AUTHOR RESPONSE TO EXTERNL PEER REVIEW COMMENTS ON DRAFT EPA REPORT:
"Watershed Modeling To Assess the Sensitivity of Streamflow, Nutrient, and Sediment Loads to Climate Change and Urban Development in 20 U.S. Watersheds"

I. GENERAL COMMENTS			
Page	**Line**	**COMMENT**	**RESPONSE**
		The report describes a modeling study to investigate the relative and combined influences of climate change and urban development on streamflow, nutrient (N and P), and sediment loads in the 20 U.S. watersheds by using the integrated existing tools (e.g., climate models, downscaling methods, and watershed models). The report deals with an interesting and important topic (on which a number of papers have appeared in the recent past). The results of the study are successful in helping to enhance the understanding of the possible impacts of future climate change and urban development on streamflow, nutrient and sediment loads in different regions of U.S., and methodological challenges associated with integrating existing tools (e.g., climate models, downscaling methods, and watershed models). The reviewer is convinced by thorough work. The accuracy of information presented, as well as the clarity of presentation, is admitted.	Comment. No response necessary.
		The report presents watershed modeling in 20 drainage basins in the US to evaluate the potential effects of climate change on water resources. Given the IPCC synthesis findings on intensification of hydrologic cycle and continued warming. Watershed systems will experience a major impact through these changes, requiring improved understanding of the complexity and response. This report is timely and develops a good baseline assessment of various watersheds spread throughout the US. This will be useful in development of risk managing strategies and in identifying vulnerable regions. A spatial spread is useful in capturing the variability in regional impacts. The use of simulation models becomes necessary to accurately predict system-wide changes in water quantity and quality. The report rightly uses SWAT and HSPF models to quantify these changes. The study has used standard procedures and datasets in developing the analysis. Use of standard procedures is useful in replication of results and also increasing the accuracy of analysis done at the scale of watersheds used. The detailed treatment of calibration and validation using observed data in these watersheds increased the reliability of the simulated results. The comparison of the two models with multiple calibration and validation methods is an appropriate and noteworthy aspect of the study. The study has followed right procedures in choosing the right gage stations for calibration and validation. Extrapolating to other sub-basins within the HUC study watershed should not seriously affect accuracy of making general conclusions, even though land use or soils might vary. Such variation on soils and land use is fully accounted in the two simulation models chosen. The implementation of climate change perturbation on climatic variables using a “change factor” approach is reasonable. In general, the study has followed scientific methods in addressing the accuracy of the information.	Comment. No response necessary.
		The report is well written and presented well. The problem is clearly articulated and described well. Information on the two models and their usage is presented and explained well. The document is planned well with good supplemental information. Figures and tables are developed well and are presented to explain methods and results well.	Comment. No response necessary.

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		The conclusions derived from the modeling study are sound. The authors carefully present the results and provide information on any caveat in study methods. The study provides clear guidelines on limits in using this study. For example, the conclusion section is clear in acknowledging that this is not a forecasting tool, but rather a sensitivity assessment of underlying watershed hydrology. The conclusions are presented with cautions on potential bias, representation of managed processes, and the nature of uncertainty in storylines. This information is important to any climate change study dealing with complex systems like watersheds.	Comment. No response necessary.
		The USEPA has conducted a comprehensive study employing two commonly used hydrologic and water quality models, the HSPF and SWAT, to simulate the plausible hydrologic impacts of future climate change and urban development in 20 different watersheds in the United States. In the face of global climate change and rapid urban development, this study is invaluable because its findings will not only contribute to our scientific knowledge of the hydrologic impacts of climate and land use changes, but also will provide many application values, enabling water resource managers and government agencies to plan for mitigation measures and adaptive strategies to ameliorate the impending consequences of climate and land use changes on water resources. The study will also contribute to the watershed hydrologic modeling community, as it has laid out important information pertinent to the assumptions of the model, concepts underlain the models, and the strength and weakness of each model.	Comment. No response necessary.
		The report furnished is well-written and well-organized. It provides a detailed description of the background information pertaining to the study and the methodology employed in the analysis, including how the climate and urban development scenarios were generated and how the models were set up, calibrated and validated. The information presented is accurate and clear. The conclusion derived from the study is sound and has certain scientific merits and application values for water management.	Comment. No response necessary.
		In general, the report is well written and contains the required information necessary to support the evaluation. The study undertaken in this report is in line with current global concerns regarding sustainable management of water resources and our environment in the changing world. The results and findings of the report are consistent with recent academic literature that shows that (1) our hydrological system is sensitive to climate change in the large scale and to land use change (primarily urbanization) and river regulations at the local scale, and (2) the simulation results are sensitive to methodological/model/scenario choices and the simulated responses span a wide range and sometimes do not agree in the direction, in some cases.	Comment. No response necessary.
		On one hand, I found the report was based on scientifically sound methodology/modeling, the study was done systematically and comprehensively, and the results were in consistent with many other studies conducted nationally and internationally; on the other hand, I do not see new findings and all the concluding points, as presented on pages 163-164, are very general and valid everywhere. I would say I knew (or have seen) these (similar descriptive) conclusions before I read the report. I would suggest (if possible) writing a few more specific/quantitative conclusions drawn from this particular study. If the report is not in its final version, I would suggest rewriting the conclusions in accordance with my comments above.	The report has been extensively revised with added detail and discussion of results in the sections on Results, Conclusions, and Executive Summary.
II. RESPONSE TO CHARGE QUESTIONS			

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CHARGE QUESTION 1, *Main Report*: Please comment on the overall organization, clarity, and general effectiveness of the main report. Is it clear what we did, why we did it, and what we learned? If not, how can the report organization be improved?

Page	Line	COMMENT	RESPONSE
		In general, the EPA's draft report is logical, clear, and concise. However, there are some errors in typing that appear in the text (some are listed under Specific Observations) and they need to be checked. Besides that, I have one comment on the literature review (Introduction section). Literature review in the draft report is incomplete. Several relevant studies are not cited. I have provided some relevant references as follows: • Tu, J. 2009. Combined impact of climate and land-use changes on streamflow and water quality in eastern Massachusetts, USA. Journal of Hydrology 379: 268-283. • Wilson, C.O., Weng, Q. 2011. Simulating the impacts of future land use and climate changes on surface water quality in the Des Plaines River watershed, Chicago Metropolitan Statistical Area, Illinois. Science of The Total Environment 409 (20): 4387-4405. • Tong S.T.Y., Sun, Y., Ranatunga, T., He, J., Yang, Y.J. 2011. Predicting plausible impacts of sets of climate and land-use change scenarios on water resources. Applied Geography 32: 477-489. • Luo, Y., Ficklin, D.L., Liu, X., Zhang, M. 2013. Assessment of climate change impacts on hydrology and water quality with a watershed modeling approach. Science of The Total Environment 450-451: 72-82.	The entire text has been reviewed and edited to cover to correct typographical errors. The four references listed in the comment are now cited and discussed in the report.
		Moreover, I have a small suggestion, that the explanation of HUC scales (e.g., HUC4, HUC8, HUC10, HUC12) should be added.	An explanation of HUC spatial scales has been added to the Study Areas section where HUCs are first mentinoned.
		The main report is organized well. Given the enormous amount of information from 20 different watersheds, the report is presented well. The main narrative outlines the issues, methods, and conclusions in a streamlined way. The study area section is lengthy and tedious – I don't have a recommendation on how this could be revised. Perhaps one way is to move study area figures to an appendix. Information on specific watersheds is presented as appendices to avoid information overload in the main report. The report is clear and well written. While there is a lot of technical content, it is written for a professional audience. The report might be accessible to a common reader without a technical background through the executive summary and other introductory sections that present general information with graphics. The report is effective in presenting this very important topic without losing focus. The report is clear on the nature, scope, and learning outcomes of the study.	The study area section has been greatly shortened, with narrative descriptions moved to the appendices. The maps and summary tables are retained in the main report and additional comparative information has been added to the tables.
		The descriptions on the model set up, as well as the configuration, calibration, and validation for each of the study areas, are also systematically organized in the appendices. With this format, the material and information pertinent to the study are disseminated in a clear and effective manner, providing the readers a distinct notion of what was being performed and studied and the important lessons learned.	Comment. No response necessary.
		In my view, the report is, more or less, well organized and clearly presented the motivation, objective, modeling procedure and the results.	Comment. No response necessary.
		The conclusion part can be improved by providing some more specific and quantitative concluding points.	Same as previous comment on Row 13

CHARGE QUESTION 2, *Main Report*: What information, if any, could be added or removed to improve the main report?

Page	Line	COMMENT	RESPONSE

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		In the main report, the results of changes in streamflow, nutrient, and sediment loads to climate change are presented, but I didn't find any information on future changes in climate variables. Thus, I suggest that climate changes (e.g., changes in precipitation and temperature) characterized by the selected projections should be added in the main report. It will make the responses of streamflow and nutrient and sediment loads to potential climate change easier to follow.	Two new tables and summary text describing projected changes in precipitation and temperature for each study area have been added to the beginning of Section 7.2, Sensitivity to Climate Change Scenarios
		The representativeness of the 20 watersheds could be elaborated more. It looks like most are within 6.19% impervious, excepting Coastal southern CA, which is 15.5%. This has a bias toward streams that could be less impaired, assuming the 10% that is suggested in many watershed studies. This should be acknowledged in some way. It might help to justify this deficiency with the parameterization in urban scenarios used later with climate change simulations. The literature on similar studies could be added. For example: • Taner et al, 2011 Ecol. Modeling • Butcher et al., 2010. ASCE Watershed Management 2010: pp. 1350-1361. doi: 10.1061/41143(394)121 • Ficklin, D.L., Y Luo, E Luedeling, M Zhang - Journal of Hydrology, 2009 • Marshall and Randhir, Climatic Change, 2008	The 20 study areas are at the HUC4 spatial scale. At this large scale few watersheds would have net imperviousness greater than 15%. However, individual subwatersheds within a study area have substantially greater imperviousness. Results in this report are presented at the HUC8 or larger scale. Additional text has been added in the report to stress the point that development effects will be larger at smaller spatial scales where development is a larger fraction of drainage area. Additional analysis is also underway and will be published sepatately based on smaller scale results to better understand the relationship between cliamte change and urban development across scales. The additional literature recommended has been cited and discussed in the report, with the exception of Butcher et al., 2010 which was an early (and outdated) conference presentation the work described in this report.
		Please include references to any previous meetings (e.g., AGU) if the results are presented.	Conference presentations are not considered acceptable references for EPA reports. Thus we do not cite presentations.
		In the section of study areas, the report provides a full description of the study areas, including their land cover, climate, and hydrologic systems. But, it does not explain why the areas were chosen, except that for some study areas, hydrologic modeling exercises had been performed. The "opportunities for leveraging the availability of pre-existing watershed models" constitute a convenient and practical reason, yet they do not provide much scientific merit. On page seven of the Main Report, it states that the study areas "…were selected to represent a range of geographic, physiographic, land use, and hydroclimatic settings." To improve the report, further description of the study areas in terms of these differences is needed. For example, in addition to Table 2, it will be helpful to state the hydro-climatic and urban development regime of each study area. Will a particular study area face drastic climate change (for example, much drier or much wetter) or land use change (such as undergoing rapid urbanization and sub-urban sprawl) in the future? Are these areas experiencing water resource problems, such as droughts, floods, and deteriorating water quality? Will these problems be expected to aggravate in the face of future climate change and urban development? By addressing the scientific significance of these study areas, the authors can justify the choice of these areas in furthering our understanding of the sensitivity of streamflow, nutrient, and sediment loads to potential climate change and urban development.	Further description has been provided regarding the hydroclimatic, physiographic, and ecoregional distribution of the study areas and the rationale for site selection has been further expanded. It is inevitable that selecting 20 areas (out of about 222 HUC4s present in the US) can provide only a small sample of the total variability that is present. Data availability for calibration was also an important, practical factor in site selecion. The expected intensity of future changes in climate or land use was not an explicit factor in site selection; rather, these issues were investigated as an outcome of the analysis. It is therefore not deemed appropriate to present these factors in the section on study area selected.
		Given the summary information provided on pages 7 to 11, and more detailed information about the study area and catchments found in the appendices, I would suggest removing from the main report the catchment descriptions appearing on pages 12 to 51. The content presented in these pages is just read from Tables 1-3. After removal of these pages, the text on page 7 can be expended a bit to provide a summary of the common features and uniqueness of the study catchments.	Descriptions of the study areas have been moved to the appendices and removed from the main report.

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CHARGE QUESTION 3, *Main Report*: Please comment on the technical quality of the main report. Are the methods appropriate to the goals of the report? Are the results and conclusions justified and adequately qualified where necessary? Can you identify any technical flaws?

Page	Line	COMMENT	RESPONSE
59	35-40	The methods used in this study are appropriate. However, I have one comment on the use of the land-use map in 2001 for the 1971 to 2000 time period. It is well known that land-use features change with time. Thus, use of a land-use map for the year 2001 to analyze hydrology and nutrient and sediment loads of the catchment for the 1971 to 2000 time period would have inherent errors. These points cannot be skipped and have to be mentioned at least in the conclusions section, if an appropriate land-use map cannot be collected for the study.	A primary goal of the study is to compare responses to climate and land use change mid-21st century to current conditions; that is, to compare ca. 2050 to ca. 2000 conditions. We do not attempt or intend to simulate responses to changes in climate and land use that occurred over the decades from 1970-2000. However, the delta change factor approach to creating future climate scenarios is based on modifying existing meteorological time series. The BASINS4 meteorological data set for approximately 1970-2000 was selected as the common basis for these time series and is assumed to provide a fairly representative picture of natural variability in climate under current conditions (thus neglecting the gradual rise in temperature that has occurred over this period). In sum, the comparison is intended to represent 2000 conditions, with weather data for 1970-2000 applied to 2001 land use. The 2001 NLCD data was selected as the base landuse because it is the only dataset with national coverage and reasonably close to the 1970-2000 base meteorological years. The calibration period used to calibrate models was selected as the latter half of the 1970-2000 period to be best match with the land use represented by the 2001 data. We agree that this issue is likely to confuse many readers and have clarified our approach in the text.
		I am not aware of any technical flaws in the report.	Comment. No response necessary.
		The technical quality of the study is sound and has followed scientific methods in analysis and presentation of the results. The methods fit to the primary goal of the report, which is to understand the vulnerability of watershed systems to climate change. The methods are designed well and are presented well in the report. The results obtained are as expected (variability over geographic space), which reflects observations of past studies. The conclusions are drawn well and qualified with good calibration and validation procedures. The techniques followed are standard procedures in hydrology and there seem to be no major flaws recognizable in the study.	Comment. No response necessary.
		The technical quality of the report is generally good. In this study, the USEPA employed HSPF and SWAT to simulate the hydrologic conditions under future climate and land use changes for five pilot study areas. The report addresses the limitations and short comings of each model. It also discusses the uncertainties intrinsic to the simulation. To simulate the future conditions, the authors adopted a scenario approach. In this regard, the report cautions that "the scenarios evaluated should not be considered comprehensive of all possible futures" (Main Report, pg. 157). Besides, it also notes that "the range of response is also limited by the particular set of climate model projections in the NARCCAP and BSCD archives" (Main Report, pg. 157). The report provides a thorough discussion of the quality and reliability of the results and reiterates numerous times that: "Results therefore represent the potential response of watersheds to different change scenarios, but should not be considered quantitative forecasts of future conditions" (Main Report, pg. 158).	Comment. No response necessary.

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		Based on the results, one model (SWAT) was chosen to simulate the remaining 15 watersheds. This approach is not only appropriate for the study goals but is also reasonable and practical, as it can save on valuable resources. The results show that "…HSPF and SWAT are each capable of providing a good fit to flow and pollutant loads for existing conditions" (Main Report, pg. 108). It also reveals that "the HSPF model provides a somewhat better fit to observed flow and water quality data for the calibration periods" (Main Report, pg. 101), because "…HSPF operates at a sub-daily time step and has a more sophisticated representation of runoff, infiltration, and channel transport processes than does SWAT" (Main Report, pg. 105). But because SWAT "…is able to represent the complex processes affecting plant growth, nutrient dynamics, and water budgets under changing climate…" and it is "…somewhat easier to set up and calibrate than is HSPF…" (Main Report, pg. 108), it was chosen as the model for the remaining analyses. However, as urbanization continues, it is inevitable that the amount of vegetation cover in these watersheds will decrease in the future. Hence, to improve the report, the authors may want to provide a more comprehensive discussion on the effectiveness of SWAT in simulating the hydrologic conditions under a decreasing vegetated but increasing impervious surface. A comparison of the performance of the HSPF with the SWAT under a highly urbanized watershed may provide more insights supporting the efficacy of SWAT in modeling the hydrologic consequences of urbanization. This may complement the conceptual argument. Moreover, it will be helpful to further discuss the plausible future changes in the land use patterns in terms of not only the amount of urban and suburban areas but also the vegetated cover.	Simulation of runoff from impervious surfaces is conceptually simple; however, as noted in the comment, hydrological processes on urban pervious land present more complexities. In addition, urbanized areas typically have extensive stormwater management, including detention facilities, manmade conveyances, and a variety of nonstructural features that influence hydrology. In many locations they also have extensive use of groundwater or imported surface water for landscape irrigation. While these issues are important, data are not readily available for detailed characterization of stormwater management. Further, the NLCD provides limited resolution of developed pervious land to characterize current land use, while the ICLUS product primarily informs us of potential future changes in imperviousness and does not provide information on changes in urban perviois areas. The simulations thus do reflect general decreases in vegetative cover anticipated in the future. We do agree that HSPF potentially provides greater capabilities for urban land simulation, primarily due to use of a subdaily time step and more sophisticated treatment of infiltration processes; however, the lack of data on the details of urban pervious cover and drainage management reduce the potential value of these differences. Additional text has been added to the discussion on the abilities of SWAT and HSPF for simulation of urban lands.
		Another way to improve the technical quality of the study is to elaborate on the IPCC future climatic scenarios. How realistic are these predictions? How do these values differ from other GCM prediction results? Such a discussion will enhance the credibility and the intrinsic values of the study, as the future climatic scenarios provide the basic framework for the hydrologic simulation exercises.	The NARCCAP and BCSD climate change scenarios evaluated in this study represent a plausible set of future conditions but are not intended as quantitative forecasts. It is stated in the text that results are conditional on these scenarios, and that use of more or other scenarios could lead to different results.
		The study adopted the USEPA ICLUS urban and residential development scenarios. A better description of ICLUS is desirable. For example, does it predict the changes in land use/land cover besides urban landscapes? What are the assumptions underpinning the urban development scenarios in ICLUS? The report mentions that ICLUS utilizes population data to generate the future urbanization scenarios. Does it consider the impacts of road networks in urbanization as well? What will be the future land use patterns in different watersheds?	For brevity we do not describe the methods used by ICLUS in detail. Additional, but not comprehensive, text has been added describing the ICLUS data set. Citations for published literature on ICLUS are listed in the report where readers can go for a more detailed description of the methodology used to generate the ICLUS data.

		Furthermore, it will be useful to have a more thorough discussion on the scale issue in the report. The findings that the "simulated responses to urban development scenarios were small relative to those resulting from climate change…" (Main Report, pg. 2) and the "results of the model scenarios that combine climate change and land use change are generally consistent with those for the climate scenarios" (Main Report, pg. 132) may be attributed to the scale used in the watershed simulation. As the authors have acknowledged, the resolution of four- or eight-digit-HUCs in the simulation may be too coarse to provide prominent effects of urbanization on streamflow, nutrient and sediment flows since, under such a scale, "…developed land is rarely a large portion of the total land area" (Main Report, pg. 127). Hence, for those watersheds, which are undergoing a rapid urbanization process, an additional analysis using a finer spatial resolution (for example, twelve-digit-HUC) may be useful to ascertain the hydrologic consequences of urbanization. With such an analysis, the authors may be able to tease out the impacts caused by urbanization, and most importantly, the aggregate impacts of climate change in tandem with land use change.	More detailed analysis of the influence of scale on urban development impacts is beyond the scope of this report. Results in this report are presented at the HUC8 or larger scale. Additional text has been added in the report to stress the point that development effects will be larger at smaller spatial scales where development is a larger fraction of drainage area. Additional analysis is also underway and will be published sepatately based on smaller scale results to better understand the relationship between cliamte change and urban development across scales.
		In general, the technical quality of the report (methods, data analysis) is good. Well-known methods and models are used in the study, which are appropriate to the goals of the report; results are clearly presented and justified.	Comment. No response necessary.
		I have the following particular concerns regarding technical issues: a. The calibration and validation results for both models (SWAT and HSPF), on both the pilot basins and all watersheds are, in general, rather poor to fair (compared with my experience of hydrological modeling in over 25 years), in terms of Nash-Sutcliffe efficiency in general, and in terms of flow volume error, in some cases. I am sure this result is not to the strength of the models, and better or much better results could be achieved, unless authors can prove that there are significant errors in the data (in this case, other catchments should be selected). This is something to do with the skills of the persons who calibrated the model and the method of calibration. In my experience, without a special reason (like global data instead of observed data used in model calibration, large data errors, etc.), I would expect the Nash-Sutcliffe efficiency ≥ 0.7 and relative water balance error $\leq 5\%$ for, at least, the calibration period, if the models were properly calibrated.	This study required development of models for 20 large watersheds, many of which have significant active management by reservoirs, withdrawals, and large point source discharges. Given the large scope of this effort, it was necessary to standardize model development for efficiency. Management activities are addressed in simplified form, either using constant assumptions or by specifying stage-storage (passive management) relationships for reservoirs. These compromises inevitably introduce uncertainty into the calibration, but are considered adequate for projecting relative changes for current versus future conditions. Certain study areas were more difficult than others (e.g., the Verde River in Arizona and the Loup-Elkhorn system in Nebraska have significant inputs from regional groundwater systems that can only be roughly approximated in a watershed model not coupled to a full groundwater model). All these conditions tend to decrease the calibration statistics. More importantly, the draft report focused on the NSE calculated with daily flows for the purpose of comparison of SWAT to HSPF (which is forced by sub-daily meteorology). SWAT operates at a daily time step and is typically evaluated relative to NSEs calculated on monthly flows, as recommended by Moriasi et al. (2007). We have now added monthly NSEs to the reporting on model calibration. Over all calibration and validation sites, the median monthly Nash-Sutcliffe E coefficient from the SWAT models was 0.74 for both the pilot and non-pilot study areas. The monthly NSEs are in most cases substantially higher than the daily NSEs previously presented (for example, the daily NSE for the initial calibration site in the Minnesota River basin was 0.79 but the monthly NSE was 0.91). Given recommendations that an NSE of 0.5 is adequate and 0.7 reflects a "good" model calibration we feel that the quality of model fit is sufficient to provide a basis for evaluation of relative responses to changes in land use and climate.

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		b. On page 71, authors present a baseline adjusted coefficient of model fit efficiency and discussed its advantage, but I do not see its results in the main report.	The baseline-adjusted coefficient of model fit efficiency is recommended for use with the monthly load series, where continuous observations are not available. This statistic is referred to in the context of monthly load predictions in the revised report in Section 6.1.1.2 and Figure 17.
		c. For the hydrological impact of climate change, it is more interesting/important to see the seasonal changes than the long-term average changes, but this is not done/presented in the report. It is well-known that climate change scenarios themselves have strong seasonality and the same is true for the hydrological impact, especially in seasonally snow-covered regions.	Given the large spatial scale of this study (20 ~HUC4 scale sites), it was necessary to simplify and reduce the amount of data presented. We chose to do this in the main report by focusing annual values. We agree, however, that changes in seasonality are important. We have added new text to the report including a data table that shows the ratio of winter to summer runoff, both by study area and by climate scenario. Full monthly results are shown in Appendices X and Y. In addition, the "Time to Centroid" metric now shown in the results section reflects potential changes in the seasonal distribution of streamflow.
		d. The impact of climate change on floods and droughts could have been studied on pilot watersheds, if not on all. (Maybe hydrological extreme is not an issue of special concern to EPA?)	Results are presented for both 7-day low flows and 100-year peak flows. These results are presented in Chapter 7 on results from all 20 watersheds and not on the section on sensitivity studies in the 5 pilot watersheds.
		e. The spatial scale used in the study is too coarse to draw useful conclusions on the impact study of land-use change to water quantity and quality.	We agree and state this in the text. Analysis is currently underway to better address this issue.
		f. The following concluding points (pages 163-164) are very general and readers of this Report know them before reading the report (because these are the conclusions found in almost any of such studies): • GCMs' projections are generally consistent in showing a continued warming trend over the next century, but offer a much wider range of plausible outcomes in other aspects of local climate – particularly the timing and intensity of precipitation and the energy inputs… • In many cases, the range of simulated responses across the different climate models and downscaling methodologies do not agree in direction. • Wetter winters and earlier snowmelt are likely in many of the northern and higher elevation watersheds. • The simulation results also illustrate….the sensitivities and uncertainties associated with use of different watershed models, different approaches for downscaling climate change simulations. • This study also suggests potentially important sensitivity of results to the use of different hydrologic models (HSPF and SWAT in this study).	The report has been extensively revised with added detail and discussion of results in the sections on Results, Conclusions, and Executive Summary.
CHARGE QUESTION 4, *Appendices* : The majority of the Appendices are detailed calibration reports repeated for each of the 20 study areas. We do not ask that you do a detailed review of each specific model described in the appendices. Rather, please comment on the general organization, clarity, and type of information presented in the calibration reports. Is there any missing information you would like to see?			
Page	**Line**	**COMMENT**	**RESPONSE**

		The information presented in the Appendices is appropriate and sufficient. Besides that, I would like to suggest that additional information on the rationale for selecting the sensitive parameters for the simulation of streamflow, nutrient, and sediment, as well as on the values of calibrated parameters, be presented in a short and precise manner in Appendices D through W.	The 20 SWAT and 5 HSPF large watershed models were developed and calibrated by multiple modelers from a consortium of four different firms. A modeling protocol (shown in Appendix A) was used to help ensure consistency, but details of the calibration were left to the judgment of the individual modeler, and inevitably vary with the characteristics of a given study area. In general, details on the parameter adjustment process are provided in the discussion of the pilot sites, while the calibration process for the 15 non-pilot sites is reported in abbreviated form. We appreciate that additional information on the rationale for selecting specific calibrated parameter values in the non-pilot sites would be of interest, but additional written details are not readily available. Further, the focus of the report is scenario development for future climate and land use conditions, not model calibration. Model input files are available on request for those who wish to delve deeper.
		The appendices are required for readers needing more information on specific watershed studies. The organization is fine. The calibration and validation information is presented well, with no major shortcoming.	Comment. No response necessary.
		The SWAT Model setup is presented without a similar one for HSPF. It would be useful to include for consistency and also for anyone willing to replicate the results.	Appendix A has been expanded to address the HSPF setup protocol.
		Appendix C is a memo on precipitation intensity. Are there similar ones developed for temperature and other climatic variables?	No. Changes to temperature are simpler. The memo on changes in precipitation intensity was done to inform our approach for representing a particularly complex, uncertain and important issue.
		The appendices are very thorough, providing adequate and detail information regarding the model setup, the procedures for model calibration, validation, and quality assurance, the preparation of future climatic data, the description of each watershed, the results of the calibration and validation of the hydrologic and water quality models for each watershed, and the simulation results for each watershed under future climate and urbanization scenarios. The information provided is well-organized, clear, and useful.	Comment. No response necessary.

CHARGE QUESTION 5, *Main Report and Appendices* : Do you have other comments, concerns or suggestions for improving the quality of main report or appendices?

Page	Line	COMMENT	RESPONSE
		To improve the report, in addition to a listing of the parameters used in the calibration, Table 10 and Table 11 can be expanded to include the new calibrated parameter values used in the modeling exercises. Also, discussion can be provided about the sensitivities of these parameters under different hydroclimatic conditions as exemplified in different watersheds. This information will be useful to SWAT modelers as they attempt to calibrate and validate their models in their research projects.	The 20 SWAT and 5 HSPF large watershed models were developed and calibrated by multiple modelers from a consortium of four different firms. A modeling protocol (shown in Appendix A) was used to help ensure consistency, but details of the calibration were left to the judgment of the individual modeler, and inevitably vary with the characteristics of a given study area. The cited tables are intended to indicate the most important hydrology calibration parameters across models; however, many other parameters are used and could be varied at modeler discretion. It is not feasible to summarize all the calibrated parameter values in a single table. Further, the focus of the report is scenario development for future climate and land use conditions, not model calibration. Model input files are available on request for those who wish to delve deeper.

AUTHOR RESPONSE TO EXTERNL PEER REVIEW COMMENTS ON DRAFT EPA REPORT:

"Watershed Modeling To Assess the Sensitivity of Streamflow, Nutrient, and Sediment Loads to Climate Change and Urban Development in 20 U.S. Watersheds"

CHARGE QUESTION 6, *Public Comments* : Please comment on the relevance and validity of public comments received by EPA on this report. Are there any public comments you strongly agree or disagree with?

Page	Line	COMMENT	RESPONSE
		Most of the public comments are valid and are related to the content of the report. I strongly agree with the comment on spatial scale to investigate the impact of urban development. The comment suggests that the authors look at the results for some of the sub-basins, especially within sub-basins that are expected to have the greatest increase in urban development to investigate the impact of urban development on streamflow and nutrient and sediment loads.	More detailed work on urbanization effects in smaller subbasins is beyond the scope of this report but this analysis is currently underway and will be submitted for publication as a jounral article.
		Observation on the Anonymous public comment: I disagree with this comment that there is huge and growing literature on falsification of IPCC findings. It is actually that literature has been increasing in confirming IPCC findings. Most statements in the comment do not agree with scientific findings in watershed science or climate change science.	Agree.
		I agree with the comments provided by the Alliance for the Great Lakes on the scale issue, the New York City Department of Environmental Protection on future climate and land use scenarios, and NOAA on climate uncertainty. It will enhance the quality of the report if the authors can address these concerns.	Agree. Text has been added to strengthen the conclusions regarding the relatively small magnitude of land use effects at the large spatial scale of this study.
		Conversely, I do not agree with the comments provided by the anonymous letter. To be credible, the author of that letter should provide more information and cite more specific references to substantiate the claims.	Agree.
		The Public Comments, as expected, represent a wide range (sometimes contradictory) of opinion/concern about the study. For example, one anonymous reviewer considers this study is meaningless, which also represents a group of scientists and politicians' views. Although I do not largely agree with the opinion of this comment, on balance, I agree with some aspects of the anonymous reviewer's argument, such as EPA's report should also acknowledge, somewhere in the report, some scientific information that is contrary to IPCC's option. For example, "there are many peer review scientific articles that disagree with the conclusions of the IPCC."	Agree.
		The evaluation report from "Alliance for the Great Lakes" brings a couple of good comments that I largely agree with.	Agree. Text has been added to the report addressing the comments made by the AGL.
		I also fully agree with the comment raised by "NYC Environmental Protection" that suggested EPA select some sub-basins that have a large percentage of land-use change to study the impact of urbanization.	More detailed work on urbanization effects in smaller subbasins is beyond the scope of this report but this analysis is currently underway and will be submitted for publication as a jounral article.

III. SPECIFIC OBSERVATIONS, *Document/Content*

Page	Line	COMMENT	RESPONSE
53	21-24	Because the HSPF and SWAT differ in the way that they present infiltration, I suggest that the information on calculation of infiltration in those two models should be presented in more detail.	Additional detail on the calculation of infiltration by the two models has been included in the text.
54	26-29	Because the HSPF and SWAT differ in the way that they present infiltration, I suggest that the information on calculation of infiltration in those two models should be presented in more detail.	Same as above comment.

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54	38-39	Because the advantage of SWAT is that it incorporates a plant growth model (including representation of CO_2 fertilization), I suggest that the plant growth model should be presented in more detail.	The SWAT plant growth model described in detail in the cited reference Neitsch et al. (2005). The plant growth model was not modified for this work. To maintain focus of this report on our objectives we think it better to cite Neitsch et al. and not decribe in detail in this report.
57	19-20	What is the reason for dividing the slopes into two categories: above and below 10 percent?	Slope is an important factor in determining flow partitioning between surface and subsurface pathways and estimating erosion potential. Multiple slope categories could be used. As described in the report, "A single breakpoint was chosen to represent major differences in runoff and erosive energy without creating an unmanageable number of HRUs."
59	35-40	It is well known that land-use features change with time. Thus, use of a land-use map for the year 2001 to analyze hydrology and nutrient and sediment loads of the catchment for the 1971 to 2000 time period would have inherent errors. This point should be mentioned.	Same as a previous comment. See response to comment on line 32.
64	3-5	How were daily records disaggregated to hourly time step? Please explain it in a short and precise manner.	A description of the disaggregation process used in developing the BASINS meteorological dataset has been added to the text. That work was done as part of a previous, unrelated project.
71	33-38	How were sensitive parameters for simulation of streamflow, nutrient, and sediment loads selected? Please explain it in more detail.	See responses under charge questions 4 and 5 above.
92	13	More detail of the house density ranges is suggested.	Text has been added to this section to add detail and clarity.
104	15-16	A figure that shows the SWAT and HSPF simulated changes in total phosphorus should be added.	This figure has been added.
2	Line 25	There is a need for better justification on minor role of urban development. Land use can be a dominant player.	We agree that urban development be a dominant influence on watersheds in cases where changes in urban land are large. It is stated in the report that the apparently minor role of urban development is due to the scale at which results are important (HUC8 or larger). At these scales, the net increase in urban development over the entire upstream watershed is generally small. More detailed analysis of the influence of scale on urban development impacts is beyond the scope of this report. Additional analysis is, however, underway and will be published sepatately based on smaller scale results to better understand the relationship between cliamte change and urban development across scales.
3		Next steps could include an analysis of the influence of scale on climatic and land use impacts.	Agree. Further analysis of the influence of scale on urban development impacts is currently underway and will be published separately as a journal article.
10-11	Tables 2-3	1. Some information in these two tables is the same, like forest%. 2. In Table 3 you have specified that the land cover data are for 2001, but land use data in Table 2 represent which year or period? 3. In Table 2, what is Acric? 4. In Table 2, Urban%+Acric%+Forest% ≠ 100%, what are others?	The tables have been revised so that they do not present redundant information. Land use in Table 2 (which was also from NLCD 2001) has been removed. "Acric" was a typographical error for "Agric", intended to represent agricultural land, but has not been removed. Land uses other than urban, "acric", and forest would be pasture, grass, range, etc.; however, land use is no longer shown in Table 2.
131	Table 36	You might need to check and comment on the value of 974.20%.	This was a data entry error and has been corrected.

III. SPECIFIC OBSERVATIONS, *Editorial (Grammar/Typos)*

Page	**Line**	**COMMENT**	**RESPONSE**
4-6		Most of the cited references have not been listed in the References section yet.	Several cited references were omitted in the draft. Missing references are now listed in the Reference section.

AUTHOR RESPONSE TO EXTERNL PEER REVIEW COMMENTS ON DRAFT EPA REPORT:

"Watershed Modeling To Assess the Sensitivity of Streamflow, Nutrient, and Sediment Loads to Climate Change and Urban Development in 20 U.S. Watersheds"

73	40	The appendices should be listed specifically.	Text revised to say … "in Appendices D through W (calibration reports)".
84	34-35	"However, in some cases (e.g., in Arizona, there is greater change in the 25-50th percentile bin)." changes to "However, in some cases (e.g., in Arizona), there is greater change in the 25-50th percentile bin."	Text has been revised.
89	6	"(as discussed in Section 5.1.2. For each of …" should be "(as discussed in Section 5.1.2). For each of …"	Text has been revised.
101	24	"(see Section 6.1.4.)" should be "(see Section 6.1.4)."	This is a result of the way the Heading 3 style was defined. Format has been corrected.
110	15	"Table 14" or "Table 15"? Please have a look at.	The comment is correct and the text has been revised to correct the error.
111	17	"HSPF output" or "SWAT output"? Please have a look at.	The text did intend to refer to HSPF output, which was not shown. This has been clarified in the text.
113	15	"Section 5.1.2. , direct comparison…" changes to "Section 5.1.2, direct comparison …"	This is a result of the way the Heading 3 style was defined. Formatting has been corrected.
118	12	"63% to 240%." According to the Table 27, it should be "62% to 240%."	Text has been revised.
118	28	"… in Table 34" changes to "…in Tables 32-34."	Text has been revised.
157	24	"limted" should be "limited."	Text has been revised.
157	37	"limted" should be "limited."	Text has been revised.
159	8	"adapation" should be "adaptation."	Text has been revised.
Appendix A, page 2	27	"bas been analyzed" should be "has been analyzed."	Text has been revised.
Appendix D, page 21	2, 4	"(Figure 6.)" changes to "(Figure 6)."	Text has been revised.
V and VI		Formatting – page numbers are not aligned.	Formatting has been corrected.
65	Fig 23	This is unreadable. Maybe re-categorize it to groups.	We agree. Previous Figures 23 and 24 were confusing and have been deleted from the final version.
79	Table 14	Move description of abbreviations to bottom of table.	Text has been revised.
165+		References are inconsistent in format.	References have been reformatted.
168	Line 1	Year is missing.	Text has been revised to include the year (2011).
6	2	You number Section 2.1, but no 2.2, 2.3, etc.	Formatting error has been corrected.
99	Figure 26	I do not think the right panel graph in Figure 26 seems correct; it looks identical to the right panel graph of Figure 25.	The comment is correct. The wrong figure was inserted. The correct figure is now inserted.
118	5, line 28	"...are summarized in Table 34" should be written as "...are summarized in Tables 32-34."	Text has been revised.