CHAPTER 7

GOING FROM REACTIVE TO PREDICTIVE POLLUTION ASSESSMENT

Working with perishable organisms has some advantages. When I returned from a field survey (Cairns 1948, 1949) for the Academy of Natural Sciences Philadelphia, the work was essentially finished. The species lists were complete, and alterations could not be made because the material was perishable. I could identify a few additional organisms from sketches I had made or, in later years, from photographs, but this activity was a relatively minor time commitment compared with the large numbers of specimens still to be identified by the other field team members. After a few years (but before acquiring the PhD), I was given some administrative responsibilities as field crew chief (Ruth Patrick designed the surveys) and for organizing the final report, meeting deadlines, and such activities.

Nevertheless, I had "free" time academically. I had minored in chemistry and physics as an undergraduate at Swarthmore College and had taken a number of graduate physiology courses. As a consequence, when Patrick was asked by various corporate executives to do toxicity tests as well as river surveys, she felt I was a prime candidate for this assignment. Money was not available at that time to hire a full-time environmental toxicologist, even had one been available. I was even more fortunate in that W. B. Hart, with his colleagues P. Doudoroff and J. Greenbank, had produced a toxicity testing manual in 1945 that used blue gill sunfish as the test species. This manual was, arguably, the first standardized methodology in this field. More important to my career was the fact that W. B. Hart regularly advised Patrick on promising areas of research.

As I reflect on the Hart, Doudoroff, and Greenbank (1945) publication from the vantage point of many years later, I can hardly grasp that this toxicity testing methodology was developed and published when it was. World War II was over in 1945, but the troops were still coming home. Of course, providing housing for returning soldiers and meeting the pent-up demand for automobiles and other items that had been scarce during the war were occupying everyone's time. More astonishing is that the research for the toxicity testing manual (as I recall, about 300 pages) was carried out while the war was in progress. Hart had the vision for such methodology and was sufficiently persuasive with the Atlantic Refining Company to gain support for the research. Had the research not been published when it was and had I not inherited the equipment used in the research, doubtless my career would have been greatly different. Belatedly, I am honoring Hart for his vision and Doudoroff and Greenbank for their essential contributions to the publication. My copy of the report was almost worn out when it was lost during my move to the University of Kansas. Had I realized how precious it would eventually become as a historic document, I would have made every effort to preserve it.

Moreover, Hart was available to show me how toxicity tests were carried out, and I received all the equipment that had been used at the Atlantic Refining Research Laboratory to carry out this pioneering toxicological study. Toxicity research, with only single species to start, was beneficial because it focused my attention on developing a predictive capability (the river surveys were reactive) for pollution control and on an important source of stress on riverine communities. Another benefit, of course, was to determine that an industrial waste or compound was toxic before it entered natural systems (predictive) rather than after harm had been done (reactive). The research also enabled toxicologists to determine at what levels no-observable deleterious effects occurred. Testing also provided a rare link between laboratory and field studies. All these features and benefits were useful to my career and also were an unexpected source of much aggravation (e.g., spills of toxic substances into natural systems often occurred on weekends and holidays).

I believed that a strong conceptual link existed between toxicity testing of industrial waste effluents before they entered an aquatic ecosystem and the pollution effects that I was observing in the system itself. Professionals who were most interested in the concepts that interested me (e.g., pollution effects in both laboratory and field) were predominantly engineers (then called sanitary engineers; now called environmental engineers) and water chemists. What I regarded as an interrelated, unified spectrum of information was fragmented into laboratory and field studies in those days. They were rarely connected and, worse yet, were studied by professionals far removed from my discipline of biology. Thus, I was faced with three aggravating situations: (1) my interests drew me away from the discipline of biology into associations with engineers and chemists because most biologists were not interested in pollution effects at that time, (2) even for biologists interested in pollution effects, a dichotomy existed between laboratory and field investigators, which still exists to some degree today, and (3) speaking out about the pollution effects upon both society and natural systems had severe penalties, as fictionalized in Ibsen's 1882 play "The Enemy of the People" (Fjelde 1965) and in real life by the saga of Carson's going public with information on the consequences of pollution trends that had mostly been restricted previously to professional journals.

This lesson (speaking out) was quite astonishing to a young investigator in environmental pollution. Being a contrarian could damage one's personal life and end a career in a nightmare. Even if one were correct, one could die before being vindicated! Although I never had the honor of meeting Carson, I may have a better understanding near the end of my own career of how she felt. Worse than the personal abuse and criticism is the feeling that perhaps I remained comparatively silent about issues that will affect both present and future generations. The academic community will always have "dissenters for hire," even to oppose ideas accepted by mainstream science. The general public often has difficulty distinguishing between genuine dissent (which strengthens science) and hired dissent (voiced by recipients of large consulting fees). However, professionals can make the distinction, and time will correct misrepresentations. Despair is in order only if young investigators begin to admire and emulate the hired dissenters.

In the 1950s, concern arose over heated wastewater discharges from steam electric power plants. The nuclear power age had not yet begun, but substantial amounts of water were used to cool the partially spent steam generated by fossil fuels from turbines that generated electric power. The question surfaced about what this thermal addition (some industrial people referred to it as thermal enrichment) would do to natural systems. As an avid "catch and release" flycaster, I could not resist the opportunity to research this question since fish are sensitive to temperature changes.

Both toxicity testing and field work with thermal discharges (Cairns 1966a) had a dramatic effect on my career because both were of interest to engineers. I received an invitation from Don Bloodgood, who was then heading the annual, prestigious Purdue University Industrial Waste Conferences, to present a plenary session address at the Tenth Industrial Waste Conference. The address was subsequently printed in much more detail (Cairns 1956a,b). This invitation was a blessing because I become acquainted with the engineers who were interested in these problems and I gained a publication outlet of many years. At that time, the classical biology journals were not interested in either toxicity testing or thermal discharge research. When I submitted articles, I was generally informed that they did not fit the interest of the readers of the biology journal. If the manuscript reached the review stage of the journal, the reviewers were generally not competent in either of these areas. As a consequence, I attended the Purdue Industrial Waste Conference every year for over a decade because my research was accepted there. The toxicity testing research was also, ironically, published in *Notulae Naturae* (Nature Notes) of the Academy of Natural Sciences of Philadelphia where I was then employed. My papers were enormously different from the almost entirely systematic or taxonomic articles that appeared in *Notulae Naturae*. However, I was an assistant curator of limnology and a staff member and, therefore, was entitled to publish in the journal. Since most engineers and others interested in industrial wastes did not read *Notulae Naturae*, I purchased large numbers of reprints and mailed them to colleagues with similar interests. This distribution was moderately effective, given the small numbers of professionals carrying out water pollution research in those days. Although the conclusions were a shock to those enamored of simplicity, the results were no surprise to anyone who had actually done some research in this area. Both regulators and industry favored tests with absolute values determined by widely accepted standardized methods that were understood and accepted by all. The legal system wanted (and still does want) such research to be simple and easily understood.

As a child, I was taught to revere the judicial system and the rule of law. As an adult, I became fascinated with the process of science. I was shocked when I began to realize how alien the thought processes of each system were to each other. Scientists, accustomed to probabilistic determinations based on evidence that can usually be validated in some way were in contrast to the justice system, where uncertainty was often used as a shield to protect the guilty. Innocent until proven guilty beyond a reasonable doubt is certainly appropriate for individuals! However, this concept should not apply to persistent, potentially toxic chemicals that may adversely affect ecosystems. As Homer remarked in the *Iliad*, "Once harm has been done, even a fool can understand it!"

The inadequacy of the judicial system to protect transitional zones, such as wetlands, has been particularly apparent. The extraordinary ecological value of wetlands as part of the hydrologic cycle has been recognized by mainstream science for decades. The judicial system still views wetlands primarily as isolated fragments of property. This discrepancy is why, in the last part of my career, I became enamored of ethics as superior to the formal law in coping with environmental problems. The impeachment trial of American President Clinton showed clearly how disenchanted the American public was with hair-splitting lawyers. Is it time for the public to take control in order to protect the planet's life support system?

Despite the misgivings just expressed, some means of determining the reliability of scientific evidence is needed. Since ecological situations differ, often dramatically, from one location to another, an effective way of establishing confidence limits for professionals and organizations is needed. An important first step in this process is to have an unknown sample analyzed by specific standard methods by an individual or organization. Steps must be taken to ensure that the sample is not "farmed out." So, despite my misgivings about prescriptive approaches to environmental problem solving, "standard methods" did appear to have some utility. As so often happens, things did not work out as I expected.

The Hart, Doudoroff, and Greenbank (1945) methodology was converted to a standard test method for the American Society for Testing and Materials (ASTM) by Roy F. Weston, Howard Baker, and me. A standardized test has the purpose of dictating precise details for any testing procedure. Such prescriptive methods always reduce environmental realism dramatically. However, these methods also increase the credibility of the results in courts and hearings. Not only are test fish confined in a small container, with no mud, plants, or other organisms, but the environmental conditions, such as dissolved oxygen concentration and temperature, are maintained at constant values. Standard methods are in stark contrast to natural systems where variations are often dramatic. As a consequence, I began to wonder what would happen to toxicity testing results using fish if numerous characteristics, such as dissolved oxygen concentration, temperature, water hardness, and bluegill sunfish body size, deviated from those in the standard methods. Also, since the standard test was 96 hours in length, I considered dramatically increasing the time period. What then would be the effects on the response to toxicants? Illustrative examples of such changes and resulting effects were published in the 1950s and 1960s (Cairns 1957a,b, Cairns and Scheier 1957, 1958a,b, 1959, 1962, 1963).

During the period just mentioned, I also considered the response of other organisms to potential toxicants, such as to detergents, which were then a cause for great concern for a variety of reasons. Research (e.g., Cairns et al. 1963) showed clearly, and not unexpectedly, that different trophic levels had different responses to toxicants and that fish were not always the most sensitive species. However, at that time, I was still testing each species in isolation from others. The quest for replicability overrode my concern for lack of environmental realism initially.

In the first phases of this research, death was used as an end point for fish and invertebrates, and reduction in rate of reproduction was used for diatoms and other microorganisms. Obvious questions concerned what physiological changes might occur prior to death (Cairns and Scheier 1964a) and how exposures of different time lengths would affect the correspondence of short- and long-term exposure (Cairns and Scheier 1964b). Evidence had already been gathered that detergents had effects on both tissues and behavior, as well as other attributes, at concentrations well below those that were considered safe (Foster et al. 1966, Scheier and Cairns 1966). Despite my pride in the fact that biologists now had a role in water pollution assessment, I had serious misgivings about the reliability of the predictions based on short-term laboratory tests low in environmental realism, especially standardized tests (Cairns 1967a,b,c).

Following these new perceptions, inevitable steps forward were to focus on water quality standards and the management of aquatic ecosystems. Both were major steps in enlarging my perspective in terms of information and in terms of the scale of the problem as well (Cairns 1967c,d). Episodic events, such as heavy suspended solid loads caused by floods or even from industrial discharges, were also an interesting consideration (Cairns 1965a,b, 1968). Instead of assessing long-term effects, research in this area focused on how much a species could endure during intense but short-term exposures.

In the mid-1960s, the government and some professional societies became concerned about water pollution and the survival requirements of organisms inhabiting natural systems. Also, concerns were expressed about the question of communication between industry and regulators, between scientists and practitioners, and between scientists and the general public. All these areas became a matter of concern to me, and I ventured into what one might call the underlying rationale for water pollution assessment, as well as the problems of communicating among disciplines and professions (Cairns 1965c,d,e,f). Venturing into the public arena with articles in the *Scientist and Citizen* (Cairns 1967b), the regulatory arena (Cairns 1965c), and the interaction of disciplines, such as biology and engineering (Cairns 1966b), was a major step for me. I was amazed at how well the engineers received a biologist and how tolerant they were of my initial lack of understanding of their field. Possibly, my just making the attempt at that time was enough. I read their articles in various journals and their books, attended their meetings, and listened to engineering presentations. At the same time, I became aware that the pressures on regulatory professionals were quite different than the pressures on the industrial personnel and academicians. Regulatory people could, and frequently did, end up in court defending their views. Many of them were not inclined to take risks that might threaten their jobs and income. This stance was quite understandable since the same situation is true of many in the academic community.

Literature Cited

Cairns, J., Jr. 1948. A study of the effects of pollution upon the protozoan population of a stream. MS thesis, University of Pennsylvania, Philadelphia, PA.

Cairns, J., Jr. 1949. The free-living Protozoa of the Conestoga Creek Basin. Pages 1-110 in R. Patrick, and H. R. Roberts, eds. Biological Survey of the Conestoga Basin and Observations on the West Branch Brandywine Creek. Academy of Natural Sciences of Philadelphia.

Cairns, J., Jr. 1956a. Effects of heat on fish. Ind. Wastes 1(5): 180-183.

Cairns, J., Jr. 1956b. Effect of increased temperatures on aquatic organisms. Ind. Wastes 1(4): 150-152.

Cairns, J., Jr. 1957a. Environment and time in fish toxicity. Ind. Wastes 2(1):1-5.

Cairns, J., Jr. 1957b. The effects of periodic low oxygen upon the toxicity of various chemicals to aquatic organisms. Proc. 12th Ind. Waste Conf., Purdue Univ. Eng. Bull. 94:165-176.

Cairns, J., Jr. 1965a. The environmental requirements of freshwater Protozoa. Biological Problems in Water Pollution, Third Seminar 1962, PHS Publ. No. 999-WP-25, pp. 48-52, Abst. pp. 385-386.

Cairns, J., Jr. 1965b. The Protozoa of the Conestoga basin. Not. Nat. Acad. Nat. Sci. Phila. 375:1-14.

Cairns, J., Jr. 1965c. A biological yardstick for industrial pollution. Ind. Water Eng. 2(6):10-13, 30.

Cairns, J., Jr. 1965d. Discussion of "Communications --sound and unsound-- on problems of pollution." Assoc. Southeast. Biol. Bull. 12(4):85, 87.

Cairns, J., Jr. 1965e. The environmental requirements of freshwater Protozoa. Biological Problems in Water Pollution, This Seminar, 1962. PHS Publ. No. 999-WP-25, pp. 48-52, Abst. pp. 385-386.

Cairns, J., Jr. 1965f. Pollution's eternal triangle. Assoc. Southeast. Biol. Bull. 12(2):35-37.

Cairns, J., Jr. 1966a. Biological concepts and industrial waste disposal problems. Proc. 20th Ind. Waste Conf., Purdue Univ. Eng. Bull. Ext. Ser. 118:49-59.
Cairns, J. Jr. 1966b. The Protozoa of the Potomac River from Point of Rocks to White Ferry. Not. Nat. Acad. Nat. Sci. Phila. 387:1-11. Plus 43 pages supporting data deposited as document No. 8902 with the AID Aux. Pub. Proj. Photoduplicate. Serv., Library of Congress.
Cairns, J., Jr. 1967a. Don't be half safe --the current revolution in bioassay techniques. Proc. 21st Ind. Waste Conf., Purdue Univ. Eng. Bull. 121(1):559-567.
Cairns, J., Jr. 1967b. Living with our natural water systems. Scientist and Citizen 9(2):28-33.
Cairns, J., Jr. 1967c. The relationship between national, state and regional water quality standards. Pages 13-14 in Proc. Am. Fish. Soc. 97th Annual Meeting.
Cairns, J., Jr. 1967d. The use of quality control techniques in the management of aquatic ecosystems. Water Resour. Bull. 3(4):47-53.
Cairns, J., Jr. 1968. Suspended solids standards for the protection of aquatic organisms. Proc. 22nd Ind. Waste Conf., Purdue Univ. Eng. Bull. 129(1):16-27.
Cairns, J., Jr. and A. Scheier. 1957. The effects of temperature and hardness of water upon the toxicity of zinc to the common bluegill (Lepomis macrochirus Raf.). Not. Nat. Acad. Nat. Sci. Phila. 299:1-12.
Cairns, J., Jr. and A. Scheier. 1958a. The effects of temperature and hardness of water upon the toxicity of zinc to the pond snail, Physa heterostropha (Say). Not. Nat. Acad. Nat. Sci. Phila. 308:1-11.
Cairns, J., Jr. and A. Scheier. 1958b. The relationship of bluegill sunfish body size to the acute toxicity of some common chemicals. Ind. Wastes 3(5):126-129.
Cairns, J., Jr. and A. Scheier. 1959. The effects of temperature and hardness of water upon the toxicity of potassium dichromate to the common bluegill sunfish. Trans. Northeast Wildl. Conf. 1:86-98.
Cairns, J., Jr. and A. Scheier. 1962. The effects of temperature and hardness of water upon the toxicity of naphthenic acids to the common bluegill (Lepomis macrochirus Raf.) and the pond snail (Physa heterostropha Say). Not. Nat. Acad. Nat. Sci. Phila. 353-11. [sic]
Cairns, J., Jr. and A. Scheier. 1963. The acute and chronic effects of standard sodium alkyl benzene sulfonate upon the pumpkinseed sunfish, Lepomis gibbnosus (Linn.) and the bluegill sunfish (L. macrochirus Raf.) Proc. 17th Ind. Waste Conf., Purdue Univ. Eng. Bull. 112:14-28.
Cairns, J., Jr. and A. Scheier. 1964a. Blood corpuscle count, hemoglobin concentration, and oxygen consumption values for the pumpkinseed sunfish (Lepomis gibbous Linn.). Not. Nat. Acad. Nat. Sci. Phila. 366:1-4.
Cairns, J., Jr. and A. Scheier. 1964b. The effect upon the pumpkinseed sunfish (Lepomis gibbous Linn.) of chronic exposure to lethal and sublethal concentrations of dieldrin. Not. Nat. Acad. Nat. Sci. Phila. 370:1-10.
Cairns, J., Jr., A. Scheier, and N. E. Hess. 1963. The effects of alkyl benzene sulfonate on aquatic organisms. Pages 43-54 in Proc. Fourth Annual Symposium on Industrial Waste Control. The Johns Hopkins University and the State of Maryland.
Fjelde, Rolf. 1965. The Complete Major Prose Plays of Henrik Ibsen. An Enemy of the People, completed in 1882. Pp. 281-386. McGraw-Hill Myerson Ltd., Toronto, Canada
Foster, N. R., A. Scheier, and J. Cairns, Jr. 1966. Effects of ABS on feeding behavior of flagfish, Jordanella floridae. Trans. Am. Fish. Soc. 95(1):109-110.
Hart, W. B.; Doudoroff, P; and Greenbank, J. 1945. The evaluation of the Toxicity of Industrial Wastes, Chemicals and Other Substances to Fresh Water Fished. Philadelphia: Waste Control Laboratory, Atlantic Refining Co., Philadelphia, PA.
Scheier, A. and J. Cairns, Jr. 1966. Persistence of gill damage in Lepomis gibbous following a brief exposure to alkyl benzene sulfonate. Not. Nat. Acad. Nat. Sci. Phila. 391:1-7.