. We have also taken great care in addressing your second comment regarding moving information from the Results and Discussion section to the Materials and Methods section. Overall, we agree and have incorporated each of your suggestions; however, there are a few specific comments that we did not fully agree with and we explicitly discuss these in the individual comments. Your suggestion to restructure the Materials and Methods chapter has greatly improved the manuscript.

Specific comments:

2) Reviewer Comment: Page 7238, lines 2-25: In the first paragraph of the introduction, you talk about ecosystem services and land use change, both of which are not really focussed on later on in the manuscript. I recommend to lay a stronger focus on the challenges involved in applying SWAT to the Haean watershed and how you used the available data and methods to improve the model setup and parameterization.

Author Response: Thank you for your comment. We have removed the discussion about ecosystem services (ESS) from the manuscript as suggested since it was a prerequisite and not a focus of the paper. We have also incorporated your suggestion for focusing on applying our data to construct and parameterize the SWAT model in Haean.

3) Reviewer Comment: Page 7241, lines 4-8: Do you have any information about where the irrigation water in the Haean watershed actually comes from?

Author Response: Yes, high elevation and dryland farming locations typically use shallow groundwater although this depends on the proximity to a stream and the landowner. Mid-elevation to low-elevation rice crops use a combination of surface water and groundwater. We have revised the statement to clarify.

4) Reviewer Comment: Page 7241, line 27 - page 7242, line 5: Please make sure that your objectives match what you present in the results and discussion section. With regard to objective (2) I think you would have to compare your results to results obtained without the spatiotemporal interpolation of precipitation in order to actually assess the potential of the new algorithm you developed.

Author Response: We believe that the stated objectives are accurate for the 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 the capability of SWAT to capture daily rainfall-runoff, and 4) quantify significance of engineered structures on flow partitioning.

In regard to the original objective 2, we did compare our interpolated and discretized results with the weather station defined locations. What we found was that when we simply used the meteorological station results and let SWAT assign them to centroid of each subbasin, the peak flow and lag time were consistently off. The primary difficulty in letting SWAT assign the meteorological data was that the centroid location could be at a significantly different elevation or aspect. In addition, the area has a high number of local convective storm events that are identified in one portion of the catchment, but not another. When we developed the interpolated discretization algorithm, these local storms were identified and prevalent. The benefit of using this algorithm is that the SWAT user can assign an interpolated value for the exact point of the centroid. This provides a much better distribution of meteorological values as long as the subbasins are arranged to account for elevation and aspect differences.

To clarify this information, we have added additional information after presenting our algorithm. The sentence now reads as “Due to the large variation in topographical complexity throughout the catchment, precipitation volume, soil moisture, and plant growth were impacted when SWAT assigned the meteorological data to each subbasin.”

5) Reviewer Comment: Page 7243, lines 11-14: If you add this here, I would recommend to name the chapter "Haean climate and hydrology".

Author Response: Thank you. We removed sub headers to better link catchment character.

6) Reviewer Comment: Page 7245, lines 8-19: Here you mix the general model description and the description of which algorithms you used in your model for the Haean watershed. This is a little confusing, so I recommend to describe this separately. S

Author Response: This is a great point and we shortened and reorganized the section to explicitly describe the general SWAT model description followed by our incorporation of some of the algorithms. We believe that adding which algorithm we used as we present how the SWAT model simulates hydrology is efficient and concise. By separating this, it would increase the length of the manuscript and force the reader to go back and forth between the model options and what we chose. We believe that the current format is a reasonable solution.

7) Reviewer Comment: Page 7245, lines 10-11: In a study we did in a mountainous watershed in China, we found out that when calculating the daily CN values based on the soil water content, surface runoff decreased with increasing slope. This happened because lateral flow increased strongly with increasing slope, which led to a decrease of soil moisture. Have you analyzed the dependency of simulated surface runoff on slope in the Haean watershed?

Author Response: That is an interesting result. Sorry, we reran the simulations on earlier scenarios and used the accumulated plant evapotranspiration for CN estimates and misstated our use of soil water content. We found that too much runoff was predicted using the soil water content and that time variable land use and crop growth had a significant impact on CN. Your statement on the China study is precisely why we switched to accumulated plant ET. In this manner, slope adjustments to the CN for slopes greater than 5% and antecedent soil moisture conditions are not an issue. Using the plant ET based estimate of CN takes into account the antecedent climate conditions, important in these monsoonal areas.

8) Reviewer Comment: Page 7246, lines 8-15: Not all variables are explained here. How were the weighting factors w1, w2, and w3 determined? What does "+ ..." at the end of the first two lines of the equation mean? The observation point aspect is denoted differently in the explanation than in the equation. Have you applied this algorithm for all climate variables or for precipitation only?

Author Response: Thank you for pointing this out. We have changed the text to incor-

porate the missing variables ïĄę, ïĄęe, ïĄęo, ze, and zo. (phi, phi_sub e, phi_sub o, z sub e, z sub o)

In previously unpublished data, we examined the variability of precipitation as a function of elevation, aspect, and spatial distance. We found that for Haean, the importance was weighted from 1) aspect, 2) distance, to 3) elevation. So we simply used average weight of each of these factors from our preliminary studies to determine the total effect on the calculated precipitation at the estimated location. The reason that we do not explicitly state this in the text is for brevity and more importantly, site specific determination. It surely will not be the same for all locations.

The “+” signs were simply provided to shorten the width of the equation. In essence, we show that weighted precipitation as a function of aspect is added to the weighted precipitation as a function of distance and is added to the weighted precipitation as a function of elevation. To alleviate this discrepancy, we have reformatted the equation as continuous (although long).

Again, the observation point aspect was clarified to match the phi in the equation. We did apply this algorithm for all climate variables but we simply presented it for precipitation as an example. As stated immediately before the equation “. The algorithm, as formulated for precipitation, is presented as…”.

9) Reviewer Comment: Page 7246, lines 10-21: This paragraph does not really describe evapotranspiration measurements as indicated in the title of the chapter.

Author Response: Good point. We changed the heading title to “Discharge and evapotranspiration estimates”, since they were calculated using the model described.

10) Reviewer Comment: Page 7248, line 15: Add "(RDA)" after "Rural Development Administration" to introduce the abbreviation used in line 17.

Author Response: Thank you for the suggestion. We added it as recommended.

11) Reviewer Comment: Page 7250, lines 26-27: What is PIXGRO?

Author Response: PIXGRO is a primary production model that couples canopy flux and vegetation structure to simulate among other things plant growth. We have added an example reference for clarity (Adiku et al., 2006).

12) Reviewer Comment: Page 7251, lines 19-22: Quite frankly, I don't really understand what you did here. Can you explain this a little better?

Author Response: Sorry about that. We have rephrased for clarity. Essentially, we want to state that since an HRU can only have 1 pothole, but there are multiple rice paddy HRUs in a subbasin, we needed to combine the HRUs to form a single rice paddy in the subbasin. Therefore, we adjusted the soil threshold in the management parameters to accommodate this.

13) Reviewer Comment: Page 7253, lines 3-5: You should have mentioned this in the Materials & Methods chapter. Why did you only mention IDW there?

Author Response: We did not initially include this result in the Materials and methods simply for brevity. The paper as both reviewers have pointed out is long and to put this in front when we describe the method used is unnecessary. These are sensitivity results and should be in the results. Therefore, we only described what we actually used in methods. However, per your suggestion, we did briefly incorporate this sensitivity into section 3.2.1 Climate data with the following sentence. "We tested several interpolation methods to grid the measured meteorology results throughout the catchment (inverse distance weighted (IDW), spline, nearest neighbor, and Kriging)."

14) Reviewer Comment: Chapter 4.2.1: In my opinion, you mix information that should be part of the introduction or materials & methods with results here.

Author Response: This is a great point. In manuscripts that describe model construction and sensitivity, there is not a clearly defined means to present this in methods or results. We struggled with placement of the sensitivity and model parameterization for some time now. Our feeling is that the Methods and model construction describes

1) general SWAT model framework, 2) which algorithms in SWAT we used, 3) model inputs including climate, discharge, and ET, 4) spatial data including DEM, soils, and land use, and 5) management inputs. There is not a clearly defined location to put in our sensitivity results used to define the model parameterization. Further, we argue that when sensitivity analysis is conducted, these results are used for parameterization and are therefore considered results. To this end, we have shortened this section and moved some of the information to the calibration and uncertainty analysis later in the paper.

15) Reviewer Comment: Page 7255, lines 1-4: How do you explain these spatial patterns of flow components? Land use, soil types, slope? Please discuss your results in more detail here.

Author Response: Thank you, that is a great point and a discussion left out of our manuscript. We have incorporated a discussion of these flow components into several locations throughout the manuscript. However, we have included a sentence immediately after the presentation of these sensitivity results in section 4.2.1. The sentence is "Since the upper elevation locations are composed of shallow, highly permeable (S. Arnhold, unpublished results) soils over bedrock; we conceptualize high infiltration rates that contribute to increased baseflow and streamflow accumulation. At mid- to low-elevation locations, higher land management and soil amendments lead to runoff and less infiltration."

16) Reviewer Comment: Chapter 4.2.2: I'm not sure if this section is necessary. The statistics have been presented in a large number of papers already and since your manuscript is very long, this might be a good opportunity to shorten it a little. If you want to keep this chapter I recommend to move it to the materials & methods chapter.

Author Response: Thank you for your suggestion. We agree that these relatively common statistics have been given in the literature and have significantly reduced (> 50%) the text and incorporated the relevant references. We think that the section does be-long at this location because as stated previously, it is not appropriate to insert it into our currently outlined Methods section. We feel that a discussion of sensitivity, followed by calibration metrics, followed by calibration procedures using these metrics is most appropriate.

17) Reviewer Comment: Page 7257, line 3 - page 7259, line 7: This should be part of the materials & methods chapter. Also, I recommend to shorten this section. For example, in my opinion a detailed description of SUFI-2 is not necessary here.

Author Response: For the same reasons previously discussed, we respectfully disagree that the calibration section should be a part of the Materials and methods chapter. We have significantly reduced the overall length of the section (~60%), which may alleviate some of the extraneous information.

Per your suggestion, we have also eliminated the bulk of the description of SUFI2 and simply referenced the Abbaspour et al., 2004 and 2007 papers. We have maintained the information necessary to provide the reader a clear and linear view of how and why we conducted our calibration and validation procedures.

18) Reviewer Comment: Page 7257, lines 3-8: To me, this is a little confusing: did you use calendar years or hydrologic years? What do you mean by "a combination of 2003, 2004 and 2009 discharge estimates"? Why didn't you use the data for 2011?

Author Response: Sorry for the miscommunication and confusion. We used calendar years rather than hydrologic years in all of our interpretations and have therefore removed hydrologic years from the text.

The 2003 and 2004 observational data were used at some locations for model verification but not included in the model validation results presented. We simply used the 2009 data for model validation. The reason that we did not include the 2011 data in validation was due to spurious observations and instrumentation results in key locations. We found that after several years of continuous instrumentation, some of the

monitoring locations developed technical difficulties and using the data was not warranted. We believe that we have enough accurate and precise data for the periods that we present, that we can draw reasonable conclusions from our study. While some of the data from years 2003, 2004, and 2011 can and were used, we chose to focus on the more complete and robust dataset from 2009.

19) Reviewer Comment: Page 7257, lines 12-15: Please explain the selection of monitoring locations for analysis in the materials & methods.

Author Response: Great and thank you for this point. We have moved the sentences that you have pointed out to section 3.2.2 Discharge and evapotranspiration estimates, after we describe our discharge procedures.

20) Reviewer Comment: Page 7257, lines 17-19: What about the impact of aspect?

Author Response: Yes, this is an important point and we have included it in the text. Again, we have moved the sentences that you have pointed out to section 3.2.2 Discharge and evapotranspiration estimates, after we describe our discharge procedures.

21) Reviewer Comment: Page 7259, lines 8-9: Please mention the hydrograph analysis in the materials & methods chapter. Which technique did you use?

Author Response: This is a great suggestion. We have added three sentences in section 3.2.2 Discharge and evapotranspiration estimates, after we describe our selection of monitoring locations.

Initially, we used 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).

22) Reviewer Comment: Page 7259, line 18: Do you mean "by maximizing the NSE"?

Author Response: This is correct. We have modified the text to "…maximizing the NSE" as suggested.

23) Reviewer Comment: Page 7259, lines 18-24: First you state the the NSE increased when using Muskingum routing instead of variable storage routing, but then you state that the change was negligible. This doesn't really make sense to me.

Author Response: You are correct, this was written vaguely. The first sentence stating that changing the routing routine caused a difference in NSE was removed. When we tested the alternative routing methods, we found negligible differences in the outlet results. Therefore, the initial statement was wrong and removed.

24) Reviewer Comment: Page 7260, lines 22-28: Which technique did you use? Please explain this in materials & methods.

Author Response: As previously discussed, we used the Eckhardt (2005) digital filter technique, among others, to estimate baseflow contribution. Per your suggestion, we have added three sentences in section 3.2.2 Discharge and evapotranspiration estimates, after we describe our selection of monitoring locations.

25) Reviewer Comment: Page 7261, lines 1-2: What do you mean by this? Please explain this a little better.

Author Response: Thank you for pointing this out. We were trying to describe that the baseflow at mid-elevation location S4 may be near the transition between baseflow

dominated and runoff dominated locations but that with long-term observational data, it appears to be more baseflow dominated. We have modified the text to incorporate this description.

26) Reviewer Comment: Page 7262, lines 8-9: Make sure that numbers and units match here.

Author Response: Great point and sorry for the mixed information. All numbers have been modified to the hundredths for consistency. Thank you.

27) Reviewer Comment: Page 7262, lines 10-12: If you refer to Figure 5 here, I would expect the 95% confidence interval to be shown in that figure.

Author Response: Yes, by saying that the majority of the simulated results are within the 95% confidence interval, it is typical or expected to see the 95PPU model uncertainty bounds on the figure. However, we chose to eliminate the "clutter" in figure 5 by showing the best simulation results, the individual observations, and then providing a text box with the calculation metric (p-factor) for the period. A 95PPU band was difficult to visualize on the figures and therefore not included. To accommodate the discrepancy, we have moved the reference to figure 5 to the preceding sentence that describes the metrics.

28) Reviewer Comment: Page 7262, line 13: Wasn't 2010 the calibration period?

Author Response: That is correct and the year was changed to 2009 for the validation period.

29) Reviewer Comment: Page 7263, line 17: Do all crops growing in the Haean watershed have a base temperature of 0.0_C?

Author Response: Yes, all of the crops in Haean were prescribed a base temperature of 0.0°C. We chose a value of 0.0°C primarily for consistency. Most of the crops were species variations consistent with East Asian or Korean climates. We initially used the database values but found that the cumulative heat units were unrealistic for harvest.

We also compared observational ET and LAI growth dynamics for each of the major crops and found a good correlation that supported maintaining a catchment wide base temperature of 0.0°C. This approach is novel in that whatever base temperature we used, we could estimate the HUSC for individual crop management operations. As would be expected, the larger the cumulative heat units are, the more precise our HUSC fraction becomes. In discussions with Jim Kiniry and Jeff Arnold, they suggested that this is a reasonable approach for our system and were supportive of our data.

30) Reviewer Comment: Page 7264, lines 10-25: In my opinion, this is not really relevant to the objectives of this paper. Have you thought about publishing a separate paper on land use change and crop productivity in the Haean watershed?

Author Response: We agree and the section describing crop information and statistics was removed for overall manuscript brevity and focus, as suggested. We have thought about a separate manuscript on land use changes affecting crop productivity, which is currently being examined. Thank you.

31) Reviewer Comment: Page 7264, lines 19-21: But then this might also be the case for all the other plots. Are they more representative of the average production in the watershed?

Author Response: That is a good point. However, as mentioned, the section describing crop information and statistics was removed for overall manuscript brevity and focus.

32) Reviewer Comment: Chapter 4.5: How did you implement this in SWAT? Were the culverts and roads integrated as part of the stream network? Did you change the subbasin delineation for the new model setups? You have to explain this better. Maybe this might even be an interesting topic for a separate paper. What was the impact on the remaining monitoring locations?

Author Response: We implemented our analysis of the engineered landscapes in SWAT by incorporating them into the prescribed river network. We simulated water be-

ing routed and exchanged with the river system with the initial prescribed river network. We then added the culverts and subsequently the roads and culverts as impervious channels with the river network. Therefore, we had three complete model constructs from the beginning to the end, 1) the rivers alone, 2) the rivers and culverts, and 3) the rivers, culverts, and roads . Essentially, the culverts were connected tributaries to the river as a stream network that were "burned" into the DEM. We superimposed 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. When we incorporated the culverts and then the culverts and roads, the subbasin delineation for the new model setups was altered with more subbasins. We were fortunate to have the bowl-shaped basin, which as described, we could parameterize according to similar locations along an elevation transect. As we discuss in the text, the majority of upland and mid-elevation locations were relatively consistent but we saw large differences at the downstream locations. This suggests that these engineered features reduce catchment wide infiltration and storage and rapidly route the water surficially to the low-elevation areas. We have modified the text to describe this more clearly and have described the implementation in section 3.3.1 DEM.

While this research alone was intriguing and to our knowledge novel, we feel that this is a major research result of this manuscript. In essence, we show that the SWAT user can use a series of techniques that we employ to improve their confidence in hydrologic partitioning; however, these engineered landscape features are a key component of how the water is partitioned.

33) Reviewer Comment: Page 7267, lines 1-3: How do you explain this?

Author Response: As stated in a previous comment, we have added our conceptualization to the end of section 4.2.1 Sensitivity and model parameterization. Conceptually, we believe that high elevation upland locations have high infiltration and are composed of shallow unconsolidated surface soils overlaying shallow fractured bedrock. This shallow soil layer contributes more relative baseflow as a percentage of the stream flow than runoff. At lower elevations, deep unconsolidated sediment at a lower hydraulic gradient contributes less baseflow as a percentage while surficial runoff dominates due to soil amendments and increased land use distribution. We believe that at high elevation, the high downward vertical infiltration (Arnhold, unpublished data) is impeded by shallow bedrock and flows laterally to weak sinks or headwater streams. At low elevations, both shallow local infiltration, which is a much lower flux (Bartsch et al, submitted), is under a lower hydraulic gradient with deeper aquifers and lower hydraulic conductivity. Therefore, the relative groundwater discharge or baseflow contribution is much lower. Hence, while the volumetric flux of baseflow may be similar between high and low elevation locations within the catchment, the relative contribution to the hydrograph is much lower at low elevations.

Since we state this in multiple locations within the text, we prefer to simply provide a concise result in the Conclusions chapter.

34) Reviewer Comment: Chapter 5: You give some new information in the conclusion, which I think should be part of the discussion instead.

Author Response: Sorry that it appears that we have new information in the conclusions. We were unable to locate specifically, which information was not discussed previously in the text. However, we have removed several statements that may be perceived as new information and hopefully, eliminates the discrepancy.

35) Reviewer Comment: Page 7269, line 3-5: You did not present any results on subdaily simulations in this paper, though.

Author Response: You are correct and although we discuss some of our subdaily results, we do not discuss our methodologies. Therefore, we removed all references to subdaily results and simulations.

36) Reviewer Comment: Table 3: What is JD? Julian day? How can you determine a specific Julian day when you based your management schedules on heat units as stated on page 7250?

Author Response: Thank you for pointing this out. JD is Julian day and it has been explicitly stated in the table as suggested.

This is a great point and we used the Julian Day value to validate the use of heat units, particularly with PHU at 0.0°C. Since we used plant growth dynamics to calibrate the model, the Julian Day should approximate the HUSC. However, the fraction provided by the HUSC would not be clear to a typical reader and therefore be meaningless. The approximate day is presented for reader clarity.

37) Reviewer Comment: Table 5: In this table, you list data for 6 monitoring locations, in the next for 10, and in Figures 4 and 5 you show data for 5 locations. This is a little confusing. Generally, the selection of monitoring locations you present and discuss results for seems a little random throughout the whole manuscript.

Author Response: Thank you. This is an accurate observation. In Figure 4, we discuss the 5 primary monitoring locations (S1, S4, S5, S6, and S7) referred to throughout the text as they are along the elevation gradient. In Figure 5, we show the same locations for calibration. Although in Figure 5, we replace S4 with SD and S6 with SK for validation purposes, as discussed in the text. In this Table 5, we inadvertently included an incorrect duplicate of S1, labeled S1a. This location and all of the associated data have been removed. Therefore Table 5 also shows data for the 5 primary monitoring locations (S1, S4, S5, S6, and S7). In Table 6, we present the primary calibration metrics for all of the monitoring locations in the basin. We do this so that the reader can have a more complete view of the metrics throughout the basin and the relative baseflow contributions.

We apologize that the monitoring location selection seems random; however, we feel that the identification, selection, and discussion of each of these locations is important and required. We also think that they are well documented throughout the text.

38) Reviewer Comment: Figure 6: The value 129 in the equation does not really seem to match the order of magnitude of the y axis. Also, I'm not sure if you can really identify a trend from that data. It seems to me that the heat sum is pretty similar for all years except 2003, 2008, and 2009. Maybe 2008 and 2009 were extreme years just like 2003. If you excluded the last two years from analysis, the heat sums would probably indicate an increasing trend.

Author Response: You are correct, the value 129 in the equation on Figure 6 does not fit the figure and was found to be a legacy text box from an earlier revision. We have removed this trend equation since it is not discussed. The slope of all of the annual maximum heat sum estimates is -74.8°C/yr (-0.205°C/d) over the period of record.

Trend analysis can always be a difficult process, particularly with n = 12. While the 3 years that you mention are significantly different than the others, which are all similar, they account for 25% of the period of record. This is a significant amount of outliers, if they are considered outliers. Therefore, we chose to include them in the overall analysis. When we remove the years 2008 and 2009, the slope is still negative but decreases to -12.4°C/yr (-0.034°C/d). Further, if the year 2003 is also removed, the slope is -15.3°C/yr (-0.042°C/d). Therefore, in each case we show a decreasing trend in the total annual heat sum. More importantly, we think that it is not appropriate to remove the "outliers" and therefore have used the assessment to support

Nevertheless, we have added a statement in section 4.4 Agricultural management and production that states a decrease annual heat sums even with the removal of the 3 potential extreme years.

39) Reviewer Comment: Figure 7: Why did you use a different color for cabbage than for the remaining crops? If I'm not mistaken, the standard abbreviation for tons is t.

Author Response: Sorry, this was a simple oversight blamed on colorblindness. All of

the marker colors for observed LAI have been coordinated and consistent. We also modified the tons to t, as suggested.

Technical corrections:

40) Reviewer Comment: Page 7240, line 6: Remove the comma after "where"

Author Response: Okay, removed as suggested.

41) Reviewer Comment: Page 7240, line 15: Replace "decreased" with "decrease"

Author Response: Okay, replaced as suggested.

42) Reviewer Comment: Page 7247, line 8: Remove brackets

Author Response: Great, corrected as suggested.

43) Reviewer Comment: Page 7247, line 19: "Priestley-Taylor" instead of "Priestly-Taylor"

Author Response: Okay, modified as suggested.

44) Reviewer Comment: Page 7249, line 11: Do you mean "forest encroachment"?

Author Response: Yes, thank you for the comment.

45) Reviewer Comment: Page 7249, line 20: Do you mean "Agriculture"?

Author Response: No, this is the correct term. However, without the commas. Thank you for pointing it out.

46) Reviewer Comment: Page 7250, line 9: Replace "were" with "was"

Author Response: Modified as suggested

47) Reviewer Comment: Page 7260, line 12: Replace "Melese" with "Melesse"

Author Response: Modified as suggested.

48) Reviewer Comment: Page 7260, line 28: Do you mean S4 here?

Author Response: Yes, thank you. Modified as suggested.

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