RESEARCH TRENDS

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Humanity's preoccupation with, attraction to, and study of water resources are as old as civilization. Ancient cities were built within easy reach of water, Roman aqueducts channeled water to Rome, the Chinese culture developed along with their efforts to intensify agricultural production by the use of irrigation, and modern cities have flourished or declined in direct relationship with the availability and management of water resources.

Yet, even today, we are still grappling with many of the same issues that have plagued humanity for thousands of years. Floods, droughts, hurricanes, energy intensive irrigation practices, waste water management, and pollution control are items that have dominated our water management specialists in the past and continue to be at the forefront of research efforts today.

What are the future trends in research efforts aimed at successfully managing our water resources? Before I answer that question, I would like to briefly discuss the difference between applied research and basic research. In order to do that I am going to describe four examples of how basic research led to solutions of practical problems dealing with water.

Research on the Shapes of Raindrops

In the 1960s Hans Pruppacher, a physicist at UCLA, asked the questions, "What is the shape of a raindrop?" Up to this point, scientists had had difficulty observing the fast-falling raindrops. They knew that contrary to popular opinion, raindrops were not shaped like teardrops, but the actual shapes remained uncertain. Pruppacher was able to gain support from the National Science Foundation (NSF), which resulted in the formulation of a theory that enabled him to predict the shape of a raindrop from its size, considering the forces that act upon the drop while it is falling freely in the atmosphere. In 1970 and 1971, Pruppacher was able to test and verify his theory by suspending raindrops in the vertical air stream of a wind tunnel. In the wind tunnel, Pruppacher could observe raindrops closely.

There was no immediate application of Pruppacher's resultant findings relating the height and width ratio of the raindrop to its specific size. It was almost a decade later, in 1979, that Thomas A. Seliga, an electrical engineer at Ohio State University, developed a new rainfall-observing radar technique based upon Pruppacher's work at UCLA on the basic geometry of raindrops.

Seliga arrived at his technique after two years of experiments. It takes advantage of the difference in signal power scattered by raindrops when the radar beam is aligned along two different planes of polarization. Although standard radar equipment that senses raindrops along the radar beam gives an incomplete picture, Seliga's radar reveals both the size and density of raindrops. This improves the estimates of rainfall and now enables meteorologists to predict floods sooner and with greater accuracy.

Salt Tolerant Plants

In the early 1960s, NSF supported research at several universities on how plants selectively absorb chemically similar minerals. By tracing the movements of radioactively "tagged" elements in the root zone, the scientists hoped to learn more about the mechanism that regulates movement, or exclusion, of metal ions across plant membranes. The plants used in such experiments normally grow in salty water. They included mangroves, which form dense masses of vegetation and tangled roots in tropical coastal lowlands.

Researchers hoped to discover what properties of root membranes permit plants to absorb potassium (an essential plant nutrient, required in large amounts) and at the same time reject sodium (one of the elements of common table salt, usually harmful to plants), even in soils with high sodium content. They also explored genetic aspects of the varying salt-tolerance of different species of plants.

By the end of the decade, research had shown that even closely related plants may differ in their tolerance of high concentrations of sodium. It was also demonstrated that these differences are directly related to the plants' absorption or exclusion of sodium.

It is this connection that a practical application of the research entered the picture. Many soils in the western United States, including New Mexico, and in other arid or semi-arid regions throughout the world are saline, that is, they contain so much salt that crops are adversely affected. On the basis of their research experience in plant physiology, some investigators, especially Emanuel Epstein at the University of California, Davis, reasoned that it might be possible to select and breed crops that would be tolerant of much higher concentrations of salt than present day varieties.

Recognizing the possible applications that could result from this line of research, the Commerce Department began supporting further experiments to test the crop irrigation potential of saline water. Initially, Epstein and his colleagues produced strains of barley irrigated only with seawater. Later they were able to produce a strain of tomatoes irrigated with 70 percent seawater. Epstein has also produced strains of wheat that grew to maturity even through irrigated with undiluted seawater. If scientists can create salt tolerant crops--and the prospects are promising--much of the world's saline, marginal land and brackish waters could be used in a much more productive manner.

Recombinant DNA

In the early 1950s, Max Delbruch, a physicist at Cal Tech, began to study mechanisms underlying genetic recombination in bacteria. This research helped set off an explosion that may well lead to the most important gains in medicine, agriculture, energy, and indeed, water management in this century. Delbruch's research group concentrated their efforts on phages--viruses that live in bacteria. A phage can attach itself to a simple bacterium cell, shuck off its own protein coat, and infilterate the host cell somewhat like the material in a syringe entering a vein. Once inside a cell, a phage takes charge of the cell's chemical activity and reproduces itself exactly. In less than half an hour, one phase produces many decendents and ruptures the bacterium, freeing the new phages to repeat the process. This led to more advanced studies that showed if two similar but different phages infect a bacterium, their progeny contain a high proportion of phages that have exchanged genetic material, DNA.

From these rudimentary beginnings came remarkable advances in the knowledge of the structure and function of the units of heredity and contributed to the rise of molecular genetics. In the late 1960s and 1970s, the expansion of knowledge in molecular genetics proceeded by quantum leaps. Aided by modern instruments, studies in the field of recombinant DNA have brought us to the threshold of applications undreamed of in the early 1950s.

Recombinant DNA work has implications for converting agricultural wastes and other wastes into low cost fuel. Already several human protein hormones have been produced by inserting human genetic material into bacteria. World food production stands to increase dramatically when crops can be genetically engineered to fix their own nitrogen from air and when plants can be engineered to live in arid climates using brackish water. The potential of genetic engineering is what led to the establishment of the Plant Genetic Engineering Laboratory at New Mexico State University and similar laboratories around the country.

Removal of Toxic Metal Ions From Water

The final example I would like to share with you comes from research in my own laboratory. More than 20 years ago my colleagues and I began studying how metal ions were involved in biological systems. It was known that many of the so called heavy metal ions such as iron, zinc, and copper, are necessary nutrients for living systems. Many of these metal ions were known to be associated with proteins, but the actual manner in which they were bound and the way they functioned in enzyme action was unknown. Through our work, as well as the work of many others, we now know much of the chemistry involved when different metal ions interact with proteins or other biological molecules.

Two or three years ago it was brought to our attention that heavy metal ions seem to have a high affinity for the cell walls of algae. Further investigation revealed that the binding of metal ions to algal cells is not dependent upon a living organism and that metal ions are bound with different affinities to the algal cell depending upon the acidity of the medium. From an understanding of how metal ions interact with other biological materials and with financial support from the New Mexico Water Resources Research Institute and the U.S. Department of the Interior, we have now been able to devise schemes by which different metal ions can be selectively absorbed and recovered from waters contaminated with heavy metal ions. Very recently, we have been able to immobilize algal cells by encapsulating them in a silica gel polymer. This material constitutes what we call an algae filter. We hope that these filters can be used on a commercial basis to remove and selectively recover a variety of toxic metal ions that may be found in industrial waste waters. It is also possible that the process will be used in the mining industry.

The Common Thread

The common thread running through all four examples is that very basic research preceded the practical application by 20-30 years, and at the time the basic research was being done, there was no practical application for it. In fact, "Studying the Shape of a Raindrop" sounds very much like the type of research that would earn Senator Proxmire's "Golden Fleece" award.

The fundamental difference between applied research and basic research is that applied research can be channeled whereas basic research cannot. Wernher von Braun once defined basic research as "What I'm doing when I don't know what I'm doing." Basic research generally begins with a single investigator or a small group of investigators setting out to attempt to understand the unknown. The plans of investigation remain flexible and the work proceeds in an atmosphere of high uncertainty. The basic facts at hand can only be suggestive enough to allow for imagining and guessing. Hypotheses must be set up for testing, but it is understood that many of these will be proved wrong. Sometimes an idea emerges from what can only be called intuition; and when the mind producing the idea is imaginative and very lucky, the whole field moves forward with a quantum jump. Applied research then results from using basic research to solve an immediate problem. However, without basic research there can be no applied research.

Unfortunately, public opinion about long-range problems rarely crystallizes into a sense of urgency. The average American, like his political representative, has a high discount rate concerning the future. The problem is typified by comments made by John Sawhill, formerly the Federal Energy administrator, when discussing the energy problem: "The president can't introduce a program until people are ready to support it, and the people won't be ready until they are in a crisis situation. Once we are in a crisis, we can shape a crash program to deal with it." He continues, "I believe in the efficacy of crash programs. It's only when you marshal all your talents and resources on a crash basis that you get good hard results."

He is right if the prerequisite basic research has been done, and all that is needed is to marshal technological forces. This was basically what happened in NASA's manned moon landing program. The basic research had been done previous to the decision to land a man on the moon. However, I submit that in dealing with the energy problems, our water problems, or our toxic waste cleanup problems, this attitude is extremely short sighted. And in view of the progress that has been made in the last few years in some of these areas, this approach can be most charitably described as trying to produce a baby in one month by putting nine men on the job.

Now let me come back to the question I first posed, "What are the future trends in research efforts aimed at <u>successfully</u> managing our water resources?" <u>Successfully</u> is the key word here. I believe support of fundamental basic research is imperative to endure the successful mamagement of water resources in the future. As many governmental agencies are pressured to "target-direct" their resources, we must not forget that without basic research, then applied research founders, and without applied research, the society we know cannot grow. Water pollution problems, water management problems, water conservation problems will not be solved.

There is no question that fundamental science pays for itself, and returns to society both cultural enrichments and continued resources for enhancing the quality of life. Moreover, given the stresses on our water resources and on the environment that the future appears to hold, an adequate base of fundamental knowledge on which to build a technological response becomes a matter of survival. One can safely assume that for all these reasons, the ideas of fundamental basic research as a national trust is both sound and profitable.