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A PERSPECTIVE ON NONSTATIONARITY AND WATER MANAGEMENT[1]

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ABSTRACT: This essay offers some perspectives on climate-related nonstationarity and water resources. Hydrologists must not lose sight of the many sources of nonstationarity, recognizing that many of them may be of much greater magnitude than those that may arise from climate change. It is paradoxical that statistical and deterministic approaches give us better insights about changes in mean conditions than about the tails of probability distributions, and yet the tails are very important to water management. Another paradox is that it is difficult to distinguish between long-term hydrologic persistence and trend. Using very long hydrologic records is helpful in mitigating this problem, but does not guarantee success. Empirical approaches, using long-term hydrologic records, should be an important part of the portfolio of research being applied to understand the hydrologic response to climate change. An example presented here shows very mixed results for trends in the size of the annual floods, with some strong clusters of positive trends and a strong cluster of negative trends. The potential for nonstationarity highlights the importance of the continuity of hydrologic records, the need for repeated analysis of the data as the time series grow, and the need for a well-trained cadre of scientists and engineers, ready to interpret the data and use those analyses to help adjust the management of our water resources.

(KEY TERMS: Water Resources Management; climate variability/change; runoff; streamflow; water policy.)

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INTRODUCTION

Much discussion has taken place since several of my colleagues and I published a perspectives article in Science Magazine (Milly *et al.*, 2008) regarding stationarity and water management. Our purpose in writing it was to get scientists and engineers to think more about these issues. We were clear in saying that we really did not have answers, but rather that we had questions and wanted to present some challenges about the need to develop new approaches to analysis, planning, and management. I still believe that we do not have the answers but we are perhaps getting better at posing the questions. In that spirit, this essay elaborates on some of the problems that the climate change issue poses to the water resources community and proposes a few ideas about a way forward.

NONSTATIONARITY IS NOTHING NEW TO WATER PLANNING AND MANAGEMENT

In water resource planning and management, we usually consider nonstationarity in those cases where

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we have a strong scientific basis for including it in our analysis. Examples include: the nonstationarity that arises from urbanization of a watershed and the changes in flood-frequency distributions that occur; or groundwater drawdown resulting in diminished base flow; or man-made reservoirs that reduce peak flows and increase low flows. We would be derelict in our responsibilities if we did not include these kinds of changes.

It is worthwhile to remind ourselves of some of the types of changes that we observe that are due to non-climatic causes. Figure 1 is an example of the role that groundwater depletion plays in streamflow, in this case, particularly, minimum instream flow. This figure shows the San Pedro River in Arizona, a record from 1936 to 2008. The annual minimum daily discharge has gone from a range of about 0.06 to 0.14 m^3/s to now falling to zero on a fairly regular basis, with most of the recent years having an annual minimum <0.05 m^3/s. This is not about climate change. It is about the depletion of groundwater in that watershed (Thomas, 2006).

Another example is the Spokane River at Spokane, Washington, another area, where there has been substantial groundwater development (Hsieh *et al.*, 2007). Looking at the annual seven-day low flows from 1890 to 2004, the trend in annual seven-day minima is downward from around 50 m^3/s to around 20 m^3/s (Figure 2).

We would be irresponsible not to include these types of nonstationarity in planning or operational analyses. They could have a profound influence on our conclusions about future water supply, future habitat, future base-flow water quality, or future assimilative capacity of these rivers. The question is: Do we have a basis for including the climate-related ones? My point here is that we should always include nonstationarity to the extent to which we can describe and understand it. Thus the real question is not whether we should consider climate change in water planning and management. Rather the question is "What do we know well enough about climate-related trends to include them in our planning and management analyses?"

One climate-related aspect of nonstationarity that I think is abundantly clear and ready to be applied is the fact that we are seeing warming and, as a consequence, we are seeing a change in rain and snow dynamics and the role of frozen ground. What we know is that for river basins where snow has been an important component of the hydrology in the past, we are seeing a change to more rain and less snow and a change in the timing of snowmelt. In certain cold regions, we see more hydrologic

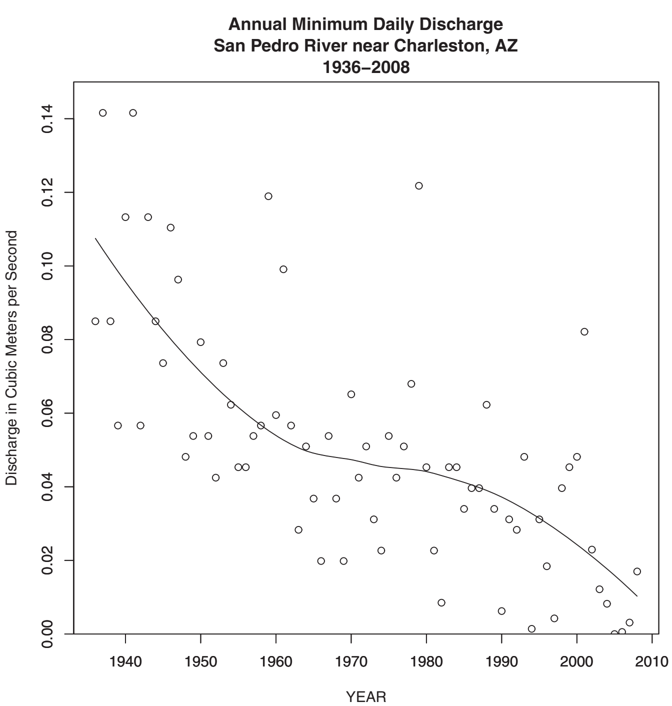

FIGURE 1. Annual Minimum Daily Discharge, San Pedro River Near Charleston, Arizona, 1936-2008. Solid line is a local polynomial regression fit. Graph shows the substantial decline in annual low flows due to groundwater development.

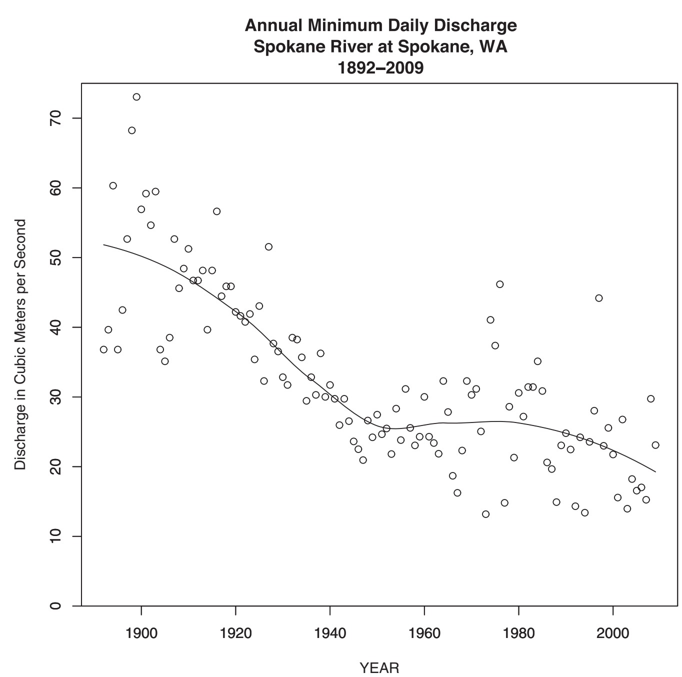

FIGURE 2. Annual Minimum Daily Discharge, Spokane River at Spokane, Washington, 1892-2009. Solid line is a local polynomial regression fit. Graph shows the substantial decline in annual low flows due to groundwater development.

variability in the coldest months because, in the past, they may have always been in a frozen state and any new precipitation fell as snow, whereas now these regions may experience some melting episodes and some rain events interspersed within the cold and/or snowy periods. The generally warmer conditions are leading to an earlier onset of the spring snowmelt period.

We are seeing these timing shifts, and they need to be considered in any studies of system operation or design. The timing change has been well documented (see, e.g., Dettinger and Cayan, 1995; Hodgkins and Dudley, 2006) and is of clear importance to water management because it influences the timing of water availability. But even with this well-documented change, there remain some major questions about the extremes: What is the nature of the changes in floods or low flows in these areas? I think the jury is still out as to whether these timing shifts associated with climate warming are actually turning into flood or low-flow magnitude changes. I think it is an important area for research.

Beyond this topic of the changing role of snow and ice in a watershed, I suggest that there is little else that we can have much confidence in with respect to the impacts of climate change on water resources. I think we need to be mindful of the wide range of climate model projections and also mindful of the wide range of changes (or lack of changes) that we see in watersheds around us. We must not only be open-minded in our search for possible climate-related impacts but also keep a perspective on the relative importance of the many drivers of hydrologic change, not just those related to climate.

THE PARADOX OF THE "TAILS"

For water planning and water management, most of the important questions are about the tails of the distribution and not about the center of the distribution, but our uncertainty about future hydrologic conditions is greatest in the tails. Information used to design strategies to mitigate floods is based on the characteristics of the upper tail. This includes design of flood-walls, levees, or bridges, or the determination of the appropriate size of the flood pool, or determination of the boundaries of high flood-hazard areas. In many cases, planning for water supply is focused on the lower tail of the flow distribution. Storage for water supplies is based on the probability of extended periods of low flow. Withdrawal permits for rivers are often based on statistics such as the 7-day 10-year low flow. Only in those cases where anticipated water use approximates the average renewable supply of the watershed, does the mean streamflow become an important statistic for planning or design purposes. This applies to a few watersheds in the western United States (U.S.) such as the Colorado River Basin and Rio Grande River Basin. Most decisions related to water quality are focused on the lower tail (for writing permits for point-source discharges) or the upper tail (for load allocations for storm water or other forms of nonpoint source pollution). From a statistical perspective, characterizing the tails and evaluating trends in variability is much more uncertain than characterizations or evaluations of trends in the central tendency. Matalas (1990) wrote about the difficulty of testing for changes in variability or skewness when compared with testing for trends in the mean. Thus, the characteristics of the probability distribution that we need to know the most about are precisely the things that are hardest to estimate or predict. This is the paradox of the tails.

A recent review of the use of climate models to make projections of future water resource conditions (Barsugli *et al.*, 2009) points to the many sources of uncertainty. They note that projections of precipitation change are less reliable than projections of temperature change and that projections of conditions for small areas (such as a watershed) are less reliable than for much larger areas. In particular, they state that "projections of average annual changes are more reliable than projections of average seasonal change, which are more reliable than projections of monthly change, which are more reliable than projections of sub-monthly change, and so on." Their assessment places "medium" to "low" reliability on estimates of variability in yield, seasonal flooding, and major storms. For flash floods, they assess the reliability of climate model outputs as "very low." Thus, in the world of model projections and in the world of statistical analysis, we have the most confidence in statements about the least important aspects of hydrology (the central tendency), and the least confidence in the most important aspects (extreme events). As a consequence, even after the last few decades during which the scientific community has been exploring climate change in relationship to water resources, we need to be clear with all audiences about just how little we actually know about these important potential changes in extremes. Barsugli *et al.* (2009) conclude that although they believe that the long-term, large-area, projections of change are more reliable than those for short terms and small areas, they state: "It could be argued that the climate models do not currently yield sufficient reliability to make forecasts of change in climate at the scale of river basins."

PRECIPITATION DOES NOT TELL THE WHOLE STORY

The analysis of precipitation is useful, but it is not a substitute for the analysis of streamflow or groundwater levels. We need to be careful that we do not assume that observed or projected changes in precipitation variables such as one-day precipitation or number of dry days will translate simply and directly to changes in streamflow. For example, some of the greatest floods that we have observed in the U.S. in the last several decades have been a part of events that were months in duration. The two great Midwestern floods of 1993 and 2008, or the Red River floods of 1997 or 2009 were results of many months of high precipitation, punctuated with some short-term high-precipitation or high-temperature events. Other reasons why trends in precipitation may not directly translate into changes in streamflow include changes in antecedent soil moisture. The climate models suggest that we are going to have drier soils in parts of the U.S. Increased precipitation, or more intense precipitation, falling on drier soils, may or may not lead to larger floods. Another factor is changes in frozen ground conditions. We may have areas that previously had frozen ground at the time of the flood-producing events that may not have frozen ground today. Again, these are things that make it difficult to just translate precipitation change into a flooding change. Contradictions between precipitation-trend studies and streamflow-trend studies are not necessarily illogical. Hydrologic responses such as changes in streamflow or groundwater levels can be thought of as a convolution integral of precipitation and the changing climate may bring about changes in the response function. In short, analyses of precipitation trends are important but they are not a substitute for analysis of trends in streamflow or groundwater levels.

THE PROBLEM OF HYDROLOGIC PERSISTENCE

Long-term persistence and human-induced trend are very easily confused. Hurst (1951) taught us about persistence many years ago. It is the natural pattern for wet years to tend to follow wet years and for dry years to tend to follow dry years. Mandelbrot and Wallis (1969) introduced the concept of "fractional noise," which is an example of a stochastic process that exhibits long-term persistence. Mandelbrot and Wallis (1969, pp. 230-231) observed that "[a] perceptually striking characteristic of fractional noises is that their sample functions [time series data] exhibit an astonishing wealth of 'features' of every kind, including trends and cyclic swings of various frequencies." Matalas (1990) commented on the problem of distinguishing trend from persistence by saying that "…a trend in the short run may be part of an oscillation in the long run." In recent years, we have learned much more about quasi-periodic variations like El Niño, Pacific Decadal Oscillation, and Atlantic Multi-decadal Oscillation, and even the ice ages. We have learned that these ocean-atmosphere related oscillations can have significant impacts on hydrology. We also know that these phenomena are still beyond the limits of our ability to predict. Given what we have learned about these sources of long-term persistence, we need to be very careful to avoid falling into the trap of seeing a pattern that plays out over several decades and calling it a trend. The best protection we can have against this trap is to use very long hydrologic records because they are likely to contain multiple realizations of many of these oscillations. This does not entirely solve the problem because long records can still be influenced by very long-period oscillations, but using long records can provide some protection. We believe that there are physical mechanisms at the root of this behavior, but we are far from understanding the persistence that we actually see in hydrologic records and we know that there are entirely natural oscillations with characteristic time scales far beyond those of things like the Atlantic Multi-decadal Oscillation (e.g., the Little Ice Age or the Pleistocene glaciations). The following are some examples of some of the persistent patterns that appear in hydrologic records and a demonstration of how easily confused we can be by these patterns.

Figure 3 is an example of the annual minimum daily discharge of the Big Sioux River at Akron, Iowa. A change shows up here about 1980. This kind of change is not limited to just one river. Many rivers in this area show a similar pattern of increased low flows around that time. Something significant happened in these basins and we do not understand it. It is much too abrupt to be associated specifically with the long-term greenhouse effect. But it just shows how dramatically hydrologic behaviors can change, moving to a very different "state" and remain in that state for many decades.

Figure 4 is an example from an area of the nation that has been experiencing extraordinary changes in flood magnitudes in recent decades. It is the Red River of the North at Grand Forks, North Dakota. The left panel shows the annual peak discharges for the years 1925 to 2009. The plot shows a local polynomial regression fit that shows a strong increase from 1925 to about 1960, then a relative level period to

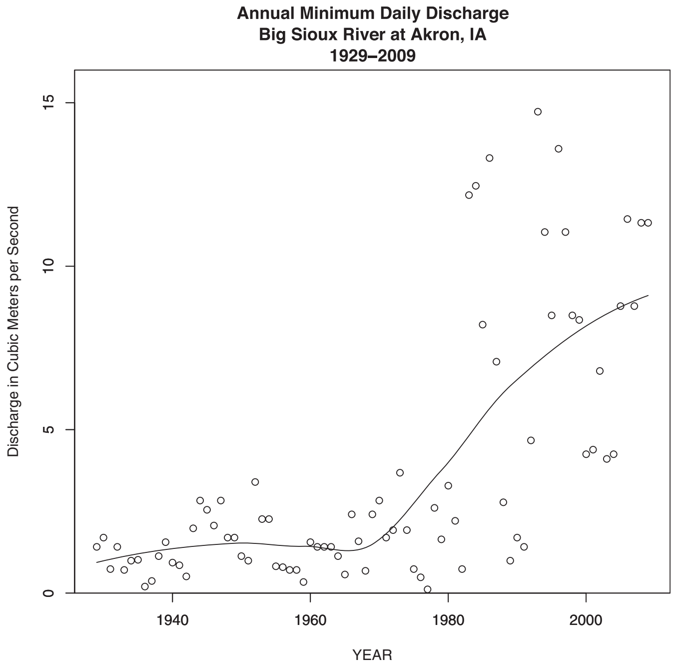

FIGURE 3. Annual Minimum Daily Discharge, Big Sioux River at Akron, Iowa, 1929-2009. Solid line is a local polynomial regression fit. Graph shows the rapid change in the characteristics of low-flow conditions that took place around 1980.

1980, and then a steep and perhaps accelerating rise since that time. If we run a regression of the log of discharge *vs.* water year, we get a trend slope of 20% per decade (which is statistically highly significant). Looking at this result, it would be easy to conclude that floods are getting larger over time and this would be very consistent with a hypothesis that increased greenhouse gases are driving this increase.

However, the record actually begins in 1882 (Figure 4, right), and if we show the data back to that point, we get a very different and more complex picture. The fitted curve suggests a period of decrease from 1882 through about 1920 followed up by an upward trend from 1920 to the present. A regression analysis now gives a positive slope (also highly significant) but of only 8% per decade. The addition of 43 years at the beginning of this record results in a major change in the inference one might draw. Now we would say that although there has been some increase in flood magnitudes over time, the pattern is no longer very consistent with a hypothesis that this is driven by greenhouse gas increases in the atmosphere. The high values in the 19th Century are inconsistent with this hypothesis. In fact, one could put forward the argument that there are two populations of annual floods at this location. One is the population that spanned the years of about 1900 to 1941, and the other population existed before 1900 and after 1942. Without the benefit of the longer record, we could easily conclude that the data were highly supportive of a greenhouse-gas driven trend in flood magnitudes, but with it we find ourselves having to entertain other highly plausible hypotheses about an abruptly shifting population, with shifts that take place at time scales of many decades. The data do not negate the possibility that greenhouse forcing is a significant factor here, but they make it much more

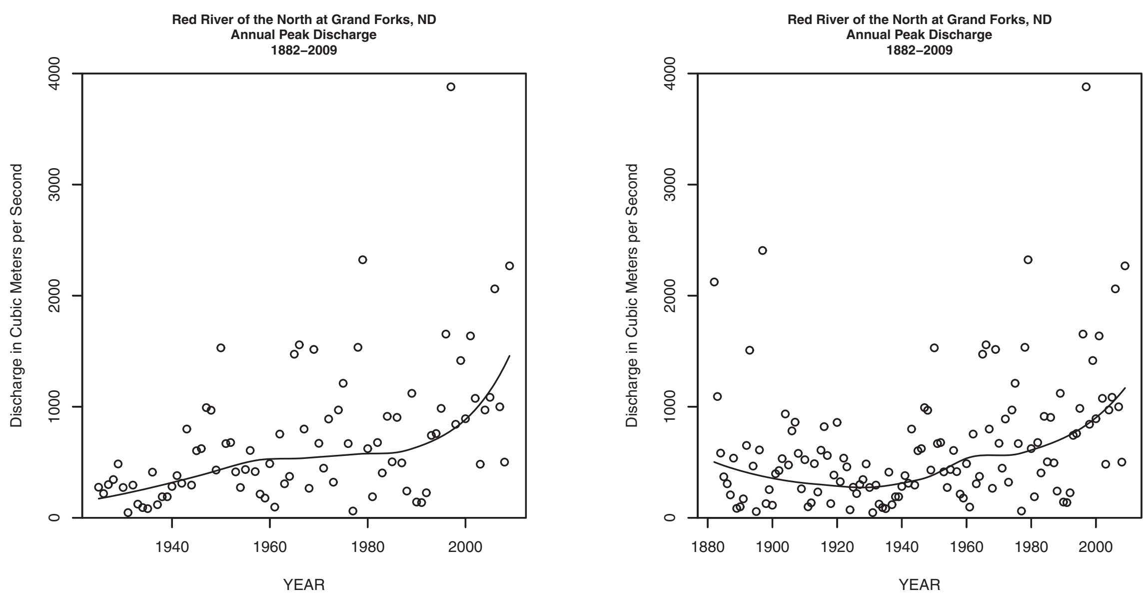

FIGURE 4. Annual Peak Discharge, Red River of the North at Grand Forks, North Dakota. Solid line is a local polynomial regression fit. Left panel is 1925-2009. Right panel is 1882-2009. Left panel suggests a strong monotonic trend in annual flood magnitudes, but right panel (using the full dataset) shows a nonmonotonic trend pattern and some suggestion of a two-state system with strong persistence.

difficult to argue that these data provide a clear demonstration of the effect of enhanced greenhouse gas forcing on flood magnitudes.

Figure 5 is yet another example of the same point. This is the Gila River at the head of Safford Valley, Arizona. If we only evaluated the record from 1921 to 1960 (Figure 5, left) and did a flood-frequency analysis using that period, our estimate of the 100-year flood would be 1,000 m^3/s (using the log Pearson III distribution). Using this estimate, the record from 1960 to present contains 8 floods that exceed the estimated 100-year flood magnitude. The expected number of 100-year floods during this period is only 0.5 floods. Clearly, an estimate of the 100-year flood using data from 1921 to 1960 would be viewed as ridiculous, and such a result would suggest that floods are responding to the greenhouse effect and shifting the flood-frequency distribution upwards over time.

The right panel of Figure 5 shows the data back to the actual beginning of the record in 1914. If our flood-frequency analysis had been done with all the data up to 1960, we would find that the estimated 100-year flood would now be about 2,500 m^3/s, and this level would only have been exceeded two times in the years 1961-2007. Inferences about greenhouse-gas increases driving increased flooding are not well supported by datasets such as this one. This record is much more consistent with a concept of quasi-periodic behavior or multiple population.

What these examples tell us is that hydrologic records can have a high degree of persistence. It suggests three ideas that need to be considered in the empirical analysis of hydrologic data. One is that persistence can be a real problem because it can easily be confused with long-term trends that might be driven by ongoing changes in atmospheric composition or driven by human activities on the landscape. The problem can be reduced, but not eliminated, by using longer-term datasets. Datasets of only a few decades in length can easily point to strong trends that are simply an artifact of some quasi-periodic variation. The second is that explicit consideration of persistent climate phenomena can be helpful in understanding the true underlying trends that may be present. Some of the persistence we see in these examples may be explained on the basis of phenomena such as El Niño. Third, it shows that frequency estimates that are based on short records can be very inaccurate not only for reasons of lack of fit and sampling error but also because shorter records may not have provided us with a full range of behaviors that we can expect from the system.

People have asked me "How should I incorporate long-term climate change into my frequency estimates?" I respond by saying that I do not think we have a particularly good answer to that question, but we can say this: The starting place should be to make sure that frequency analyses use all of the data currently available. We should also strive to bring in paleo-data and historical information just to broaden our perspective on the kind of variability that can exist at this site. If the hydrology is changing over time, we want to make sure that our estimates incorporate the newest data and the extremes of the

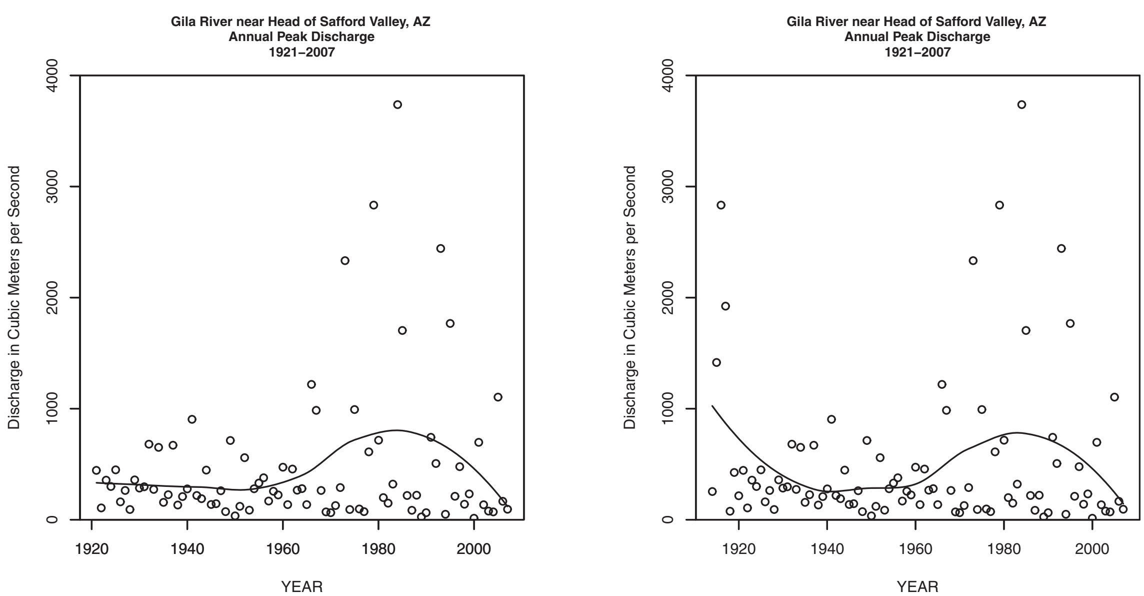

FIGURE 5. Annual Peak Discharge, Gila River Near Head of Safford Valley, Arizona. Solid line is a local polynomial regression fit. Left panel is 1921-2007. Right panel is 1914-2007. Left panel suggests a stationary process from 1921 until about 1962 followed by a steep increase and then a decrease. Right panel, showing the entire record, suggests a two-state system with a population of high values from 1914 to about 1920 and from 1962 to about 2007.

distant past. Too often, we use flood-frequency estimates that do not include the most recent data. This would be unwise even if we had no concern about nonstationarity, but becomes dangerous in the presence of either nonstationarity or strong persistence. Starting with a frequency analysis that assumes stationarity and uses all of the available data, one can also conduct sensitivity analysis that involves assumptions about possible trends or mixed population models as alternatives, but the starting place should be an up-to-date dataset. The recognition of long-term persistence should also lead us to place much wider uncertainty bounds on our estimates of flood frequency, recognizing that there is a real possibility that the future may not be like the past, even if human actions were known to have no impact on floods.

TWO PATHS TO FOLLOW: MODEL-BASED AND EMPIRICAL

The research related to climate change and water resources should follow two paths simultaneously. One path is to use climate models (global and regional) to drive hydrologic models, but the other path is to explore the hydrologic record to see what we can learn about hydrologic change that might help guide our ideas about the future. It is very important for these two paths to provide feedback to each other, using data to test the models and the models to suggest questions to pose to the data.

Today the use of climate models is the dominant path. The approach used here is to take climate model projections, and by various approaches, "downscale" the outputs to be appropriate and reasonable input variables to drive hydrologic models that are then used in simulations of system operations. In contrast, the empirical path is predicated on the idea that the past century as an unplanned experiment that we must exploit to learn more about how the climate/hydrology system behaves and will behave in the future. The experiment is this: mankind has added substantially to the concentration of greenhouse gases in the atmosphere over the past century. Global CO_2 is about 35% higher than it was at the beginning of the industrial revolution. It is very logical to expect that this change in the radiative properties of the atmosphere has already changed the hydrologic cycle. We need to use this experiment that is going on today, and tease out as much information as possible from the hydrologic record and see what we can learn from it. Although we only have one Earth and thus only one run of this experiment, in another sense we have many experimental subjects. These are the individual watersheds. Each one is responding in its own way to the changing global atmosphere. Particularly when we have hydrologic records approximating a century in length, we can look at their behavior before there were substantial additions of greenhouse gases and then explore the last several decades when there has been a rapidly increasing level of greenhouse gases. Using this approach, we hope to learn how this "experiment" plays out in watersheds of different sizes and different geologic and climatic characteristics.

Both of these paths (model and empirical) are extremely flawed. But they are the only methods that we have at this point, and we need to pursue both of them and try to see to what extent they are pointing us in the same direction, with similar conclusions – although it is possible they could both be wrong – or whether they are diverging. We need to have a much higher level of communication between the two different approaches and cross-checking between them.

As I said, both approaches have severe limitations. I am not going to spend a lot of time on problems of using climate models to make projections of future water resource conditions. Some of the weaknesses described in The Water Utilities Climate Alliance (Barsugli *et al.*, 2009) "options report" relate to simulation of precipitation, especially intense precipitation; to simulation of orographic effects; and difficulties in reproducing long-term persistence driven by quasi-periodic ocean phenomena such as El Niño.

Is downscaling the answer? This is a hotly debated topic and much more work is needed to identify the pros and cons of this approach. Some of the problems with this approach are associated with the inherent inaccuracies of the climate models. Another problem is the way that climate model results are captured simply as changes to precipitation and temperature rather than as changes to many aspects of the entire water and energy balance at the land-atmosphere interface. As concerned as I am with the downscaling approach, I believe it should still be pursued and improved, but there should also be a vigorous research effort at the same time to see if downscaling can properly hindcast the hydrologic changes (or lack of changes) that have been observed to date.

The empirical approaches have severe flaws as well. One of those flaws is the difficulty in sorting out what might be a land-use signal from a climate-related signal, because there are so few hydrologic measurements at locations that are completely free of any land-use-related changes. Another flaw is the potential that what appear to be hydrologic trends related to the trends in greenhouse gases may be nothing more than long-term persistence. Another flaw is that there may be significant nonlinear effects. We may identify a linear relationship between a hydrologic variable and greenhouse gas concentrations. However, we cannot assume that this linear relationship will continue in the face of the further increases in greenhouse gas concentrations that we can expect to happen. There may be major hydrologic changes that take place abruptly if we get important shifts in circulation and storm tracks and our historic record of gradual change in greenhouse gasses will not enable us to project these types of hydrologic changes. But, even with these caveats, it is crucial that we exploit the data we have to the greatest extent possible to help us anticipate the kind of hydrologic changes that we can expect in the future.

Given the importance of the need to understand and project future changes in water resources I am advocating that we need both a model-based approach and an empirical approach. We need more research that operates from the premise that our watersheds are the experimental subjects in a global unplanned experiment, and within that premise we need to try a diverse set of empirical analytical approaches. I would argue that the hydrologic literature is very thin at the present time on the analysis of the long-term records of precipitation, runoff, and groundwater data that we have accumulated.

ONE EXAMPLE OF THE EMPIRICAL APPROACH

What do we actually know about the amount of trend we see in the records we have? This particular analysis explores trend in annual flood peaks in the U.S. Figure 6 is based on an analysis of 200 USGS streamgages in the conterminous U.S. that have operated for more than 85 years that have no significant upstream regulation or urbanization. The triangles represent the direction and slope of the trend over the period of record. Record lengths range from 85 to 127 years (ending with water year 2008). Several other analyses have been performed with these data looking at 60- and 80-year periods. The results of these shorter periods are very similar to what is shown here. The trend slopes are shown on the map without regard to statistical significance because the focus here is not on the result at any one particular streamgage. Instead the focus is on exploration of common patterns across the U.S. To facilitate this exploration, the U.S. was subdivided into four quadrants (shown on Figure 6) and the slopes are shown in Figure 7 as boxplots for the dataset as a whole and for each of the four regions. The results show a strong tendency toward negative trend slopes in the Southwest, a rather even division between positive and negative in the Northwest, and slight tendencies toward positive trends in the East, slightly more in the Northeast than in the Southeast.

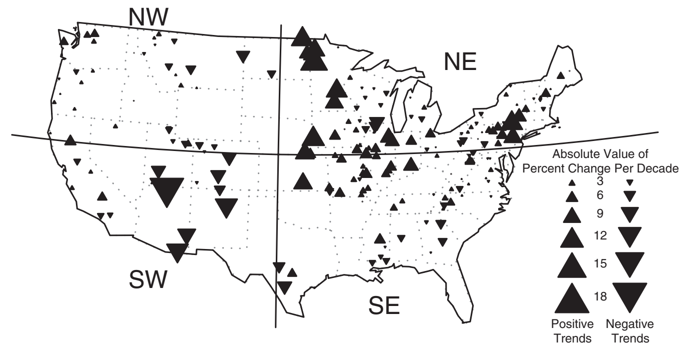

FIGURE 6. Annual Peak Discharge Trends at 200 Streamgages With Record Lengths of 85-127 Years, All Ending in Water Year 2008. Trend slopes are expressed as percentage change per decade, based on a linear regression of the log of the annual peak discharge *vs.* water year. The triangles indicate direction and magnitude of the trend. Lines indicate the boundaries of four large regions. Results show clustering of increases in upper Midwest and parts of the Northeast, and a cluster of increases in the arid Southwest.

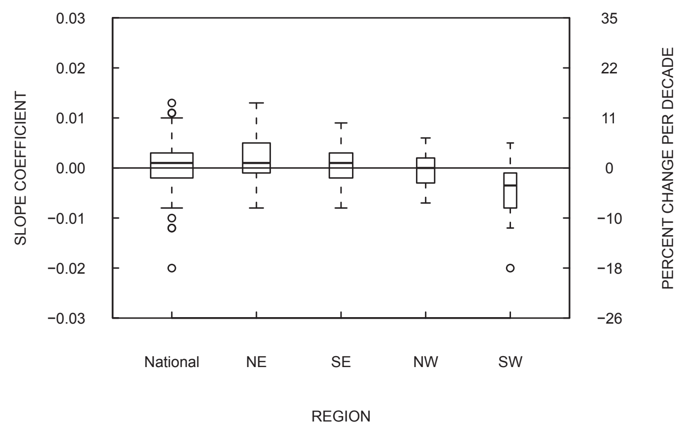

FIGURE 7. Boxplots of Annual Peak Discharge Trends, Nationally and by Region for the Results Shown in Figure 6. Trend slopes are expressed as percentage change per decade, based on a linear regression of the log of the annual peak discharge *vs.* water year. Boxes show the distribution of slopes across all 200 streamgages, and boxes for each of the four regions. The Southwest region shows negative trends in more than 75% of the streamgages. The Northeast and Southeast have more positive than negative slopes but only slightly so. The Northwest is evenly split between positive and negative slopes.

Of the 200 sites, 54 of them were significant (at $\alpha = 0.05$) and, of these, 19 were significant negative trends and 35 were significant positive trends (under a null hypothesis of no trend, we should expect about 5 significant upwards trends and 5 significant downwards trends). Of the significant trends, those in the Southwest were all negative and, for the other three quadrants of the U.S., there were more positive trends than negative, but there were some negatives in each of these quadrants. Note that, in this analysis, a trend estimated as 5% per decade translates to about 63% per century and a trend estimated as −5% per decade translates to a decrease of about −39% per century. The median of the absolute values of the trend slopes is 2.63% per decade and the largest is 17.9% per decade (which happens to be a negative slope). Thus, at time scales of a century or more, many of these represent very substantial changes in flood magnitudes. The flood data analyzed show very little indications of changes in the variability of the log discharge values over time, so a reasonable interpretation of the results is that the rates of change observed would apply to all quantiles of the distribution (so a 5% trend per decade applies to the 2-year flood, 10-year flood, 100-year flood, etc.).

What does this example tell us? It tells us that there is a great deal of site-to-site and regional variability in how flooding behavior is changing in U.S. rivers. The West behaves differently than the East and, in all regions but in the Southwest, there is a mix of increasing and decreasing flood magnitudes. The Southwest is dominated by decreases. We also see that flooding is "clumpy" in both space and time. These observations should give us reason for caution when model results suggest widespread increases in flood magnitudes. Perhaps in time, as greenhouse gas concentrations increase, a very coherent pattern of change in flooding will emerge above the considerable noise in the system, but for now the longest datasets we have do not provide a basis for anticipating such a pattern of change.

CONTINUITY OF OBSERVATIONS AND DATA ANALYSIS

We need to redouble our efforts to observe the hydrologic system, describe what we see, and apply what we see. But my point is not just about the importance of observations. It is also about interpreting the data and incorporating this new knowledge into water-resources design and operation.

My approach is to say, "It is non-stationarity. Get over it." To me, it is given that hydrologic systems are changing and the task of the scientist is to describe the nature of the change and use that to develop a predictive understanding of how it may change in the future under a future set of stresses. We should put our emphasis on describing and understanding the changes that are occurring in the hydrologic system, taking into consideration the full range of possible drivers: for example, land-use change, dam building and removal, groundwater development, and climate drivers. The tools of choice should be those of exploratory data analysis more than the use of hypothesis tests.

Another quote from Milly *et al.* (2008): "In a nonstationary world, continuity of observations is crucial." To make this point about the importance of measurements, I turn to a paper by Ralph Keeling (2008) who tells the story of Dave Keeling (his father) and the efforts he undertook to develop and then sustain the monitoring of atmospheric CO_2 at the Mauna Loa Observatory, and the scientific interpretation of those data.

I know that some of the hydrologists have what I call "CO_2 envy" (see Vörösmarty, 2002). We wish we had a time series that was as clean and understandable and clear as the Mauna Loa CO_2 record to help us tell our story of change. The atmospheric scientists are dealing with a well-mixed fluid and one where the change is large in relationship to natural variability. Hydrologists deal with a poorly mixed fluid (water in the atmosphere and on land) and one for which natural variability is very large in comparison with temporal change. These differences make the task of interpreting our records much more difficult than the task of the atmospheric chemists, but we must strive to collect the data, interpret it, and describe to others the story it contains.

Ralph Keeling's paper tells how difficult it was for his father to continue to get funded to collect this absolutely crucial data on the condition of the planet. The people who reviewed his proposals in the funding agencies said "Where's the hypothesis? This is just monitoring." To quote from Ralph Keeling: "A continuing challenge to long-term Earth observations is the prejudice against science that is not directly aimed at hypothesis testing. At a time when the planet is being propelled by human action […], we cannot afford such a rigid view of the scientific enterprise." In other words, among the most important scientific efforts to undertake are those that measure the state of the planet. These will lead to the formulation of important hypotheses and will be the basis for learning how the planet operates and will provide the reality against which the models can be tested.

Keeling goes on to say that: "The only way we can figure out what is happening to our planet is to measure it. And this means tracking the changes, decade after decade, and poring over the records."

And in the cases of my agency, the USGS, and our colleagues at the National Weather Service, I think of the importance of bringing flood and low-flow frequency analyses (USGS) and precipitation frequency, intensity, and duration analyses (NWS) up to date. Both agencies are making efforts on these fronts, but I would argue that both agencies are behind the curve in being able to provide the nation with the kind of up-to-date information that is needed. Regardless of whether we think climate change is important to engineering design and operations, we need to base the design and operations on the most up-to-date information. We need to pore over the records much more than we do today.

There is another important issue here and that is the continuity of records. Recognizing that the world is nonstationary really heightens the importance of keeping our longest observational records going. The only way we will observe change and potentially sort out trend from persistence is to have records that stretch toward 100 years and beyond. What has been disturbing many of us in the hydrologic community is the difficulty that we have in keeping the funding going so that we can keep the streamgages operating. If we just look at streamgages that have operated for at least 30 years, we had to shut down about 100 of them in 2007 due to funding gaps. These losses of long-record stations have had their ups and downs. There were about 150 losses of streamgages per year in the mid-1990s, only about 20 losses in 2001 (a year of improved streamgaging budgets), but the rate of streamgage loss has been on the rise again in recent years. To provide a more concrete example in one part of the nation, we can look at the Pacific Northwest. At the end of 1979, we had 317 streamgages operating that had started operations in 1930 or before. As of 2007, we had 220 of these still operating, a loss of 31% of the total. Given the issues of snowfall, snowpack, streamflow timing, and instream flow for fisheries in this region, this kind of loss of monitoring assets is troubling, to say the least. As mentioned above, we should look at climate and hydrology as an unplanned global experiment (the experimental treatment being the addition of greenhouse gases to the atmosphere) and we should think of every watershed as an experimental subject. The streamgages are how we measure the effect on each experimental subject. The experimental subjects are not totally independent of each other, so the loss of statistical power is small if we have lost a streamgage that is highly correlated with one or more of the streamgages that remain in operation, but to the extent that they are independent of each other, the loss of each one results in a loss of ability to detect and describe the hydrologic results of the experiment.

NEW APPROACHES TO DECISION MAKING

We need a new multidisciplinary attack on water-resources planning and management, given the high degree of uncertainty about the potential changes in water resources not only from drivers such as climate change, but also from land-use change and groundwater depletion, or other human actions on the landscape. This is a point made in Milly *et al.* (2008), "Stationarity is Dead: Whither Water Management." The point we made there was that the basis of our current practices in engineering, economics, and decision theory emerged from the Harvard Water Program (Maass *et al.*, 1962), and these implicitly assumed that streamflow is a stationary process. Starting with this premise the engineering-economic task was to do some kind of optimization on a risk *vs.* cost trade off.

Once we recognize that we have nonstationarity for a variety of reasons, things like urbanization, groundwater development, as well as climate change, we really have to rethink our approach to planning and operations. It is going to take a concerted effort by a combination of statisticians, economists, operations researchers, hydrologists, climatologists, and civil engineers working together in a think-tank kind of environment to create a whole new approach to decision making. "Finding a suitable successor is crucial to human adaptation to climate change" (Milly *et al.*, 2008). An excellent discussion of ideas about decision making under uncertainty, as it relates to the question of climate change can be found in Morgan *et al.*, 2009.

THE IMPORTANCE OF HUMAN CAPITAL

A very important part of what we need for successful adaptation of water resources to climate change is human capital. The next generation of water-resources professionals (planners, designers, operators, researchers) needs to be well educated and they need to be employed in the agencies and companies that do the analyses to keep our water-resource systems abreast of the changing hydrologic system. So many of the analyses of important characteristics of our watersheds (e.g., low flows, flood volumes, flood peaks, flood hazard zones) are seriously out of date and need to be updated on a continuous basis to provide the foundation of knowledge on which we can plan and operate our systems. Furthermore, as our climate changes, there will be many hydrologic changes that we will need to track and understand (soil moisture, frozen ground, nutrient dynamics, algal dynamics, and many other topics). Effective planning and operations depend on having on staff, or on contract, a workforce that understands hydrology and atmospheric science and that is able to devote the time needed to describe and understand the changes that our water resources are undergoing and to make thoughtful projections of how the system will evolve decades into the future. Our ability to adapt to all of the hydrologic changes that are taking place (related to climatic and other drivers of change) depends on graduate and undergraduate education of water professionals and the staffing levels of water agencies. We need professionals with the knowledge and motivation to keep monitoring the resource, to learn from the changes that are taking place, and to use that ever-changing knowledge to adjust our designs, plans, and operations on an ongoing basis.

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