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Hydrology and Earth System Sciences Discussions

Interactive comment on "Is streamflow increasing? Trends in the coterminous United States" *by* N. Y. Krakauer and I. Fung

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Review of the paper entitled "Is Streamflow increasing? Trends in the coterminous United States" by Krakauer and Fung.

Karakauer and Fung investigate by data-analysis whether streamflow in the US has been increasing during the last 120 years and how such an increase, if existent, is related with the increase in precipitation, CO2 and the associated increase in temperature. I believe that they present a clear case and a concise and clear paper.

However, there are a number of statistical issues I take issue with and should be clarified or further discussed The statistical methods used in their analysis should be without question, because it is the basis of all their conclusions. Yet there are numerous remarks to make:

- The authors want to relate streamflow increase to an increase in precipitation and temperature. For this they first interpolate streamflow to a grid and then perform a regression analysis on the grid values. This is not how I would do it for two reasons: 1. By regressing interpolated quantities instead of the observed values itself, you effectively use regression a modelled (read interpolated) value, which is generally smoother and has different statistical properties than the original observable. 2.In case of runoff, you are interpolating runoff data from catchments of different size onto a grid, which is basically statistically merging of apples and oranges, while not taking account of runoff divides. It would have been better to estimate yearly average rainfall depth and average temperature in each catchment by interpolation and relating that directly to observed yearly average runoff depth (normalized if required) for that catchment. This would directly link, in a hydrologically logical way, runoff to its driving forces within the catchment. If you then required maps of temporal differences or regression coefficients or correlation coefficients, these could have been interpolated from the runoff observation points, e.g. by ordinary kriging.

- The authors never state what type of kriging was used to make the runoff maps: ordinary kriging, simple kriging, universal kriging?

- When making a map of normalized runoff the authors have to use yet another gridded data product (also a model), i.e. that of Fekete et al (2002). They could have also interpolated these values (e.g. by kriging) from the runoff locations, thereby avoiding using multiple sources in one result. Note that by using the approach proposed by me, normalization would have been possible with the mean and standard deviation of observed yearly runoff, avoiding this extra interpolation step altogether.

- When calculating the continental average streamflow and its uncertainty (as used in Figure 4 and 5 and 6 etc.), it is not clear how these were exactly obtained. There are three options:

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1. Taking the unweighted average of the streamflow data at the stations and use the correlation function of Figure 3 to calculate the variance of the unweighted mean. In case Z(x_i) the value at station located at x_i and i=1,..N the number of stations:

Average runoff =(1/N)Sum_i{Z(xi)}

Var(average runoff) = (1/N^2)Sum_i Sum_j {sigma(x_i)*sigma(x_j)*corr(x_i - x_j)}

2. The same as above, but now applied to the gridded estimates obtained from Kriging.

3. Using Block-kriging applied to the US land mass.

Theoretically, only option 3 is correct. Option 1 is incorrect and suboptimal as it does not correct for the preferential sampling in space of the streamflow stations as is obviously the case as seen in Figure 1. Option 2 does correct for the preferential sampling, but is not entirely correct because the crosscovariance between the interpolated (gridded) data is not exactly given by the stationary covariance of Figure 3. So only option 3 is exactly right. Option 2 yields a reasonable estimate of the continental mean discharge, but the variance is wrong. Option 1 yields a biased estimate of the continental mean discharge itself.

So far, the statistical problems I have with the paper. Apart from that, I do have my doubts about the interpretation of some of the results, certainly with respect to the effect of CO_2 on streamflow in the great plains explained by the rainfall falling in the growing season, which is intercepted by plants. First, "interception" is not a good word to use here, because to hydrologists that word refers to interception water remaining on the canopy and then evaporating directly. So interception water is not used by plants for transpiration. The authors probably mean water used by plants. Also, I think it is not at all clear that increased CO_2 content will result in more discharge. First, if there is enough water during the growing season ($P>E$), then there is no reason for plants to increase their water use efficiency, as water is not limiting. They might thus instead start to increase growth, which will increase at least evaporation by interception and

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thereby decreases runoff. In water limited conditions (P<E) , it can be argued that increased CO_2 concentration will plants to loose less water at the same growth rate, and thus be more water use efficient. But this then would also increase the length of the period after the rains that the soil remains wet and allows plants to grow at full capacity. This then allows plants to increase their biomass until the same amount of water was effectively used as before, thus keeping the runoff constant. Given that the statistical case is not so strong due to the large correlation between CO_2 increase and T-increase, I am not sure whether any conclusions about the effect can be drawn at this point.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 785, 2008.