Response to Anonymous Referee #1

Interactive comment on "Integrated hydrological modeling of the North China Plain and implications for sustainable water management" by H. Qin et al.
Anonymous Referee #1 Received and published: 15 May 2013

[Original comments in black; Authors' response in red]

Qin et al. (2103) introduced a coupled surface water-groundwater model for the North China Plain (NCP, total area: 140000 km2). I found that the paper is interesting and it can be a good contribution to the literature because there are currently still limited studies of coupled surface water-groundwater model in a large scale setting. However, I have to say here that I do not feel to be in position to evaluate their perspectives on sustainable water management.

The paper addresses relevant scientific questions within the scope of HESS. The results are sufficient to support the interpretations and conclusions. Nevertheless, I found that the overall presentation of the paper, especially the explanations of the scientific methods and assumptions, should be improved.

I would like to propose that the following comments should be considered. These comments should not be misinterpreted as negative critiques, but as suggestion to further improve the work.

Abstract. Page 3694, lines 11-14. The performance values (RMSE and R) for the simulated groundwater heads should be given.

Response: The performance values for the simulated groundwater heads are now given in the revised manuscript.

Page 3694, lines 17-18. "The water balance results indicate that more than 69% of water leaving the flow system is attributed to the ET component." How much fraction of the ET is taken from the (saturated) groundwater part?

Response: The fraction of the ET taken from the saturated groundwater part is very low. On average for the simulation period, approximately 0.25% of the actual ET is taken from the saturated groundwater part. We have added a sentence in the revised manuscript to clarify this.

Section 2.1. Page 3697, lines 10-14. A map/figure (showing four main hydro-geological zones of the NCP aquifer) will be very useful.

Response：We agree with the reviewer's comment. We have added the four main hydrogeological zones of the NCP aquifer in Figure 1.

Section 2.3. Page 3698, lines 24-26 and page 3699, lines 1-2. I suggest changing the codes/indexes "10", "20" and "30" to something else.

Response: We agree with the reviewer's comment. We have changed the indexes"10", "20" and "30" to "piedmont", "central", and "coastal" in the revised manuscript.

Section 2.4. Pages 3699-3700. This section needs further explanation and clarification.

Page 3699, lines 22-27. Could the authors illustrate the locations of pumping stations used for irrigation purposes? From which aquifer layers is the irrigation water taken from? Did the authors consider the return flow of the irrigated water?

Response：The method for dealing with irrigation pumping is implemented in the MIKE SHE code. In the NCP, farmers use shallow groundwater wells for irrigation, where the locations of the wells may not be mapped exactly. Therefore, extraction of groundwater for irrigation is carried out from the shallow wells at the same model cells where the water is used. In our current model setup, all groundwater used for irrigation is pumped from the shallow aquifer. The irrigation method is by using sprinklers, which means the irrigated water is added to the precipitation component in the model, and hence added to the return flow since irrigation is calculated as part of the total groundwater recharge. The corresponding modification has been made in the revised manuscript.

Page 3700, lines 3-6. The positions of (some) important cities and industrial areas should be indicated in the figure.

Response: The capital cities (Beijing, Tianjin, Shijiazhuang and Jinan) and other cities are shown in figure 1 in the revised manuscript. These cities are the most important industrial and domestic areas in the NCP area.

Page 3700, lines 6-9. I missed the explanation about the “upper” and “middle” parts of groundwater zone. How much water is extracted from each aquifer zone/layer?

Response: We think the expressions “upper” and “middle” are not precise. The water extracted for domestic and industrial use is assumed to be pumped from the deep aquifer (layers 2 and 3, or aquifer units III and IV). No water is assumed to be pumped from the shallow aquifer. We have revised the sentence in this part to explain this assumption more clearly.

Page 3700, lines 9-23. How was the location of each pumping well verified? I think the explanation and presentation of this method should be improved.

Response: We use the concept of “virtual” wells in the present study. That’s to say, the pumping wells for domestic and industrial purposes are all artificially allocated in the model, with the purpose of simulating the groundwater extraction for domestic and industrial uses on the condition that we do not know exactly the number of wells and their locations in the NCP. The corresponding modification has been made in the revised manuscript.

Section 3.1. Page 3701, lines 25-26. Please rephrase the sentence. I find the statement and reason to inactivate snowmelt not intuitive.

Response: The sentence has been rephrased to “Since the precipitation is small in winter and the data are not available for the NCP model, snow melt is not considered in the model simulation.”

Section 3.2. Page 3702, lines 9-13. So, the models have three unsaturated zone layers and three saturated zone (aquifer) layers? Am I correct? I suggest rephrasing these lines and not using the phrase “computational layers”.

Response: These sentences are formulated not entirely clear. Actually the model grids in the subsurface are categorized into three layers for the aquifer system (includes both the unsaturated and saturated zones). The first layer includes the first two aquifer units (I and II in Fig. 3), while the second layer and third layer correspond to aquifer units III and IV, respectively. The four aquifer units are adopted from Cao et al. (2013) and shown in Fig.3. We have also added some sentences describing the four aquifer units. And we have deleted “computational” in these sentences.

Section 3.4. Page 3703, lines 16-19. I found that the explanation in this important section is unclear. I recommend to extend the explanation, especially about the methodology to calculate the ET in the model.
Response: The explanation to the method of ET calculation has been extended and further clarified. The corresponding modification has been made in the revised manuscript.

Section 3.5. Pages 3703-3704. Did the authors define any aquitard layers? I missed their explanation.
Response：No, we do not define any aquitard layers in the NCP model.

Page 3704, lines 4-5. The sentence “Since the geological units adopted from ...” does not flow.
Response: The sentence has been rephrased to “The geological units (and their parameters) adopted from Cao et al. (2013) represent categorized zonal index using a continuous K field, therefore they do not have hydrogeological meanings.”

Section 3.6. Pages 3706, lines 9-12. This paragraph does not flow.
Response: This paragraph has been moved to the first paragraph in Section 3.6, after the sentence “The NCP model is calibrated using AutoCAL as an optimization tool, which is included in MIKE SHE software package (Madsen, 2003).” It is now more cohesive with the tool AutoCAL in this section.

Section 4.1. Table 6. I missed some useful information about the description of each hydrogeological unit. Could the authors briefly describe the lithological or geological description of each zone?
Response: The geological units (and their parameters) adopted from Cao et al. (2013) represent categorized zonal index using a continuous K field, therefore they do not have hydrogeological meanings. We have added some sentences in Section 4.1 to describe the source of the hydrogeological parameters that are adopted from Cao et al. (2013).

Section 4.1. Page 3707, lines 14-15. I suggest to also calculating MAE and RMSE after bias correction.
Response: It is not very clear to us what the “bias correction” is referring to, as we have not made any bias correction and this is not common practice in this type of groundwater modeling. As shown in Table 7 the 226 wells show a mean error (ME) of 1.43 m. However, as indicated in Fig. 5, the simulated groundwater head does not appear to have a general or uniform bias. Corrections of simulated values by subtracting ME values (whether uniform of spatially varying) would improve the performance criteria, but we cannot see the purpose of doing it in our study.

Section 4.1. Page 3707, lines 15-17. I am not sure that there are groundwater level variations of about 60-70 m. These are extremely large.

Response: The groundwater table has many cones of depression in the NCP. In the primary cones of depression, the maximum water level declines can be up to 60m in the shallow aquifer zone and 70m in the deep aquifer zone. Cao et al. (2013) and Fei et al. (2009) have pointed out this fact in their published papers.

Section 4.1. Page 3707, lines 19-26 and page 3708, lines 1-5. Did you consider the influence of the spatial support discrepancy between the model resolution (2 km) and measurements (point scale)?

Response: We are aware that there are discrepancies in the spatial supporting scales between the simulated and observed groundwater heads. The associated scale uncertainty arises mainly for two reasons: first, the simulated groundwater head usually represent the values at the center of the model grids; however it is quite seldom that the boreholes are located precisely at those places. Second, the model is not able to account for the local heterogeneity at the scale smaller than the model grid. As a result, both the model simulation and the field data could be reasonably correct at their own supporting scales, but the calculated uncertainty (e.g. RMSE) is not minimized to zero. Furthermore, the method used to identify the locations of the groundwater abstraction wells in the cities, as described in Section 2.4, could also introduce considerable uncertainty. We have improved the manuscript text by including the discussions above.

Page 3707, lines 25-29. This reasoning is unclear for me. Please rephrase.

Response: We have rephrased the reasoning in the revised manuscript. Hope it is clear now.

Section 4.3. Page 3710, lines 8-15. I could not find the location of the discharge points/locations in Figs. 1 and 3.

Response：The points' locations in Fig.3 (Fig.4 in the revised manuscript) are based on the chainage of the river. The numbers behind the river names in Fig.9 (Fig.10 in the revised manuscript) are the chainage numbers counted from the upstream where the chainage is 0. It is difficult to mark the location in the figure. Therefore, we have added an extended description in the caption below the figure.

Section 4.4. Pages 3710-3713. Did the authors compare the water balance analysis to other studies, e.g. Cao et al. (2013)? How much percentage of ET fluxes do come from the saturated groundwater zone? I would recommend that the authors calculate the contribution fractions of unsaturated and saturated zones to ET fluxes.

Response: As the parameters and assumptions of the groundwater flow components are adopted from Cao et al. (2013), we think it is less meaningful to compare with the results of Cao et al. (2013). Instead, we have compared our results with the results from other studies where different hydrogeological conceptualizations are used, and we have added some sentences in this section in the revised manuscript. We have also calculated that 99.75% of ET fluxes come from the unsaturated zone while 0.25% comes from the saturated zone.

Page 3711, lines 14-15. Please change the unit in Fig. 11 to m.

Response：We have changed the unit to m and a new figure is presented in the revised manuscript.

Section 4.5. Pages 3710-3713. I suggest moving this part to a separate discussion section.

Response: As Section 4.5 is on pages 3713-3714 and not on pages 3710-3713, the meaning of this comment from the referee is not quite clear to us. We think the structure of Section 4 (“Results and discussion”) is suitable for our manuscript, and we do not support the idea to move Section 4.5 to a separate discussion section.

Section 5. Page 3715, lines 22-26 and page 3716, lines 1-2. I suggest rephrasing this paragraph. I found that this paragraph seems not really supporting or connected to the statements mentioned in the abstract (page 3694, lines 17-18). In the abstract, the authors emphasize the significance of the ET component in the water balance analysis (”... more than 69% of water leaving the flow is attributed to the ET component”), while in the conclusion the authors mainly discuss the significance of pumping activities.

Response：We have rephrased this paragraph to make it connect to the statements mentioned in the abstract.

Response to Anonymous Referee #2

Interactive comment on "Integrated hydrological modeling of the North China Plain and implications for sustainable water management" by H. Qin et al.
Anonymous Referee #2 Received and published: 20 May 2013

I enjoyed reading this paper which is well written and conveys a significant message on how to address the sustainable water management for the North China Plain (NCP) hydrological system. For that area, in order to take account of the dynamic of the evaporation and the irrigation effects a coupled surface water-groundwater model using the well-known MIKE SHE was came up with. Evapotranspiration estimates based on remote sensing are employed as well.
In my opinion, the paper has the merit to present a methodological contribution that attempts to address how to cope with the over exploitation of groundwater due to the rapid economic growth of the China. However, this merit is hidden to reader because of contents sometime confused and analysis and results incomplete. Details are given below.

1) Considering the groundwater component, I expected a comparison of achieved results against findings of previous studies done in the same area, e.g. by using MODFLOW. This is necessary to showing how and in which terms the use of the proposed model is of benefit for a correct analysis of the NCP hydrological system and in particular of the groundwater one.
Response: We agree with the reviewer that a comparison of previous studies and our model is necessary. Many of the previous studies focused on small areas in the NCP, such as Shijiazhuang, Beijing, Tianjin and etc. We have added a comparison of achieved water balance results, which we think are important for the NCP hydrological model, against previous studies in Section 4.4. A corresponding modification is also carried out in the revised manuscript.

2) As regards data. Precipitation is collected by TRMM having a resolution of 25 km and then resampled to 10 km. It's not clear what is the procedure adopted for the resampling. For that, did the authors use rainfall recorded at the meteorological network? In this regard, it would be very useful to add in Table 2, the average annual temperature observed at each station along with the reference Evapotranspiration (ET) computed by Penman–Monteith.
Response: The precipitation data used in this study is a fully distributed, satellite-based product which is generated using a combination of different satellite based information from microwave and thermal infrared sensors. Based on the remote sensing data, the precipitation rates are adjusted by the surface rain gauge data on monthly time step. The above process is one of the precipitation algorithms developed by NASA, and is not part of our study. But some of the stations that we use for calculation of reference ET might be used by NASA for that purpose.
We have slightly rephrased the paragraph to provide a little more details on the precipitation product. We have also clarified that the reference ET was already calculated using the Penman-Monteith equation, based on the data from the meteorological stations. We have also added yearly averaged Reference ET (yearly average of the years 2000 to 2007) and mean temperature to table 2 as requested.

3) As far as the groundwater pumping is concerned, two aspects need to be clarified.

The first, how is it possible that a total of 580 pumping stations are generated if the constraints are i (city) <21 and Ni (well) <20? On average one should have Ni=27>20. It's likely that I didn't realize well how the random procedure is applied, but I think that the authors need to address this point carefully.

Response: The pumped groundwater generated here are for two purposes: domestic (urban and rural) use and industrial use. Each purpose has its own pumping wells and the number of pumping wells for each purpose is equal. Therefore, the total number of pumping wells is the sum of pumping wells for domestic use and industrial use. That's to say, each purpose has 290 pumping wells. This is the number used to constrain i (city) <21 and Ni (well) <25(a mistake of Ni<20 is made in the old manuscript). In the revised manuscript, we have presented this aspect clearer so that the reader will not be confused.

The second aspect regards Eq. (1). Based on that, the two water demand pumping rate, DW and IW, are uniformly distributed at each well and this should be unrealistic considering that groundwater might be exploited in a different way according to the groundwater sensitivity to the abstractions, less for larger recharges. However, I guess that the authors employ the random approach just to allow for the spatial variability of the wells that should be unknown. If so, this makes sense. Please clarify this aspect.

Response: Yes, we agree with the reviewers comment. There are a large number of pumping wells in the NCP, and their locations are not documented. It is not yet possible to represent all the wells in the NCP in our model. Based on this fact, we employ the random-point approach to represent the groundwater pumping activities in the NCP. We have clarified this aspect in the revised manuscript.

4) Honestly, I didn't understand how the sensitivity analysis of parameters has been addressed. I have gone over the manuscript but I didn't find it. I think that this should be the focus of the analysis and it has to be thoroughly detailed in a separate paragraph. The features need to be clarified are: a) what the authors mean about "the most sensitive parameters" and to what; b) where do the parameters listed in Table 6 come from? Which data are used; c) what is the criteria adopted for which both the horizontal conductivity has to be log-transformed and a parameter may be tied to a different one.

Response: a) We have used AUTOCAL in MIKE Zero to perform a formal sensitivity analysis and then selected the most parameters. Based on this result, we chose the most sensitive parameters for calibration.

b) The initial values of parameters in Table 6 are adopted from Cao et al. (2013) and the finial values are the results of the calibration process. Most hydrological parameters are from Cao et al. (2013) while we carried out the calibration process to get the values of the most important parameters for our model, in order to get and use parameters that are suitable for our model.

c) A logarithmic transformation is generally recommended if the feasible range of the parameter varies over orders of magnitude. And it is a common practice in hydrological modeling to limit the number of parameters in the sensitivity analysis as low as possible, since they may be correlated. Thus, some parameters are tied to other parameters in our model's sensitivity analysis.

We have added a paragraph in Section 4.1 of the revised manuscript to simply describe the sensitivity analysis.

5) In Table 7, some Nash-Sutcliff values are negative and this needs to be justified. In this case, the observed mean is a better predictor than the model and this results in the residual variance described by the model is larger than the data variance. In other words, for these situations the model should be useless.
Response: The reviewer is correct that when the Nash-Sutcliffe efficiency is lower than zero, it indicates that the model predictions are less accurate than the average of the observation data. As discussed various places in the manuscript there can be several reasons for such poor simulation results at some points, e.g. the simplified geological model and the uncertainty on the actual location of the water abstraction wells in the city areas. Altogether the results with negative Nash-Sutcliffe efficiencies for some wells illustrate that the model has limited predictive capabilities at point scale. This, however does not make the model useless for its main purpose, namely to provide an improved insight in the various water balance components and groundwater head trends for the NCP area as a whole. The above discussion has been incorporated into the revised manuscript. The Nash-Sutcliffe efficiency is a standard performance measure for discharge simulations, and it is less often used for groundwater head simulations. As the aim of the present NCP model is not to reproduce the groundwater head dynamics at individual locations but rather to evaluate the overall water balance, we have decided not to show the Nash-Sutcliffe coefficients in the revised manuscript.

6) Please, clarify how Modis data are combined with interpolated data from meteo network.
Response: The ET-Watch data was obtained from an external source: Institute of Remote Sensing Applications, Chinese Academy of Science. It is a rather complicated algorithm which includes input data from several different sources, such as remote sensing data and data from meteorological stations. Data from meteorological stations include: sunshine hours, air temperature, air humidity, air pressure and wind speed. These station data are interpolated to daily maps with a spatial resolution of 1km, but we are not familiar with the details of their method. We do not find that a detailed description of the methods within ET-Watch is relevant for this study, why we refer interested readers to the papers on ET-Watch by Wu et al 2012. We have rephrased the first paragraph of section 4.2 slightly to make it clearer that this merging of data is done as part of the ET-estimation within the ETWatch setup.

7) In Figure 9, it would be useful if the average areal rainfall was plotted along with the discharge for each identified river site. In this way, one can infer if the trend in the discharge can be affected also from the rainfall.
Response: We agree with the reviewers comment. The average areal rainfall is useful information for the readers. Therefore, we have plotted the average areal rainfall along with the simulated discharge in the revised manuscript.

Finally, I would like to add a general comment about the topic. I deem that no measure can be envisaged to cope with the water shortage if climate changes are not considered in the analysis. Besides, whatever measure one could plan, this one has to be tested in terms of cost necessary to accomplish it. This results in the need to develop optimization algorithms in order to identify the correct water allocation and meet demand in the investigated area. Based on that, it seems to me that this work can be considered as preliminary and this has to be pointed out in the abstract, introduction and conclusions.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 3693, 2013.

Response: Yes, we agree with the reviewers comment. This paper presents some preliminary results of our hydrological model for the NCP. The next step considering the climate change and scenario analysis has already been planned and we are working on it for the moment. We have added some sentences in the 'discussion of future work' sector to point out that we will continue the modeling work of the NCP to consider the climate change and scenario analysis.

Additional remarks on the revised manuscript:

As we are preparing the responses to the reviewers' comments and revising the manuscript, we have discovered some aspects that we can make improvements to the modeling work. These aspects include: improvement of land use and soil type around Beijing City, consideration of domestic and industrial water return flow to the model, and plot change of figure 5 (figure 6 in revised manuscript). Although the reviewers did not request these aspects, we think it is beneficial and worthy for us to make these improvements. Therefore, we have made three major improvements for the model and the corresponding modifications of the manuscript have also been made. First, we have refined the land use and soil type around Beijing City; secondly, we have reduced the groundwater abstraction for domestic and industrial use to be a way of representing the return flow effect; thirdly, we have used an averaged value of observed/simulated head of each one of the total 226 wells and then plotted these points in figure 5 (figure 6 in revised manuscript). As we think it does not make sense to show R value of total wells along with R value of individual well in the same table, we do not show the R value in the bottom line of table 7. Based on these improvements, the model is recalibrated and rerun and new results and modifications are presented in the revised manuscript. Of course the main structure and conclusions will do not change under these improvements.