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Discussions

Interactive comment on "An integrated water system model considering hydrological and biogeochemical processes at basin scale: model construction and application" *by* Y. Y. Zhang et al.

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Response to Anonymous Referee #2

Dear Review,

Thanks very much for your useful comments and suggestions on our manuscript. We have revised the manuscript accordingly, and detailed corrections are listed below. The revised manuscript is also provided in the Supplement.

The manuscript describes a novel attempt for integrated water modelling with relying on existing submodels. The model description is coupled with a case study in the Shaying catchment (East China), where a partial comparison is made to the SWAT model based on a previous study. The topic is timely and would nicely fit into the scope of HESS. I think, however, that the manuscript needs significant changes to be acceptable.

Certain parts (Introduction and conclusion) of the paper promise more than the study actually does/did. While this is mainly a stale issue, there are more severe problems with the general presentation of the work. The introduction is built around the idea that the (in my opinion old yet persisting) challenges of integrated modelling need to be addressed by new models because old models have intrinsic problems (being limited in scope or too simplistic) that prevent their meaningful usage in integrated assessments. While this may be true, the reader gets surprised in the following section that the new model developed to solve these problems is mostly a different mix of the old ingredients of the same conceptual models (for example significant bits of QUAL2K and SWAT are re-used). For this reason I think that the presentation of this indeed interesting model and case study should be done in a different way. As SWAT is really encompassing many of the proven thematic submodels, it is not a shame that HEXM has a significant structural similarity to it. Additionally, SWAT is very far from being perfect from both practical and theoretical points of view, so alternative models have their raison d'être as well. I would less emphasise the general problems of SWAT because - as a close relative - HEXM shares most of them. The focus could be put on the advantages of HEXM stemming from the different scope (for example dams) and different submodels (hydrology, soil, etc).

Response: Thanks for your comments. The introduction and conclusions sections were revised greatly. In the introduction section, the main attention was paid to the advantages and disadvantages of most existing models to resolve the current complex water issues caused by the interconnections among water and other related environmental processes. Most of existing models focus on one or two major processes in the field or basin and could just get satisfactory performance of these major processes. SWAT is a typical integrated water system model, which simulates most of water related processes over long time periods at large scales and has been widely used all over the world. Its model structure and powerful functions are considered as a landmark in the field of water system modeling. However, not all of water related processes could be captured well in the practice, due to the module applicability and inaccurate descriptions of some modules (See P4 L8-25).

An integrated water system model is developed by considering the more accurate hydrological and biogeochemical processes with the aim to improve the model performance of some key water related elements in complex basins. The main features of our proposed model and its key differences from SWAT were specified in the Section 2 (Model Framework) (See P6 L3-P7 L7).

The conclusions section was replaced by the section of conclusions and discussion and restructured greatly. The theoretical implication, scale issues and practical implication of the proposed model were discussed. The possible future works were also discussed. (See P21 L3-P22 L30)

A structural problem is the lack of sufficient discussion. While the model itself and the results of the case study are presented in acceptable detail, the theoretical implications of putting these submodels together and a structural comparison of the new model to the criticized predecessors is completely missing. It would be interesting to learn about the problems when conceptual models operating on different spatial and temporal scales (for example hydrology operates on subcatchments, soil biogeochemistry and erosion are on the site scale) are connected in an integrated framework. These issues have to be addressed if HEXM is considered as a new integrated model and not a modification/extension of SWAT.

Response: Thanks for your comments and suggestions. The conclusions section was replaced by the section of conclusions and discussion and restructured greatly. The theoretical implication, scale issues and practical implication of the proposed model were discussed. The possible future works were also discussed (See P21 L3-P22

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L30) .

Furthermore, the main features of HEXM, its key difference from SWAT and the multi-scale solution were given in Section 2 (model framework) (See P6 L25- P7 L7) .

Specific comments

P 9222 L 5: I think that this hindrance will actually continue in the future too.

Response: Thanks for your suggestion. It was revised as "hinder sustainable development in many regions over the world". (See P2 L16,17)

P 9222 L 7: This is difficult to understand: What do you consider as "the traditional hydrological method" for solving water pollution and ecological degradation?

Response: Thanks for your comments sorry for the less clear sentence. We accurately meant the single disciplinary approach such as hydrological models or water quality models. The sentence was revised to "It is impossible to address these water problems using only the traditional single disciplinary approaches (viz., hydrology, environmental sciences or ecology) because of the interconnections among water and other related environmental processes in the complicated water system (Kindler, 2000)" (See P2 L20-23).

P 9222 L 9-11: Process-oriented modelling may indeed be the most efficient tool, but here no justification is given. What are the arguments against other methods?

Response: Thanks for your comments. As suggested by the same reviewer later, we put less emphasis on whether a model is process-oriented or not. This sentence was revised as "The integrated river basin management might be one of the most sensible frameworks to comprehensively tackle these problems at basin scale. Thereinto, the integrated water system model is a reasonable practice to simultaneously simulate water related elements (flow regimes, nutrient loss, sediment and water pollution) (Kirchner, 2006), and also an effective tool to support water resource allocation, environment flow management, river ecological restoration (Arthington, 2012).." (See P2 L23-29)

P 9222 L 14: I think that “energy process” is rather “energy fluxes”.

Response: Thanks for your suggestion. It was revised accordingly. (See P3 L2)

P 9222 L 15: These sentences can be made much shorter without any loss of information: “For example, the physiological and ecological processes of vegetation affect evapotranspiration, soil moisture distribution and infiltration, and nutrient sorption and movement. On the contrary, soil moisture and nutrient content directly affect crop growth. Overland flow affects the pollutant loads to water bodies.”

Response: Thanks for your suggestion. It was revised accordingly. (See P3 L3-8)

P 9222 L 27: What is the basis for this statement?

Response: Thanks for your suggestion. The references were added, i.e. “(Wigmosta et al., 1994; Singh and Woolhiser, 2002; Burt and Pinay 2005)”. (See P3, L8)

P 9223 L 4: “Darcy’s”

Response: Thanks for your careful review. Yes it should be “Darcy’s”. However, these sentences seem to be redundant as suggested by Reviewer 1. Therefore, they were deleted in the revision.

P 9223 L 8: Here I think “remote sensing” is more appropriate than GIS. Moreover I don’t see the relevance of “GPS”.

Response: Thanks for your careful review. However, these sentences seem to be redundant as suggested by Reviewer 1. Therefore, the sentence was deleted in the revision.

P 9223 L 11: Here you state that the combination of these rather old yet practical knowledge (in the previous paragraph) into an integrated model system stems from the 1980s. Then I think it is unnecessary to have so detailed description of these relationships in the previous paragraph. The following sentence essentially summarises the whole story: “Several models have been developed based on the mature models of

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different disciplines (hydrology, environment and ecology)."

Response: Thanks for your suggestion. The detailed description of these relationships in the previous paragraph was deleted in the revision (it was also suggestion by Reviewer 1).

We revised the first few sentences in this paragraph as suggested as "Several models have been developed based on the mature models of different disciplines (hydrology, environment and ecology) since the 1980s (Singh and Woolhiser, 2002)" (See P3 L17-18) .

P 9223 L 14: "based" is probably not the best term here. I would suggest to write that different models put the emphasis on different processes.

Response: Thanks for your comment. Instead of categorizing the different type of processes, we revised the sentence as "most of existing models focus on one or two major processes (e.g. hydrology, water quality, biogeochemistry) in simulation." . (See P3 L18-20)

P 9223 L 17: I think that the difference between true empirical and process-based conceptual models is overemphasised. The attributed real-world meaning of individual parameters and processes of conceptual models is not guaranteed to persist when the model is calibrated (see e.g. Mantovan and Todini, 2006; Mantovan et al. 2007). At the same time, calibration-free models are rather rare in environmental modelling. This means that most of our (sub)models are actually empirical to a certain degree. That's why I think one should not overemphasise the distinction between "process-based" and "empirical" models. The main message of this part seems to be that due to the complexity of the integrated system it is rare that each process is simulated with the same detail. Thus one gets best performance for processes that are described with the most detail and only approximate results for others outside of the model's focus.

Response: Thanks very much for your suggestions. We revised the sentences accordingly to the suggestion as “due to the complexity of the integrated system it is rare that each process is simulated with the same detail. Thus one gets best performance for processes that are described with the most detail and only approximate results for others outside of the model’s focus. (see, for example, Mantovan and Todini, 2006; Mantovan et al. 2007)" (See Page 3 L21-24) .

The references were also added in the revision (See Page 31 L30- P32 L4).

P 9224 L 7: This highlights the tradeoff of integrated models. SWAT covers enough processes to be called an integrated model, but from the perspective of specialised models SWAT’s processes are overly simplified. However, the entire integrated system is so complex that it is one of the largest environmental models and there is practically no chance to calibrate everything.

Response: Thanks very much for your discussion which elaborate more about SWAT and integrated models in general. We extended the discussion in the revision as “So far, SWAT is a typical integrated water system model, which simulates most of water related processes over long time periods at large scales (Arnold et al., 1998). Its model structure and powerful functions are considered as a landmark in the field of water system modeling . However, not all of water related processes could be well captured in practice, such as daily flow and extreme events (Borah and Bera, 2004), soil nitrogen and carbon (Gassman et al., 2007), the performance in regulated basins (Zhang et al., 2012). The probable reasons were the applicability and inaccurate descriptions of some modules (Neitsch et al., 2011). In particular, two methods are adopted in SWAT to estimate surface runoff, viz., Soil Conservation Service (SCS) curve number method and Green-Ampt infiltration model. The SCS equation is usually given priority, but it is developed for rural watersheds in the United States and the applicability of curve number to other regions is questioned (Rallison and Miller 1981). The Green-Ampt infiltration model is usually limited to simulate flow events at micro scales (time: hours or minutes, space: fields or 10-1 to 10 km2 watersheds) (Brakensiek, 1977; King et al., 1999). Furthermore, it is much more difficult for SWAT to capture the complicated dynamic processes of soil nitrogen and carbon accurately compared with other biochemistry models, such as DNDC (Li et al., 1992; Gassman et al., 2007). ” (See P4 L8-L25)

P 9224 L 7: “described in its model setting” can be deleted.

Response: Thanks for your suggestion. It was revised accordingly.

P 9224 L 12: As mentioned before, “over-simplified” is only true at the scale of single processes.

Response: Thanks for your comments. As suggested by both reviewers, the over-simplification of SWAT’s modules was not emphasized in the revision. This paragraph was rewritten to focus on the applicability and mechanisms of the typical integrated water system model (SWAT) (See P4 L8-L25)." So far, SWAT is a typical integrated water system model, which simulates most of water related processes over long time periods at large scales (Arnold et al., 1998). Its model structure and powerful functions are considered as a landmark in the field of water system modeling . However, not all of water related processes could be well captured in practice, such as daily flow and extreme events (Borah and Bera, 2004), soil nitrogen and carbon (Gassman et al., 2007), the performance in regulated basins (Zhang et al., 2012). The probable reasons were the applicability and inaccurate descriptions of some modules. In particular, two methods are adopted in SWAT to estimate surface runoff, viz., Soil Conservation Service (SCS) curve number method and Green-Ampt infiltration model (Neitsch et al., 2011). The SCS equation is usually given priority, but it is developed for rural watersheds in the United States and the applicability of curve number to other regions is questioned (Rallison and Miller 1981). The Green-Ampt infiltration model is usually limited to simulate flow events at micro scales (time: hours or minutes, space: fields or 10-1 to 10 km2 watersheds) (Brakensiek, 1977; King et al., 1999). Furthermore, it is much more difficult for SWAT to capture the complicated dynamic processes of soil nitrogen and carbon accurately compared with other biochemistry models, such as DNDC (Li et al.,

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1992; Gassman et al., 2007). "

P 9224 L 18: It's not a "new challenge", this is the original challenge of integrated modelling from the very first moment. Just like the tradeoff of resource allocation in integrated water management. Nevertheless, the statement is right in the sense that integrated management has just been adopted in the last decades and these issues are now becoming obvious for the practitioners.

Response: Thanks for your suggestion. This paragraph was revised accordingly. (see P4 L29-30)

P 9224 L 26: And most importantly, there is MUCH more data than before, which allows for more detailed analyses.

Response: Thanks for your careful review. The data sets to support the more detailed analyses were high resolution of spatial information data (DEM, land use and crop distribution), chemical and isotopic data from field experiment (Kirchner, 2006), and so on. In the revision, we revised the sentence as "more observation data can be obtained including high resolution of spatial information data (DEM, land use and crop distribution), chemical and isotopic data from field experiment (Kirchner, 2006)" and also presented sentences in different paragraph for better connection (See P3 L14-16).

P 9225 L 1: "non point source pools" of which pollutants/nutrients?

Response: Thanks for your careful review. The nonpoint source pools included nitrogen, phosphorus and carbon. We added the details in the revision. (See P5 L8,9)

P 9225 L 9: "to lay the scientific foundation to promote the implement of integrated river basin management all over the world" is a bit overstated, so I suggest to remove.

Response: Thanks for your suggestion. It was removed in the revision.

P 9225 L 10: "as follows."

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Response: Thanks for your suggestion. It was changed accordingly (See P5 L15).

P 9225 L 21: TVGM is a nice member of the class of saturated path models where the proportion of runoff (=the proportion of saturated area in the (sub)catchment) is the function of the areal mean soil moisture. However, just like the other members of this class, TVGM is a conceptual model. While using TVGM may be a step forward from totally empirical (just like the SCS curve number method) models, SWAT's other built-in runoff function (Green-Ampt, a discrete simplification of the Richard's equation) is even closer to the physical description of runoff formation on small scale. So (i) SWAT's empirical nature should not be emphasised so much and (ii) should not be used as the main argument for developing HEXM.

P 9227 L 13: Same remarks as for P 9225 L 21

Response: Thanks very much for your suggestion. In the revision, the SWAT's empirical nature was not emphasized. Our proposed model was developed by focusing on the improvement of the existing models in the practice and module applicability. Several key processes were considered in more detail in our proposed model including hydrological and soil biogeochemical processes, dam regulation. It was expected to get better performance of key water related components including runoff, water quality concentrations, and nonpoint source pollutant load. Following all the reviewers' suggestions, we summarized the contribution of our proposed model in the revision (See P6 L3-P7 L7).

P 9225 L 25: A reference should be provided for the DNDC model.

Response: Thanks for your suggestion. The reference was added as Li et al., (1992). (See P6 L30)

P 9226 L 3-5: These can be deleted: "based on hydrology", "based on ecology", and "for environment".

Response: Thanks for your suggestion. They were deleted in the revision.

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P 9226 L 7-11: It is a pity that there is no Discussion section, because this sentence could be used to generate valuable content there. The coupling of (sub)models working on such different spatial and temporal scales poses severe theoretical difficulties. The outputs of submodels are only meaningful on their spatial and temporal scale. This means that a surface runoff calculated on a HRU or subcatchment scale cannot be directly used in a field-scale model of for example diffuse pollution. The multi-decade struggle of hydrologists to transfer knowledge between hillslope and catchment scales provides a nice illustration of these theoretical problems (see a summary in: Kirchner 2006).

Response: Thanks very much for your suggestion. This sentence was expanded in Section 2 (See P6 L3-24). The multi-scale solution of our proposed model was also specified in Section 2.6 (See P13 L4- P14 L4). The section of conclusions and discussion was added in the revision (See P21 L3-P22 L30).

P 9227 L 10: SWAT was criticized to be overly simplistic, but the authors chose a simplified PET estimation method instead SWAT's optional more accurate and robust Penman-Monteith due to limited data availability. On one hand this is a perfect illustration of the unavoidable tradeoff of modelling between detail and usability. On the other, it somewhat devaluates the arguments against SWAT.

Response: Thanks for your comment. In the revision, the simplification of SWAT in some processes was not overly emphasized any more. The objective of this study was clarified as to develop an integrated water system model (HEXM) for more accurate simulation in hydrological and biogeochemical processes and to improve the model performance of some key water related elements in complex basins. However, the other processes of HEXM were suitably described based on the tradeoff of modelling between detail and usability. We chose Hargreaves method (Hargreaves and Samani, 1982) to calculate the potential evapotranspiration because it only uses the daily maximum and minimum temperature data. Although the Penman-Monteith method is more accurate and robust, it needs more detailed weather data including solar radiation, wind speed, water vapor pressure, and temperature.

Following all the reviewers' suggestions, we summarized the contribution of our proposed model in the revision (See P6 L3-P7 L7).

P 9231 L 1: Is eroded material anywhere retained along the overland transport paths? This is a significant factor that actually determines the particulate diffuse loads of streams. Usually a tiny portion of eroded particles manage to reach the nearest stream.

Response: Thanks for your comments. Yes. In HEXM, the eroded materials are divided into insoluble and soluble forms. The first form is absorbed in sediment and transported with the sediment into the nearest stream; the second one is soluble in overland flow and reaches the stream with the overland flow (see the appendix of MMM). Both of them are retained along the overland transport paths and only a tiny portion reaches the stream. We clarify this in the revision as "The eroded matters are divided into insoluble and soluble forms and the main sources are the erosion of soil and urban area, the sewage discharge of rural living and livestock breeding. The soil erosion, as the primary source in most catchment, is estimated using DNDC (Li et al., 1992) and the other sources are estimated using the export coefficient method (Johnes,1996). The overland migration processes contain the soluble matter migration with overland flow, the insoluble matter migration with sediment, and the loss during the migration. All of these processes take place along the overland transport paths." (see P11 L8-16)

P 9231 L 11: Same problem as with SWAT: QUAL2 was blamed to be "subject to computational instability and time consuming due to its complexity" on P 9223 L 26. Then why is it used here? (I mean it's OK to use but then it should not be presented as something inappropriate for integrated modeling)

Response: Thanks for your comments. In HEXM, QUAL2 is solved at the subbasin scale in order to improve the calculating efficiency, rather than at the fine grid scale for the numerical solution of water dynamics equations. We clarify this in the revision as "In order to avoid the computational instability and improve the calculating efficiency, the model is solved at the subbasin scale, rather than the fine grid scale for numerical solution." (see P11 L26-28)

P 9233 L 2: It would be great to learn more about the uncertainty analysis method (Bayesian approach). According to section 3.2 it must be a kind of informal method because the evaluation functions presented there are no formal likelihood functions. But that's the most the reader can guess about the uncertainty assessment.

Response: Thanks for your suggestion. Five indices were provided in HEXM to evaluate model performance including bias (bias), relative error (re), root mean square error (RMSE), correlation coefficient (r) and coefficient of efficiency (NS). The RMSE is always used to calculate the likelihood function. As a demonstration of our proposed model, in the case study, we use only bias, r and NS are used in this study. We clarify the reason of using the simple objective function in the revision as "The bias, r and NS are used to evaluate model performance in the case study as a demonstration of our proposed model." (see P16 L4-5).

P 9234 L 9-18: Would be interesting to know the number of people living on the catchment.

Response: Thanks for your suggestion. The social economic data of this study area were added in the revision as "The average annual population (2003-2008) is 32.42 million including 23.70 million rural population. The average annual stocks of big animals and livestock are 8.30 million and 178.42 million, respectively. The average annual amount of chemical fertilizer is 1.55 million ton." (See P15 L1-4)

P 9235 L 7: This sentence about LH-OAT is just confusing the following description of calibration by SCE-UA, so it could be moved to section 3.3.

Response: Thanks. It was revised following your good suggestion. The sentence was revised to "To reduce the dimensions of the calibration problem, we restricted SCE-UA to calibrate only the sensitive parameters as defined by LH-OAT." following the suggestion by the same reviewer later (See P15 L30,31) and we moved description to Section 3.3.1(See P17 L2-3).

P 9235 L 18-24: This paragraph could be shortened because most of these performance measures are well known.

Response: Thanks very much for your good suggestion. This paragraph was shortened (See P16 L10-13) .

P 9235 L 21: The optimal value of NS is not close to 1, it IS 1.

Response: Thanks very much for your careful review. Yes, they are one. However, the sentence was deleted after the paragraph was shorten (See P16 L10-13).

P 9235 L 24: What are the reasons for not using NS for NH4? NS does not have any criterium on the amount or frequency of measurements.

Response: Thanks very much for your good suggestion. We clarify this in the revision as "NS is sensitive to extreme value, outlier and number of data points and is not commonly used in the environmental sciences (Ritter and Muñoz-Carpena, 2013). Thus it is not used to evaluate the NH4-N concentration simulation." (See P16 L6-8)

P 9236 L 1: The references here do not fully explain why the different objective functions have to have these weights. It seems to me that "Madsen 2003" and "Efstratiadis and Koutsoyiannis, 2010" both describe that different objectives can be merged into one by weighting, but I haven't found any explanation for these actual values. Are these weights specially tuned for the Shaying catchment?

Response: Thanks for your comments. Weighting method is introduced as a widely accepted way to comprehensively handle different objectives in Efstratiadis and Koutsoyiannis (2010). In this study, these objective functions are simply aggregated to a single objective (frunoff and fNH4-N) because the case study is only a demonstration of our model performance (See P16 L8-12). In further study, multi-objective optimization algorithm would be used to address this problem. (See P22 L24-30 in the discussion)

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P 9236 L 5-9: This description is a bit confusing. I would rephrase this as follows: "Over 200 parameters (93 lumped, 112 distributed) control the hydrological, ecological and environmental processes of HEXM. To reduce the dimensions of the calibration problem we restricted SCE-UA to calibrate only the sensitive parameters as defined by LH-OAT."

Response: Thanks for your suggestion. The sentence was revised following your suggestion (See P16 L25-P17 L3) .

P 9236 L 10: These parameter abbreviations (WMc, WM, ...) do not make too much sense when one has to look them up in the appendix. I would keep the textual definitions only.

Response: Thanks for your suggestion. All the parameter abbreviations were explained in the manuscript (See P17 L5-9) .

P 9238 L 18: Calling an NH4 model as "environmental simulation" is a bit overstated again. Rather "Water quality" or even "Modelled Ammonium concentrations".

Response: Thanks for your suggestion. The environmental simulation was replaced by water quality simulation. (See P18 L28)

P 9240 L 10: Due to the differing sizes of subcatchments it does not make too much sense to report absolute annual yields. I would suggest to make them specific to catchment or cultivation area.

Response: Thanks for your suggestion. The yield of each subbasin was reported as "The average annual yields ranged from 0.08 t km-2 year-1 to 326.95 t km-2 year-1 with the mean of 76.84 t km-2 year-1." (See P20 L22-23)

P 9243 Appendices: These could be significantly shortened if they would concentrate on those parts which are not published elsewhere. Or an even more complete description (with units!) could be provided as supplementary material.

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Response: Thanks for your suggestion. The appendices were shortened to focus on the different modules with SWAT. The descriptions of similar modules and equations were provided as supplementary material. The units of all the variables were added (See P23 L9,10,13,17; P56-63.)

P 9243 L 5: I would recommend to use the notation of surface runoff (for Rs), fast flow/interflow (for Rss), and baseflow (for Rg) instead of having 3 types of runoffs.

Response: Thanks for your suggestion. It was revised (See P23 L13,17,20) .

P 9255 L 3: “improved USLE”

Response: Thanks for your careful review. It was changed (See P61 L5) .

P 9263 Table 1: Qual2K has a simplified 1D channel hydraulic model inside, but it doesn’t do anything with hydrology.

Response: Thanks for your careful review. It was revised (See P35) .

P 9266 Table 4: What is the reason for the high bias of SWAT in certain subcatchments (Yingshang, Zhoukou) during calibration? One would assume that the calibration mechanism tries to eliminate bias as much as possible when calibrating.

Response: Thanks for your comments. We used bias, correlation coefficient and coefficient of efficiency to assess the model performance of runoff. However, these is a tradeoff among three indices and the optimum of bias does not indicate the other indices get the optima. Thus, a weighted average method is used to aggregate these three objective functions to a single objective in the model calibration and the optimal statistical values are 0. The calibration principle is to get the entire optimum of these three indices. The explanation was given in the revision as " The obvious biases were caused by the tradeoff among these three evaluation indices." (See P17 L25).

P 9267 Table 5: Interesting to see NS values like -9 and -81. These must be as wrong as possible. What may be the reason?

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Response: Thanks for your suggestion. The low flow simulation is actually a difficult task, especially in the regulated river basins. The common evaluation criteria are disadvantageous to evaluate low flow simulation and the discrepancy was usually quite large to the optimal value. However, the model performances considering regulation were improved obviously comparing those without considering regulation. In the future, the low flow simulation will be improved further. More discussion was added in the revision as "the simulation performance still need to be further improved. The probable reasons were that, on the one hand, the low flow forecasting is actually a difficult task especially in the regulated river basins, and the common evaluation criteria are disadvantageous to evaluate low flow simulation (Pushpalatha et al., 2012). One the other hand, the dam regulation module of HEXM is still difficult to fully capture the low flow events." (See P18 L20-27)

P 9268 Table 6: For some stations (Zhoukou, Huaidian, Fuyang, Fantaizi) SWAT seems to be less biased, although HEXM has less variance in bias among the stations. Why is HEXM mostly wrong by about 30%?

Response: Thanks for your comments. We used a combined objective function to assess the model performance of NH4-N including bias and correlation coefficient. The calibration principle is to reach the entire optimum of these two indices. Comparing the SWAT simulation, the performance of HEXM is improved obviously. Moreover, according to Moriasi et al., 2007, the performance is at the good rating if the bias of NH4-N simulation ranges from 25% to 40% or from -40% to 25%, and the performance is at the satisfactory rating if the bias ranges from 40% to 70% or from -70% to -40%. Thus, the model performance of HEXM was good or satisfactory at most stations. More discussion was added in the revision as "Although the bias of HEXM simulation for some stations was greater than that of SWAT, the overall simulation performance was improved greatly by coupling N cycle model (DNDC) because the values of fNH4-N were less than that of SWAT." (See P19 L2-3; L17-21)

P 9272 Figure 4: A part of the WQM figure was taken from the QUAL2 manual. This has to be indicated!

Response: Thanks for your suggestion. The part of instream water quality module was indicated as QUAL-2E model in Figure 4. (See P46)

P 9276 Figure 8: There seems to be an issue with the dam regulation module because 4 out of 7 stations frequently have tiny simulated values when the observed flow was between 10 and 100. How can the red trend line belonging to the grey dots be above the black when most grey points are below the black ones? (cf. Fuyang, low flow)

Response: Thanks for your comments. This figure seems to be difficult to illustrate the model performance of low flow and high flow. The evaluation results were clear in Table 5 (See Page 40). The bias of low flow simulation considering regulation was much less than those without considering regulation at all these stations. The entire low flows were overestimated at most stations (bias < 0.0) except Zhoukou station (0.48), although there were still lots of seriously underestimated points. The low flow simulation is actually a difficult task, especially in the regulated river basins. The dam regulation module of HEXM is the generalization of dam regulation rules and still difficult to well capture the low flow events. In the future, the low flow simulation and dam regulation module will be improved further. Figure 8 was removed as the suggestion of review 1 because it was confusing. The explanation was given as " However, the entire low flows were overestimated at most stations (bias<0.0) except Zhoukou station (0.48), and the simulation performance still need to be further improved. The probable reasons were that, on the one hand, the low flow forecasting is actually a difficult task especially in the regulated river basins, and the common evaluation criteria are disadvantageous to evaluate low flow simulation (Pushpalatha et al., 2012). One the other hand, the dam regulation module of HEXM is still difficult to fully capture the low flow events. "(See P18 L20-27).

P 9277 Figure 9: The simulated outbursts of NH4 seem to be attached to very low simulated flow. Can these be identified as the seriously underestimated points on fig

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8?

Response: Thanks for your comments. Yes. The obvious discrepancies between NH4-N simulation and observation appeared at the seriously underestimated points of low flow. The explanation was given in the revision as " Moreover, the obvious discrepancies between the simulation and observation often appeared in the period from January to May due to the poor simulation performance of low flows." (See P19 L12-14) Furthermore, the improvement of low flow event simulation was discussed in the section of conclusions and discussion (See P22 L18,21,22) .

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/11/C4666/2014/hessd-11-C4666-2014-supplement.pdf

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 9219, 2014.