CHAPTER 5

THE JOYS OF BEING ON A TEAM OF SCIENTISTS

For most of my tenure at the Academy of National Sciences Philadelphia (ANSP), I had administrative charge of the river survey crew on field trips. Ruth Patrick designed the sampling programs, picked station locations, oversaw the overall design and extramural funding, and, especially in the early days, frequently collected diatoms and other algae. I made arrangements with motels, shipped equipment, and the like. Of course, I did not have the detailed taxonomic and ecological knowledge of each of the groups that the specialists on the team had. Typically, the team consisted of an algologist, an entomologist, an invertebrate zoologist, a protozoologist (generally me), an ichthyologist, and a water chemist, who also did minimal bacterial counts such as total counts, coliform counts, etc. A field assistant also helped set fishnets and other sampling devices for fish and collected fish. The entire crew collaborated on dredging, which was a labor-intensive activity. During most of my time at ANSP, I identified protozoans; in the 1950s and early 1960s, I also collected fish when no ichthyologist was available. The camaraderie of the team was exceptional, and no one showered or changed from field clothes until the entire team had returned to the motel. Since the chemist and I worked with highly perishable samples, we were always the first to finish in the field. Sometimes I checked the fishnets and other sampling devices, such as hoop nets, before collecting protozoan samples. The entire field crew would collaborate on seining and other fish collecting activities that required substantial numbers of people.

The aquatic toxicology program was continuous since Arthur (Art) Scheier, a chemist, was never in the field and could work continuously on toxicity testing. Art and I worked together for many years and published numerous articles together. The collaboration was perfect, even though Art was only a half-time employee in the beginning. He also had a degree in optometry but was just getting a practice started. Even after he became established, we continued to work together because we enjoyed the research. When I left the Academy in 1966, Art took over the toxicity program. Before Art died, I had the pleasure of seeing him again when his son married the daughter of our friends in Blacksburg.

Although toxicity tests could be carried out by one person when the tests involved only one species and short time spans in containers low in environmental realism, we worked as a team as the number of species gradually increased to communities and both the complexity and time span increased. Our major interest was primarily in complex systems, which are best studied by a team. The river surveys were conducted in different parts of the contiguous United States, in Canada, and occasionally elsewhere, such as the Amazon River in South America. Generally, these surveys were pre-construction surveys for industrial plant sites. One early employer of the river survey team was E. I. du Pont de Nemours & Company. Just after the completion of the Conestoga and Brandywine River surveys (which brought me to ANSP), the two field crews, totaling some 35 persons, were "downsized" (to use current jargon) to just a few of the original crew members: Charles Wurtz, John Wallace, Jackson Ward, Thomas Dolan V, and me.

At that time, Crawford Greenwaldt was chief executive officer of du Pont and was also a member of the board of trustees of ANSP. His hobby was photographing and studying hummingbirds, which is undoubtedly at least one of the reasons why a captain of a major industry served on a board of an essentially biological organization. When Patrick told Greenwaldt what the Limnology Department was planning, he decided in favor of a baseline pre-operational river survey before each new du Pont plant began operating. The biological conditions, as well as the water chemistry conditions, would be known before the du Pont plant had an opportunity to affect them. The design of the surveys included reference or control stations upstream of the proposed plant site, a second sampling station just below the area where the engineers predicted the waste discharge would be fairly well mixed with the river water, a third sampling station downstream in an area where an oxygen sag might be expected to occur if organic waste were involved, and, finally, one or more delimiting stations well below the area where any effects were likely to occur. Generally, a simulated plant waste was used for toxicity testing, which, together with other evidence on predictive rates of waste transformation and the like, could be used to set the delimiting stations. This experience in river surveys heightened my awareness of the connection between laboratory predictive models and the response of complex natural systems. The survey team returned after the plant became operational to decide if the stations had been set properly. The team also observed any effects that might be attributable to events other than the plant's operations, such as channel dredging. For very important sites, such as the Savannah River, Sam Roback and I often did several "mini-surveys" a year to determine if either insects or protozoans showed any indication of deleterious effects.

What worried me about the sampling were the long intervals between surveys, even if mini-surveys decreased the intervals. Patrick reduced this concern by developing the Catherwood Diatometer, which was placed permanently in the river at various sampling stations. Slides were collected and shipped to ANSP at weekly or greater intervals to investigate any appreciable changes in diatom community structure. This design was a vast improvement. However, lag time was still significant because shipping took a few days, even by air, and the analysis of the slide could take several days to a week. Although river surveys done in the late 1940s and early 1950s were predominantly for plants under construction or to be built, surveys inevitably began to be conducted on a significant number of existing plants.

I treasure the field period and realize it was a necessary precursor to my most productive research years as measured by both quality and quantity of publications. The most valuable part was sharing ideas with and learning from other team members, which constitutes an incredibly valuable experience—an experience not easily obtained in any other way. Although I benefited enormously from the skillful and patient guidance of Robert Enders while I was an undergraduate, I did not have the opportunity to compare my performance in depth with others in my peer group due to lack of interaction with them (discussed earlier). My first opportunity to make a comparison of my performance to others was service on one of Patrick's two limnological field teams while I was still a MS candidate. Within my field team, and compared with most members of the other field team, I felt I was doing an acceptable job. I had less experience than virtually all the other taxonomists, especially Dr. Mary Gojdics, the protozoologist on the other field team. Consequently, I was fixed on my responsibilities. Later correspondence with Herbert W. Levi, Professor Emeritus at Harvard University, has really startled me. He remembered that evenings and weekends were free for the crew to play tennis, to go home weekends if they lived in Philadelphia, or to explore the Lancaster County area. I hasten to add that Patrick, with responsibilities as a professional algologist and also head of the entire operation, was in the laboratory at least as much as I was on the evenings and weekends. It is probably fortunate that I did not realize how much I was playing catch up with the rest of the crew, despite the fact that I was working with perishable samples and their strict time deadline. I gained confidence in my academic ability that I had never had before and probably could not have acquired easily under other circumstances, even in graduate school. Equally important was the fact that I had managed the transition from an isolated individual to a member of a team.

Some people worked with highly perishable material (Mary Gojdics, the bacteriologist, the chemist, and me), and some with material that could be stored for enormous periods of time before identification was carried out (especially diatoms used by Patrick and John Wallace). Some people had to sort and preserve their collections, and even make preliminary identifications. Consequently, a gradient of time was allocated for both collection and identification, which depended on the individual group. I knew that I had to start new collections within 48 hours. If I did not, some specimens would have died or reproduced, thus distorting the community sample. Some species I worked with were very fragile and low in numbers. Preservation would have eliminated valuable characteristics, such as a contractile vacuole, so I had to use speed identification with drawings when the keys did not provide enlightenment. In some cases, I had to be satisfied with the generic name without identifying to species because the community structure was of major importance to my investigation. I needed the number of different kinds of species, with some attention to abundance, rather than extremely precise identifications for each component species. Fortunately, training in rapid aircraft identification during World War II prepared me to gather multiple attributes or characters simultaneously rather than sequentially, as one is tempted to do with a dichotomous key. Nevertheless, team needs had a major influence on the way I approached problem solving.

The team was studying a large system and, even though I was only studying a component of the large system, the work still provided a system perspective. The entire project was financed with extramural funding, which gave me my first exposure in depth to that important area of my future professional life. Also beneficial was my being able to participate in the completion of a final report suitable in condensed form for publication in a professional journal (e.g., *The Proceedings of the Academy of Natural Sciences of Philadelphia*), an experience that was to benefit me greatly throughout my career. In a very real sense, being on a team provided a means of self-evaluation that would have otherwise not been easily possible while, at the same time, enlarging my perspective enormously. I learned a great deal through observing Patrick lead two teams at one time.

Money has always been important to most scientists for purchasing equipment; providing salaries for technicians, graduate students, and hourly employees; and ensuring that one had some degree of control over personal time management (Isaac Asimov is reported to have replied, when asked to define academic freedom, "extramural funding"). The ability to see the connection between one's own area of interest and other parts of mainstream science, as well as society, increases substantially the probability of acquiring extramural funding. For example, Patrick saw the connection between kinds and abundance of diatoms and other species of aquatic organisms to water pollution, convinced others (both academics and corporate executives) of its utility as an index of pollution, and was able to obtain substantial extramural finding for a long and productive academic career. Additionally, she was able to give others, including me, a start on academic careers with this funding. She also saw the connection between applied and theoretical research—primarily that a good research project design could include elements equally useful in both areas. Funding not available to individual specialists was available to teams of specialists focused on a unifying theme. Ironically, to learn the factors leading to a significant degree of academic independence required sacrificing that independence temporarily.

One also sees quite a variety of personality types on field teams. Some members like to sleep late (in terms of field teams, to 7 or 8 a.m.). These same individuals may cheerfully work till 1 or 2 a.m. on a regular basis. On the other hand, some field crew members like to get out on the river soon after sunup, especially in summer when this time is the coolest part of the day. These individuals generally retire early. Some people are not particularly sensitive to local customs. Since the crew often depended on local citizens for information, supplies, help with repairing motors, etc., individualism had to be redirected sometimes so that it did not have deleterious effects on team acceptance locally. I remember one situation where a male crew member wore purple toreador pants to a restaurant in Allendale, South Carolina, which in the early 1950s was a sleepy town on US 301. Although Patrick directed almost every operation at the outset of the Conestoga River Survey, I gradually acquired, as the department grew, other non-academic responsibilities for making arrangements on field surveys for such things as motel rooms. None of us on the team could replace any others in the areas of specialty, but some things I did could have been done by anyone on the team who was willing to do so. Somehow I gradually acquired more and more administrative responsibilities. This management duty expanded my perspective, but I also incurred a price in loss of time for academic matters, such as preparing manuscripts for publication, etc.

The downside of serving on a field team is quite apparent: (1) the pace and direction are set by the group, (2) the perception of others that field team members cannot function well as individuals, (3) time away from home is too much for many individuals (being part of team is not the only way to be stressed in this way; research projects near or far can take an individual away from home), (4) the level of physical stamina is difficult, but not impossible, to maintain in the later part of one's professional career (individual field work can also be very demanding, but one may have more options to reduce stress when working alone that one has as a team member; mid-course adjustments are always easier for an individual than a team), (5) the opportunity to attend many important professional meetings is reduced because of conflict with team schedules, and (6) more dependence on the performance of others than does "so called" independent research (which still generally involves others). Some of my colleagues participated in team investigations for their entire professional careers, but they were notable exceptions. It is possible to do individual research and also be on a team, but it is backbreaking work in terms of the effort required. One also hopes to have a satisfactory family life as well. It was clear that this was possible since I observed Patrick managing this tricky time management problem. The toll for even the successful individual was clearly a cause for concern. I managed to maintain this team schedule for 19 years (1948–1966). Did I spend too much time on this balancing act? I'll never be certain!

A major problem of working in teams is keeping the balance. Small teams of two or three people, who work together reasonably harmoniously, are in an entirely different category than teams of 14 or more. As the group increases in size, responsibility to it diminishes for many individuals, and vulnerability that results from individual negligence or lack of responsibility increases dramatically. On the other hand, the cash flow that maintains research momentum is definitely enhanced by team funding, which permits activities to continue that otherwise might be interrupted. Everything considered, it seems to me that being on a team increases the diversity of experiences and capabilities of an individual and, thus, leads to the sort of stability in extramural funding and associated activities that diversification in a stock index does. This association also ensures that one cannot achieve the excellence that comes from totally independent research that is continuously funded.

Today, a scientific team is often a collection of individuals who are temporarily assembled for a specific task. Possibly the best known scientific team is the one that assembled the atom bomb during World War II. However, the teams most familiar to American citizens are baseball and football teams and, in other countries, teams such as cricket, basketball, and soccer. When I was a boy in the 1920s and 1930s, sports teams were relatively stable because players remained on one team for perhaps their entire careers. Teams of that era also had a loyal following and, presumably, were also loyal to the area that supported them. Today, players have enormous mobility, and entire teams disregard loyalty to the area and change geographic locations depending on facilities and contracts offered. I raise these issues, despite the fact that I am not a knowledgeable sports fan, because some of them parallel situations in the academic world. I have struggled with some of these issues (with modest success) throughout my career, but remain ambivalent to this day, as the following brief discussion shows.

When I joined the two river survey teams in summer of 1948, both were led by a single person (Ruth Patrick) and shared a chemist (Jackson Ward) and a bacteriologist (Raymond Smith). Although composed primarily of specialists in various subdivisions of limnology and aquatic biology (see Appendix 1 in Chapter 1), the teams had a common objective. Their individual efforts, while important in their own area of specialization, were only notable in terms of the grant if the results could be integrated effectively and conclusions drawn from a synthesis of all the individual efforts. The teams suffered from turnover of personnel. Patrick and I were the only two members still employed from the original 1948 survey crews when I left ANSP in 1966. However, many original field team members remained my colleagues for a decade or longer. Even so, substantive personal and professional adjustments were necessary each time a new team member arrived. A team with extended relationships among members has a much better chance of achieving information synthesis than a recently assembled team. The danger is that the team may become too satisfied with a particular approach—too rigid both individually and collectively. The dangers of a standardized approach are dramatically increased if competition introduces "efficiency" (i.e., cutting costs) as a major factor. Teams financed with "soft money" are particularly vulnerable to this danger.

During my last year at ANSP, the pollution problems being assessed required a much greater mix of disciplines than were available on a fixed composition team. Furthermore, each problem was sufficiently unique to require a somewhat different array of team members. Teams became more diverse in disciplinary composition and the research teams were newly structured for each problem.

In the early 1960s, both Dr. Charles Reimer (a field team member) and I were offered faculty positions by a midwestern university of intermediate national ranking. We were then both full curators (comparable to full professors) with tenure at ANSP. We were both offered assistant professorships without tenure by a full professor with tenure whose publication record was markedly inferior to each of ours. The rationale was twofold: (1) service in a university was so different from that in a research organization that we would be starting over and (2) our presumed rapid promotion would enable larger salary increases later than would be possible for an immediate appointment as a full professor. We both immediately declined! Soon after, I was offered a superb position as a department chair by a university enamored of interdisciplinary activities. This offer came because of my administrative experience with interdisciplinary teams. The salary was nearly half again as much as I was making, and the fringe benefits were even more impressive. I enjoyed administration, and, although it may be immodest to say so, I did it well. However, I could not envision giving up research entirely. Had my involvement with team research become a trap with the only exit labeled "administration" or "starting over" as if I had just obtained the PhD? Fortunately, it had not, but only my many hours of individual research had saved me from the stereotype of a team person.

When I went to the University of Kansas in 1966, I began working with H. W. Shirer, who had both a MD and a PhD in electrical engineering—just the person I needed to develop computer-interfaced biological early warning systems. I also worked extensively with Roger Kaesler, a paleontologist in the Geology Department, who studied community structure in ways that were of great interest to me. The three of us, together with our graduate students, formed a team whose scope was broader than the teams I was accustomed to working with, but whose total number was smaller. However, when I came to Virginia Polytechnic Institute and State University at the invitation of Robert A. Paterson, then head of the Department of Biology, my specific assignment was to form an aquatic ecology group. Since I was hired in a research position, I had time to devote to first identifying research problems and then acquiring the funding to investigate them. As might be expected for that era, most members of the aquatic ecology group were in their first professional position after acquiring the PhD. Extramural grants provided research assistantships for graduate students and money for technicians, equipment, travel, and the like. The system was quite successful for about a decade, with over 60 graduate students and 7 faculty members at its peak, although one faculty member flatly refused to work on teams. The aquatic ecology group was understandably modeled after the ANSP group, but redesigned to fit a university situation with graduate students. The aquatic ecology group was a team only in the sense that the group was able to acquire funds not readily available to young PhDs. Problems I was called upon to consider required a more diverse group of disciplines than I had been part of earlier, and the mix of disciplines was different for each particular problem. In order to compete for this extramural funding, I quickly started adding members of other disciplines such as engineers, statisticians, geologists, and even psychologists and historians. This diversity necessitated approaching not only other departments and colleges but also sometimes other institutions.

A comprehensive university with substantial extramural funding cannot escape some administrative problems, such as distribution of overhead monies generated by team research. Therefore, after two years at Virginia Polytechnic Institute and State University, Leslie Malpass (Academic Vice-president) approached me about forming an interdisciplinary center for environmental studies. As director, I would have the authority to negotiate directly with faculty members and their department heads and could escape the time consuming chain-of-command procedures. The tremendous advantage of this system was that I could gather the mix of specialists needed to solve a particular problem. One disadvantage was that many of the individual faculty members had probably never worked on a multidimensional team before and were unaccustomed to exchanging information with other disciplines. Most had never faced the problem of synthesizing information from a variety of disciplines. Another disadvantage was the availability of faculty members to their home departments. If the individual were participating on a team and not fully involved in the department, this divided loyalty was often viewed with great suspicion by some department heads and colleagues. Funding for research and graduate students and equipment for research helped dispel or dampen some fears. Also, team members had access to a huge data pool, which would not have been available to a lone investigator. Increased data and information strengthened many publications. Nevertheless, time management problems still existed since many team members were usually carrying a full department load in addition to the team activities. Also, some teams grew rather large and unwieldy, the largest being 51 faculty members from 14 departments in 3 colleges, plus associated graduate and undergraduate students. Even the small teams required tracking for accountability because some sponsors required monthly reports and all required quarterly reports. This aspect of accountability would not have been possible had I not had Kenneth L. Dickson as my assistant director; he excelled in this type of activity.

Multidimensional individuals are now carrying out many of the problem solving activities that once required a small team; regrettably, many universities have gotten on the interdisciplinary bandwagon when the parade was nearly over. Transdisciplinarity is now increasingly common. Both top-down and bottom-up strategies are essential to resolve global and other large systems issues (Cairns 2003). My primary point is that universities and other institutions should consider interdisciplinary activities as an essential step toward transdisciplinarity rather than as an endpoint. I started with a modest mixture of disciplines because interdisciplinary activities were then quite rare; then I moved beyond that level. Even that level of complexity was a major challenge to a novice. At present, over a half century later, I am still seeking transdisciplinarity. Even my former students will probably spend their professional careers on this quest.

In the late 1980s and early 1990s, state-supported universities came under fire because faculty members were perceived to be ignoring teaching responsibilities to ensure research success. Since most of the money I acquired in extramural funding supported student research, I have always felt that my research was part of teaching. The research experience was, in fact, an invaluable experience for the transition of many students to both academic and non-academic positions. However, this evaluation of the co-existence of research and teaching is not a general perception among legislators and the public. This misperception is quite understandable since universities did not make as great an effort to demonstrate the benefits of this co-existence as, in retrospect, they should have. My own belief is that the interdisciplinary team, which is becoming increasingly unwieldy and difficult to operate in a university, should be replaced, to some degree, by consulting firms and research organizations or by multidimensional people with some experience in more than one of what used to be classic, isolated disciplines. The Worldwatch Organization, founded by Lester Brown, is a good example of individuals carrying out a synthesis on such problems as transportation, feeding China, and other issues. The word *interdisciplinary* implies that the classical disciplines will still dominate, despite the fact that they often are hampered by jargon only understood by a relatively few people. Some disciplines still rely on artificial constraints on the boundaries established, which are the construct of the discipline for quality control and are not necessarily easy to surmount for solving the complex, multivariate problems that the world faces today. Therefore, multidimensional people are needed to replace the old construct of interdisciplinary teams, and then some of the problems associated with an aggregation of individuals not accustomed to working together will be resolved. However, both faculty and students need to get their training somewhere. Naturally, part of that will be outside of educational institutions, but, ideally, the most important learning experience will be in them.

Experiencing An Institutional Paradigm Shift

Institutions also experience paradigm shifts. Before Patrick formed the limnological survey team, the ANSP research staff was entirely world-class systematists. They were specialists who published almost entirely within their specialty. This research continued uninterrupted, and both groups benefited from the presence of the other. Predictably, frictions arose between the two groups, but the institutional paradigm shift of adding a new approach while maintaining the well established research activities succeeded.

Thomas Dolan, an original Conestoga/Brandywine survey crew member, has been most helpful in checking my recollection of the early team period. Others, now deceased, were John M. Ward (chemist, was part-time) and Charles B. Wurtz (malacologist). Donald Reihard, Jr. was a bacteriologist and was half time. John H. Wallace (algologist) continued to be associated with the group but was pursuing a graduate degree at the University of Pennsylvania full time. Stuart S. Bamforth (laboratory assistant), Hazel D. Barner (laboratory assistant), Edward Haldeman (laboratory assistant), and James F. Bergseng (field assistant) were also employed until December 31, 1948, although all but Jim Bergseng were part-time. The two crews carrying out the Conestoga/Brandywine river surveys were headquartered at Franklin and Marshall College in Lancaster, Pennsylvania. Neither Tom Dolan nor I recall having any substantive interaction with personnel at ANSP during that summer, nor does Herbert Levi. We, of course, knew what ANSP was like, to some degree, but it was not until we went to work there the following fall that we realized how different we were from the other curatorial staff. We worked as a team despite our individual specialties; other ANSP curators worked primarily alone. We were involved with pollution problems; other staff members sought areas where there were none. They were supported by long-term endowments and the like; we were on "soft money." In terms of our activities on the river survey group, we were all taxonomists, although our use of the information was quite different from the other curatorial staff at ANSP who named new species and constructed new taxonomic hierarchies. Our information was used in ways that affected industrial and societal decisions or, at least, was intended to.

Many of the curatorial staff at ANSP historically had other professions (e.g., Joseph Leidy), and many who were employed when I first went to work there were in endowed chairs or independently wealthy. The source of funding for the river surveys during the summer of 1948 was the Sanitary Water Board of the Commonwealth of Pennsylvania, definitely a unique source of funds in ANSP's history. The dichotomy in both professional activities and sources of funding became even greater when the grant from the Commonwealth of Pennsylvania ended and we began carrying out investigations for industrial firms, such as E. I. du Pont de Nemours & Company and the Commonwealth of Virginia. Had the extramural funding for the Limnology Department ceased, we would have been without salaries because ANSP did not have reserves for this purpose. My recollections of that period do not include the deep concern about funding that would have been quite reasonable because this was a new undertaking to which even industry was unaccustomed. At that time, the idea that biologists had a role in pollution investigations, although not new in the professional literature, was quite rare for industry where pollution studies were the realm of chemists and sanitary engineers (now environmental engineers). In one period, directly after the Commonwealth of Pennsylvania's funding ended, salaries were reduced from $3,600 per year to $3,000 per year—something I remember vividly because my wife and I had just purchased our first house. Fortunately, my mother, who died rather young, had left me enough money for a substantial down payment, and the blow was not as severe as it otherwise would have been. Additionally, extramural funding increased over the next year; our original salaries were reinstated and then subsequently increased. Clearly, financial security was not the primary concern of team members.

However, the uncertainty about the source of funding, the need to prepare quarterly and sometimes even monthly reports, and the need to send out competitive grant proposals certainly caused a great dichotomy in outlook between the well established curatorial staff and us. The other curatorial staff had a much greater level of security than we did, were much more isolated from competition for money, and, thus, could unhesitatingly embark on long-range studies while we could not. However, the enormous benefit to this situation was that, while I was still completing my graduate degrees, I was exposed to the intricacies of obtaining extramural funding, which has benefited me for my entire career.

The Guadalupe River survey was the Limnology Department's first industrial survey, and it was a thrill to the entire team! Tom Dolan remembers it vividly, despite the half century that has elapsed since then. The river survey team was applying Patrick's then well validated methodology in a very common situation during the rapid industrial development that followed World War II. The Guadalupe River flows into the Gulf of Mexico, and the team was to survey the river before completion of a new du Pont synthetic fibers plant to be located near Victoria, Texas. The team was examining a river ecologically quite different from the ones we had studied, and we were a substantial distance from the large library in Philadelphia. Field work was simpler in those days, and the survey equipment for the entire team fit in one of the then-common station wagons of that era (when I left ANSP in 1966, we were carrying tons of equipment in large trucks). Such things as boats and even outboard motors were rented, a pattern that continued for some years.

The team stayed in the Hotel Victoria in 1949, which had a lobby and some adjacent rooms that resembled a museum. We worked long hours, but fortunately in Texas, especially when construction crews were in the area, our muddy appearance did not cause any sensation when we walked through the lobby at the height of the cocktail hour. Our post-field collection investigations were carried out in a metal Quonset hut, which boomed every time the metal expanded or contracted in response to the heat of the sun. Since the Guadalupe River flowed into the Gulf of Mexico not too distant from the proposed plant site, we used a shrimp boat crew for baseline shrimp and oyster samplings. It was the beginning of my many encounters with other cultures on a working basis, which made life very interesting. I learned that local people were willing to help if I took the time to get acquainted with them. For example, our food allowance went much further because the crew of the boat boiled freshly caught shrimp on a Primus stove in a red powder called "shrimp boil," which to this day I have never found an equal for flavor, even in the most expensive restaurants.

This survey also launched a lifetime for me of explaining biodiversity, aquatic community structure, and other esoteric matters to industrial executives, engineers, regulatory personnel, and local citizens. Had I not been on a team, it is unlikely that these experiences would have been open to me.

I also had my first experience of completing toxicity tests on the simulated industrial waste streams in the ANSP laboratories in Philadelphia. I also observed and worked on the river where the effluents would be discharged. The tests were the crude, single species, short-term laboratory tests in containers low in environmental realism, but they were state-of-the-art for that period. Most important, I was forced to make the laboratory-field connection so uncommon for toxicologists and ecologists, which became a major focus of my entire career.

Quite different teams replaced the ones I first experienced. I speculate that multidimensional people will replace teams in many areas in the future. The teams on which I started my career were groups of people gathering information, at a fixed time and a fixed location, and meant to be assembled for a specific purpose. We were primarily focused on point source discharge pollution coming from an industrial or municipal pipe. We primarily counted "critters," with chemical/physical information as a background. Although we occasionally looked at entire drainage basins, only a segment of a drainage basin was usually surveyed, with five or six points of reference, including a control. This operation was expensive (despite our relatively low salaries) because of housing, per diem, and transportation in the area of the survey.

Multidimensional people, instead of interdisciplinary teams, will enjoy a number of advantages: (1) communication problems will be reduced, (2) as much time as necessary can be spent at a particular locus rather than the arbitrary time generally characteristic of teams, (3) the direction can be quickly changed by new information, and (4) exhaustive detail, when necessary, can be obtained from a specialist. The synthesis in the report is more likely to be understood by non-specialists since the person writing the report is a generalist who is knowledgeable, to a certain degree, about all the areas and, most important, cognizant of how the information will be used. While the teams on which I served were aware of how the information would be used, they did not commonly meet with the persons who would use the information. A spokesperson, generally Patrick or sometimes me, would convey the information, which is essentially what a multidimensional person would do if substituting for a team.

In this scenario, the camaraderie of a team would be gone, although a different type of camaraderie may develop between the decision makers and the persons furnishing the detailed information. While the detailed information gathered by the specialist would not be shared in the same way, a different kind of professional growth, likely more enduring, would result, since the multidimensional person could easily incorporate the information selected for the diagnosis than when it was predetermined by the specialist.

These dynamic, continuous changes have kept my interest intense for over a half century.

Reference

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