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Hydrology and Earth System Sciences Discussions

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Interactive Comment

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

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We thank the reviewers for their comments and critiques, and appreciate the opportunity to submit a revised and improved version of our paper. Our responses to the points raised are as follows:

1 Review I (Bierkens):

a) *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 re-*

gression 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.

We believe that our treatment of the stream gauge measurements is appropriate given the nature and limitations of the data and the stated objectives of this study. In response to point 1, meteorological observations (such as precipitation) are, in general, not available at the specific watersheds for which stream gauge observations are available, and thus must be somehow interpolated. For our goal of determining regional trends in streamflow and correlations with regional and global climate, we see no clear advantage in interpolating available precipitation time series to the stream gauge locations over interpolating available streamflow measurements and comparing their trends and interannual variability with a quality-controlled gridded precipitation product with similar effective resolution. Point 2 would be a substantial concern if we were looking at river catchments that are large compared with the streamflow correlation length scale of ∼700 km, but this is not the case. As we pointed out, “Most of the streams in HCDN drain small watersheds (median drainage area: 740 square km; 10th-90th percentiles: 73-6800 square km). In interpolating measured annual streamflows onto a grid, we therefore treated them as point measurements on the much larger scale

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(8×10^6 square km) of the coterminous United States."

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

We used ordinary kriging, and now specify this in the paper.

c) *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.*

While we agree that using the Fekete et al. product introduces some error in translating normalized streamflow departures to absolute amounts of water, we prefer it to directly interpolating the streamflows per unit area from our data set because HCDN consists of small streams which may not be representative of the average amount of runoff on a larger regional scale. In mountainous areas, precipitation and runoff in a particular small watershed can potentially differ by an order of magnitude from the regional average, depending on its elevation and aspect. We therefore normalized observed HCDN streamflows by the mean and standard deviation of their observed yearly runoff and, after interpolating the normalized anomalies, scaled these by the Fekete et al. product to more accurately estimate absolute mean annual runoff departures on regional scales. We have added language to Section 2.1 to clarify our methodology.

d) *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:*
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.

We used option 2 (area-weighted mean of the gridded map derived from point kriging, with the uncertainty of this mean derived from the full posterior error covariance matrix of the gridded point estimates). This is equivalent to option 3 (block kriging) when the same grid used for the point kriging is used to approximate the point-to-block covariance and the block prior variance (see Olea, *Geostatistics for Engineers and Earth Scientists* [Kluwer, 1999], p. 205 for a derivation of this equivalence). Thus, our estimates of both the aggregated discharge and its error variance are formally correct.

e) *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.*

We have changed our phrasing accordingly.

f) *Also, I think it is not at all clear that increased CO2 content will result in more dis-*

charge. 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 thereby decreases runoff. In water limited conditions (P<E) , it can be argued that increased CO2 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.

We agree that it is not self-evident that increasing CO_2 concentrations results in increases in river discharge. It is possible (depending on such factors as the timing and instensity of rainfall/snowmelt, the water-holding capacity of the soil, and the extent to which plant biomass is water-limited) that increasing CO_2 concentrations do not have this effect, or have it only under certain conditions. The differing interpretations of streamflow increase of Gedney et al. and Piao et al. discussed in the Introduction to our paper largely hinge on different (modeled) answers to these sorts of questions. Our study aims to empirically investigate the net result of these interactions on evapotranspiration and streamflow on a regional scale.

g) *Given that the statistical case is not so strong due to the large correlation between CO2 increase and T-increase, I am not sure whether any conclusions about the effect can be drawn at this point.*

We agree that our study does not allow confident attribution of streamflow increases to increasing CO_2 concentrations. We emphasize that precipitation changes are the main driver of observed trends in streamflow. We do propose that increasing CO_2 concentration has contributed to relatively greater increase in streamflow in regions of summer-rain dominance in the central US, but highlight in the Discussion the tentativeness of this attribution and possible alternative explanations, such as changes in the seasonality or intensity of precipitation, for our finding of regionally increased streamflow.

2 Review II (Bouwer):

a) *The effect of CO_2 on streamflow is claimed in the paper, but it is unclear what this is based on. The significance levels (p-values) for the different regression coefficients are not presented in the text, nor in Table 1, except for the regression between precipitation and runoff. The text mentions that for CO_2 "the association is not significant for the coterminous US". What then, is the claim based on that CO_2 does have an effect on runoff? Are some of the regressions for different precipitation regimes displayed in Table 1 then significant? If yes, for which regions? This could be clarified the table and Figure 9, by identifying the statistically significant results.*
In order to support the conclusions of this paper, Table 1 should be set up in a clear way, so that the different regressions are easily recognisable. This is now unclear with various footnotes. Crucially, the p-values of the different regression coefficients need to be presented here. If no significant link is found between runoff and CO_2 levels for the coterminous US, this should be one of the main conclusions. This would be an important conclusion for other studies looking into global impacts of elevated CO_2 levels. Perhaps at the regional level such links do exist, so please present more clearly.

We have added more significance tests to the paper, particularly for Table 1. We have also rearranged the presentation of regression coefficients in Table 1 for clarity.

b) *Temperatures exhibit interannual variation that is linked to runoff, while CO_2 levels change gradually. It is likely that therefore no significant link could be found. It may*

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be possible to attribute runoff change to CO2 levels, but probably not in a regression analysis. This should be elaborated in the paper.

We discuss the difficulty in separating the direct impact of higher CO_2 concentration from the impact of greenhouse warming. While we do find possible impacts of CO_2 concentration on streamflow, additional analytical approaches and more direct observations (e.g. of evapotranspiration) can usefully complement our regression approach.

c) *For the reader to appreciate any of such possible links between runoff and the different variables, it would certainly help to present the timeseries of temperature and CO2 levels, in a similar fashion as was done for precipitation (Figure 5b).*

We have added these to Figure 5.

d) *Further, the physics behind the links between temperature increases and runoff reductions (section 3.2) could be further clarified. Evaporation is mainly determined by radiation, windspeed and humidity. Reduction in evaporation rates would increase the sensible heat flux (temperature) over the latent heat flux, Therefore, in areas where water is limiting (in summer time precipitation dominated areas), decreasing evaporation rates due to water shor tages would lead to temperature increases, rather than the other way around. What is probably meant here is that an increase in the average temperatures creates conditions for the air to contain more water vapour. This should be explained, as any increase in temperatures may be modulated by local as well as global and regional (circulation, global warming) processes.*

We show regression of streamflow against global temperature rather than regional temperature, and now elaborate on this more fully in the paper (Section 3.2): "For the regression analyses involving temperature just described and shown in the bottom rows of Table 1, we used global temperature as a predictor variable in order to quantify the impact of global warming on streamflow adjusted for precipitation and CO_2 change. Similar regressions using Northern Hemisphere or local (GHCN) temperature series instead of global temperature give qualitatively similar results (not shown), with a negative correlation of streamflow with temperature alongside a (usually nonsignificant) positive correlation of streamflow with CO_2 level."

e) *The title of the article is somewhat misleading; although a trend is the runoff dataset is determined (p. 791), the main thrust of the ar ticle seems to be the attribution of any variation in runoff. At least, the significant correlation between precipitation and runoff found (Figure 6) is largely based on their interannual co-variation, rather than the trend in precipitation and runoff alone.*

We have heard similar comments from others as well since the original submission of our paper, and with the editor's permission propose to change the title of the paper to "Mapping and attribution of change in streamflow in the coterminous United States" in the revision.

3 Review III (Piao):

a) *It is shown in Table 1 that the regression coefficient of streamflow vs. precipitation varies with changes in precipitation seasonality (the proportion of summer rain). This interesting observation deserves some more discussion. For example, why is streamflow less covariated with precipitation at the regions with* $> 65\%$ *summer rain (higher precipitation seasonality), than at the regions of* $35 - 65\%$ *summer rain?*

We have added brief discussion on why this might be so.

b) *Please provide the predictive power (R2) of each variable (precipitation, temperature, and atmospheric CO2) on streamflow in each regression used in Table 1.*

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We have added R^2 values for the regressions presented in Table 1.

c) *In Figure 5, placing in temporal trends for streamflow and precipitation will help readers perceive the dynamic tendency among fluctuations across different periods. In addition, the meanings of different colored lines in Figure 5 have never been clarified in its caption.*

We have added trendlines and clarifications to Figure 5.

4 Comment (Castellarin):

I would recommend the Authors to include in their review of the literature the study by Douglas et al. (2000).

We now cite this paper in the Introduction.