Hydrol. Earth Syst. Sci. Discuss., 9, C5531–C5533, 2012
www.hydrol-earth-syst-sci-discuss.net/9/C5531/2012/

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.

S. Thompson (Referee)

sally.thompson@berkeley.edu

Received and published: 29 November 2012

Thank you for the opportunity to review this paper. The paper describes the authors' analysis of hydrological and climatic data over the Nenjiang River Basin in Northeast China. The paper embodies an application of the hydrological sensitivity analysis method of Milly, Koster et al. The premise of which is: i) Changes in runoff within a basin can be subdivided into those attributable to climate and those attributable to human activity ii) Changes in runoff due to climate can be separately computed via a water balance forced with suitable climatological data to quantify trends in precipitation and trends in evaporation. iii) Human-induced change is then given by the residual of the observed runoff change and the estimated change due to climate.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

To apply this method the authors draw on ground-based data from 39 meteorological stations, observe land-use changes from LandSat data, and look at runoff change through several nested basins. They then draw conclusions about the distribution of changes throughout the basin.

I have a few concerns about the study – some methodological and some regarding novelty and contribution.

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.

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 justifiable. Moreover, I could not see any description of how the authors determined the value of "w" for the basin?

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.

The treatment of uncertainty is insufficient. 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 final computation of the human effects receives the cumulative impact of errors in all the other parameters! At a minimum a propagation of

HESSD

9, C5531–C5533, 2012

Interactive Comment

error approach should be considered. It cannot be justifiable to report these values to 3 significant figures!

Editorially: The paper spends a lot of space presenting derivations and definitions 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 figure at the scale at which it has been presented.

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 scientific 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.

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