

Have We Dropped the Ball on Water Resources Research?

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In Peter Rogers's excellent editorial (Rogers 2008), he suggests three general classes of hypotheses associated with global warming and suggests that adaptation of water resources systems is critical regardless of the hypothesis. How do we plan for adaptation in water management, the necessity for which Peter argues is becoming increasingly apparent?

In a recent *Science* paper, Milly et al. (2008) argue that stationarity, the cornerstone of most of our planning methods, is dead. To those of us in the hydrologic community who came of scientific age in the 1970s, the argument may seem like déjà vu all over again, as we remember the contentiousness of the debate over the Hurst phenomenon, which essentially was an argument about the relevance of stationary versus nonstationary statistics to hydrologic time series analysis. Viewed 35 years later, what is apparent is that although the participants in that discussion recognized that water resources systems were susceptible to climate variability and even change, there was presumed to be no prior knowledge as to its direction.

Now, at least in some contexts, we have an idea as to the direction of change (a particular example is streamflow in the western United States, where most indications are that temperatures have warmed over the past 50 years and seem likely to continue to do so, resulting in shifts in the seasonality of runoff). Furthermore, climate change does not have a corner on the market for nonstationary methods—we now increasingly recognize the role that change in land cover and land use has in hydrologic variables.

Certainly the profession has been slow to acknowledge these changes and acknowledge that fundamentally new approaches will be required to address them [see also Howe (2008), as well as the very interesting series of papers in the special issue of the UCOWR *Journal of Contemporary Water Research and Education* that follow his preface]. In my experience, the response of those in the water resources planning community (populated largely by engineers, meaning our students) has been to dig in their heels. Further, they say that there is lots of uncertainty in the planning process; thank you, but all of that uncertainty (including that attributable to future hydrologic changes) is already dealt with in our traditional planning methods.

What I've seen happen increasingly is that outside review, often by citizen or political oversight, has asked questions such as "how are you dealing with future climate change in those projections?" although it may be that climate (or land cover) change is just one of many terms in the uncertainty equation, that perspective is a difficult sell, especially given the widespread visibility afforded to the IPCC process. So the heat is on, so to speak, to develop new approaches that explicitly deal with climate and other types of environmental change.

I think that it is reasonable to assert that understanding hydrologic change will be one of the key challenges to the hydrology

community in the coming decades. There are numerous examples where land cover and climate change have affected both natural (i.e., streamflow) and managed water resources—not to speak of water demand. Setting aside for the sake of this editorial our ability to predict climate change (itself a large source of uncertainty), let's just ask what kind of job we're doing in training the next generation of scientists and engineers to address these problems in the context of water management. In my mind, the answer is pretty obvious—we basically aren't doing the job in any meaningful way.

Why not? The proximate reason that those responsible for water resources planning generally ignore climate and land cover change is that they are using methods (that those of us in the academic community taught them) that don't really address the problem. So how do we break that cycle? The simple answer is, the impetus has to come from the academic community, which is charged with being the source of new ideas and methods. But what drives the academic community? To paraphrase my friend Dave Dawdy, "You surely don't think that universities exist for the pursuit of knowledge? They exist for the pursuit of money."

Yes, that sounds crass, but money—in particular research funding—supports graduate students, and the nature of that funding determines the expertise that is available in the Ph.D. market. My university recently ran a search in an area that we termed "sustainable water resources." Among more than 100 applicants, I counted only one or two whose background could even remotely be termed water resources management. Among a very talented field, and considering the hydrology area alone, we had lots of applicants in areas such as land-atmosphere interactions, remote sensing, ecohydrology, and so on—but essentially no one in water management.

Why is that? To me, the reason is obvious—there's no funding in the area, hence the most talented Ph.D. students work in other areas. But that means that there are no young faculty members in the area, hence no new ideas. Sure, I'll grant you that universities are hardly the source of all new ideas, but we do have a responsibility to be forward looking, and we aren't meeting that responsibility.

So where does the source of the problem lie? At least in part, the hydrology community has been a victim of its own success. The so-called Eagleson Report (NRC 1991) essentially rode the water resources engineering community out of hydrology. Hydrology was defined as a science, and at least in surface water hydrology, the primary funding agencies (NSF, NASA, and NOAA) don't deal with water management problems. (That is not entirely true—both NASA and NOAA fund some "applications" research, but it is fairly specific, e.g., work to figure out how to apply remote sensing products or climate products—and not fundamental work dealing with how water systems are operated.) NSF won't touch water management—its Hydrological Sciences program is just that, and although the Natural Hazards program in the Engineering Directorate makes mention of floods and droughts among its priorities, the program has been shrinking and is in practice an earthquake engineering program.

Why has water management research been allowed to fall on hard times just when one would think it would be most needed? The most obvious answer is that no vocal academic community is advocating for it. Engineers don't tend to be terribly outspoken by nature, and we could take a lesson or two from our colleagues in the sciences. I recently served on the NAS Decadal Review of Earth Science and Applications from Space (ESAS). That rather substantial activity (which involved about 100 scientists on an executive panel and seven subpanels) was structured along themes (such as weather, climate, water cycle) rather than disciplines. The stakes were substantial, as NASA had stated (and appears to be following through) that the next generation of earth satellites would follow the ESAS priorities. The oceanography community was not at all happy that there was no oceanography subpanel—instead, oceanographers were diffused among several subpanels. The result was a letter from more than 700 oceanographers, basically complaining that their voices were not being heard. I can tell you that it's hard to ignore 700 screaming oceanographers—basically they got what they wanted. So where are the 700 screaming water resources engineers?

Assuming that there are people who read these editorials, and a few who might actually agree, the question is, how should the problem be resolved? In my experience with various efforts to sell research agendas, the first step is to establish a credible group (could be an ASCE committee of some kind, for instance) and to formulate a plan. This effort doesn't necessarily have to be very long (probably better that it isn't). The group needs to lay out the scientific and engineering basis for the perceived issues as forcefully and concisely as possible. It then needs to lay out a plan for what needs to be done—both in terms of the research and the

organizational structure to foster it. I think that an argument can be made that the “who” is NSF, and that the target needs to cross somehow between the Engineering and Geosciences directorates. I should point out the Geosciences Directorate in particular will soon (in fact, by the time this editorial appears) be under new management, which is always a good time to make such an approach.

In summary, water resources research has been allowed to slide into oblivion over the past 30 years. From a time when the lead journal in the field took the name of the area, fundamental research has declined basically to zero. I see tremendous opportunities in the area, and find it hard to understand why a convincing argument hasn't been made (it may well be simply that the activity level is so low that critical mass has been lost) to resolve this problem. The need is great, and I think that it borders on irresponsibility that the community has allowed the current situation to come to pass.

References

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