Response to Review #1

by Hao et al.

Comments:
Manuscript review for hess-2015-12: "Urbanization Dramatically Altered the Water Balances of a Paddy Field Dominated Basin in Southern China" by Hao et al.
The authors analyzed hydrologic responses to urbanization in a paddy dominated basin in southern China. The essence of those similar topics should reside in attribution analyses related to "what's the most important impact factor and how to quantify its impact". Similar discussions in general extensively exist in literature already, and most of them rely on detailed observations or modeling approaches. I believe the methodology in attribution used in this study could be substantially improved, which would also enable their results to be more persuasive (see specific comments below). I would suggest a major revision of this manuscript before possible consideration in the publication of HESS.

Response: Based on the Reviewer's suggestion, we have added a standard attribution study that intended to separate the individual contribution of climate change and variability or landuse/land cover change from the total observed changes (increase) in streamflow. We used two types of models, Climate Elasticity Model, and Rainfall-Runoff Model, to examine the sensitivity of streamflow changes in response to P and PET (CEM), and P (RRM). We found that P explained about 15% of the increase of streamflow while the 85% of streamflow increase was attributed to LULC. These additional attribution analyses were consistent with earlier findings using the mass balance method and further confirmed that the observed increase in streamflow during the past recent decade was most likely due to urbanization, a major factor influencing the hydrological changes in the study basin.

New analysis was added in Section 2.4 Line 194-225, and Section 3.5 Line 280-290.

Specific comments:
1) Line 18 of abstract, the expression of "water-dominated to a human-dominated landscape" is not accurate, paddy field is also "human dominated" to some extent, please modify.

Response: we modified the sentence as: "…an artificial wetland-dominated landscape to an urban land use- dominated one ….".

2) Necessary details of statistic methods and corresponding results (for instance, sen's slope, DHR) should be provided.
Response: we added the statistic methods and results. See line 166-175.

3) How reliable is the "Baseflow index program" in your case? The uncertainty will definitely jeopardize your results regarding to the changes of baseflow, please clarify.

Response: the method we used was standard baseflow separation method and was widely used in the literature. We added Figure 5 to provide more details about how to determine *N*.

We found the results on the significant increase in baseflow are consistent with other water table data from a several wells. We have added groundwater data to support our hypothesis (Figure 10). Thus, we believe our analysis results are reliable.

4) section 3.3, "our results showed that N…..", please rephrase this paragraph which does not make evident sense to me.
Response: We added a few sentences to better describe the uses of N number. See line 185-192.

5) section 3.3, "the increase in baseflow or low flow…as a result of groundwater management". Section 4.2, "the large reduction in ET from paddy fields might overwhelmed…". What do you think is the main factor that leads to the increasing trend of baseflow in your basin? I believe the authors need to elaborate their attribution analyses.

Response: Many factors may result in an increase in baseflow in an urban setting, such as an increase in groundwater recharge due to a reduction in ET, leaking from drinking water supply systems, reduction of groundwater pumping, or an increase in rainfall. In the study basin, we think it is more logical to attribute the baseflow increase to an increase in groundwater recharge due to reduction in ET as a result of paddy rice conversion to urban use. The detected rise of baseflow was consistent with water table data (Fig 10) that show a significant rising trend.

6) section 4.1, "a decrease in ET is normally… ". please explain.

Response: Thanks for pointing out the mistake. There was a typo in this sentence. Now the sentence reads as 'A decrease in ET is normally caused by a decrease in P and/or PET (Sun et al.., 2005; Sun et al., 2011a, 2011b).

7) please check the basic usage of scientific terms in hydrology, such as , streamflow / runoff, and their units.

Response: Done. We used a consistent unit for streamflow (mm). We made sure that we used streamflow as a standard term to represent discharge at the watershed outlet demoted as Q.

8) what's the temporal coverage of streamflow observation? I suggest tend analyses of all variables (e.g. streamflow, precipitation, ET, PET, baseflow, etc) should be framed within an overlapped time window.

Response: We have acquired streamflow data from the two hydrological monitoring stations. The annual data cover from 1986 to 2013. However, daily data for the 'rainy season' (May –Oct) are available only from 2002 to 2013. MODIS ET data are from 2000-2013. After we found a 'break point' for annual streamflow change around 2002, we focused our attribution study in two periods, the baseline 1986-2002, and second period 2003-2013 that experienced the most dramatic urbanization.

9) please swap the presenting orders of table 1 and table 2.
Response: Done.

10) Fig 3 could be dropped off, since it is less relevant to the topic.

Response: we would like to retain this Figure to show the increase in Temp. We add trend analysis for the recent period 1990-2013 to show the acceleration of warming and thus evaporative potnetial.

11) Words / expression should be revisited for the whole manuscript, to name a few: Line 10, P8, “are control”; Line 4, P16, “practices”, etc.
Response: Corrected. We have one native English speaker review the entire manuscript for grammars.

Response to Anonymous Referee #2

Received and published: 8 March 2015

General:

This study discussed the impact of urbanization on water balance in the Qinhuai River basin, a region undergoing rapid development. I believe it is an interesting topic. However, I do not feel the results presented in the paper could well support the main hypothesis, i.e. decreases in ET resulted from urbanization contribute greatly to the reduction in streamflow. In the manuscript, the data were presented in an inconsistent way, and trends were compared for various different periods. There are also many general hypotheses in the discussions that have not been tested. I recommend at least major revision.

Response: The reviewer had concerns about our conclusion that the observed increased in streamflow was mainly caused by the decrease in ET. To address the reviewer's concerns we have made two major efforts in this revision:

1) conducted an attribution study using two models (Climate Elasticity, and Rainfall-Runoff models) and consistent results achieved – over 85% of the increase in flow was caused by land cover/land use change and 15% by the increase in Precip. Since the land cover change was characterized as converting to urban uses that directly resulted in a decrease in ET due to a decrease in leaf area index. The decrease in ET was detected both by MODIS ET data and water balance method (Precip-Streamflow), two independent data source giving more confidence that the decrease in ET was the main cause of increase in streamflow. In addition, we argue it is plausible that rise of baseflow was caused by the decrease in ET in the study basin. The increase in impervious surface was not likely the main cause of the observed increase in streamflow. It is well known the impervious area mainly elevates stormflow. We found that all flow percentiles increased during recent years
2) In the revision, we focused our analysis on annual streamflow for two periods, 1986-2002 (reference period), and the period 2003-2012 (rapid urbanization period as confirmed from remote sensing data. We updated Fig 2). Since MODIS data are available only for the period 2000-2012, we limited our analysis on examining trend of LAI, ET, and PET for this period. We intended to use all pieces of information to solve the puzzle and test our hypothesis.

See Section 2.4 and Section 3.5.

Specific comments:

1. The land cover data statistics, which was arranged from different data sources, is somehow inconsistent, and this uncertainty should be addressed in the manuscript. For example, Fig. 2 (Section 2.1) shows 17% increases (of the total basin area) in the impervious surface areas from 2003 to 2012, while the areas of rice paddy only accounts for about half of the changes (~8% of the total basin area from 2001 to 2012). However, both the statistics in Table 2 and conclusions indicated the increase in impervious areas was mainly due to conversion of the rice paddy fields in the 2000s. Please clarify this. Also, why not give the statistics for the same period in the text for better comparison?

Response: The impervious surface area (1988-1994) data were from Du et al. (2012) and Du and Chen (2014). In the revision, we have reanalyzed the Lansat 7 TM + imagines for 2000-2012 to derive urban bult up areas, and made sure the land use data are consistent. We have updated Table 2 to reflect our new analysis.

2. This study used multiple data sources (including land cover data, surface meteorology, streamflow, Remote sensing data etc), which were presented at various periods. This makes the paper hard to follow.

Moreover, trends from different periods are generally incomparable (especially for a short period). Yet, this study compared the trends of different variables (including P, ET, runoff, LAI etc) at different periods, and used these results to support the main hypothesis, which renders this questionable (see comments #3). E.g. MODIS LAI and ET data were analyzed for the period from 2000 to 2013; river flow data were analyzed from 1986-2013 (Fig. 10) and various sub-periods from 2000 to 2013 (Fig. 7, 8 & 9).

Response: Streamflow data were acquired from hydrologic monitoring stations. Unfortunately, the data did not come with the same temporal resolution. Consequently, we could not analyze the hydrological variable for the same period, the longest one from 1986-2013 that was used to examine annual hydrological change. However, for baseflow and flow duration curve analysis we could only examine a shorter period, 2006-2013, and 2002-2013, respectively. We have updated the Double Mass analysis for up to the period from 1986-2013.

3. The major hypothesis of this study is that ET reduction caused by land cover change is a big contributor to the increase in stream flow from 1980s to present. However, there is no independent ET data to test this hypothesis. The study used water balance (i.e. P-Q, Fig. 10) to estimate annual ET from 1986 to 2013, which may subject to great uncertainties due to likely substantial changes in the water storage. For example, the MODIS ET data (Fig. 5) somehow shows inconsistent inter-annual variability as the ET time series estimated using P-Q (Fig. 10).

Response: As in any method to estimate ET, there was uncertainty of the P-Q method, such storage change in ponds, soils, and groundwater. However, we argue that over the long term (25 years in this study) the trend of P-Q reflects the true trend of ET. We found a similar decreasing trend for MODIS ET, providing more confidence that P-Q is a reliable approach to detect the change in ET and provides a plausible explanation of the increase in streamflow. Attribution study using two empirical models further confirmed that precipitation was not the major contributor to the increase in streamflow (15%), but rather 85% of the increase in streamflow was due to landcover/landuse change indicating the ET was a major driver for the increase in streamflow.

4. What factors control the ET variability in Qinhuai River basin? This may need further clarification. - MODIS ET algorithm uses MODIS LAI data as an input (Mu et al. 2011), and therefore these two datasets are not totally independent. - Section 4.1, Line 2-3:

Response: The reviewer's concern could be valid. However, we used a different LAI dataset from Mu et al. (2011). The LAI data set was derived by Beijing Normal University (Yuan et al., 2011) using a modified temporal spatial filter (mTSF). So we argue that the positive correlations between MODIS ET and LAI were not an artifact. Again, the P-Q also showed a decreasing trend consistent with the decrease in basin wide mean LAI.

"A decrease in ET is normally caused by an increase in P and PET": this needs further consideration.

Response: This was a typo. It should be stated as 'A decrease in ET is normally caused by an decrease in P and PET"…

The relationship between ET and PET/P may vary in different climate zones. The MOD16 product also produces PET; this product may be more suitable for diagnosing the relationship between ET and PET, since both variables are likely affected by the uncertainty in surface meteorology inputs.
Response: We argue that the FAO PET values based on local weather stations provide stronger evidence that the PET and ET had different trends since PET data were derived from independent variables used in calculating MODIS ET.

5. How much do the changes in precipitation frequency and distribution contribute to the changes in streamflow characteristics? Should this be included in the discussions related to Figs 7-10 as well?

Response: The Reviewer had an excellent point and suggestion. Therefore, we added an attribution study using two empirical models to examine precipitation effects on the streamflow. We found that precipitation contributed about 15% of the increase in streamflow during the rapid urbanization period (2003-2013) and the rest of the change in streamflow could be attributed to land cover/landuse change.

Minor comments: (technique corrections)

1. Section 3.2 Line 21-22: why add this sentence? This has nothing to do with the trend analysis presented here (i.e. from 2000 to 2013).

Response: The sentence was removed.

2. Please switch Tables 1 and 2.

Response: Done.

Response to Anonymous Referee #3

Received and published: 20 March 2015

This study "Urbanization dramatically altered the water balances of a paddy field dominated basin in Southern China" discussed an important hydrological consequences of urbanization over the typical agricultural landscape of the southern China over which paddy field dominates. The conclusion that urbanization will increase the total streamflow and decrease ET is within our common sense, while this article provided more evidence and data to support this idea and highlighted the larger magnitude in the increase of runoff coefficient due to the urbanization over paddy fields than other vegetation cover. My major concern is the robustness of statistical analysis over this short time period (i.e. 2000-2013) over which natural fluctuations due to climate variations may overcast the human influences. Although the authors provided several different statistical methods to support their conclusions, I still have doubt on their conclusions because of some missing data and assumptions (see detail comments that followed).

Response: The authors appreciate the reviewer's careful review, insightful comments, and suggestions to make the study more robust and convincing. Therefore, we have added an attribution analysis to quantify the effects of climate variability and land use change on streamflow separately. The new findings were consistent with the previous conclusion that the main reason (85%) for the observed increase in streamflow was due to land cover change and 15% the increase was due to increase in precipitation. The rise of air temperature and thus evaporative potential (PET) masked somewhat the decrease in ET as a result of decrease in water use by vegetated surfaces dominated by rice paddies. The new analysis provided us more confidence to our early conclusions.

As suggested by the Reviewer that it seems to be a common sense that urbanization increases impervious surface areas, thus stormflow and total streamflow increase as well. However, to our knowledge, there are no studies in the literature that have comprehensively examined water balances in paddy field-dominated watersheds using relatively long term hydrometeorological data. Very few studies have devoted to paddy field watershed hydrology, but mostly using engineering hydrological models with a sole interest of stormflow or flooding events (e.g., Du et al., 2012). We believe that to have a full understanding the impacts of urbanization on watershed hydrology, a process-based study, starting from ET and associated controlling factors, is needed.

To make it publishable, this manuscript need major edits in the language. Figure 1: Capitalize "Overland flow" and put it in one line; Separate each item with ";" or line break in each text box; increase the width of lines. Make a brief description of this figure in the text.

Response: We have fixed the grammar problems and have a native English speaker to review the manuscript. We have added a description of the Figures.

Page 1945, Line 27: add "to" between "was" and "understand";

Response: done.

Rephrase the title of Figure 2 as "The location and land use change of the Qinhuai River Basin during 1988-2012". Replace "The insert map..." with "The land use map was classified from Landsat ETM+ images of the year 2012".

Response: done.

P1946, L23: The rate should be clarified. according to Fig. 3, the rate should be 0.26 Celsius degree / decade since 1961. The "1990s" represents a 10-yr period. Is it exactly what the authors want to report that the temperature increased 0.44 Celsius degree in this decade?

Response: we have rewritten the sentence as: “Mean air temperature (1961-2013) across the study basin has increased drastically at rate of 0.44 ^{o}C/decade from 1990 to 2013 (Figure 3), suggesting an increasing trend in evaporative potential during the past two decades.

Figure 4: replace "P" with "PPT"; spell out the "SD".

Response: done. we used P throughout the paper in the revision.

Figure 5: Rephrase the title, such as "Changes in MODIS ET and LAI during the peak growing season (July - August) during 2000-2013. What’s the unit of ET?

Response: The unit of ET is mm/ two months.

P1946, L25: Add "per unit basin area" after "streamflow". P1948, L10: Delete "are".
P1950, L5-6: It’s better to replace these two "change" with "linear trend".
Response: Good suggestion. Done.

Figure 6: Need to explain why using different time scales on LAI (annual mean?) and PET (in the peak growing season) when calculating their correlations with departure of ET (annual).

Response: In Fig 6, all variables for the peak growing season were used. We have clarified this the Figure title.

Figure 7 & P1950,L13-15 & L24-27: This figure and analysis are misleading since the data for the first time period is not complete, i.e. missing 2004 & 2005.

From the diagram of runoff coefficient during May-October in each year, I am arguing that there might be no significant change during 2006-2011 and the big increase in the last two years may due to climate variations. I am concerning the robustness of statistical method for this short time period.

Response: Unfortunately, we do not have daily streamflow data for the period 2004-2005. Thus, our analysis was limited to show the flow duration change for selected periods.

The reviewer had a good point on the uncertainty of our statement on the streamflow increase during the ‘flood season’ as a result of short data series. However, at the annual scale, we have demonstrated that the streamflow rise was a result of land use change not climatic variation. Since most of the annual streamflow was from the ‘flood season’, so we argue that it is likely that the change in runoff coefficients reflect the hydrological regime change in the study basin.

P1951,L10-12: As authors mentioned in P1950,L22-24 that groundwater levels were on the rise in recent decade, estimating ET with "PPT - Q" might be questionable; while it may also indicates that ET might decrease in a higher rate than the current estimations.

Response: Good point that supports our hypothesis. We have added this argument – an increase in the water storage term may suggest the ET estimated by the P-Q method might represent lower end of ET reduction, i.e. the true ET could even lower.

P1951,L15-17: Fig. 10 shows the trends of annual streamflow during the period of 1986 - 2013, but here the authors are talking about the trends over the period of 2000 - 2013. Do these two time periods have the same trends in the magnitude?

Response: We added the time period for Figure 12 (1986-2013). Using different data, we tried to show streamflow at multiple temporal scales (annual flow and summer flooding wet season) all increased. Yes, all the data show the same direction of change.

P1951,L22-24: The strong correlation between MODIS ET and LAI is most likely from the equations on which MOD16 product based (Mu et al., 2007). I question the usage of this information to support author's point at here. But without doubt, the authors can make this claim that MODIS ET also have the same result as detected with other methods (in this article).

Response: In this study we did not use LAI data from Mu et al. (2007). Instead, we used a LAI products released by Beijing Normal University (Yuan et al., 2011). This product represents an improved data over the previous data used in Mu et al., (2007). So the MODIS ET data stream was somewhat independent from LAI data.

As the reviewer suggested, the trend of MODIS ET was similar to tread of ET estimated by the P-Q method, so we have confidence that the ET reduction was negatively related to trend of LAI.

P1952,L1-2: Here the authors claim that "regional annual ET is generally controled by PET, P, and ..." which is inconsistent with their previous claim (P1951,L20), i.e. "LAI is a major controlling factor for ET". Do these control factors' role change in various spatial and temporal scales? The authors should clarify in the text. P1952,L2-3: I believe there is a typo. Should replace "an increase in P and PET" with "a decrease in P and PET".

Response: The Reviewer is correct. There was a typo that caused the confusion. The sentence has been corrected as "A decrease in ET is normally caused by *a decrease* in P and/or PET (Sun et al.., 2005; Sun et al., 2011a, 2011b).

LAI is a major factor at the shorter time scale (i.e., monthly) than longterm such as annual ET since LAI does not change from year to year in general for most land covers (unless extreme drought or disturbances). We have clarified this point to stress the temporal scale effects ET controlling factors.

P1954,L22-23: The claim of "The changes in water balances were mainly through the reduction in growing season ET and not by increasing impervious surfaces alone" is confusing. One conclusion from this study should be that the decrease of ET is mainly caused by the shrinkage of rice paddy fields and expansion of impervious surface area. Are there other reasons that could decrease ET? Are the authors mentioning decreasing LAI (is it not dominantly from urbanization)?

Response: We have rewritten the sentence as "A significant increase in streamflow and a decrease in ET in the study basin were detected, and the changes were considered to be associated with urbanization characterized as shrinkage of rice paddy fields and an increase in impervious surface area. Urbanization that resulted in a reduction in LAI during the peak growing season overwhelmed the hydrological effects of climate warming and precipitation variability during the study period."

P1952,L6-7 & P 1950,L24-25: Why does this shift in runoff coefficient happen around 2003?

Response: We believe that the main reason was urbanization – converting paddy fields to urban uses. We provided new remote sensing (Landsat 7 TM+ data) to show paddy field change land use change (Figure 2 insert). The urban built-up areas increased from 9% (222km^2) to 12% (301 km^2) from 2000 to 2004, but jumped to 23% (612 km^2) in 2012, and the area of rice paddy fields decreased from 45% (1188km^2) of the total land area in 2000 to 43% (1112 km^2), and jump to 36% (932 km^2) in 2012. Prier to 2000, we used published data by Du et al. (2012) and Du and Chen (2014).

References: Mu, Q., F.A. Heinsch, M. Zhao, and S. W. Running, (2007), Development of a global evapotranspiration algorithm based on MODIS and global meteorology data, Remote Sensing of Environment, 111, 519-536, doi: 10.1016/j.rse.2007.04.015