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Hydrology and Earth System Sciences Discussions

Interactive comment on "Effects of climate change and human activities on runoff in the Nenjiang River Basin, Northeast China" *by* L. Q. Dong et al.

L. Q. Dong et al.

dongliqink@hotmail.com

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We thank the commenter for taking the time to carefully review our manuscript and providing the thoughtful comments. We would like to respond as follows:

[Comment] Methodologically: The hydrological sensitivity analysis method implicitly ignores factors that might be "natural" or at least "not human" but which don't arise immediately from changes in atmospheric forcing. For instance, changes in vegetation cover from grasslands to desert are noted by the authors. It is unclear whether these changes are accounted for in the point-scale computation of PET by the authors. A spatially-based approach for PET estimation would be advisable.

[Response] We used climatic records collected from 1956-2010 weather stations to calculate evapotranspiration. The estimated point-scale PET rates were then used to compute average basin-scale PET with the Thiessen polygon method. Changes in vegetation cover over time was handled in our analysis through a model parameter "w" in equation 10, see more explanation below.

[Comment] Similar concerns arise with respect to Equations 13 and 14, which explicitly treat the parameter w as if it is time-invariant. Given that the authors also explicitly note that w is dependent on vegetation, and vegetation has changed, this assumption does not appear to be justiïňĄable. Moreover, I could not see any description of how the authors determined the value of "w" for the basin?

[Response] Parameter "w" is indeed treated as a time-dependent parameter, as it changes with the change of vegetation. In this study, we first estimated actual evapotranspiration "E" through equation 9, and then calculated "w" through equation 10 for different time periods. To clarify this, we will add the following sentence at the end of page 11528 (i.e. after equation 10):

"We first estimated E by Eq. (9) and then calculated w by Eq. (10) for different time periods to reflect changes in vegetation cover."

[Comment] The data used for the computation of PET included hours of sunlight, but not explicit measurements of solar radiation. Given the much discussed observations of "dimming" in East Asia due to aerosol emissions etc, it seems quite possible that changes in radiation may also contribute to hydrological changes.

[Response] A number of studies have reported declines in Pan evaporation in many countries of the Northern Hemisphere. As you pointed out, the phenomenon has been attributed mainly to the decreasing solar irradiance during the 1950s and the early 1990. The decline in evaporation varied from 2 to 4 mm per year from the tropics to tundra regions (Roderick and Farquhar, 2005, International Journal of Climatology). Specifically for China, the decline has been estimated for about 3 mm per

year (Liu/Xu/Henderson/Gong, 2004, Journal of Geophysical Research 109: D15102 doi:10.1029/2004JD004511). While solar radiation is certainly an important factor affecting PET, the “dimming” effect (i.e. 3 mm/yr) is very marginal when compared to an evapotranspiration of several hundred mm (our estimated average PET is 880 mm/yr). Therefore, the change in solar radiation should have no effect on the overall finding from our hydrological assessment.

[Comment] The treatment of uncertainty is insufïňĄcient. Similar discussions of land use change and its effects on hydrology in Italy (relying on distributed hydrological models) have concluded that the uncertainty in the analysis was equal to the observed effects (see e.g. Brath and Montanari). Given that this method relies on differencing of multiple observations and estimations, the ïňĄnal computation of the human effects receives the cumulative impact of errors in all the other parameters! At a minimum a propagation of error approach should be considered. It cannot be justiïňĄable to report these values to3 signiïňĄcant ïňĄgures!

[Response] Your point is well taken. We agree there are several error sources in this modeling approach, as we have partially discussed in the paper. Due to the large river basin area with hundreds of sub-watersheds and a large quantity of data (dozens of weather stations, dozens of river gauge stations, land use, etc.), it is both conceptually and computationally challenging in making a realistic estimation of error propagation. In our ongoing studies we focus on several smaller sub-watersheds within the Nenjiang River Basin, and we are making efforts to improve uncertainty analysis with reduced size and better parameter control.

[Comment] Editorially: The paper spends a lot of space presenting derivations and deïňĄnitions of existing methods. It is not necessary to include detailed descriptions of commonly used techniques like a Kendall Test or Pettitt Test etc. Please just provide a good reference, justify the choice of technique. Please also consider an alternatively way to present the data in Figure 4. It is almost impossible to interpret the ïňĄgure at the scale at which it has been presented.

[Response] Your point is well taken. While the techniques used in our study are well documented, detailed descriptions of the methods can be especially helpful, based on our experience, to researchers in underdeveloped countries where access to foreign publications can be a real problem. If space is not an issue, we prefer to have the technical descriptions included in the publication.

Changes in different land use/land cover are quantitatively given in Table 4. Figure 4 is intended to give the readers a visual presentation of these changes in space.

[Comment] Contribution: It is a unclear how this paper advances methods or understanding in hydrology and change analysis generally. It doesn’t critically evaluate the technique used, propose a new method or confront the observations in the Nenjiang Basin with anticipated results. While the authors have done a decent job of compiling data sources etc, I don’t see the scientiïňĄc novelty and contribution of the work. Perhaps some more detailed science questions can be advanced to provide better motivation? As it is, the motivation of this work seems purely for local analysis and the contribution it makes to international science is not obvious to me.

[Response] In our opinion, this study contributes to at least two areas: 1) a technical approach to clearly separate the effects of climate change and human activities on runoff, and 2) extension of the knowledge base of climate change effects on hydrology in high-latitude river basins.

As we discussed in the Introduction section, there have been numerous studies on climate change and human activity effects, each alone or combined, on regional hydrology. However, many river basins in the world have undergone changes due to human activities in the past century, and separation of the climate change effect from the human activity effect is not easy. Studies concerning this aspect are few, as well as they give little consideration to spatial heterogeneity. Our study demonstrates a technical approach in doing that.

Climate change studies have shown that global warming will have profound effects on

hydrologic processes especially in higher latitude regions. Since a large portion of the Nenjiang River Basin is in both a high latitude and a high altitude location, the findings of our study contribute to broadening the literature body on climate change effects in cold regions. The work was done for a specific location, but what is not “local” in Earth Science research? Similar to the phrase in American politics - “All politics is local,” studies in Earth Science are often motivated by local problems and interests.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 11521, 2012.